Fieldwork and Footnotes

Studies in the History of European Anthropology

Edited by

Han F. Vermeulen

and Arturo Alverez Roldán

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Fieldwork and footnotes

Doing fieldwork and writing ethnographic texts are the primary tasks for anthropological practice. What are the origins of this practice? How has anthropology evolved in the many national traditions in Europe? These studies, focused on the history of European anthropology, provide new responses to these questions, and reveal that anthropology/ethnology is much older than has been generally assumed. The editors and contributors believe that the history of anthropology is itself an anthropological problem and should be investigated as such. They provide an overview of current themes in the history of anthropology in Europe, the first such volume to appear in English.

The contributors examine a wide variety of anthropological impulses within Europe—from the seventeenth century to the late twentieth century. They explore key issues in the history of social and cultural anthropology in the UK, France, Germany, the Netherlands, Sweden, Poland, Slovenia and Romania, and in Mexico by way of the influence of Spanish anthropologists. The importance of historical figures such as Lord Monboddo on the Orang Outang; Enlightenment and Romanticism in A.Bastian; H.J.Nieboer on slavery; and Malinowski and Witkiewicz on the conceptualization of culture is discussed. The differences between anthropology, ethnography and ethnology are explored, as is the problem of modernism and postmodernism with regard to the Malinowskian revolution.

Fieldwork and Footnotes reflects the great diversity of anthropological traditions in Europe, and provides an invaluable international, comparative framework which will make the book of great interest to historians of science as well as to anthropologists and ethnologists.

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European Association of Social Anthropologists

The European Association of Social Anthropologists (EASA) was inaugurated in January 1989, in response to a widely felt need for a professional association which would represent social anthropologists in Europe and foster co-operation and interchange in teaching and research. As Europe transforms itself in the nineties, the EASA is dedicated to the renewal of the distinctive European tradition in social anthropology.

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Han F.Vermeulen
Arturo Alvarez Roldán

Introduction

The history of anthropology and Europe

Han F.Vermeulen and Arturo Alvarez Roldán

In 1962 A.Irving Hallowell stimulated a conference on the History of Anthropology sponsored by the Social Science Research Council in New York (Hymes 1962). Since then the history of anthropology has become an established field, especially in the United States, with the *History of Anthropology Newsletter* and a 'History of Anthropology' series, starting in 1973 and 1983 respectively, both edited by George W.Stocking, Jr. Although from the beginning promoters of the new subdiscipline attempted to create a single forum for historians and anthropologists (Stocking 1983), the history of anthropology has in fact increasingly become a specialty within the history of science rather than within anthropology.

In Europe the history of anthropology has not had as systematic a development as in the United States. Several books and collections have appeared, and four specialized journals covering the field are published in Europe,[1] but these are relatively new journals and the first initiatives to co-ordinate European scholars working in the field have been taken only recently. At the first conference of the European Association of Social Anthropologists (EASA), held at Coimbra, Portugal, in September 1990, a workshop on the 'History of European Anthropology' was organized for the first time.[2] Eight papers on the history of British, French, Georgian, German and Dutch anthropology were presented, three of which were published (Barnard 1992, Alvarez Roldán 1992, Herrero Pérez 1994). There was considerable interest in a conference whose participants were anthropologists, and not specialists in the history of ideas.

This interest was even greater during the second workshop of the same title, held during the second biennial EASA conference in Prague, in August 1992. During two sessions, eighteen papers and a historical film on Sir Raymond Firth were presented.[3] Of these papers, fourteen

have been selected for publication in this volume, while two other papers were published separately (Jerábek 1992, Stagl 1994). We decided to publish this selection as an EASA Monograph for two reasons. First, we wanted to prevent the dispersion of papers in journals, which militates against the emergence of the 'history of anthropology' as a domain in its own right. The other reason for publishing a selection of the papers together is that they are the results of important new research from varied countries in Europe, and reveal a diversity of research traditions and a liveliness of both these traditions and of historiographical research which is promising and stimulating.

One of the essential differences between this attempt to professionalize the history of anthropology in Europe and the attempt originating in the USA in the 1960s is that here it is practising anthropologists and not professional historians who are taking the responsibility of writing their history. It seems that anthropologists are no longer prepared to consider themselves passive subjects in a history written by others. We share with Adam Kuper (1991) the opinion that this opens new perspectives for the subdiscipline, since the practitioner may have advantages over the outsider in writing the history of anthropology.

But while the history of anthropology can have great relevance for current anthropological debates, it is also necessary to establish it as a domain of inquiry in its own right, in order to gain practitioners' confidence. It is our view that histories of anthropology can be written by and addressed to anthropologists. This should not be understood, however, as a defence on the part of anthropologists simply wishing to retain historical knowledge of their own practice within the boundaries of their discipline. The enterprise can enrich both anthropology and history, in the same way as works written by and addressed to professional historians. We hold with George Stocking to 'the ideal of a history of anthropology which is both historically sophisticated and anthropologically informed' (Stocking 1982a:xviii). Therefore we expect that this book on the history of European anthropology will be welcomed both by historians of science and by active anthropologists.

The book is divided in three main parts. The first presents studies which deal with the origins of anthropology in Europe; the second discusses individual contributions to European anthropology; and the third focuses on anthropological traditions in Europe.

The four chapters in Part I present the results of current research on crucial episodes in the development of anthropology in Europe. Michael Harbsmeier offers an overview of the prehistory of ethnography in early modern travel literature in the seventeenth and

early eighteenth centuries. Han Vermeulen traces the origins and formation of the concepts 'ethnography' and 'ethnology' in the late eighteenth century, and their institutionalization in ethnographical museums and ethnological societies in the early ninteenth century. Gheorghita Geana focuses on the discovery of the 'whole of humankind', by looking at the genesis of (social/cultural) anthropology through the Hegelian looking-glass. Klaus-Peter Koepping identifies the roots of an epistemological contradiction with which the science of humankind is still struggling—between the Enlightenment search for universal laws and the Romantic quest for particularity—in the work of Adolf Bastian of the mid-ninteenth century.

Part II presents studies on contributions made by individuals to the development of anthropology in Europe, and focuses on well-known and lesser-known figures in the history of anthropology. Alan Barnard shows the legacy which derives from one form of 'noble savage' in the Enlightenment: the *Orang Outang* as conceived by the Scottish judge Lord Monboddo. Jan de Wolf contextualizes the work of the Dutch ethnologist H.J.Nieboer on slavery (1900–10), which moves beyond evolutionism towards (early) functionalism. Peter Skalník makes a detailed analysis of the relationship between the Polish writers Malinowski and Witkiewicz in terms of the difference between science and art in the conceptualization of culture. Arturo Alvarez Roldán analyses how Malinowski invented the ethnographic method of participant observation in the early twentieth century, by making a historical comparison of the ethnographic experience of this author in Mailu and his subsequent work in the Trobriand Islands.

The chapters which make up Part III deal with the study of anthropological traditions or research programmes in various countries. The last publications on this subject appeared more than ten years ago (Diamond 1980; Gerholm and Hannerz 1982). The list of these traditions does not seek to be exhaustive. Instead, it serves to draw attention to the many-coloured picture of anthropological traditions in Europe. Tomas Gerholm applies centre/periphery concepts to the disciplines of ethnology (or folk-life studies) and anthropology in Sweden. Zmago Šmitek and Božidar Jezernik trace the lines that (cultural) anthropology of extra-European countries has followed in Slovenia, alongside the study of Slovenian folk culture. Zbigniew Jasiewicz and David Slattery outline the dynamics of the history of Polish (cultural and social) anthropology and ethnography (or folk studies) that determined the shape of ethnology in Poland. Nikola Bock discusses the reasons why historical anthropology (seen as a special branch of history) appeared so late in the landscape of science in

Germany, and explains this by drawing on the pre- and post-war history of ethnology. Hugo García Valencia deals in his chapter with the development of Latin American anthropology by Spanish anthropologists exiled in Mexico. This last part of the book is concluded by a contribution by Thomas Schippers on the history of the research traditions meeting in the anthropology of Europe today.

At least three important problems are dealt with by the authors published here. First, the origins of anthropology and the problem of periodization. Second, the problem of modernism and post-modernism in regard to the Malinowskian 'revolution'. And third, the position of ethnology and ethnography within European anthropology. We shall discuss these problems in this order.

THE ORIGINS OF ANTHROPOLOGY AND THE PROBLEM OF PERIODIZATION

It is, perhaps, symptomatic that George Stocking, the doyen of historians of anthropology, has not systematically approached the problem of periodizing the history of anthropology, which, although difficult, seems to be vital to the field. Stocking has written about specific episodes, such as classical evolutionism of the nineteenth century, the concern of his *Victorian Anthropology* (1987), but for the rest restricted himself to identifying three 'paradigmatic traditions' in the history of anthropology: the biblical or 'ethnological', the developmental or 'evolutionist' and the polygenetic or 'physical anthropological' (1990).

The question of the origins of anthropology remains unresolved because of the lack of agreement on the criteria that should be taken into account to determine the starting points of anthropological thought and ethnographic studies. These attempts have been criticized as 'presentist' (Stocking 1982b). However, criteria are necessary, since a history of anthropology without any epistemological commitment, if possible at all, would result in a blind history that creates confusion. That is partly the situation in the field: a variety of periods and circumstances under which the formation of the discipline is supposed to have taken place have been put forward. Some scholars argue that the Greeks, Romans and Arabs were the first in formalizing anthropological knowledge about human culture (Mühlmann 1948; Hymes 1974; Darnell 1974; Honigmann 1976; Palerm 1982). Other authors suggest that anthropology emerged either in the Renaissance (Cocchiara 1948; Rowe 1965; Hodgen 1964; Darnell 1974) or in the Enlightenment (Evans-Pritchard 1951 and 1981; Foucault 1966; Harris

1968; Moravia 1970; Duchet 1971; Diamond 1974; Voget 1975; Copans and Jamin 1978; Llobera 1980).[4] A third group of historians recognize the existence of anthropology only from the nineteenth century onwards when the discipline achieved professional status (Penniman 1935; Lowie 1937; Burrow 1966; Mercier 1966; Poirier 1968 and 1969; Service 1985). Next to the interest in origins, there is an increasing tendency among historians of anthropology to concentrate on 'modern' anthropology, from the early twentieth century onwards (e.g. Kuper 1977, 1983).

Pluralism may be profitable for the history of anthropology and we have attempted to preserve it in this volume. Our intention has certainly not been to favour any specific approach to the problem of the origins of anthropology, but pluralism does not necessarily mean 'eclecticism'. In order to assess the validity of each particular approach to this problem, it is necessary first to know the underlying criteria to every option. There have been at least four different approaches to the genealogical problem of the origins in the historiography of anthropology.

First, a 'problem' orientation. This approach takes as the marker for the rise of anthropology the posing of specifically anthropological problems. Anthropology is taken to have begun when certain authors framed certain questions and made some efforts to answer them. This orientation is represented in this book by Geana, Koepping and Barnard. From a Hegelian perspective, Geana states that a scientific discipline emerges only when its object becomes perceived as a whole. Following Hegel, the history of anthropology may be divided in two phases: conceptual and preconceptual. Anthropology reached its conceptual phase in the mid-nineteenth century. But first it had to surpass its preconceptual phase, in which the great anthropo-geographic discoveries between the mid-fifteenth and mid-seventeenth centuries were the most important events. The object of anthropology—humankind—was conceived as 'a whole' at the same time as the whole planet was discovered.

According to Koepping, the central problem in anthropology is its inner epistemological contradiction: the combination of cultural relativism with an aspiration to find universal explanations. The roots of this contradiction lie in two currents of thought, namely those derived from the Enlightenment and those derived from Romanticism. The work of Adolf Bastian, particularly his notions of elementary ideas *(Elementargedanken)* and of folk-ideas *(Völkergedanken),* can be seen as one of the first attempts to combine both currents and to overcome the fundamental paradox in the study of humankind.

The debate about the definition of 'man' that took place in the eighteenth century enables Barnard to reclaim Lord Monboddo as one of the founders of social anthropology. Monboddo examined the definition of the species 'man' in light of the philosophy of Aristotle, speculations on the origin of language, the existence of feral children, of travellers' tales and of scientific descriptions of the speechless race known as the *Orang Outang*. Monboddo's significance lies in 'the exploration of the relation between the categories *Man* and *Orang Outang* in terms of language, political organization, material culture and capacity for learning'. His legacy is 'a paradigm for probing the common humanity at the root of all cultures'.

Second, there is a 'conceptual' orientation to the origins of anthropology. This orientation pays attention to the formation and distribution of concepts of the discipline, as well as to its names and their transformation. Following this orientation, Vermeulen advances the thesis that anthropology in the form of ethnography and ethnology originated in the late eighteenth century when concepts were coined to represent a 'science of nations and peoples'. He observes that by the time these concepts were established in ethnological societies (1839–43) they had been given a different meaning.

Third, there is a 'professional' orientation, which stresses the importance of academic and professional institutions in establishing the discipline. This orientation is not represented in this volume, but on the contrary is criticized by some of the contributors. Harbsmeier states that travel accounts from the early modern period (the seventeenth and eighteenth centuries) constitute an important chapter in the 'prehistory of anthropology'. Such accounts often contain crucial ethnographic information, but can also be fruitfully interpreted as 'involuntary self-descriptions' of the travellers themselves and of their contemporary readers. Vermeulen points out that the period of professionalization must be seen against the background of the preceding period of 'conceptualization'.

Fourth, there is an 'epistemological' orientation. In this approach, anthropology is considered as a science that in order to exist requires that its theories and methods are accepted by a community of anthropologists. Although anthropology probably never had any 'paradigms' in the specific sense given to the term by Thomas Kuhn (1962, 1970), it might be possible to speak of the existence of 'quasiparadigms' in the history of anthropology, as opposed to Kuhnian pre-paradigms. These quasi-paradigms, comparable to Foucault's *epistèmes,* indicate the emergence of anthropology as a social science. From this point of view it makes sense to distinguish

between a history and a prehistory of anthropology, as do Harbsmeier and Vermeulen. Malinowski's development of the ethnographic method in the field and its importance to the quasi-paradigmatic main line of inquiry in anthropology is stressed by Alvarez Roldán.

Among the essays presented here, there is a consensus on the main stages that a chronological scheme of the history of anthropology should cover: a prehistory of ethnography (Harbsmeier), the origins of anthropology and ethnography in the late eighteenth century (Barnard, Vermeulen), the rebirth of anthropology in the nineteenth century (Geana, Koepping, Schippers), the constitution of modern anthropology in the early twentieth century (de Wolf, Skalník, Alvarez Roldán).

MODERNISM AND POSTMODERNISM WITH REGARD TO MALINOWSKI

Although Malinowski is probably the anthropologist to whom historians of anthropology have devoted most attention, two new contributions are included in this volume. At first sight, the chapters by Skalník and Alvarez Roldán are contradictory since the former emphasizes the importance of Malinowski's life experience to his subsequent work, and the latter denies these influences. According to Alvarez Roldán, Malinowski became one of the key figures in the development of a modern research programme in anthropology, by discovering a method *in the field*. Skalník states that such a programme was reductionist in comparison with the understanding of culture proposed by Malinowski's Polish friend Witkiewicz. Witkiewicz's conceptualization of culture evokes the well-known conception suggested by Clifford Geertz in 'thick description' (1973), and some authors have asserted that this conceptualization lies at the roots of anthropological postmodernism. It seems that here again the debate between modernism and postmodernism arises, taking Malinowski's life and work as a starting point.

The debate about postmodernism does not seem to be concluded in anthropology. In current Polish anthropology a rising interest in postmodernism, more particularly in Rorty, is observed (Jasiewicz and Slattery). 'Textualism' and 'ethnographic authority' have been two of the key issues introduced into anthropological debate by postmodernists. According to some postmodern anthropologists 'ethnographic authority' derives from the style of writing, which in the case of Malinowski was realist (Clifford 1983, 1988; Marcus and Cushman 1982; Clifford and Marcus 1986; Marcus and Fischer 1986;

Geertz 1983; Van Maanen 1988). In a recent paper Joan Bestard (1993) states that the characterization of modern ethnographies as realist texts by postmodern anthropologists can be understood only by setting up these kinds of ethnographies against postmodern ones. Bestard argues that Malinowski's monographs are a cultural product of the anti-realist modernist intellectual and artistic movement arising in Central Europe at the end of the ninteenth century. In this sense Witkiewicz's conceptualization of culture would be as modernist as Malinowski's, and Skalník points to the influence of the movement of Young Poland on both.

It does seem doubtful that the authority of Malinowski's ethnographies depends on their 'realistic' style. What then is the basis of their authority—if there is any? In his chapter, Alvarez Roldán suggests that Malinowski's ethnographic authority has to do with the epistemological concept of validity. In order to write valid ethnographies the anthropologist needs valid tools, valid data, inferences and explanations. According to Alvarez Roldán, Malinowski was one of the first anthropologists who became conscious of the validity problem and attempted to resolve it by suggesting important canons for doing ethnography (see also Sanjek 1990).

The comparison of Malinowski with Nieboer, whose comparative study of labour relations is contextualized by Jan de Wolf in this volume, reinforces Skalník's thesis that Malinowski's functionalism was not the result of his fieldwork but a theoretical framework he took to the field. By formulating general laws about the relations between land, labour and capital, Nieboer abandoned the evolutionary path of his tutor Steinmetz and adopted a functionalist perspective in analysing slavery. This proves that functionalism was not an invention of Malinowski in the Trobriand Islands, but part of the *Zeitgeist*. It also proves that Malinowski's revolution in method did not rest on his discovery of functionalism.

ANTHROPOLOGY AND ETHNOLOGY IN EUROPE

'Anthropology' is used in this volume as a general term for a group of studies including ethnology, ethnography, social and cultural anthropology, folklore studies and biological anthropology. This broad definition is necessary to do justice to the wide range of the chapters, covering aspects of the diverse anthropological traditions that have been, and in most cases still are, present in Europe. This is not meant as an attempt to define what anthropology *is,* but as an attempt to pay attention to answers that have been given in the past (see, for instance,

the definition by Kollár quoted by Vermeulen, the Hegelian definition presented by Geana, and the redefinition by Bastian quoted by Koepping). In fact, the recent definition given by David Schneider of anthropology as the study of culture defined as a system of symbols and meanings which have to do with 'ways of living life' (Schneider 1993) is a nice synopsis of discussions in Swedish and European ethnology in the mid-1930s (see Gerholm and Schippers in this volume).

In this context, a comparison of the famous 'four-fields' approach in Northern America with the approaches current in Europe is revealing. Schneider suggests that the separation of biological anthropology and archaeology from social and cultural anthropology would be 'highly desirable, perhaps even necessary'. In Europe, this situation has been the case for a long time. Physical anthropology and archaeology do not play a role in the curriculum of the humanities and/or social sciences anywhere in Europe. Therefore, in Europe the opposition is not between social and cultural anthropology on the one hand and biological anthropology and archaeology on the other (as Schneider suggests for North America), but between anthropology (be it cultural or social) and ethnology (or ethnography or 'folk studies', in whatever kind of denominations).

The four fields that Schneider mentions, however, do not correspond to the divisions set out by Boas, Powell and Brinton.[5] Particularly linguistic anthropology, so vital for the formation of ethnography in the eighteenth century, is overlooked in Schneider's scheme, whereas ethnology is equated with social and cultural anthropology. Historically, however, the rise of social anthropology in the 1920s must be seen as a reaction to ethnology as practised earlier (cf. Malinowski discussed by Skalník in this volume and Radcliffe-Brown quoted in Barnard [1992:14]).

In addition, the chapters in this book suggest that ethnology and ethnography as 'folk' or 'national' studies have remained very much alive in Europe and dominate in the Eastern parts of the continent. While social and cultural anthropology dominate in the Western, Northern and Southern parts of Europe, ethnology and ethnography are still in existence there as well, although they have been renamed several times.

In Scandinavia, particularly in Sweden, ethnology (or *folklivs-forskning*) has a strong history and remains central to the tradition of 'folk-life research' in Europe. It developed in opposition to 'general and comparative ethnography', which was practised in museums and universities. This term was abandoned in the late 1960s and early 1970s

in favour of social and cultural anthropology, but in spite of the change in terminology, anthropology in Sweden remained rather peripheral to the American, British and French 'mainland' of social and cultural anthropology (Gerholm).

In Poland, ethnography was strongly restricted to *ludoznawstwo* (or folk studies), although there were ethnographic studies of extra-European cultures which formed a link between *etnografia* and *antropologia*. The opposition between *etnografia* (folk studies) and *antropologia* was influenced by the communist 'ideologization' of Poland, which resulted in ethnology losing its independence and its transformation into 'historical and descriptive ethnography'. It was only in the mid-1970s that (social or cultural) anthropology was allowed to return to ethnology, and even more recently, after the fall of communist power, that it could be added to the title of several institutes (Jasiewicz and Slattery).

The same development occurred in Slovenia, where it was denied that there ever was a (cultural) anthropological tradition. Instead, it was supposed that in Slovenia only an ethnographical tradition exists, which was concerned with *narodznanstvo* or 'knowledge of the nation'. The chapter by Šmitek and Jezernik was written to prove otherwise. They point to a line of authors indicating that there was an anthropological tradition in Slovenia, which started in the sixteenth and seventeenth centuries with travel accounts and continued into the twentieth century until it was abandoned in favour of ethnography as a 'national science'.

The influence of Soviet ethnographers on research traditions in Eastern Europe has not been sufficiently documented yet. Although studies have been published on the beginning and the end of 'Soviet ethnography' (Slezkine 1991, Tishkov 1992), as well as volumes on the 'state of the art' of ethnology and (physical) anthropology in the Soviet Union (Bromley 1974, Gellner 1980), the vital period of Soviet influence in the 1950s and 1960s still needs to be described (but see Jerábek [1992] on the situation in the Czech region and Buhocia [1966] on the situation in Romania). It appears that the Soviets were strongly opposed to Western 'ethnology' which was seen as an imperialistic science and which was discarded in favour of an ethnography which should study both non-European and European peoples, i.e. should include 'folk studies' or *narodovedenie*. By this definition they remained faithful to the roots of ethnography as formulated in the eighteenth century.

The problem of external influences also arises in the case of historical anthropology, a branch of history which was able to establish

itself in Germany only with great difficulty and without the help of social history or ethnology. Nikola Bock observes that the terms with which the subject was disregarded, namely 'irrational', 'subjectivist' and 'total', are similar to the terms used to blame German *Volkskunde* for its involvement with National Socialism. By drawing on recent research on the rather tight relationship between the ethnological sciences (*Völkerkunde* and *Volkskunde*) in Germany and National Socialism, she argues that the opposition to historical anthropology was related to unresolved experiences. The terms used 'still seem to be frightening to German scholars working in the humanities today, dedicated as they are to preventing new "irrational" fascism by reinforcing rational and structural explanations of human behaviour and social life'.

The same subtlety in argument is displayed by García Valencia, who concentrates on the construction of social anthropology in Mexico with the help of Spanish exiled anthropologists. In a detailed discussion García Valencia compares the development of anthropology in Spain and Mexico and observes that the 'holistic model' in Mexico, derived from the tradition of anthropology and folklore developed in Spain from the second half of the nineteenth century, broke down in the 1960s to give way to 'a more specific one, namely social anthropology'.

The book is closed by an analysis of Thomas Schippers, who discusses the history of ethnology (folklore studies) vis-à-vis social and cultural anthropology in Europe between 1920 and 1980. He observes that at least three types of anthropology have been applied to the study of European societies after the Second World War: a social anthropological orientation (mainly in France and Britain), a cultural anthropological orientation (mainly in the USA), and an ethnological orientation that continued 'the variety of disciplines more or less federated within the European Ethnology project founded by Sigurd Erixon (in the 1930s)'. It would seem that these orientations have something to learn from one another's history and results, particularly when they are concerned with the same social groups and local cultures.

One of the merits of assembling data from a variety of research traditions in one continent is that changes in one tradition may be revealing for changes in another. This applies particularly to changes in terminology which suggest paradigmatic shifts 'hidden' behind them. Thus, the coining of the new terms *Ethnographie* and *Völkerkunde* around 1770 in the Germanic countries implied a shift in interest from the study of customs and *mores* to the study of nations

or national cultures; the subsequent change from a science of nations to a 'science of human races' which occurred around 1840 (Vermeulen) foreshadowed the abolition of the ethnological societies and their transformation into anthropological societies in England and France in the 1870s and 1880s (Stocking 1971). The change of name at the London School of Economics in 1927, when the Department of Ethnology was renamed Department of Anthropology (Skalník), marked the bipartition between (diffusionistic) ethnology and the social anthropology of Radcliffe-Brown and Malinowski. The rise of social anthropology, and its growing tendency to specialize on extra-European societies, then influenced the emergence of the new subject 'European Ethnology' in Sweden and in international forums around 1935/37 (Schippers, Gerholm), as an alternative to the older terms *Volkskunde* and folklore studies—and in contrast to *Völkerkunde* translated as 'non-European ethnology' (Jerábek 1992), 'foreign ethnology' (Jasiewicz and Slattery) or 'overseas anthropology' (Schippers).

After the Second World War the old and familiar term ethnology as the name of departments and curricula was traded in for social anthropology or cultural anthropology in the Netherlands (early 1950s), France (1960) and Sweden (late 1960s). In Eastern Europe in the 1950s the term ethnology was replaced by ethnography, accompanied by folkloristics. This last development was reverted only after 1989/90, when, due to the collapse of communism, departments changed their names back to 'Ethnology' (Bratislava, Slovakia) or to 'European Ethnology' (Brno, Moravia), or added the subject of 'cultural anthropology' (as in Ljubljana, Slovenia, and in Poznan, Poland).

These examples show that it is essential for historiographic purposes to pay attention to the specific terms used, as well as to changes in meaning and scope, which are often related to (paradigmatic) shifts in theory and method. This corresponds to our conviction that the history of anthropology *is* an anthropological problem and should be developed as such, by describing developments from within and by historizing and contextualizing as much as possible.

Doing fieldwork and writing ethnographic texts still seem to be the primary tasks of anthropological practice, which should be accounted for in the history of anthropology. The title of this volume *Fieldwork and Footnotes* refers to these two aspects of scholarship in their European guise (as clearly appears in the chapters of Alvarez Roldán and Schippers).

We believe it is important to record and reflect on the enormous

diversity of anthropological impulses within Europe, which have come to their present form through time and which are constantly being adapted to meet new requirements. Therefore it is vital to pay close attention to the specific terms used to designate the field(s), to changes in terminology, as well as to changes in the functions that these approaches have in the societies in which they are being developed. We invite readers of this book—practising anthropologists as well as historians of science—to discover this *diversity* and *specificity* in the following chapters on the history of anthropology in Europe.

NOTES

1 *History and Anthropology* (London 1984), *Gradhiva: Revue d'histoire et d'archives de l'anthropologie* (Paris 1986), *History of the Human Sciences* (London 1988) and *Boletín de Historia de la Antropología* (La Laguna, Spain 1988).

2 The workshop in Coimbra was organized by Fernando Estévez González (La Laguna, Spain) and Arturo Alvarez Roldán (Granada, Spain). A call for papers was published in the very first *EASA Newsletter,* no. 1 (October 1989), p. 8.

3 A report on the workshop in Prague was published in *EASA Newsletter,* no. 8, October 1992, pp. 10–11 and in the *History of Anthropology Newsletter,* XIX (2), December 1992, p. 21. A direct result of the sessions in Prague was the establishment of the 'History of European Anthropology Network' (HEAN), an EASA network with the aim of organizing meetings and of facilitating communication between scholars working on the subject. The secretarial address of HEAN is: Dr Jan de Wolf, Department of Cultural Anthropology, Utrecht University, PO Box 80.140, 3508 TC Utrecht, The Netherlands. At the third biennial EASA conference in Oslo (June 1994) a workshop on the history of European anthropology was held for the third time, this time under the auspices of HEAN.

4 The French volumes edited by Rupp-Eisenreich (1984) and Blanckaert (1985) contain contributions which fall into both categories.

5 See the discussions on the four-fields approach in *Anthropology Newsletter,* October 1992–January 1993.

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Part I

The origins of anthropology in Europe

1 Towards a prehistory of ethnography

Early modern German travel writing as traditions of knowledge

Michael Harbsmeier

> Sein Famulus, ein Theolog, der Philosophie mit Theologie nicht zu vereinbaren wußte, fragte Kant einst um Rat, was er wohl deshalb lesen müßte?—Kant: Lesen Sie Reisebeschreibungen. Famulus: In der Dogmatik kommen Sachen vor, die ich nicht begreife. Kant: Lesen Sie Reisebeschreibungen.[1]

BEGINNINGS

Neither the handbooks in the history of anthropology nor the increasing number of specialists in the field seem to agree on the question of the historical origins of the very discipline they study. Many textbooks give students the impression that social and cultural anthropology originated in the second half of the nineteenth century, when several evolutionist scholars began to explore 'primitive culture' rather than, as their immediate predecessors did, studying 'inferior races'. According to Marvin Harris's *The Rise of Anthropological Theory* (1968) among others, the Enlightenment of the eighteenth century provided, at its earliest stage, the comparative and 'developmentalist' theoretical framework deemed fundamental for all later developments as a model. For example, Guiseppe Cocchiara, in his history of ethnological theories entitled *The Myth of the Noble Savage* (1948), turns to Montaigne and the Renaissance as the point of departure. Still others—among them Han Vermeulen in this volume, and to some extent myself (Harbsmeier 1989, 1992b)—have had a closer look at the historical development and *Begriffsgeschichte* of the very names *Völkerkunde* and 'ethnography' and their equivalents, ending up with Göttingen and the last decades of the eighteeth century as the crucial formative period for the discipline.

Others again—including, for example, Wilhelm Mühlmann (1948)

and Francesco Remotti (1990)—are less inclined to put exclusive stress on the institutional set-up with chairs, academic institutions and associations characteristic of the late nineteenth century, the theoretical *problématique* inherited from the Enlightenment, the scepticism and self-criticism of the Renaissance, or on the very words describing the discipline invented in the German academic *Spätaufklärung*. Ethnography, the art of describing 'other' cultures and societies on the basis of some kind of eye-witness observation, appears to be much older than many historians of anthropology seem prepared to admit. Even the most superficial accounts of the discipline's prehistory cannot deny that very few historians, theologians or philosophers have been able, even in the most remote past, to avoid thinking seriously about how differently people in other parts of the world tend to live.

A prehistory of anthropology, that is a history of ethnographic writing and anthropological thinking prior to the academic institutionalization of *Völkerkunde* and ethnology, would thus seem an almost impossible task. Observations of other people and 'theoretical' reflections on the importance of such observations are surely just as old as human culture itself. Linking these two dimensions by making empirical evidence relevant for general and theoretical issues, as well as allowing the experiences to serve as a 'test' for the theory, surely has changed dramatically over time—in Europe as well as in any of the other great literate traditions and civilizations.

In a general, maximalist sense of the term, a prehistory of anthropology should take into account all the variations of linking statements about 'other' forms of human life and existence to a deepened understanding of one's own. From a more modest and much more Eurocentric point of view, however, the crucial issue seems to be the (Weberian) question: what modes of linking theory to practice, observation to generalization, experience of 'otherness' to understanding selfhood did immediately precede and finally lead to the successful establishment of something like *Völkerkunde* or ethnology as institutions specialized in academic learning and teaching based on disciplined, scholarly research? This is the question that I attempt to answer in this chapter.

TRADITIONS OF KNOWLEDGE

Ethnographic performances, acts of telling stories about how 'real' life is different elsewhere in the world (or the other world), are a widespread phenomenon in human history. Institutionalized forms of

recognizing the value and importance of such performances, through literary genres such as travel accounts, for example, can, however, be found only in some literate civilizations. Linking such performances, moreover, to other more important forms of public articulations of the social order, such as philosophical and political discourse as articulated in ethnographic museums or world exhibitions, is a specialty of Europe and the West.

Trying to understand the events cumulatively making up anthropology's prehistory, can lead us to many places indeed. Making literate or cultural capital out of one's personal experience with 'the other' has been particularly profitable in post-Columbian (perhaps even post-Marco-Polian) Europe. Trying to analyse and understand the innumerable ethnographic and intercultural performances, acts and investments, which actually did succeed in reshaping the world from Marco Polo or Columbus onwards to the beginnings of anthropology proper with Schlözer, Gatterer, Meiners and the German *Spätaufklärung* at the University of Göttingen, is in itself an anthropological task.

As Fredrik Barth has repeatedly insisted, there should be no difference between how anthropologists deal with their ancestors and predecessors, on the one hand, and how we treat those people anthropologists study as 'objects', on the other. Barth criticized Edmund Leach for his structuralist approach to other cultures, a contrast to his historicist attitude towards his predecessors and his own tradition. Analysing variations in cosmology among closely related groups in Highland New Guinea, Barth rightly insists on the processual character common to cosmological, ethnographic and anthropological performances. He argues they all should be analysed as social processes, which only sometimes—and for reasons analytically to be proven rather than taken for granted—happen to crystallize into 'theories', 'models', 'structures', 'cultures', 'societies' or even 'worlds'. He alludes to Adam Kuper's *Anthropology and Anthropologists: The Modern British School,* when Barth writes that he would have liked to call his book about ritual variations in Highland New Guinea *Cosmology and Cosmologists—the Modern Ok School.*[2]

Following Barth, this chapter also could have been called 'Ethnography and ethnographers—the early modern German school'. I will attempt to analyse the ethnographic performances of early modern German travellers as social processes embedded in specific contexts, as traditions of knowledge. They can tell us a great deal about the cultures performing them—and perhaps also a bit about the nature of ethnographic knowledge in general.

The best *comparative* work on a European basis on the history of early modern ethnography and anthropological discourse is doubtless Giuliano Gliozzi's (1977) *Adam and the New World. The Birth of Anthropology as Colonial Ideology: from biblical genealogies to racial theories (1500–1700)*. Gliozzi has focused rather narrowly on a critical exposition of the various theories and hypotheses about the origins of the inhabitants of the New World. This enables him to cover broadly all the Spanish and Portuguese, Dutch and German, French and Italian, Scandinavian and English debates and discussions through more than two centuries. In what follows, I shall concentrate on developments in Germany only, but try to take a broader look at the issues involved in analysing the social fabric of early modern ethnographic performances.

TRAVELLING, READING AND TEACHING

In the German *Spätaufklärung,* not only was the general public preoccupied with reading travel accounts, but also the university professors were. They were the first to use the words *Völkerkunde* and ethnography as labels in their lectures and textbooks (Fischer 1970; Vermeulen 1993 and this volume). The many collections of travels, histories of mankind, universal histories and world histories written by an astonishingly large number of both amateur and university scholars during the last decades of the eighteenth century can all be seen as attempts at comprehending the overwhelming amount of information provided by contemporary travellers of all professions and nationalities (Marino 1975; Harbsmeier 1989).

The late eighteenth century was characterized by a well-developed division of labour between, on the one hand, the travellers providing first-hand information and, on the other, a large number of both professional and amateur collectors and readers processing this information into various kinds of publication—lectures, books, articles. But as Han Vermeulen has rightly pointed out only the systematic efforts to orchestrate this division of labour, only the series of exploratory expeditions inaugurated by the Russian Imperial Academy of Sciences in St Petersburg (founded 1724), finally led, albeit indirectly, to the first occurrence of *Völkerkunde* and 'ethnography' as a name for a new academic discipline.

Describing the customs of the different peoples of the vast and still expanding Russian empire was far from the main concern of the instructions issued by Tsar Peter I and his successors. Yet many of the participants of the great Russian expeditions did publish extensive

ethnographies in their accounts and reports to the Academy. Peter Simon Pallas, today considered the most important of these explorers, had devoted so much attention to ethnographic issues in his *Reisen durch verschiedene Provinzen des Russischen Reichs* (printed in three volumes from 1771 to 1776) that he also could publish no less than three volumes of extracts exclusively dealing with the 'peculiarities' *(Merkwürdigkeiten)* of the various peoples and nations he had encountered on his voyage (1773, 1777a, 1777b).

Seen as a group, the international participants of the great Russian expeditions distinguished themselves from other contemporary traveller-explorers by a much higher level of academic training and disciplinary specialization. Comparable in many respects to the French and English voyages around the world headed by Bougainville and Captain Cook, the expeditions over land organized by the Russian Academy were characterized by a much closer integration of imperial (geo-) political goals and ambitions and purely scholarly and academic purposes. Mapping, analysing and describing as systematically and completely as possible all the natural and human resources to be found and exploited inside one and the same empire required a more effective co-ordination of tasks and available energies than the exploration of some unknown distant islands in competition with other European powers.

Georg Forster's *A Voyage Round the World,* published in English only one year after the last volume of Pallas's *Reisen durch verschiedene Provinzen des Russischen Reichs,* also contained large amounts of ethnographic data on different peoples and nations. These data are an integral part of the narrative and could hardly have been published separately. Forster's readers could be found largely outside the strictly academic circles where systematic observations of natural and human resources could become a basis for training future generations of an imperial ruling class. If Pallas can be seen as an example for the professionalization of ethnography, Forster represents an earlier stage of the process—much more in line with the majority of the travel accounts of his time.

As a hothouse for the production of knowledge, the Russian Imperial Academy did produce at least one decisive condition for the professionalization of ethnography: the subordination of travels and travellers to the institutionalized needs of an expanding empire. Looking at this professionalization's prehistory, we can find similar attempts to subordinate travels and travellers under forms of academic institutions.[3] All characteristically failed due to the absence of a

framework as powerful in linking methods to observations and theory to practice as the Russian Imperial Academy.

But St Petersburg and its academy were far from the only point of reference for the historians at the University of Göttingen trying in their lectures and textbooks to make systematic sense of the masses of information provided by other less trained and specialized travellers and explorers of the time. Christoph Meiners for example, Professor of *Weltweisheit* in Göttingen from 1772 to 1810 and author of an amazingly large number of books and textbooks in several disciplines, seems to have spent most of the time he was not writing, reading travel accounts (for details see Harbsmeier 1989). And in his *Grundriß der Geschichte der Menschheit* we find yet another very explicit attempt at establishing ethnography as a new academic discipline, even though Meiners—apparently in conscious opposition to his colleagues Gatterer and Schlözer—insisted on calling it 'history of mankind'.

Trying to define his new science, Meiners describes it in contrast to universal history: while universal history deals with chains of real events, Meiners's new ethnography deals with what man 'has been or now still is' *('was er war oder noch jetzt ist')*. Universal history deals with men, events and peoples important for the sufferings and wellbeing of large numbers of people, while Meiners's history of mankind pays respect to 'the savages and barbarians of all continents, who did not have the slightest noticeable effect on the fate of humanity' *('die Wilden und Barbaren aller Erdtheile, die in den Schicksalen des ganzen Menschengeschlechts nicht die geringste bemerkbare Veränderung hervorgebracht haben')*. Universal history sticks to a strictly chronological order, while Meiners's new science deals with 'nations, deeds and events infinitely separated from one another through time and space' (Meiners 1785:21–3).

As presented in his *Grundriß,* Meiners's new science mainly consisted in systematically extracting any ethnographic information from the huge number of travel accounts available at his university's library. The bibliography at the end of his book contains a complete list of more than 500 entries referred to in the copious notes at the end of each chapter. Meiners tried to compensate for his travellers' poor training and specialization compared to those sent out and instructed by the Russian Imperial Academy, by exponentially increasing the sheer number of his sources, by his notorious *Belesenheit*. Whatever we may think of the merits of his new science, Christoph Meiners's way of dealing with travel accounts surely was more characteristic of his time than the rather advanced and innovative methods advocated by some of

his more respectable colleagues (for more details see Harbsmeier 1989, 1992b).

Yet a third approach to the problem of making travellers and travel writers fit the needs and schemes of academic and theoretical teaching and presentation was also tried out in Göttingen only a few years earlier, when Johann David Michaelis, Professor of Oriental Languages, gave substantial advice to a Danish expedition to Arabia Felix. Michaelis's *Fragen an eine Gesellschaft gelehrter Manner, die auf Befehl Ihro Majestät des Königes von Dännemark nach Arabien reisen* (1762) was by far the most comprehensive text of its kind at that time. Apart from including a short list of questions issued by the *Académie des inscriptions et de belles lettres,* Michaelis not only reproduced the instructions authorized by the Danish king, but also formulated one hundred questions himself. Michaelis's questions mostly deal with medical issues, with all sorts of disease and bodily disorders, with natural history and innumerable species of plants, animals and other natural phenomena, and with a few oriental habits and customs. All of this was closely related to Michaelis's fundamental interest in the study of the Old Testament.

Michaelis admits that his exclusive concern with questions raised by reading and translating the scriptures might seem excessively theological. According to Michaelis, however, the Bible deserves attention not only as the basis for 'all our religion', but also by forcing upon us innumerable problems of natural history, ethnography *(Sitten der Morgenländer)* and geography. Based upon the Bible, many of Michaelis's questions had Hebrew terms as their titles. Philology entered into his kind of natural history much more than in other questionnaires and travel instructions of the eighteenth century. Rather than directly addressing issues of geography, astronomy, botany, zoology and medicine, Michaelis wanted his expedition to engage in what today would be called ethno-medicine, ethno-botany, ethno-zoology, etc. The list of birds for example, dealt with for some fifty pages in the final question, only examines what he calls 'unclean' and 'forbidden' specimens. To Michaelis, the Bible, and especially the Old Testament, served as a looking-glass through which to address the issues that other contemporary authors of questionnaires and instructions for scientific expeditions confronted directly.

Most explicitly this shows up in Michaelis's astonishingly few strictly ethnographic questions. In the official instructions, only polygamy and its demographic effects are singled out among the habits and customs to be studied, and only six of Michaelis's questions were explicitly devoted to 'customs and habits': sexual intercourse during

periods of menstruation; male and female circumcision and methods of castration; signs of virginity preserved after marriage; and questions about ritual spitting, the meaning of taking off one's shoes, and leviratical marriages.

As the only expedition member to survive and return, Carsten Niebuhr, a surveyor by training, did his best to answer Michaelis's detailed instructions in the first of his extensive travel accounts. This account became famous for its detailed description of the ruins of Persepolis. Michaelis's attempt, however, at making the Danish expedition into an exercise of biblical studies was only a partial success. His treatment becomes yet another apt example of the difficulties involved in subordinating travelling observers' primary experiences to academic needs characterizing this late stage in the prehistory of ethnography as a discipline (for a more detailed analysis see Harbsmeier 1992a).

During the eighteenth century, the number of travellers specially trained for purposes of scientific observation and registration in fields such as geography, natural history and ethnography slowly increased to make up a very tiny, but important, minority. Travelling inside Europe had for a long time been the privilege of a well-educated elite—and the fate of an illiterate multitude of people forced to move. Only the first could possibly have written and published accounts of their travels—and did so to an increasing extent. From the first decades of the eighteenth century onwards, some *Hofmeisters* began to publish books about their experiences as supervisors of the Grand Tour of young noblemen entrusted to them, thus providing rich material for a (south) European *Völkerkunde,* that never was established as a discipline.

As we shall see shortly, travels and travel writing beyond the confines of Christian Europe through the preceding century had, in contrast, been an almost exclusive privilege of notoriously under-privileged and uprooted men who were forced by social and economic circumstances to sell their labour-power to foreign agencies such as the Dutch East Indian Company. In spite of their background, many of these travellers have left written records of their overseas adventures, most published with the active assistance of some more learned editor. During the early eighteenth century, this pattern also changed when a few academically trained Germans began to take part in European overseas expansion and to write about their observations.

Carl Friedrich Behrens for example, whose *Reise durch die Südländer und um die Welt* from 1737 was reprinted as *Der wohlversuchte Südländer* only one year later, had taken part in

Roggeveen's voyage round the world from 1721 to 1722 as 'Sergeant oder Commandeur von der Militz' before settling down as a baker. The ethnographic parts of his accounts are in many respects very similar to those of his less learned predecessors. However, Behrens not only claims to have visited the island of Robinson Crusoe and Friday, but also discusses very critically the methods of 'discovery' adopted by the Dutch on earlier voyages (Behrens 1737:111–41). He dwells extensively on the mysterious inscriptions on Mount Adam allegedly written in the language spoken before the Tower of Babel.[4]

A similar academic obsession with historical and philological issues can be found in Philipp Johann Tabbert's comprehensive, historical-geographical account of 'the Northern and Eastern parts of Europe and Asia', which he published under the pseudonym Philipp Johann von Strahlenberg in Stockholm in 1730. Having spent several years as a prisoner of war in Siberia, Tabbert did not content himself with a narrative of his captivity, producing instead a learned treatise starting with more than a hundred pages introductory *Forschungsbericht* (Robel 1987:225) and including a *tabula polyglotta* of no less than thirty-two different 'Tatarian' languages, a vocabulary of Kalmuk phrases, a great map, as well as a series of copperplates featuring 'Scythian-Asiatic' antiquities. Even without instructions from any imperial academy, Tabbert managed to produce a clearly professional and academic piece of travel writing.

The contrast with the less learned predecessors is even more conspicuous in the third of our examples of early academic travel writing: Johann Gottlieb Worms's *Ostindian- und persianische Reisen* from 1745. Feeling unsatisfied by the mere narrative of Worms's adventures as employee of the Dutch East Indian Company, the editor of his account felt it necessary to include a description of 'all Dutch colonies and conquests' from other sources. And in a supplement on the uses of oriental travels, the learned editor dwelt extensively on how observations on the manners and customs at the 'Theatrum aller biblischen Geschichte' might help readers to understand better the 'Oriental and therefore figurative and hieroglyphic' messages of the Old Testament (Worms 1745:1000).

None of the three last-mentioned travellers had any impact on later developments in the prehistory of ethnography. All of them, however, represent an important stage in the professionalization of travelling observers in the first decades of the eighteenth century. But it was only through the efforts of professors such as Michaelis and Meiners and, finally, the Russian Imperial Academy that the observation of the

manners and customs of other peoples and nations became a discipline of specialized teaching and training.

COMPANIES AND COSMOLOGIES

In the seventeenth century the genre itself appears to have prospered in the hands of those numerous travellers who, due to the effects of endemic wars and serious economic and social decline in the German-speaking territories, had to leave home to sell their souls and labour-power to the Dutch East Indian Company (VOC) founded in 1601. Signing a contract with the VOC, these travellers not only received a job and a salary for some years, but also had the chance to earn some fame by publishing their overseas adventures, if they returned.

On coming home, most of these travellers still did not have much more to live on than what they were paid for while abroad: their labour-power. Trying—often desperately—to discover a way of getting reintegrated into the social network, surprisingly many of them found that writing and publishing a book about their voyage could be a great help in re-establishing themselves as part of the social order from which they had been away. Having received only a very rudimentary education at school, most had to rely on active help from somebody more learned and influential to act properly as the author of a book. Most of their accounts, thus, should be read as the result of a combined effort of the generally poorly educated and untrained traveller and someone culturally more competent—a sponsor, editor and adviser. Their books still offer a unique possibility for studying the (proto-) ethnographic performances of a series of non- or pre-professional observers of other cultures and societies.

At first glance, the corpus of German *East and West Indian Voyages* of the seventeenth and early eighteenth centuries[5] appears as an extremely heterogeneous collection of first-person narratives about various voyages taken as a sequence of events, on the one hand, and an even more chaotic series of ethnographic descriptions of all sorts of 'other' cultures and societies, on the other. Focusing on the 'static-descriptive' passages spread throughout these texts,[6] I shall try in two steps to show that these descriptions can tell us a great deal about the art of ethnographic description prior to its professionalization.

FROM THE CAPE OF GOOD HOPE TO BATAVIA

The ‘Hottentots’, whom almost all the travellers deemed worth a more detailed description at the beginning or at the end of their accounts, were invariably portrayed as the most abhorrent and disgusting people under the sun, devoid of all signs of civility, propriety and humanity, lacking both human language, cooking, housing, religion and political institutions. Because of this, they had an important function for the texts describing them: the descriptions guided the traveller as well as his readers from the world at home to those out there, no longer on religious grounds, but on the more properly ethnographic basis of various inventories of what was deemed essential for a civilized as opposed to savage or barbarian forms of life.

Of course the early descriptions of the ‘Hottentots’ could be taken as a distorted image of the people today called the Khoi-Khoi of southern Africa and their culture. Due to the observers’ obsessive concentration on the body and bodily performances of the Hottentot, and the fact that most of these observers moreover claimed the Hottentot not even to speak a recognizably human language, the results of such an attempt at ethno-historical reconstruction and recovery predictably would be rather meagre—and themselves distorted and misleading.

More rewarding, I think, would be the attempt to understand the image of the Hottentot as a distorted picture of the travellers themselves. Having left home to sell their labour-power to the company’s *Seelenverkäufer* in Amsterdam, to one of the first major non-military and non-religious, purely profit-orientated multinational corporations in history, these travellers had themselves as little to call their own as had the so-called Hottentots. Most of the travellers left no other written traces of their life than the account of their voyage, and as a rule they had to find the support of somebody else in their efforts to get an account of their travels published. Telling and writing about their adventures on their return was for them a way of getting reaggregated into the society which they had left in a very literal sense. By claiming themselves to have seen, with their own eyes, a people utterly lacking all the basic assets for a respectable human and social existence out there in southern Africa they could claim at least some respectability for themselves at home.

At the other end of the spectrum of people and places described by almost all authors of East Indian voyages in the seventeenth and early eighteenth centuries we find Batavia, now called Jakarta, the capital of the Company in Java. The opposite of the Cape of Good Hope in almost every respect, Batavia was invariably described as a Utopian

place: a city more wealthy and more beautiful than the finest cities in Europe (which most of the observers never had the chance to see), a city governed and ruled more righteously than any German or European town, and—most important perhaps—a city inhabited by a bewildering multitude of different peoples and nations. Many of the observers contented themselves with enumerating the various nations represented in Batavia, but most of them also indulged in lengthy descriptions of the Chinese and their obsession with hair-dressing, all kinds of games and theatre, and the Javanese and their occasional running amok through the streets.

The multiculturalism and *Völkervielfalt* of Batavia, the peaceful coexistence of so many different nations and peoples under the same, righteous government of the Dutch East Indian Company, was as prominent a theme in the German East Indian travelogues as the Hottentots. These descriptions of Batavia surely can teach us about the ethnography of an early colonial city, yet perhaps they make even more sense when interpreted as microcosmic representations of the ideal world of wealth and beauty to which the poor German travellers hoped to get access by selling their souls to the Dutch East Indian Company. Having seen and observed this truly wonderful world contributed to letting the travellers themselves have a share of that world—at least in the eyes of their contemporary audience and readers.

The few examples discussed so far already suggest that the ethnographic descriptions, like digressions woven into the narrative of the voyage itself, are fundamentally incomplete and fragmentary because only the most remarkable and curious features get mentioned. Reading the accounts of these travellers is like entering one of the contemporary *Kunst- und Wunderkammern* or cabinets of curiosity, which also bring together strange and striking, but always incomplete bits and pieces of natural history and ethnography. There is no aim to give a complete and rounded picture of any of the other worlds, of which all these bits and pieces are taken as signs (see, e.g., Pomian 1988; Lugli 1983). To understand the ethnographic information contained in travel narratives, the modern reader, and especially the anthropologist, almost automatically feels forced to do exactly what was done when these early collections were dismantled during the nineteenth century to be reorganised into museums.

Recontextualising the ethnographic pieces, the ethno-historian might try to reconstruct more complete images of these other cultures, rather than the other worlds from which they now are seen to derive. Taking seriously the notoriously incomplete and

fragmentary, the fundamentally distorted character of all the inherently incommensurable bits and pieces has led a number of scholars recently to undertake another recontextualization. According to this view, the very fragmentary character of these descriptions should be taken as evidence of some kind of colonial (or proto-colonial) strategy of conquest. The accounts take possession of these other cultures through a discourse of systematic differentiation. Whatever we can learn from them in other respects, however, these analyses of travel accounts as colonial discourse and as ethno-historical documents respectively, tend to overlook what I think is a fact of fundamental importance. For the contemporary readers of travel accounts in the seventeenth and early eighteenth centuries travellers to Africa, Asia and America were supposed to have eye-witnessed not so much other cultures and societies—they never used such terms in fact—but what they themselves often referred to as 'other worlds'.

OTHER WORLDS

Looking at the ethnographic digressions found throughout the East and West Indian voyages of the seventeenth and early eighteenth centuries from a thematic rather than a geographical perspective, two themes deeply fascinated most of our travellers: death and the Devil. Neither of these two themes was dealt with in every major descriptive digression. On the other hand, all the travellers' accounts, on some occasion or another at least, touched on the question of how the living dealt with their dead and how the Devil dealt with the living.

According to our travellers, the Devil appeared almost everywhere in Africa, Asia and America, and assumed a bewildering variety of names and shapes. In spite of the numerous descriptions of his presence and influence through all kinds of worship, sacrifice and consultation, however, none of the travellers ever claimed to have seen or talked to the Devil in person. In the presence of a Christian, so it was argued, the Devil kept both hidden and silent. Following some of the more curious travellers, such as Samuel Brun, Johann Wilhelm Müllier, Heinrich von Uchteritz or Adam Olearius (who in this context mostly acted as an editor of others' accounts), and their vain attempts at getting in touch with the truth and origin of all the signs they thought they were shown, one can see the obsessive invocation of the master of the other world. The endless attempts to find all kinds of signs of his presence again and again confirmed that what the travellers had observed really was that other world ruled by the Devil.

Even in these descriptions there might be some traces of valuable information for the ethno-historian of Asian, African and American religions. Describing people as victims and allies of Satan himself, of course, has also played a role in legitimizing colonial and pre-colonial ambitions and hegemonies. Experiencing the world out there as under his sovereign rule also contributed substantially to making the travellers familiar with the world they had just returned home to when performing their reaggregational ritual.

The numerous descriptions of rituals surrounding death in Africa, Asia and America illustrate how ethnographic descriptions in Germany in the seventeenth and early eighteenth centuries can be fruitfully analysed as involuntary auto-ethnography, rather than ethnography proper or colonial discourse.

Apart from a few but significant exceptions (which I shall discuss shortly) death and the dead outside of Europe appear to have been an extremely noisy affair. Looking closely at the very long series of descriptions of death, burial and mourning throughout the world, the loud, tumultuous and vociferous reactions and behaviour of the bereaved must have been as striking for contemporary readers as they are for readers today. Most observers seem to have taken the various forms of noise and lamentation or—as in the case of widow-burning described in almost all sources—even orchestrated, joyous jubilation, as a sign of heathen claims to immortality, the absence of the capacity to accept mortality on the part of the bereaved.

The most important exception to this rule, the solemn silence of the Chinese in Batavia when confronted with death (which is described in many sources), is easily explained: worshipping the ancestors as if they were living and offering food to the dead—food which reportedly was often taken away by hungry European visitors to the Chinese cemetery near Batavia—amounts to much the same thing: a systematic denial of mortality. Human sacrifice, such as widow-burning in India or the massive killing of slaves on the occasion of the death of African kings, was taken as evidence for exactly the same phenomenon by almost all German observers.

Denial of mortality has apparently been taken as a universal sign of a heathen other world. The only descriptions making the exchange of memories and recollections of the life of the deceased part of the grief of the bereaved belong to the New World. There, however, corpses are neither cremated nor buried, but simply thrown away (Ultzheimer 1616:85–6), or eaten by the bereaved (Schmalkalden 1652:21–3). In Protestant Europe, in the milieu of our observers, talk and memories of the life of the deceased during the seventeenth and early eighteenth

centuries were increasingly made part of the funeral liturgy. In the other worlds, it was just the other way round.

German travellers to the East and West Indies during the seventeenth and early eighteenth centuries have certainly left accounts containing some ethnographic information of interest for the ethno-historian of the cultures and societies which these travellers tried to describe as best as they could. As employees of the Dutch East Indian Company, moreover, many of these travellers also involuntarily aided European commercial expansion by legitimating worldwide European dominance. By looking more closely at what they actually described I hope to have demonstrated that, in their written accounts, they have given us an important clue to how they and their contemporary readers tried to make sense of the world which they tried to make their own again after having been away for so long. Talking about and describing other worlds involves analysing and understanding other cultures. Analysing and understanding ways of talking and writing about other worlds can also be a clue to a better understanding of those cultures who did the talking and writing.

NOTES

1 Walter Benjamin, 'Eine Geschichte, in der Kant sich kurz faßt', *Gesammelte Schriften* (vol. IV, 2:809). I owe this reference to Bremer (1987:63). This paper summarizes the last two chapters of my *Wilde Völkerkunde. Andere Welten in deutschen Reiseberichten der frühen Neuzeit* (Harbsmeier 1994b). I gratefully acknowledge the generous support of the Danish Research Council for the Humanities.

2 'I propose that we should work on the hypothesis that the ritual occasions of the Ok people are major occasions in the genesis of *their* tradition of knowledge, as significant in it as the major monographs, seminal articles, and innovative lectures are in ours. The distribution of symbols, ideas, meanings and world views among the Ok is a product of such events. As their anthropologist, I should like to write a book which might be called *Cosmology and Cosmologists—the Modern Ok School*' (Barth 1987:19).

3 Attempts at domesticating and subordinating travels and travellers to the more or less institutionalized needs of a sedentary elite go back as far as Plato and his detailed instructions on how to deal with the danger to the ideal republic arising from uncontrolled travellers' appreciation or even propagation of foreign and contagious manners and customs. In early modern times, apodemic literature from the late sixteenth century onwards followed up on Plato's concerns by systematically urging travellers to follow certain methodological rules when making observations. Justin Stagl, who has analysed the development of this literature to the end of the eighteenth century in great detail, has little to say about its actual impact on contemporary travel accounts.

4 'Diese Insul Ceylon ist eine von den allergrößesten, schönsten, reichsten und fruchtbarsten Insulen in gantz Asien. Weswegen auch gar viele auf die Gedancken gefallen, es müsse bey der ersten Welt, der Garten Eden, oder das sogenannte Paradies/allda gestanden seyn. Und solches wollen sie aus dem Grabe des Adams daselbst bekräftigen, welches man biß auf den heutigen Tage sehen kan, nemlich auf dem sogenannten Adams-Pück, oder Berg, welcher einer von den allerhöchsten Bergen in gantz Indien ist: denn die Einwohner sagen: der erste Mensch, Adam, soll allda begraben liegen; und wolen das aus der dabey befindlichen Grab-Schrifft erweisen, welche schon viele fremde Passagier gelesen, oder vielmehr gesehen, die Figuren der Buchstaben auch auf das accurateste nachgezeichnet haben. Alleine, so vielen von denen Grundgelehrtesten Männern und Liechtern der Gelehrten Welt, die solche auch gezeiget haben/hat doch niemand derselben sich dieser Sprache entsinnen, die Schrift nicht lesen, noch weniger aber deren Inhalt herausbringen können. Ja selbst die Orientalischen Völker, die doch, bey ihren Nationen/ihre uralte Sprache führen und behalten, haben keine Erkänntnüs von dieser Schrifft. So daß man also notwendig muthmassen muss, es müsse von der allerersten Sprache seyn, welche man, noch vor der Babylonischen Verwirrung der Sprachen geredet habe' (Behrens 1737:245–6). On Behrens see also Harbsmeier (1991).

5 Apart from the texts reprinted in Naber's collection this corpus includes: Ultzheimer (1616), Brun (1624), Fernberger (1628), Augspurger (1644), Schmalkalden (1652), Andersen (1669), Iversen (1669), Cortemünde (1675), Müller (1676, although he was working as a priest at a Danish colony in Africa), Parthey (1687), Tappe (1704), Langhansz (1705), Uchteritz (1705), Vogel (1716), Barchewitz (1730), and Volckart (1735).

6 The distinction between 'dynamic-narrative' and 'static descriptive' passages characteristic of travel accounts has first been suggested by Nicolas Troubetzkoi in 1927 (see Harbsmeier 1982).

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2 Origins and institutionalization of ethnography and ethnology in Europe and the USA, 1771–1845

Han F.Vermeulen

INTRODUCTION

In the history of anthropology much attention has been paid to the relative age and meaning of the concepts ethnology, ethnography, *Völkerkunde, Volkskunde* and anthropology vis-à-vis one another.[1] The discussions about the history of these concepts have been heated and often biased, by nationalistic points of view among other things (as Fischer 1970 has observed). Within the larger process of the 'unification' of Europe, the time may well have come to concentrate again on these issues, by bringing into perspective some new findings.

Following recent discussions in Germany and elsewhere on the origins and early development of the concepts *Ethnographie, Ethnologie, Völkerkunde* and *Volkskunde,* it becomes clear that these concepts all emerged during a specific period, namely the early 1770s and 1780s. Of these concepts, the coining of the term *Ethnographie* is probably the most significant. It appeared as early as 1771, in a historical work by the German historian and linguist August Ludwig Schlözer in Göttingen, and as an equivalent of the term *Völkerkunde* (Vermeulen 1992). The term *Völkerkunde,* which appeared simultaneously, was followed ten years later by the term *Volks-Kunde* (1782, also in Göttingen). A prototype of these concepts, *Völker-Beschreibung,* was used in a German manuscript written in Russia in 1740, but reappeared in print only in the early 1780s.

These data constitute important corrections to long-held views in France and England. The general dating in England is that the concept *ethnography* first appeared in a journal in 1834 and then as the name of a new gallery in the British Museum in 1845, while the concept

ethnology first occurred in 1842/43 with the founding of the Ethnological Society of London. In France it is generally believed that the concept *ethnologie* was coined by Alexandre-César Chavannes, in 1787 in Lausanne, before it was established by the foundation of the Société Ethnologique de Paris in 1839. Chavannes' use of the term ethnology is generally accepted as the first reference to the concept, although *Ethnologie* also emerged in the same year 1787 in Halle (as was discovered by Lutz in 1973). As a consequence of the EASA conference in Prague it can now be revealed that none of these claims is justified since the concept *ethnologia* had already appeared four years earlier, in a Latin work published in Vienna in 1783 (which was established by Tibenský in 1978 but remained unknown in the West, see below).

The similarity between these concepts is that they all referred to a study of peoples or nations that was descriptive and historical. It seems that the difference between *Völkerkunde* and *Volkskunde* was that the first concept applied to the study of all peoples, whereas the second applied to the study of one people only. Thus, the opposition between 'Western' and 'non-Western' (European and extra-European), by which the distinction between *Volkskunde* and *Völkerkunde* is usually explained, was not valid for the late eighteenth century.

It is my intention to present these distinctions in greater detail, and to pay attention to the social and political processes that influenced the formation and adoption of these five concepts at the end of the eighteenth and the beginning of the nineteenth century in several countries in Europe, as well as in the USA. This exposition will be concluded by looking more closely at the foundation of the so-called 'ethnological societies', established in 1839–43 in Paris, New York and London, which precedes the commonly accepted dating of the beginnings of anthropology with the work of Tylor, Morgan, Bastian and others in the years 1859–71. The earlier data taken together point to a process which can be called the conceptualization of ethnology or *Völkerkunde* as a 'science of nations'. This was followed by its institutionalization in ethnographical societies and museums, as well as the concurrent transformation of ethnology into a 'science of races'.

PLURAL ORIGINS OF ANTHROPOLOGY

Contrary to general consensus, anthropology is not a young discipline. If we accept the thesis that the discipline must have been in existence at least by the time its very names, ethnography and ethnology, were

coined, we must conclude that the discipline as such goes back to the late eighteenth century. Elsewhere I have shown that the concepts *Völkerkunde* and *Ethnographie* first surfaced in 1771 in Göttingen, a university town in northern Germany, whereas the concept *Volks-Kunde* appeared in 1782, also in Göttingen (Vermeulen 1994). The concept *Ethnologie* now appears to have been introduced in 1783 in Vienna, and only later, in 1787, appeared in Lausanne (French-speaking Switzerland) and Halle (a university town in eastern Germany). Thus, we are led to the conclusion that the discipline of ethnology or ethnography, as the Greek neologisms of the German concepts *Völkerkunde* and *Volkskunde,* was conceptualized in the years 1771–87 as part of the Enlightenment endeavour to create some order in the growing body of data on peoples, nations or *Völker* in the world of that era, and against the background that such a discipline was lacking and needed to be formulated.

This interpretation stands in marked contrast to the general belief that anthropology as the comparative study of 'other societies' originated in the mid-nineteenth century with the works of founding fathers such as Tylor, Morgan, Bastian, McLennan, Maine, Bachofen and Lubbock. This is the standard view, embraced by most anthropologists and taught to students throughout the world. According to another view, put forward mostly by historians of anthropology, anthropology started as an independent discipline in the 1830s and 1840s, with the foundation of the ethnological societies. (Peculiarly, the establishment of specialized ethnographical museums in the same period, from 1836 onwards, is largely neglected—apparently due to lack of comparative analyses, see below). As a variant of this view, academic institutionalization by means of university chairs is mentioned, a process which started much later, at the end of the nineteenth century and the beginning of the twentieth.

Although these views have the advantage of being relatively clearcut, I would say that the datings involved are limited in view and biased by presentism, in that the processes which went on before these dates are neglected. For instance, there is evidence that Tylor and Morgan were not exclusively concerned with 'other' societies but had a perspective which included Western, 'developed' societies, whereas both Tylor and Bastian based their own work partly on studies which go back to the century before their own (Leopold 1980; Koepping 1983). I therefore propose to focus in greater detail on the period before the works of anthropology's 'founding fathers' were published, i.e. on the late eighteenth and early nineteenth centuries. This would have the advantage of an increased time-span and could supply us with

a fuller understanding of what happened afterwards, in the late nineteenth and early twentieth centuries.

Within this broader perspective, there is a historicist way of going about the history of anthropology which is by looking at the formation and development of the concepts with which this discipline has been designated. Although there are a number of problems attached to this form of conceptual history or *Begriffsgeschichte,* it is clear that if we use the dates and the meanings involved as indicators of more general developments the method has great potential.

This is not a new method. As stated above, within the history of anthropology much attention has been given to the relative age and meaning of the concepts mentioned. The time may be appropriate to concentrate again on these issues, but now from a historicist point of view. In the following I shall concentrate on the origin and reception of the concepts *Völkerkunde, Ethnographie, Volkskunde* and *Ethnologie,* and I shall try to connect these processes to more general developments such as rationalism and empiricism on the one hand and racialism and nationalism on the other.

THE INTRODUCTION OF ETHNOGRAPHY AND ETHNOLOGY, 1771–91

As indicated above, the concepts *Ethnographie* and *Völkerkunde* first appeared in the work of the historian and linguist August Ludwig Schlözer, who had worked in Göttingen and St Petersburg in the 1760s and became a professor in Göttingen in 1769. Schlözer used these terms as equivalents of each other, in an important book on the history of the nordic countries entitled *Allgemeine Nordische Geschichte* (Halle 1771), in an effort to rewrite the history of Northern and Eastern Europe and Russia, including Siberia and Mongolia (Vermeulen 1992). Schlözer also used the concepts in a work drafted in the same period, *Vorstellung seiner Universal-Historie* (Göttingen 1772), to which Stagl (1974) has drawn attention. We have no absolute proof that the concepts are Schlözer's invention, but the circumstantial evidence is rather convincing. Thus, Schlözer implicitly claimed the concept *ethnographisch* to be his own in a polemic with Johann Gottfried Herder in 1772/73 (Stagl 1974:80–1).

Second, Schlözer used the concepts *Völkerkunde* and *Ethnographie,* as well as such derivations as *ethnographisch* and *Ethnograph* (ethnographer!), much more often than did his followers. Furthermore, it is known that he was an innovator in matters of language and that he liked to invent new concepts. Finally, the concepts appear in significant

passages in his books and all point to the desire that history should be rewritten in an ethnological way, thus paying attention to the history of nations or *Völkergeschichte*. To that end, Schlözer developed an 'ethnographical method', which was practised by his students till well into the nineteenth century.

The idea that next to a history of states a history of nations was indispensable had already been suggested by Schlözer's older colleague, the historian Johann Christoph Gatterer. Gatterer apparently adopted the concept *Völkerkunde* from Schlözer's teaching or personal communication, using it in the same year 1771 in a volume entitled *Einleitung in die synchronistische Universalhistorie*. He again used it in his review of geography (*Abriß der Geographie,* published in 1775/78), in conjunction with the word *Ethnographia (sic)* and as its equivalent (Fischer 1970:170). Gatterer, however, classified the discipline not within the domain of history, but within that of geography. Although geography in Gatterer's view was an auxiliary discipline (or *Hilfswissenschaft*) of history, it is likely that ethnography could not be classified among the historical sciences because some of the peoples it should study were considered to have no (written) history (Vermeulen 1992). Gatterer's views were influential in that the geographical conception of ethnography remained common in Germany at least until Ratzel.

Central to the conception and classification of *Ethnographie* and *Völkerkunde* was the University of Göttingen, to which Schlözer returned in 1767, after an absence of five years in Russia. It was a young university, where academic liberalism was embraced, collections of books, drawings and artefacts *(Kunstsachen)* were built up and where the Enlightenment principles of rationalism and empiricism were taught as instruments to educate the state elite of Hanover and to improve society. Although this university was founded in 1734/7, it reached its first zenith in the 1770s and 1780s and was instrumental in the process known as the German *Spätaufklärung*. During this period Göttingen was in close contact with the British, as well as with the Russian empire.

An important factor in shaping the tradition of ethnography was the massive exploration being undertaken in the Russian empire. Russia in the eighteenth century had developed into a multinational state and this had important consequences. Thus, it was official policy that in order to govern the diverse nations which were part of the empire, they should be studied intensively. To this end numerous expeditions were sent out by the Imperial Russian Academy of Sciences, from the 1720s onwards and culminating in the 1770s and 1780s (Donnert 1983). These

expeditions had, next to geopolitical goals, scientific aims in that the study of natural history, geography, history, languages and peoples was actively demanded and supported. It is significant that many German scholars were employed as members of these expeditions, and they supplied the learned world with travel journals and detailed descriptions. The man for whom Schlözer had worked, G.F.Müller, was a member of the second Kamchatka expedition (1733–43); Müller's successor on this expedition, J.E.Fischer, was a close colleague of Schlözer; whereas Peter Simon Pallas, an old friend of Schlözer, became famous as an explorer of Russia (most notably in the years 1768–74). Schlözer, although he never became an explorer himself, came into contact with their and other publications on the peoples and nations in the newly explored territories during the five years that he worked in St Petersburg (1761–5, 1766–7) and this must have influenced him into formulating his ideas on the history of the northern nations in both Europe and Asia.

Equally important in shaping ethnography was the development of comparative linguistics. Schlözer especially was a gifted linguist and in his early years made important contributions to this field. He applied the dictum of Leibniz that for the study of ancient history it was not books and annals that were essential, but the comparison of languages (1771:288). By applying the method of historical linguistics, Schlözer reshuffled the early history of all northern nations and shook up the genealogical views propounded for centuries on the basis of a prejudiced reading of the Bible. Thus Moses, according to Schlözer, had never known anything about northern Europe, let alone Siberia, and therefore the genealogical tables in his *Völkertafel* (Genesis X–XII) were not reliable. These views were probably shared by a select group in Göttingen and other circles in Germany at the time, especially among scholars who came to study the Bible as a historical document, i.e. as written by human beings and therefore fallible and subjective.

Other important factors were developments in the domains of history, geography and 'statistics'. In the domain of history a breaking up of the field of ancient 'polyhistory' occurred, which resulted in a systematic reordering of the branches and subdisciplines of this (Renaissance) discipline. Ethnography or the study of *mores* was one of them and there is evidence in Schlözer's and Gatterer's work that developments in the study of *Völkergeschichte,* or the history of nations, were instrumental in calling forth the new discipline. The same applied to geography, which was considered by Gatterer as an auxiliary discipline of history. I have already referred to the importance of the

geographical, astronomical and natural historical expeditions sent out throughout the Russian empire, but it is significant that the subject of the customs and manners of the peoples in these territories was explicitly included in the questionnaires and instructions passed out to the members of these expeditions.

In opposition to geography the subject of 'statistics' *(Statistik)* was being developed by Achenwall, and later by Schlözer. It was a comparative study of states, intended to complete the study of *Weltgeschichte* or universal history. The study of peoples and nations was apparently meant as a complement to this study of states, and probably as an alternative to the physical anthropology developed simultaneously by Buffon, Camper, Blumenbach and others. In the context of these expeditions and developments in history, geography, linguistics, 'statistics', and (physical) anthropology, the subject of *Völkerkunde* or *Ethnographie* was introduced and developed, most notably, but not exclusively, in Göttingen.

What is striking, however, is that after they had been introduced, the concepts were quickly adopted and amended outside Göttingen. In 1776 the term *Völkerkunde* was used in a long article about 'Die Völker' by F.C.Fulda, who worked as a small-city pastor in Mühlhausen but for his historical studies maintained close connections with professors in Göttingen. In 1781 it was used in the title of a journal published by the natural historian and South Sea explorer Johann Reinhold Forster and his son-in-law, the historian Matthias Christian Sprengel, who were both working in Halle but also were in close contact with Göttingen: the well-known *Beiträge zur Völkerund Länderkunde* (published in Leipzig in fourteen volumes between 1781 and 1790, with a continuation in thirteen volumes from 1790 to 1793). In 1782 it appeared in the journal *Litteratur und Völkerkunde* published by J.W.von Archenholtz, who worked in Dresden (published in Dessau in nine volumes between 1782 and 1786, with another five volumes between 1787 and 1791). One year later the concept *Völkerkunde* appeared in the *Magazin der Erd- und Völkerkunde* by Theophil Friedrich Ehrmann who was working in Strasburg, although the journal was published in Giessen in 1783–4 (Vermeulen 1994).

At the same time, variants were introduced. A particularly important one, but comparatively unknown, was *Völker-Beschreibung,* the literal translation of *Ethnographie* in German. It appeared in a handwritten instruction by the historian and geographer G.F.Müller to J.E.Fischer, his successor as a member of the second Kamchatka expedition in 1740 (Russow 1900:83). It reoccurred forty years later in the title of a

journal edited by Peter Simon Pallas, working in St Petersburg, entitled *Neue Nordische Beyträge zur physikalischen und geographischen Erd- und Völkerbeschreibung, Naturgeschichte und Ökonomie* (7 vols, St Petersburg, 1781–96), as well as the third volume of J.P.Falk's *Beiträge zur topographischen Kenntniß des Russischen Reichs,* edited by J.G.Georgi and published in St Petersburg in 1785–6. This volume was subtitled 'Beiträge zur Thier-Kenntniß und Völker-Beschreibung' (Contributions to the Knowledge of Animals and to the Description of Nations). Georgi had accompanied Falk during the second Ohrenburg expedition (1770–4), and published an important description of nations in the Russian empire (*Beschreibung aller Nationen des Rußischen Reichs,* 4 vols, St Petersburg, 1776–80), which was beautifully illustrated by the engraver C.M.Roth.

In the 1780s two other forms were introduced as variants on the same theme. The concept *Volks-Kunde (sic)* appeared in 1782 in a journal by Friedrich Ekkard, who was secretary of the university library in Göttingen and collaborated closely with Schlözer (Kutter 1978). Five years later the concept *Volkskunde* occurred in the 'statistical' work of Joseph Mader, an Austrian professor working in Prague (1787), and in 1788 in the popular work of the Romantic poet C.F.D.Schubart, writing in Stuttgart (Möller 1964; Narr & Bausinger 1964).

At about the same time, the concept *Ethnologie* appeared. It was first used by the Slovak historian Adam František Kollár in Vienna, who introduced the term and gave it its first definition in 1783.[2] Kollár defined *ethnologia* as:

> notitia gentium populorumque, sive est id doctorum hominum studium, quo in variarum gentium origines, idiomata, mores, atque instituta, ac denique patriam vetustasque sedes eo consilio inquirunt, ut de gentibus populisque sui aevi rectius judicium ferre possint
>
> (*Historiae iurisque publici regni Ungariae amoenitates,* Vindobonae 1783:80)[3]

It is significant that Kollár, who was head of the imperial library in Vienna, praised the work of Schlözer, whereas Schlözer in his work of 1771 quoted an earlier study by Kollár. They both focused on nations and peoples in Northern, Central and Eastern Europe, from a historical and a contemporary perspective. Kollár's inspiration came from the ethnic composition of the two Pannonias, which had just been liberated from the Turks; Schlözer's inspiration came from the origins of the Russian state in the ninth century with the arrival of the (Nordic)

Waraeger in Kiev. Despite their differences in interest, both men were working on the same research problem, the origins of nations, with the same material: dictionaries, grammars and chronicles.

The concept *Ethnologie* reappeared a few years later, in two different places simultaneously: it was used by the Swiss theologian Alexandre-César Chavannes, working in Lausanne, and by the German geographer and historian Johann Ernst Fabri, working in Halle, in the year 1787. Chavannes used it in his *Essai sur l'éducation intellectuelle* as a designation of *'l'histoire des progrès des peuples vers la civilisation'* (as was already pointed out by Topinard in 1888 and 1891). Fabri used the concept *Ethnologie* as an alternative for *Ethnographie* and saw it not as a designation of *Völkerkunde,* but as something bigger, combining both *Völkerkunde* and *Volkskunde* (Lutz 1973:24); he later gave the discipline a prominent place in his encyclopaedia of historical sciences (1808),

The synchronicity of this development makes it likely that both authors borrowed from another source. It has been thought for a long time that Chavannes coined the word and that it was a French invention (cf. Topinard 1888 and 1891; Berthoud 1992:257). However, on the basis of the reference to Kollár's work, we can infer that the German-language area was seminal and that the word *Ethnographie* served as the root of it. Fabri, for one, had studied in Göttingen, whereas Chavannes was widely read in German literature and maintained close relations with Isaak Iselin who also had studied at Göttingen (Stagl 1994).

It must therefore be concluded that the concepts *Völkerkunde* and *Ethnographie,* as equivalents of each other, are the oldest and most distinctive. They applied to a new discipline of nations or peoples *(Völker),* as is clear also from the possible prototype, *Völker-Beschreibung*. The concepts *Volkskunde* and *Ethnologie* arrived later, as variants on the same theme, all referring to a discipline which was descriptive and historical. Together, the introduction of these concepts points to a phenomenon which can be called the conceptualization of ethnology.

As mentioned above, the difference between 'Völkerkunde' and 'Volkskunde' can probably be seen as a relative one: *Völkerkunde* was descriptive and inclusive, thus dealing with the study of all peoples; *Volkskunde* was the study of one people only. This formula is based on the reading of contemporary works, although one does not find it explicitly. We can be sure, however, that the later (and current) distinction between 'Western' (or European) and 'non-Western' (or extra-European), with which the distinction between *Volkskunde* and

Völkerkunde is usually connected and 'explained', was *not* valid for the publications of the German Enlightenment. This distinction apparently came to the fore much later, probably in the early twentieth century, under the influence of colonialism and other factors. *Volkskunde* in the late eighteenth century did not have the connotations of a study of peoples in the country of the researcher, or of classes within his/her society considered as backward by the norms of that society. These are all later developments. In fact, *Volkskunde* as envisaged in the late eighteenth century was not a Romantic science, but—like *Völkerkunde*—a product of the German Enlightenment.[4] It is remarkable, therefore, that later Romantics such as Goethe and Herder seldom used the words *Volkskunde* or *Völkerkunde*. These scholars are known for having contributed to the subject by paying much attention to the study of the folk-life, and to folk-songs and *Volkspoesie,* regarded as the spirit of nations *(Volksgeisf)*. Their work did influence the later, Romantic, conception of *Volkskunde*.

By this time, however, the concepts had already become accepted in various circles in Germany and German-speaking countries. Several learned journals had been launched and the first review articles on the new subject were published, beginning in 1787 with the 'Kurze Ubersicht der Völkerkunde' by Ehrmann, who returned to it in 1791 and 1808, and long expositions by the historian and geographer colleague J.E.Fabri in 1808. Furthermore, the discipline had become known outside Germany, namely in Russia, Austria, Bohemia and Switzerland—albeit in all cases still through its connection with German-language users.

By 1791, when the illustrated *Ethnographische Bildergallerie* was published in Nuremberg (as was noticed by Bastian in 1881, but thereafter forgotten), the adoption had been so rapid that in the twenty years between 1771 and 1791 the concepts *Völkerkunde, Ethnographie, Volkskunde* and *Ethnologie* appeared in more than forty books, journals, articles and letters (see appendix 2 in Vermeulen 1994). We can thus conclude that by 1791 anthropology in the form of *Völkerkunde* existed as a new field of study: it was formulated and it was practised.

RECEPTION AND INSTITUTIONALIZATION OF ETHNOGRAPHY AND ETHNOLOGY IN OTHER COUNTRIES, 1791–1845

After the first adoptions and emendations in Germany, Russia, Austria, Bohemia and Switzerland, the concepts also found their way into other countries of Europe, as well as into the USA. Very little is known about this process, but we can be sure that the developments in Germany did not pass unnoticed abroad. Evidence in support of this supposition is the occurrence of the concept *Völkerkunde* in the correspondence of Josef Dobrovský, the famous Czech linguist, who used it in 1789 in the formulation: 'alle Liebhaber der slawischen Völkerkunde und Sprache' ('all supporters of Slavic ethnology and language', quoted in Krbec and Michálková 1959:12). Although Dobrovský was bilingual, in Czech and German, this does not apply to all authors who adopted (one of) the concepts, as can be seen from the following exposition of the occurrence of the concepts in The Netherlands, France, Hungary, Bohemia, Slovakia, Russia, Denmark, England and the USA.

In The Netherlands the word *volkenkunde* was present in 1794 when a certain Fokke spoke about 'eene handel- en volkenkundige reis door geheel Europa' ('a commercial and ethnological journey through the whole of Europe'). In 1811 and 1812 the word *Volkskunde* was used by the linguist Buys and the orientalist Willmet. In 1821 the Reverend R.P.van de Kasteele, director of the Royal Cabinet of Curiosities in The Hague, spoke about the necessity of compiling an ethnological collection ('een Ethnologische verzameling'). In 1822 the historian and geographer N.G.van Kampen used the combination 'Aardrijks- en Volkenkunde' ('Geography and Ethnography'), which he may have adopted directly from the German authors since he based himself on a handbook by Fabri. In 1824 the concept *ethnographie* was accepted in the dictionary of P. Weiland, while in 1826–7 Johannes Olivier published the journal *Cybele: Tijdschrift ter bevordering van land- en volkenkunde* (6 issues). In November 1830 the very first lecture on the subject in the Dutch language was given by van de Kasteele ('Over de Volkenkunde') before the Diligentia Society in The Hague. In 1836 the first academic chair for the subject was established, significantly at the Royal Military Academy (KMA) in Breda, which was designated as a chair in 'de Maleische taal en de Land- en Volkenkunde der Oost-Indische bezittingen' ('the Malay language and the Geography and Ethnography of the East-Indian colonies'). In

1837, finally, the founder of the Japanese (later Ethnographical) Museum in Leiden, Ph.Fr.B.von Siebold, a medical doctor who had studied in Germany and worked for the Dutch on Deshima, wrote about the necessity of founding an 'ethnographisch Museum' or a 'Museum van Land- en Volkenkunde' ('a Museum of Geography and Ethnography') in The Netherlands (cf. de Josselin de Jong & Vermeulen 1989:281–6).

In France the situation is less clear. L.-F.Jauffret, a member of the *Idéologues* and one of the founders of the short-lived Société des Observateurs de l'Homme (1799–1804), knew the German contributions to the new field very well and used the work of Christoph Meiners extensively (Rupp-Eisenreich 1983). However, the word *ethnographie* in France has not been found before 1820, when it appeared in the *Portefeuille géographique et ethnographique* by G.Engelmann and G.Berger (Blanckaert 1988:26). The term *ethnographie* was taken up in the dictionary of Pierre Boiste in 1823 and it appeared in the title of the *Atlas ethnographique du globe* of the Italian geographer Adriano Balbi in 1826, which is, however, an 'atlas' of linguistic tables! After the early and soon-forgotten reference by Chavannes in 1787, the concept *Ethnologie* made its reappearance in the work of the physicist André-Marie Ampère, who during the years 1829–34 gave the discipline its place among the 'sciences anthropologiques' in his classification of sciences (de Rohan-Csermak 1967, 1970).

By this time a development took place in France towards the integration of ethnic and racial history, in which the term ethnology was given a different meaning. It became the object of the Société Ethnologique de Paris, established by W.F.Edwards and others in 1839 with the purpose of studying 'l'organisation physique, le caractère intellectuel et moral, les langues et les traditions historiques'. This implied a shift away from the original content, which is particularly important because the French society became the example of the ethnological societies in England and America: the Ethnological Society of London founded in 1843 and the American Ethnological Society founded in New York in 1842 (Mühlmann 1968:77–9; Fischer 1970; Stocking 1971; Blanckaert 1988). With these societies ethnology entered its institutional phase.

It is remarkable, however, that the discipline by this time had undergone a transformation in that ethnology was practised with concern for a curious mixture of geographical, historical, linguistic and physical (i.e., racial) characteristics, which ethnography as conceived earlier and practised simultaneously did not possess, or in any case not

to the same extent. In France and The Netherlands, for instance, ethnography maintained its position vis-à-vis ethnology in specialized ethnographical societies, such as the Société d'Ethnographie Américaine et Orientale (founded in 1859), and the Koninklijk Instituut voor Taal-, Land- en Volkenkunde van Nederlandsch-Indië (KITLV, or Royal Institute of Linguistics, Geography and Ethnography of the Netherlands Indies, founded in 1851 with preparations starting in 1844). These societies were geographically and linguistically orientated—in competition with the ethnological societies, that pursued a racial kind of ethnology. Thus, the concept *Ethnographie,* introduced in the *Dictionnaire de l'A'cadémie française* of 1835, was defined as 'partie de la statistique [i.e., the comparative study of states] qui a pour objet l'étude et la description des divers peuples'. In 1831 a commission was instituted with G.Cuvier and E.-F.Jomard as its members for the 'formation d'un Musée ethnographique à Paris' (Bastian 1881:9), to which subject Jomard returned in 1845 in an open letter to Von Siebold about 'des collections ethnographiques'. By this time (1839) Jomard had defined 'l'objet de l'ethnologie' in a much broader sense than the members of the Société Ethnologique de Paris did in the same year, in which the problem of race was included but not as its sole object (Fischer 1970:178).

The establishment of an ethnological society with the object of retracing the original (primordial) races in the history of (mainly European) nations, as Topinard (1885:119) summarized Edwards's position, created confusion. By this time a physical study of humans had existed for a good half century and had, through the work of Buff on, Blumenbach, Soemmering, White, Cuvier and others, become known under the general rubric of anthropology or 'natural history of man'. This was the reason why between 1839 and 1871 heated discussions were held on the differences between the two approaches (and the history of the respective names!), which ultimately led to the abolition of the ethnological societies and their restructuring as anthropological societies.

By this time, the concepts were (re)introduced in Central and Eastern Europe. In Hungary, the term ethnography was used by the (Slovak) scholar János Csaplovícs who wrote in 1818 that nations *(Völker)* are to be distinguished 'durch Sprache, physische und moralische Veranlagung' ('by language, physical and moral disposition'). Csaplovícs published a book in German on Hungarian national dress *(Ethnographische Erklärung der von Oberstlieutenant Heimbacher gezeichneten und in Kupferstichen herausgegeben 78 ungarischen Trachten)* in 1820 and introduced the term *Ethnográphiai*

in a series of articles entitled 'Ethnográphiai értekezés Magyarorszagról' ('ethnographical dissertation on Hungary'), published in the journal *Tudományos Gyüjtemény* in 1822 (de Rohan-Csermak 1970:705; Podolák 1988:230).

In Bohemia the introduction of the term ethnography took place in the same period. The concept *ethnographia* and its Czech equivalent *národopis* were used in 1821 in the first volume of the journal *Krok. Weregny spis wsenaucny pro Wzdelance Narodu Cesko-Slowanskeho* by Jan Svatopluk Presl (vol. I, 1821:10). It was repeated in this definition in the Czech-German dictionary of J.Jungmann in 1836 (*Slownjk cesko-nemecký,* Prague, vol. II, col. 611).[5] However, the concept *Völkerkunde* was known much earlier, as is indicated by the letter from Dobrovský in 1789 mentioned above.

For Slovakia the work of Kollár has already been mentioned. Kollár worked in Vienna but published in Latin. Although he signed himself as *Hungarus,* he was designated by his contemporaries as the 'Slovak Socrates' (Tibenský 1983). His countryman Csaplovícs also was considered a Hungarian by nation and published in that language, but by this time (*c.* 1820) Slovak identity was beginning to be identified as such.

About the situation in Russia we have very little data, but the information that we have is of the utmost importance. Thus, the concept *Ethnographie,* although probably formulated under the influence of the expeditions in the new territories of the Russian empire, as far as we presently know occurs as late as 1836, when an Ethnographic Museum was founded as a department of the Kunstkam(m)er (established in St Petersburg in 1713). The date 1836 is important, however, since it makes the St Petersburg museum the oldest ethnographical museum in the world, the only other ethnographical museums that have claimed to be 'the oldest' having been established in Leiden in 1837 (by Von Siebold) and in Copenhagen in 1841 (cf. Avé 1980:11, n. 2; Troufanoff 1966). The *Etnografisk Museum* in Copenhagen was founded and reordered by C.J. Thomsen on the basis of earlier collections in 1841. This was followed in 1843 by the museum of Gustav Klemm in Dresden, and the opening of a new gallery in the British Museum in 1845.

However, the 1836 reference to ethnography in St Petersburg is probably only the proverbial tip of the iceberg since the term *Völker-Beschreibung,* as we saw, already occurred in 1740 in the instruction by Müller to Fischer as his successor to the second Kamchatka expedition, and reappeared forty years later in the title of the journal published by Pallas mentioned above (1781–96) and in the subtitle of the third

volume of the *Beiträge* by Falk edited by Georgi: 'Beiträge zur Thier-Kenntniß und Völker-Beschreibung' (1786). This *Völker-Beschreibung* is the exact equivalent of the concept *Ethnographie* and it is significant that we find it in the context of the Russian natural historical, geographical and ethnographical explorations of the eighteenth century. We would therefore not be surprised if future research would reveal that there are many other early references to these concepts in Russian literature and that, perhaps, the word *Ethnographie* was even coined in the international context of St Petersburg, before it was introduced by Schlözer in Göttingen in 1771.

In the USA we have an early reference to the term *ethnological* in the questionnaire supplied to the members of the Lewis and Clark expedition in 1802, which contained an appendix 'Ethnological Information Desired' (Hallowell 1960:17). The questionnaire was drafted by Thomas Jefferson, who earlier published important *Notes on the State of Virginia* (1785), in which the comparative study of languages was also seen as the key to the origin and migration of nations. Forty years later, the concept 'Ethnology' was accepted for the dictionary of Noah Webster, *An American Dictionary of the English Language,* 2 vols, New York 1828 (as was pointed out by Lutz (1973:20)).

In Britain the adoption proceeded more slowly than in the other European countries and occurred in any case later than in America. The term 'ethnography' surfaced as late as 1834 in the second volume of the *Penny Cyclopaedia,* a journal published by Charles Knight and edited by George Long (vol. II, 1834:97). It subsequently appeared in an address by Cardinal Nicolas Wiseman on the 'Connection between Science and Revealed Religion' (1835, published in 1836), as well as in the third edition of the *Researches into the Physical History of Mankind* by the well-known natural historian James Cowles Prichard (1836, vol. I:110). The term 'ethnographical' first appeared in an official report in 1845, when the British Museum opened a large new gallery 'for the reception of the ethnographical collections' (Braunholtz 1970:37–8, n. 7).[6]

We have no real explanation of the fact that the concepts reached England so late, but we observe that the meaning which obtained in the 1830s and 1840s implied a radical departure from the Continental view maintained in Germany, France (partly), The Netherlands and Central Europe. These data thus substantiate Stocking's view that the term ethnology 'was still new to English usage' when Richard King published a prospectus to establish an Ethnological Society in London in 1842 (Stocking 1971:372). It must be stressed, however, that the

formulation used by King ('Ethnology: The Natural History of Man'), while in line with the view of Edwards in Paris, implied a deviation from the original content of the concept 'ethnography', as equivalent to *Völkerkunde*. This is evidenced by Webster's definition of ethnology as 'a treatise on nations' in 1828 (Lutz 1973:20), as well as with that proposed by the *Penny Cyclopaedia* of ethnography as 'nation-description' in 1834 (Bendyshe quoted in Hunt 1865:xcii). On the other hand, the definition by Wiseman in 1835/36 of ethnography as 'the classification of nations from the comparative study of languages' (Hunt 1865:xcv; Topinard 1885:122) is in accordance with the conception of Balbi (1826), and goes at least as far back as Schlözer 1771.

The concept 'folklore' (or 'folk-wisdom') was introduced by the Scottish scholar William J.Thoms in the *Athenaeum* of 22 August 1846 (who signed as 'Ambrose Merton', Haddon 1934:110), evidently as a neologism for the new meaning which the older German term *Volkskunde* had acquired in the meantime.

TRENDS AND CONCLUSIONS

We have seen that ethnology in the 1770s and 1780s was conceived as a descriptive and historical study of all nations, and how in the 1820s and 1830s it underwent a transformation in that the subject became influenced by nationalistic ideas on the one hand (especially in Central and Eastern Europe) and by racial ideas on the other (particularly in France and England).

This last development, indicated by the fact that the physical characteristics of nations had become part of the object of the ethnological societies, created considerable confusion. This was the reason why ultimately in 1871 in England (in 1885 in France) the term anthropology was favoured above that of ethnology (Stocking 1971). This anthropology was distinctively more physical in outlook than ethnology had ever been, but if there was a physical anthropology and a physical ethnology, there was no need to maintain the second, as Topinard (1885:215) reasoned. In the other European countries, however, particularly in the ethnographic societies and museums, the mainstream of ethnography continued to be directed towards *Völker-Beschreibung* or 'nation-description'. The use of the term became problematic only in that there should also be a theoretical discipline as implied by the noun *logos* in the term 'ethnology'. (It was this last development which ultimately was realized by Bastian, Tylor, Morgan and others.)

This was not the only important development in the early nineteenth century, however. It is well established that during this period the phenomenon of nationalism raised its head in most European countries, among other things due to political decisions taken at the Congress of Vienna (Lemberg 1950; Hobsbawm 1990; Smith 1991; Dann 1993). In this context, it is extremely interesting that in the 1820s the concept of ethnography reappeared in Central Europe, in two countries belonging to the Austrian empire. In both cases a specific term was coined to bring it in accordance with the national or regional language: in Hungary the noun *Ethnográphiai* appeared and in Bohemia the word *národopis* (resp. in 1822 with Csaplovícs and in 1821 with Presl).

It is likewise significant that in the view propounded by Csaplovícs the physical dimension *was* included. In fact, the formulation by Csaplovícs (1818) can be taken as a fine synopsis of the current view at that time: peoples or nations are to be distinguished by languages (this had been Schlözer's position), by physical condition (the position adopted by Edwards and others in 1829–43) and by moral condition (the position of Scottish and French Enlightenment writers such as Hume, Smith, Montesquieu and Voltaire). However, as we saw, this physical dimension was not dominant, until it became so with the work of William Edwards, who was very influential in calling forth a racial ethnology of European nations. For historical analysis, therefore, it is important to realize that the foundation of the ethnological societies in the years 1839–43 implied a shift away from a geographical, historical and linguistic type of ethnography, towards a physical and racial type of ethnology.

In this context, it is necessary to remember that the use of the concepts *Volkskunde* and *Völkerkunde* by Joseph Mader and Josef Dobrovský in Prague in 1787–9 occurred *before* the heyday of nationalism in Czechoslovakia, Germany and other countries. Thus, we must conclude that *Völkerkunde* in itself had no intrinsic connection with nationalism, but that the *second* wave of occurrences *was* connected with national movements.

The data outlined above indicate that there was an ethnographical tradition that probably originated in Russia, was encapsulated in a scientific format in Germany and Austria, and radiated out from there to be relatively quickly adopted in other European countries and in the USA.

The relevance of these insights is that they point to the influences of Enlightenment rationalism and empiricism instead of to those of nationalism and racialism, although these developments played a

significant role in the second wave of introduction of the concepts. Thus, these data contradict the view that is generally held about the history of anthropology, i.e., that anthropology is a Romantic discipline that originated from the contacts between European travellers, traders, missionaries and colonial officers, and the peoples outside Europe. Although these confrontations were obviously influential, it must be noticed that these contacts with 'others' did not in themselves produce a form and a programme with the help of which the peoples of the earth, including those of Europe, could be studied, described and compared. The rapid adoption of the terms *Völkerkunde, Ethnographie, Volkskunde* and *Ethnologie* points to the need felt for such a discipline, given the fact that these contacts had been intense over the years but had failed to result in a systematic body of knowledge (as Rousseau, for one, explicitly stated in his second *Discourse*). Thus, before the 1770s there was no category under which these studies and observations could be subsumed, nor a separate discipline to which a contribution could be made.

It can be observed now, after more than two hundred years, that the concept of ethnography especially has proved powerful. It refers to the description of living peoples, nations or tribes—concepts which have increasingly become problematic, due to the historical and political events of the past two centuries. But the idea of it still makes sense, especially if we look at it from Lowie's point of view, who wrote (1937:3) that ethnography is 'that part of anthropology (in the English sense of the word, the whole science of man)', which 'deals with the "cultures" of human groups.'

NOTES

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1 See Hunt (1865), Bendyshe (1865), Topinard (1876–91), Bastian (1881), Möller (1964), de Rohan-Csermak (1967), Fischer (1970) (who mentions most of the older contributions), Lutz (1973), Stagl (1974), Kutter (1978) Rupp-Eisenreich (1983) and Vermeulen (1992, 1994).

2 This first definition of ethnology was discovered by Ján Tibenský (1978). See also Urbancová (1970, 1987) and Belaj (1989). I owe the reference to Kollár to my colleagues Šmitek and Jezernik from Slovenia, and Skovierová, Jakubíková and Profantová from Slovakia. In the West it is generally believed that Chavannes was the creator of the neologism *ethnologie* (Berthoud 1992:257).
3 In English translation: 'Ethnology, which I have mentioned occasionally above, is the knowledge of nations and peoples, or, that study of learned men, by which they inquire into the origins, languages, customs and institutes of various nations, and finally into the fatherland and ancient seats, in order to better judge the nations and peoples in their own times.'
4 I owe this idea to Helmut Möller from Göttingen, who was the first to retrace the late eighteenth-century roots of *Volkskunde* (Möller 1964). See also the work of Gerhard Lutz (1973, 1982).
5 These data were kindly supplied by Václav Hubinger from Prague, during our correspondence preceding the EASA conference.
6 The reference to the British Museum was kindly pointed out to me by Andrew West, from the University of Humberside, Hull.

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3 Discovering the whole of humankind

The genesis of anthropology through the Hegelian looking-glass

Gheorghita Geana

OBJECT, CONCEPT AND THE DIVISION OF HISTORY

One of the main questions requiring the attention of historians of social/cultural anthropology and demanding a complex answer is: when did this scientific discipline properly begin?

The reason for this question lies in the plurality of alternatives whose meaning depends upon the lack of a unique starting criterion. This does not mean that an alternative in itself does not have a meaning, but simply that this meaning is only partial and, when taken singly, it cannot justify the beginning of our science.

The desired starting point seems to depend upon one or another of the particular aspects, variably considered as a *conditio sine qua non* of a sound anthropology. The origin slides along the axis of time, as follows:

Possible conditioning criterion	*Starting point for anthropology*
The coining of 'culture' as a central anthropological concept	Edward B.Tylor's modern and technical definition of 'culture' in 1871[1]
Direct field work	(a) Intensive fieldwork. Henry Schoolcraft's eighteenyear experience (1822–40) amidst the Chippewa American Indians[2] (b) *Ad hoc* field trip. The French expedition to the Atlantic and Indian Oceans (1800–4), with François Pérón as entitled 'anthropologist'[3]

Theoretical or philosophical basic principles	(a) Charles Darwin's evolutionary theory[4] (b) Eighteenth century's Enlightenment[5] (c) The Renaissance[6]
Differential perception of 'other cultures'	Antiquity[7]

In so far as each pair of elements is sufficient motivation, there is a logic to this synopsis. Nevertheless, the arbitrary is also a potential dilemma, since a science cannot come into being at random. This contradiction between the seemingly logical and the arbitrary is suspended if, within the long story of informal anthropological discourse, we distinguish two phases.

First, a *preconceptual phase,* lasting from ancient times until the mid-nineteenth century. During this period, various predicates of what we understand today by 'anthropology' occurred, but this happened disparately and more or less at random. However profound, none of them could serve on their own as a substitute for the whole pattern of predicates which make up the (modern scientific) *concept* of anthropology. Contingent factors—both logical and chronological—did not allow them to merge into a disciplinary pattern.

Second, the *conceptual phase,* which began in the mid-nineteenth century and still continues; in fact, this phase encompasses the infinity of time, because human knowledge has its end in an ultimately intangible absolute truth. In this second phase, anthropology truly acquired its own identity. All its predicates or determinations flocked together and formed a disciplinary pattern (note: in Latin, *concipio, -ere, -cepi, -ceptum* means, *inter alia,* 'to take together'). A main characteristic of this phase is that, in Hegelian terms, 'the concept corresponds to its object, and the object corresponds to its concept' (Hegel 1988:62, 65). This correspondence is not at all uniform: it improves with the advancement of the discipline. One can even assert that the development of any science, anthropology included, involves a closer correspondence between its object and concept—aspiring to perfection in the ideal state of spirit. However, once this state is reached, knowledge itself would be closed.

This movement of becoming which underlies the relationship of the object to its concept also connects anthropology to humankind. During the whole preconceptual phase, the relation of anthropology to humankind was like that between two unfulfilled entities. Their relation acquires authenticity only in the conceptual phase. Therefore the

statement that humankind is the object of anthropology must be read as follows: *the concept of humankind is the object of the concept of anthropology*.[8]

The division preconceptual/conceptual, introduced here, distinguishes between the purpose and the development towards this purpose. This division is not hierarchical, and does not contribute to knowing one phase or the other. In the last analysis, the hierarchy vanishes even upon the ontological level. As Hegel says, 'the thing is not exhausted in its purpose, but in its progressing, and the result is not the real whole either, but only together with its becoming' (Hegel 1988:5).

THE GREAT DISCOVERIES: ONLY GEOGRAPHICAL?

The interval between the mid-fifteenth and mid-seventeenth century was commonly known in many written sources as the 'age of great geographical discoveries' (Maghidovici 1959:139–494; Otetea 1969:157–60; Langer 1972:383–91; Davies 1979:1039–42, etc.). It is a commonplace that anthropology proper is an aftermath of this crucial but complex event (Shapiro 1964; Hallowell 1965). The main protagonists were plenty of motley travellers: sailors, missionaries, colonists and (later) scientific explorers; but the ramifications affected the whole spectrum of human behaviour.

The economical and political echoes of the great geographical discoveries are well known. As Wallerstein (1974) has pointed out, 'world economy' originated in the sixteenth century, through the emergence of capital as 'the expression of infinite into the economic domain' (Papu 1967:25). By and large, all that concerns a world-scene or world-system—from the material to the spiritual dimensions of man's social existence—invokes this epoch directly or indirectly. One can further exemplify this by examining the science of international law, founded by Hugo Grotius at the beginning of the seventeenth century, or the idea of *Weltliteratur,* promoted by Schiller and Goethe, or philosophy of history, inconceivable before but flourishing after the planetarization of history (and having Vico, Voltaire, Turgot, Herder and Hegel as brilliant figures).

Two further examples illustrate the stretched and subtle effects of broadening the geographical horizons. Modern art of the twentieth century, the first example, adopted from primitive art (i.e. from the art of newly discovered peoples) images, themes and technical procedures.[9] The painter Paul Gauguin perfectly applied to his domain the method of participant observation, as if he had learned it from a

handbook of modern anthropology! The second example is epistemological, referring to the problem of the rationality of science. From Plato to Frege, the rationality of science came to be judged by its conformity to the formal criterion of logico-mathematical coherence and systematicity. In opposition, Thomas Kuhn, Stephen Toulmin and other contemporary philosophers emphasize the *historicity* and *social context* of theories. Toulmin especially (1972), imbued with anthropological ideas, has written explicitly that geographical discoveries represented a complex challenge against the absolutist way of thinking. 'It took the circumnavigations of such men as Cook and Bougainville and the reports of European travellers to South and East Asia to open up the last and remotest part of the globe, so displaying the full spectrum of human variability' (1972:47). The scientific constructs themselves (concepts, theories) belong to the same spectrum of variation. This induces Toulmin to conclude that: The burden of "rationality" then consists in the fundamental obligation to continue reappraising our strategy in the light of fresh experience' (1972:503).[10]

These shifts in both art and philosophy exemplify the numerous conceptual changes caused by this global expansion. Terrestrial space became a stage where the whole drama of the human species is acted out. And, as in any great performance, the stage (with its dimensions and arrangements) affects all that happens upon it.[11]

Thus the so-called 'great discoveries' were geographical, but not only geographical. They were geographical because of the invincible presence of space, which, along with time, is an existential category. But the true determination of the great discoveries is twofold, namely *geo-human*. If we can speak about the substantial changes the great discoveries wrought on human destiny, it is surely because those discoveries from the outset had a human component. This component deserves to be included in the label. The complete denomination of the events we are dealing with is *great anthropogeographical discoveries*. It was not merely new lands, but equally new populations and cultures that were brought to the light of human perception, centred at that time in the European zone of civilization. As a matter of fact, it can hardly be upheld that scientific geographical interest was the prime mover of the famous travels: 'In the 100 years from the mid-15th to the mid-16th century, a combination of circumstances stimulated men to seek new routes; and it was *new routes rather than new lands* that filled the minds of kings and commoners, scholars and seamen' (Davies 1979:1039; my italics). A new route to India and China was at stake. The old overland ones were obstructed by the instability of the Mongol empire as well as by the ascent of the Ottoman empire. Those routes—

either old or new—were initially commercial, possibly missionary. Only later on did purely scientific interest prevail, or in any case grow considerably in importance: ‘While, as in earlier centuries, traders and missionaries often proved themselves also to be intrepid explorers, in this most recent period of geographical discovery the seeker after knowledge for its own sake played a greater part than ever before’ (Davies 1979:1042).

THE RULE OF COMPLETENESS

It is easier now to realize that anthropology could not reach its conceptual phase until its object of study—mankind—had been integrally embraced in the *perceptive pattern* of the knowing mind.[12] This was not the only discipline in this situation.

The first comparative example is—as stands to reason—geography. What did this science appear to be in the days of Ptolemy? Then geography was a sum of naive descriptions which could not travel beyond the Mediterranean world. Alexander the Great, through his military campaigns, moved the lines of the ancient horizon to a distance far from the Mediterranean shores. Geography was but an ancilla, a sort of ‘mesography’ (Mehedinti 1943a:127)—an environmental description—in the service of either military men, or merchants, or (at best) historians. This situation was of long standing. Magellan’s expedition around the globe only formally demonstrated that geography is able to offer—in Mehedinti’s terms—‘hologeic’ and ‘holochronic’ descriptions (i.e. to observe a phenomenon over the whole planet and in its whole evolution through time). A few centuries were to elapse before that ability was actualized.

Another suggestive example is biology. It is an inescapable truism that the cornerstone of modern biology was that the theory of the evolution of species applied to humans. It is generally accepted that this revolutionary idea was systematically formulated by Charles Darwin. However, in his early youth, Darwin shared the commonly held belief that species were immutable. The idea of evolution germinated in his mind during his five-year voyage round the earth, on board the *Beagle*. As a naturalist, Darwin used the occasion to observe many species of plants and animals, both living and fossil. He could not see, of course, all the species, but surveyed a representative sample of them. No wonder the voyage on the *Beagle* was assessed by Darwin himself as ‘the most important event’ in his life (1958:76), and by Penniman

(1974:56) as 'the great event in the biological world, and one which was to revolutionize all the sciences'.

Now let us turn to a discipline whose object of study is absolutely free from geographical space: chemistry. Chemistry owes its modern existence to the order revealed by Mendeleev's *Opyt systemy elementov* (Outline of the System of Elements). The idea of completeness is clearly implicit. The famous table was, from the very beginning, composed with the purpose of containing not only all elements known at that moment, but also of presenting the places of those elements yet to be discovered.

It is not desirable to exaggerate the meaning of these facts, but one cannot ignore their convergence either. A conditioning rule seems to preside over such a crystallizing moment in the history of particular sciences, namely: *perceiving the whole object of study is a prerequisite for the genesis of a scientific discipline*. Let us call it the *rule of completeness*. Obviously, the term 'completeness' refers to the perception of the object of study and marks the moment when a science goes into its conceptual phase.

Physics seems to escape our rule. Physics appears like a protean assemblage of different theoretical bodies—classical mechanics, thermodynamics, optics, nuclear physics, etc.—rejecting the strategy of a single base and of a unitary task. There was, however, a widely shared opinion (by Huygens, Hertz, Kirchhoff, Helmholtz, Wundt, all quoted in Nagel 1961:154–5) that mechanics must be the most general, perfect and basic physical science. Therefore, as Hertz declared, 'the task of physics is to reduce the phenomena of nature to the simple laws of mechanics' (Nagel 1961:154–5). When, at the beginning of the twentieth century, the shortcomings of classical mechanics were pointed out, the same task was entrusted to the theory of relativity. But, as Einstein himself underlined (1992:108),

> Although today we know positively that classical mechanics does not offer a satisfactory background to the whole of physics, it continues to lie at the core of our whole thought in physics. The motive is that despite our considerable advancement since Newton, we have not yet reached a background of physics from which to be sure that all complexity of the investigated phenomena could be logically deduced.

Or, classical mechanics, this branch of knowledge to which the greatest physicists turned as their disciplinary ideal, was built by Kepler, Galileo, Newton, by extending physical notions and laws from the

nearest environment to the heavenly bodies, and even to the infinity of space. Why could one not see the infinite as the absolute dimension of completeness?[13]

As soon as man is 'a vital constituent of the earth-space pattern' (Bird 1989:335), anthropology is inevitably marked by the rule of completeness. Human groups—races, varieties, ethnicities—have been disseminated since remote times all over the planet. And by its diversity in configuration—mountains, hills and plains, small islands and continental masses, arid and fertile regions, etc.—geographical space has contributed much to the great diversification of humankind in biological and, especially, in cultural categories. The long distances, harsh climate and, not least, the low level of human technical knowledge kept many populations and their cultures outside the integrative perception centred within the European area. Practically, until the great anthropo-geographical discoveries, humankind had not yet seen all its faces and images; it had not yet perceived its image as a whole.

However, where anthropo-geographical perception was incomplete, the perceiving mind completed it with an unbridled imagination. Strange *horror vacui*! Where he had not planted his foot and his sight had not reached, man peopled space with all kinds of monsters. Travel literature swarms with such representations. Among the ancient authors, Herodotus (1961:3, CXVI, and 4, XXVII) reported from indirect sources (which, however, he did not swear to) that Arimasps (one-eyed people) lived in northern Europe. In the seventh century, Isidorus of Seville (1970:574) wrote in his *Etymologies: 'Hyppopodes in Scythia sunt, humanum formam et equinos pedes habentes* ('There are hyppopodes in Scythia, [beings] with human face and horses' legs'). Fantastic images proliferated during the Middle Ages, spread sometimes even by travellers. Langness (1980:6–7) reprinted from Hartmann Schedel's *Liber Chronicarum* of 1493, the *Prodigiorum* of Lycosthenem of 1557, and Edward Fenton's *Certaine Secret Wonders of Nature* of 1569, wood-cuts representing people with dog-like heads, acephalous people with their faces on their breasts, people with ears extending down to the ground. An episode in Shakespeare's *The Tempest* (III, 3) reveals how these images echoed in the public imagination:

Sebastian: Now I will believe
That there are unicorns; that in Arabia
There is one tree, the phoenix's throne; one phoenix
At this hour reigning there.

Antonio: I'll believe both;
And what does else want credit, come to me,
And I'll be sworn 'tis true: travellers ne'er did lie,
Though fools at home condemn 'em.

We should not insist on details which are marginal in our context. A large spectrum of this distorted world accompanied by exact sources and quotations and followed by historical or aesthetical comments can be found in some basic works about medieval mentality (Papu 1967; Baltrusaitis 1955; Le Goff 1978, 1985). Other points are of primary interest in our discussion. Why, for instance, did the extension of perception move from realistic images to fantastic ones? In other words, why did not a simple extension of already verified perception occur? In fact, the dissemination of fantastic images (both anthropomorphous and extra-anthropomorphous in their basic structure)[14] expressed not only a shortcoming of knowledge, or a simple *horror vacui,*[15] but also an exuberance of man's imaginative faculty. When examining them, one must have in mind Gilbert Durand's appreciation that the imaginary is 'the mark of an ontological vocation' (1977:533).

Another problem arises from the difference in time between the moment of closing the perceptive anthropo-geographical circle and the transition to what we have called the 'conceptual phase' in the history of anthropology. Three and a half centuries passed between Magellan's expedition around the earth and Tylor's modern anthropological definition of culture. This signifies that single perception does not yet deliver the 'concept' and neither does it automatically express the 'self-consciousness' of an entity, being—as in the Hegelian phenomenological scheme—only a step towards this purpose.

ANTHROPOLOGY: THE SCIENCE, SELF-CONSCIOUSNESS AND REASON OF HUMANKIND

As everybody will have noticed, Hegel's ghost has been hovering from the outset over this chapter. His philosophical view deserves special attention as a possible way of understanding the history of anthropology.

Hegel is oracular, both in his mysterious (often obscure) style and in his revealing ideas and sentences. In spite of the risks, the approach here attempted is inwardly justified. As has been said, 'Hegel's world is a system of existential forms, where, no matter the level of reality, he finds the endeavour of the inferior to the superior, and, in the last

analysis, the endeavour of all forms towards the concrete fulfilment of Mind—end and simultaneously propulsive mover of the becoming' (Rosca 1967:93). Or, both humankind and *anthropology* may be taken for such 'existential forms'.

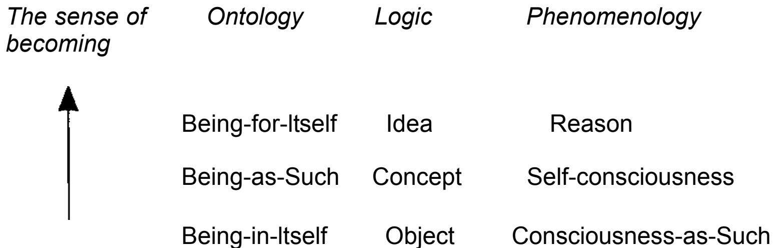

Figure 3.1 Lines of becoming in Hegel's philosophical system

Figure 3.1 is made up of those aspects of Hegel's general philosophical scheme suitable for reflecting upon the becoming of the two entities. The schemes' rigidity (in any case, the cliché of three-time thinking, which could evoke the unpleasant episode of unilinear evolutionism) should not be the focus; the content of the view and the enlightening force of some terms and phrases should be. It is highly suggestive, for instance, to review the concept of 'man' as it was split in the categories of 'primitive' and 'civilized' (perhaps the most provocative anthropological consequence of the great anthropo-geographical discoveries), in the light of the triad: Being/Being-as-Such/Being-for-Itself (in the original: *Sein/Dasein/Fürsichsein*) (Hegel 1989:182–209). One may overlook Being (more exactly Being-in-Itself) as an empty abstraction. Furthermore, as a Being-as-such, humankind receives some determinations: 'In so far as man truly wishes to be, he must be as-such, and, for this, he must limit himself *(sich begrenzen)*' (Hegel 1989:197). Of course—we shall note—humanity is infinite in its spirit, but this does not absolve it from knowing its inward limits. These limits are either quantitative (e.g. population size, anthroposphere) or qualitative (races, ethnicities, cultures, etc.). The distinction between so-called 'primitive' and 'civilized' functions like a limit (in fact, a limit of limits) of the second category. But all the limits are surpassed in the process of becoming; thus, the Being-as-such passes into the Being-for-Itself, which is the 'accomplished quality *(vollendete Qualität)*' (Hegel 1989:203). To discuss the act of surpassing, Hegel used the verb *aufheben,* a word (as the great dialectician underlined himself)

with two opposed meanings: 'to deny', 'to abolish'; 'to keep', 'to preserve'. In our case, the *Aufhebung* of the limit between 'primitive' and 'civilized' might be understood, on the one hand, as preserving their particular attributes, and, on the other, as abolishing the value discrimination between them. This principle of cultural relativism, expresses the *Fürsichsein* of cultures and accomplishes the *Fürsichsein* of humanity.

Another interesting theme for reflection is the becoming of 'humankind' and 'anthropology' phenomenologically. In this perspective, all steps that enlarged the perceptive horizon on humankind, as well as any new operational term, action or research method—e.g. the appearance of the term 'anthropology' (introduced in 1501 by Magnus Hundt), the first participation of an entitled 'anthropologist' in an expedition, the trajectory of the 'culture' concept from Klemm's definition to the synthesis made by Kroeber/Kluckhohn, etc.—may be considered as moments when our discipline was on the way towards its *self-consciousness*.

These suggestions are meant to explore the possibility of reconstructing the history of anthropology on a Hegelian basis. This reconstruction seems to be possible through the three lines of becoming: *ontology, logic* and *phenomenology*. They become more and more mutually resonant, so that, in the last stage, Idea, Being-for-Itself and Reason are the same entity. In this project, humankind and anthropology—their becoming—could be pursued separately as two existential forms. Nevertheless, simply through its existence, anthropology adopts 'humankind' as its object. By assuming an object, an entity becomes the consciousness of that object. The becoming of humankind towards self-consciousness represents a process of knowing which is anthropological knowing: anthropology itself. Through the process of their becoming, humankind and anthropology melt more and more into each other, serving as a unity of objectivity and subjectivity in the universality of reason. As the science of an object endowed with consciousness, anthropology even becomes the selfconsciousness of that object and, in the ideal stage, its reason. Therefore anthropology in the Hegelian sense is: the *science, selfconsciousness* and *reason* of humankind.

NOTES

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1 As a matter of fact, nobody has started off the history of social/cultural anthropology with Tylor, but, as a pure possibility, it would not be beyond any logical order. Perhaps Robert Lowie was not indifferent to this motivation when he opened his classical historical work (1937) with Gustav Klemm, who was quite near (in time and conception) to Tylor in defining culture.
2 'Henry Schoolcraft deserves recognition as the first true fieldworker in the science of social anthropology' (Hays 1958:41; and really the first chapter of Hays's 'Informal History of Social Anthropology' is entirely devoted to Schoolcraft).
3 See Hewes (1968).
4 'When I knew that the first edition would appear in 1935, I found that the title *A Hundred Years of Anthropology* was very apt, as 1835 was the year when Darwin visited the Galapagos Islands and discovered the key to his future researches which were to culminate in *The Origin of Species* in 1859' (Penniman 1974:12). See also Kardiner & Preble (1966), in which the first biography among those which follow each other chronologically is Darwin's.
5 'All that is new in anthropological theory begins with the Enlightenment' (Harris 1971:9); and also: 'The beginning of social science, the generlized base on which both anthropology and sociology would be erected seems best located in the eighteenth century' (Voget 1973:7). Radcliffe-Brown's attempt to link the origins of anthropology to the Enlightenment of the eighteenth century (see Barnard 1992) is an insufficiently founded desire, that is a subjective fact, explicable by his aspiration 'to validate his brand of anthropology, and his position within international anthropology generally, with reference to the greatest intellectual milieu of the modern world' (Barnard 1992:14). As a matter of fact, all schools of anthropology put fieldwork at the centre of the discipline (Jarvie 1967:223). The ideas of human nature and progress, taken over from the Enlightenment, were merely the ideological base of what will be anthropology proper in the nineteenth century.
6 See Rowe (1965). It was also revealed that 'social anthropology…does not begin until the sixteenth century, when a substantial body of travel accounts became available', and, moreover, social phenomena ceased to be considered as 'supernaturally caused' (Slotkin 1965:XIII, 1).
7 'The Bible, Homer, Hippocrates, Herodotus, Chinese scholars of the Han dynasty—to take only some of the more obvious examples—showed an interest in the distinctive life-ways of different peoples' (Kroeber and Kluckhohn 1963:3–4).
8 When saying that humankind is the object of anthropology, we establish a *cognitive* (not logical) relationship between the two entities. This relationship existed throughout the whole history of anthropology, without interruption. Even the presumed discontinuity between ancient Greek-

Roman and Renaissance traditions in the ethnographic domain was seriously refuted (Hoffman 1973).

9 According to Robert Goldwater (1974:29), modern art is based on three related domains: primitive art, prehistorical art and children's art products.

10 It is an open question why some echoes, like those in art and philosophy of science, occurred so late. First, a common answer: both of them were conditioned beforehand. Then, particularly speaking, in modern art such an echo depended on the training of aesthetical taste for the primitives' products, as well as on the sufficient accumulation of those artistic products in ethnographical museums; moreover, in the Renaissance (which is contemporary to the great geographical discoveries—see Papu 1967), the artists' fascination with the ancient Greeks was too fresh and could not be abandoned so quickly. As to philosophy of science, it had to wait previously for the mathematization of modern sciences. The first and persistent echo which the newly discovered world had in philosophy was the launching of the notion of *le bon sauvage* (Mouralis 1989), but this was a moral notion, not an epistemological concept.

11 This syncretism of events and framework was expressed by Mehedinti in his theory of the geographical phases of history (1943b:308–19). For thousands of years, Mehedinti says, people lived in the 'continental phase'; the few centres of civilization scattered over the globe (European region, Mesopotamia and Egypt, Trans-Saharian Africa, South and East Asia, Mexico, Peru) existed in a total lack of mutual communication. The second was the 'oceanic phase', in which the technical inventions enabled expeditions on water over long distances. In addition, the seas and oceans have an international regime that also facilitated travels. It is over water that man for the first time succeeded in circling the planet. Finally, the third phase is the 'aerial' one. It was inaugurated at the beginning of the twentieth century by aeroplane flights. It is notable, regarding this phase, that 'air is more international than water' (Mehedenti 1943b:308–19).

12 The phrase 'perceptive pattern' evokes the concept of *habitus perceptuel,* coined by Pierre Bourdieu and meaning an ensemble of schemes on which scientists' action and perception rely. Luc Gauthier, to whom I owe the revelation of this concept, achieved an application of it to the astronomers' scientific practice (Gauthier 1992). His particular analysis could also be suggestive for other scientific disciplines, including anthropology. In this regard, as far as the perception on mankind was not complete, or, as we shall see, was imbued with fantastical elements introduced by laymen, one may not speak of a proper anthropological perceptive habit. It seems that the concept of 'perceptive habit' is linked to that of 'scientific community' in T.S.Kuhn's view (1964); or, one can hardly speak of such an institutional framework regarding anthropology until the nineteenth century. Thus, it is proved once again that 'anthropology is a very recent development in the intellectual history of Western culture' (Hallowell 1965:24).

13 It is a wonder that in his theory of scientific change Thomas Kuhn did not pay attention to the moments when the scientists' perception is extended over new regions of reality. It is true that he took into account 'new instruments' and 'new places' as changing factors in new scientific paradigms. But in his view, a new *Gestalt* (a new perception of the world)

is merely a rearrangement of the same old constitutive elements (1964:110ff).

14 The fantastic images not only include human forms, but also animals, plants, and even the realm of the lifeless (see Le Goff 1985, esp. his chapter on the 'medieval marvellous').

15 Edgar Papu (1967:69ff) speaks about a 'fantasy of ignorance', as opposed to the 'mythical fantasy'. While the latter is suggested and nourished by real phenomena (e.g. sirens by the whirlpool between Scylla and Charibda), having, therefore, a metaphorical existence, the fantasy of ignorance has nothing to do with functional reality.

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4 Enlightenment and Romanticism in the work of Adolf Bastian

The historical roots of anthropology in the nineteenth century

Klaus-Peter Koepping

QUESTIONS IN THE HISTORY OF ANTHROPOLOGY

Many writers on the history of the discipline like to set the start of truly modern anthropology with either Boas or Malinowski (see for example Evans-Pritchard 1951; Hymes 1969; Bauman 1973; Kuper 1983; Stocking 1987). This view is justified if by modern we mean only the emphasis on empirical fieldwork in depth within a single socio-cultural group. Evans-Pritchard formulated it cogently:

> The viewpoint in social anthropology today may be summed up by saying that we now think we can learn more about the nature of human society by really detailed intensive and observational studies, conducted in a series of a few selected societies with the aim of solving limited problems, than by attempting generalizations on a wider scale from literature.
>
> (Evans-Pritchard 1951:91–2)

The shadow of Durkheim as the theoretical godfather certainly loomed as large for Evans-Pritchard as is implied in Bauman's critical assessment of the Durkheimian 'vaccine injected into the blood of the modern study of culture in its infancy…by its midwives, Malinowski and Boas' (Bauman 1973:45). Modern anthropology also depends upon the tenets of empirical validation conventionally labelled participant observation. Less clear is the resolution of the Boasian 'eternal tension' between 'seeking to subsume a variety of them [phenomena] under a general law' and 'seeking to penetrate the secrets of the individual phenomenon' (Stocking 1987:xvi).

Our problem is merging two seemingly incompatible epistemological paradigms: the subjective and objective, the comparative and the unique, the inner view and the outside analysis,

the general and the particular. Did these tensions really arise between the 1890s and the mid-1920s? Can all criticism of as well as homage to this yoking together of opposite epistemologies be laid at the feet of the two historical icons, Boas and Malinowski?

As I shall show, this kind of study, combining the universal human trait of the creation of culture with the malleability and variability of human nature as shown through the expression of different and specifically unique cultural manifestations, was already present in the mid-nineteenth century. It is doubtless a result of the convergence of diametrically opposed currents of thought: the Enlightenment urge for generalized statements about generic human nature, and the Romantic attempt to salvage the importance of cultural creations as unique expressions of specific collectivities. These two currents, of the universalism of rational criteria for comparison and of the particularism of unique creativity (the Romantic notion of genius in its collective forms), re-emerged in different discourses throughout the nineteenth as well as the twentieth century—about nature and nurture, about natural and cultural evolution as well as about structuralist and interpretationist approaches.

I shall trace this fusion of the twin impulses of the Enlightenment and Romanticism in the work of Adolf Bastian (1826–1905). Bastian's work is a focal point because his first written statements appear at the same time as the nature-nurture controversy. This debate had already occupied the eighteenth-century savants (see Voltaire) and resurfaced in this period through the scientific publications of Darwin. Bastian can be considered a pivotal link for the emergence of modern anthropology as his work, though only marginally mentioned in recent histories of the discipline, exerted a considerable influence on the works of later 'founders' of the anthropological enterprise. Tylor, as well as Boas, was aware of considerable portions of Bastian's works, particularly the notion of the 'psychic unity of mankind' (see Tylor 1865:378; Tylor 1871; Boas 1911:43 and 154ff.). The variety of anthropological influences on such modern founders as Boas has to a large extent been demonstrated (Kluckhohn and Pufer 1959; Stocking 1974; Koepping 1983:124ff.). Yet why the epistemological conundrum as inherited from Enlightenment and Romanticism appears in all its acuteness by the middle of the nineteenth century is less well understood.

After showing the relevance of anthropology's double aims in its modern foundations through the figures of Malinowski and Boas, the ensuing discussion will centre on the expressions which the Enlightenment and the Romantic impulse found in Bastian's writings.

MALINOWSKI, BOAS AND MODERN ANTHROPOLOGICAL SENSIBILITIES

Both Boas and Malinowski's statements reveal the inherited tensions. While Malinowski maintained at one time that 'to grasp the native's point of view, his relation to life, to realize his vision of his world… we have to study man, and we must study what concerns him most intimately, that is, the hold which life has on him' (1922:25), he later insisted on the following requirement for anthropological comparative work: 'The principles of social organization…have to be constructed by the observer out of a multitude of manifestations of varying significance and relevance' (1935, I:317).

One may, as Leach has done, charge Malinowski with epistemological naivety (Leach 1964:134). Yet even those authors who facetiously challenge the paradigm of the 'inside view' as a form of 'ventriloquism' (see Geertz 1988) admit to the twin vision of anthropology, the necessity of merging the micro- with the macro-view. 'We are the miniaturists of the social sciences, painting on lilliputian canvases with what we take to be delicate strokes. We hope to find in the little what eludes us in the large, to stumble upon general truths while sorting through special cases'(Geertz 1975:4).

The epistemological conundrum is clearly stated in these remarks: are we really able to accept both the rationality of scientific construction and the relativity of world-views? Do we really stress differences—at least on an intellectual level—while looking for the universal? The more theoretical aspect of this very same question is voiced by Boas as a goal of anthropological work: 'Which are the social tendencies that are general human characteristics?… Thus a critical examination of what is generally valid for all humanity and what is specifically valid for different cultural types comes to be a matter of great concern to students of society' (Boas 1940 in 1968:261).

Anthropologists may have contributed to an emerging awareness of the precariousness of the empirical method, along with contemporary sociological and philosophical epistemologists during the first third of this century. However, it does appear that combining inside and outside views was a hope posited without much reflection. Anthropology had to wait for this pointed reminder in Lévi-Strauss's despairing personal notes, who puts the problem on the existential rather than the epistemological level: 'Either the anthropologist clings to the norms of his own group, in which case the others can only inspire in him an ephemeral curiosity in which there is always an element of disapproval; or he makes himself over completely to the objects of his studies, in

which case he can never be perfectly objective, because in giving himself to all societies he cannot but refuse himself…to one among them' (Lévi-Strauss 1971:381). Lévi-Strauss's 1955 statement epitomizes what in more recent times has been called the 'predicament of culture' (Clifford 1988), which I take to be really the predicament of the anthropologist.

THE MID-NINETEENTH-CENTURY FUSION OF TWO CURRENTS IN ANTHROPOLOGICAL ORIENTATION

The 'anthropologization' of the world took place through the systems devised by the savants of the eighteenth century (Evans-Pritchard 1951; Foucault 1966; Harris 1968; Diamond 1974; Lepenies 1976 and 1988). The Romantic Johann Gottfried Herder's firm rejection of the mindless and mechanical external classifications of French Enlightenment thought, and his plea to judge each time and culture by its own canon of values, brought about the new emphasis in anthropology on specific cultural configurations as collective expressions of a 'folk'. The more general question about the nature of culture *sui generis* remained. The tension continued surrounding connections between nature and culture on the one hand, and between the diverse synchronic and diachronic manifestations of culture on the other. There is a considerable gap between Herder's work and Bastian's first publications in 1860. Intervening important writers throughout the humanities spread the message of the Romantic revolt after 1800, from folklore and mythology studies (Jacob Grimm; Lazarus) to linguistics and folk psychology (Steinthal), from legal studies to historiography, from literary criticism (Friedrich and August Wilhelm Schlegel) to philosophy (Schelling and the Neo-Kantians), to mention but a few of the important intellectual currents of the first half of the nineteenth century in the humanities (Koepping 1983:77–94). But the key methodological impulse encapsulated in the above-mentioned existential quandary of Lévi-Strauss, namely the impossibility of committing oneself to a singular culture without denying all other cultures, appears clearly in the following outburst of Herder:

> Admittedly, we could derive from it all the common-places about the right and the good, maxims of philanthropy and wisdom, views of all times and peoples for all times and peoples. For all times and peoples? That means, alas, precisely not for the very people whom the particular code of law was meant to fit like clothing.
>
> (Herder in Barnard 1969:201)

Herder had earlier seen the epistemological problem of combining the two modes of generalization based on classificatory principles and of giving one's due to the creative genius of each unique cultural or historical production: 'Nobody in the world feels the weakness of general characterization more than me. One depicts a whole people, era, region—what has one depicted?… Who has not noticed what an inexpressible matter is the uniqueness of a person: who is able to speak with discernment about difference?' (Herder's *Journal* of 1769, in Herder 1976:36, my translation); and further: 'Each estate, each form of life, has its own customs' (1976:27).

It only needed the fieldworker, the first-hand collector of data on the diversity of customs, to combine Herder's critique of the Enlightenment with the Romantic revolt which gave uniqueness its place. Adolf Bastian was the scholar who proposed a programme of anthropology similar to that endorsed by Boas and Malinowski in this century.

The most direct connection can be established between Herder's idea of the importance of the social formation of an ethnic group *(Volk)* and its animating impulse in the folk-soul *(Volksseele)* with what Bastian was to call the 'soul of society' *(Gesellschaftsseele)*. This socio-psychological concept of the collective mind finds its objective expressions in material culture as well as in art, religion or legal custom. Once elaborated it becomes, as I shall show in more detail, the basis for those 'folk-ideas' *(Völkergedanken),* which are but the diversely patterned forms of collective representations *(Gesellschaftsgedanken),* expressed in their culturally unique formations. Bastian takes his cue from Herder whom he quotes approvingly: 'Though complete in itself, the individual is endowed in such a way that it can reach the highest form of actualization when it fits itself into a totality, as a fulfilment of its destiny, because the individual is a means and an end both for itself and also for higher purposes' (cited in Bastian 1900:119). He had much earlier found the adage 'It thinks in us' (Bastian 1868:1).

The main problems of the dichotomous approach of anthropology—the scientific and the humanistic, the positivist and the hermeneutic—were discussed by Bastian. We may nowadays criticize the hasty, almost breathless, execution of the programme as well as his inept writing which either undermines his good intentions or makes it almost impossible to judge how far he succeeded in fulfilling his theoretical goals. As he wrote his first three-volume compendium in 1860, he would normally be incorporated among the many cultural evolution prophets of the nineteenth century (the controversy has been raised by

Bidney 1968 and critically discussed by Koepping 1983). However, Bastian had a practical and healthy scepticism towards simplistic evolutionary progression theories. His five journeys around the world, his erudition as well as his scientific and medical training, all contributed to this scepticism.

He was trained as a medical practitioner, and taught the fields of evolutionary biology, comparative anatomy and physiology by his lifelong friend, the pathologist Rudolf Virchow. Bastian employed his grasp of nineteenth-century scientific knowledge towards analysing cultural data and the vast diversity of the 'collective representations' of groups, what he called anthropology's main aim (his *Gesellschaftsgedanken*). This abstraction was balanced by his demand for first-hand field investigations into the manifold manifestations of the collective representations in the form of collective 'ethnic idea frames' or his *Völkergedanken*.

BASTIAN AND THE ENLIGHTENMENT VISION: THE LINK THROUGH ALEXANDER VON HUMBOLDT

Reading Bastian's turgid prose, one becomes aware of the two strains of our anthropological ancestry: the Enlightenment of mainly French persuasion (on the precepts of English seventeenth-century philosophy as well as on the base-line of Descartes) and the Romantic movement as a conscious German counter-movement (led by Johann Gottfried Herder, who quite openly relied on English predecessors, such as Shaftesbury or many of the Scottish representatives of the Enlightenment). To appreciate the marriage of these two disparate streams in Bastian's scientific anthropology it is necessary to summarize and to delve into these two positions.

The French Enlightenment provided the scientific, even scientistic, orientation towards human phenomena which pervaded positivism (and its offshoots evolutionism and Marxism) from the nineteenth century onwards. Both human affairs and nature were believed to be governed by the same kind of laws. These rules of necessity could be discerned by applying the power of reason. Cartesian rationalism and English empiricism became the two ruling paradigms for the study of society. Finding the natural laws of society (and of religion, education etc.) would enable people, who were seen as infinitely perfectable, to attain a better state of existence through steady progress. Reason replaced authority and tradition as the principle governing human conduct. Montesquieu exemplified this when he stated: 'Man, being a physical being, is, like other bodies, governed by invariable laws…. It is of

course essential that the intellectual world, the world of the mind, should be as well regulated as the physical world' (Montesquieu quoted in Hazard 1965:375).

What may seem a splendid vision when put into Kant's terms—where Enlightenment is paraphrased by the adage of Horace, *sapere aude,* 'dare to think'—becomes, in the writings of the French savants, a threatening vision of man as machine, a totalitarian nightmare of education and control. Interestingly, this joyless vision of human affairs, with its presumed lawlike conformity to universal progress through rationality, had but a short life.

The Enlightenment tended to assume liberty and equality were ruled by the laws of reason, on the one hand, and by the rules of etiquette, property relations and hierarchies of natural organization on the other hand. This vision became the paradigmatic presupposition for the foundation of the social sciences (see Koepping 1982). Through Comte and Durkheim, the conservative stream of the Enlightenment engendered the following propositions:

1 Society can be studied through the application of the scientific method, and its lawful empirical results can and should be used to improve the state of society;
2 Society rests on organization and hierarchy for survival (these conservative visions were shared by Turgot, Holbach, d'Alembert, Voltaire and Condorcet).

There was a more radical stream among the savants, represented by Saint-Simon, Rousseau, Diderot and Helvetius, many of whom believed in the possibility of true equality through education. Their ideas were to influence Marx as well as English liberal reformers such as John Stuart Mill. All of them, radicals as well as conservatives, in the period between the Enlightenment and the mid-nineteenth century, believed in laws underlying human conduct, in progress, in universals and in the empirical accessibility of human reality.

The Enlightenment played out in a modern discourse the very same contradictions which have beset the notion of natural law since antiquity. The conservative stream was represented both by Plato, in regard to hierarchical organization and totalitarian control of social life, and by the Stoics, who believed in the universal laws of necessity (though in circular form) in nature, society and the mind. Liberal streams, such as the Sophists, interpreted natural law as an indication that customs which are socially created could be overthrown.

Bastian received this influence of the Enlightenment vision directly through one of his teachers, Alexander von Humboldt (1769–1859;

Bastian spoke at his funeral; Bastian 1869). His vision of the natural and human world which Bastian was to adopt and cite is worth quoting: 'we hope to find laws which regulate the difference of temperature and climate…before we can hope to explain the involved causes of vegetable distribution; and it is thus that the observer…is led from one class of phenomena to another, by means of the mutual dependence and connection existing between them' (Alexander von Humboldt 1844:viii).

It would be but a small step from this to connect the kingdoms of nature and humanity, and to apply insight from one to the other. Alexander's brother, Wilhelm, summed up these expectations and became at the same time a prophet for the job Bastian was trying to accomplish. Wilhelm von Humboldt wrote: 'if anybody is able to do it, I would say it is my brother, for he might connect the study of physical nature with that of moral nature, and thus bring the universe as we know it into true harmony, or if this surpasses the abilities of one man, prepare the study of physical nature in such a way that the second step will become easier' (Wilhelm von Humboldt 1836 in 1967:159).

The ultimate vision is one of harmony in nature and society, by equalizing differentials. Bastian shared this belief with Alexander von Humboldt, who summed it up in his *Cosmos* as follows: '[natural harmony] embraces within its wide scope the remotest nebulous spots, and the revolving double stars in the regions of space, no less than the telluric phenomena included under the department of the geography of organic forms, such as plants, animals and races of men' (Alexander von Humboldt 1844:x).

Bastian echoes Humboldt's idea of the universal harmony as the heritage of the best of the Enlightenment optimism (really a scientific eschatology) in the following words:

> Being part of the totality that constitutes the world, man can only perceive those connections through which the world relates to him…. Being juxtaposed in space, all things react to each other …inasmuch as their totality makes up the whole of the universe, they must, as parts, be interdependent…the microcosm is in reality only a mental distillation of the macrocosmic realm in an individuality which is but a part of it. Properly speaking, the mind and the body are one, and together make man. This unity of mind and matter, created anew each moment, is the essence of the nature of man.
>
> (Bastian in Koepping 1983:179, originally 1860)

BASTIAN'S ADOPTION OF HERDERIAN CONCEPTS

The Romanticist reaction against this Enlightenment vision can be summed up in a short quote by Herder, giving the gist of the argument:

> O, that another Montesquieu would come and really offer us the spirit of the laws and governments of our globe, instead of a mere classification of governments into three or four empty categories. … A classification of states, based on political principles, is also of little avail…. Least of all are we in need of a scissors and paste approach, where examples are assembled at random from all nations, times and climates, until we can no longer see the wood for the trees; the genius of our earth as one entity is lost.
>
> (Herder 1774, in Barnard 1969:325)

And even more pronounced:

> This is a time when the art of legislation is considered the sole method of civilizing nations. Yet this method has been employed in the strangest fashion to produce mostly general philosophies of the human race, rational axioms of human behaviour and what-have-you! Doubtless the undertaking was more dazzling than useful.
>
> (Herder, in Barnard 1969:201)

From this Bastian derives his emphasis on the specificity of each cultural creation as expressed in his *Völkergedanken*. The link is made explicitly. Lévi-Strauss, in his 1962 Geneva address for the 250th anniversary of Rousseau, traced the foundations of comparative ethnology to the saying of Rousseau that it was deplorable not to find a savant of the order of Montesquieu or Buffon to study people and their customs instead of stones and plants (cited in Lévi-Strauss 1973, chapter 2). This same demand is uncannily pre-empted by a quote which Bastian takes from Herder (who undoubtedly knew his Rousseau well): 'As Herder said with great amazement, it is about time that, having studied the kingdom of minerals, plants and animals, we make an attempt to understand man.' Bastian then continues with a definition of ethnology: 'Ethnology really is directed towards the study of the "ethnos", the collective representations of social groups or what I have called social thoughts *(Gesellschaftsgedanken)*.' A few lines previously, Bastian referred to his notion of the folk-idea: 'The folk idea itself *(Völkergedanke),* if used merely as a mental crutch, is useless unless it is supported by detailed micro-studies.' (Bastian in Koepping 1983:174

and 175, originally Bastian 1893–4:20, 53, 58 ff.) Thus the notion of variability of cultures and the specificity of each unique ethnic creation go hand in hand with the demand for close observation.

Those who think that research into the subjectively meaningful forms of ideas, the 'native's point of view', was first introduced by Malinowski should read Bastian's thoughts on the study of folk-ideas:

> The main aim for ethnology has become the securing and collecting of these folk ideas.... In contrast to the classical sciences of minerals, plants and animals, the science of man has to take cognisance of the subjective angle, the object being man himself in the subjectively created world of ideas.
>
> (Bastian in Koepping 1983:171; originally Bastian 1893–4:20–1)

This quote shows that Bastian tried to stress the subjective view of social action, and to capture that through a collection and analysis of the idea-systems. For Bastian ideas encompass items of material culture, since, for him, the world of human-made things gives us access to thought processes. Whereas Herder remained a historian, interpreting different cultures on the diachronic axis through written sources, Bastian enacted the same programme by travelling throughout the mid-nineteenth-century world on the synchronic axis with living oral cultures.

Herder maintained there are certain universal requirements for the development of civilized social life. He included the value of *Humanität,* by which he meant the reliance on reason and common sense *(Vernunft und Billigkeit).* Here, perhaps, lies the true source of all modern anthropological dilemmas. There is an insoluble contradiction between relativism, the equal importance and value of all ethnic groups and their expressions in language, art, or social organization, and the axiological demand for a binding universal morality (which for Herder included fraternity, without which liberty or equality would be useless; see Berlin 1980; Spitz 1955). For Herder the common people did not need high-flown philosophical theories, but common sense (Herder, *Werke,* vol. V, 1982:18; *Briefe zur Beförderung der Humanität,* orig. 1793). This reliance on the common-sense philosophies of populations attracted Bastian. This may well have influenced his aim to collect evidence for pervasive folk-ideas which persist in bounded ethnic groups within specific territories.

ELEMENTARY AND FOLK-IDEAS AS TOOLS OF ANALYSIS

Having traced the roots of Bastian's cohesive anthropological programme, with its double roots in Enlightenment and Romanticism, it is necessary to clarify finally the distinction between elementary ideas and folk-ideas and their connection in Bastian's system of and programme for the analysis of cultures. Elementary ideas, *Elementargedanken,* for which Bastian uses also the Stoic term *logoi spermatikoi,* 'pregnant thoughts' or thought-seeds, are never directly observable, but are only indirectly deducible from the plethora of folk-ideas. Elementary ideas are abstract generalizations which cannot be located in real social life. They are hidden in the cloaks of ethnic diversity. Yet, because Bastian starts from the proposition that all humans have an equal intellectual potential (widely quoted as his idea of the psychic unity of mankind), a developmental sequence or a mental process of some kind must lead from elementary to folk-ideas.

The *basic* prerequisite for Bastian is the biologically given mental endowment, equal in all individuals and collectivities, to solve problems. Given that people everywhere tend to hit on the same solutions with monotonous regularity, how can elementary ideas change and vary? Until proven, Bastian did not believe in diffusion, though he never denied it. Ratzel mistakenly implies a denial in a heated and polemic controversy in the 1880s: modern literature on the history of anthropological theories continues this mistake (see Bastian 1873; 1885; Ratzel 1887; Bastian 1894; see Koepping 1983:65ff.).

The forces for changing elementary ideas are of a twofold nature. First, there is an inbuilt potential in an idea which can be expanded to its utmost. Bastian uses the analogy from the natural sciences, in this case from physics, about potential and kinetic energy. Elementary ideas are energy-loaded mental seeds which follow an *entelechetic* law, developing in diverse directions up to an initially given expenditure of energy. Second, there are *external geographical and historical* forces at work: the environment works on ideas and shapes them according to the demands of human survival, while history and/or migration of population groups changes idea-systems constantly.

The notion of *folk-ideas* may nowadays not require much further elaboration, as the concept has become common in the anthropology of ethnicity, identity, the diversity of time perception, on notions of space, of person or any other category. Folk-ideas largely coincide with the modern concept of culture prevalent in American cultural anthropology and the nineteenth century *Kulturwissenschaften*. Both are defined as a

learned pattern of behaviour and thought, shared by an ethnically bounded community and transmitted by enculturative practices. Bastian expresses it in one of his more elaborate definitions thus:

> The object of our study is the ethnically coloured world of ideas, that world of the creations of the Folk Ideas *(Völkergedanken)* which are expressions of the collective social representations *(Gesellschaftsgedanken),* and we are to study them in their variability deriving from their historical-geographic conditions.
>
> (Bastian 1893–4, vol. IV:311)

The concept of folk-ideas can easily be identified as the notion of the cultural repository of an ethnic group and be traced to the Herderian concept of *Volksseele*. The *elementary idea* has remained a largely puzzling concept in both origin and connection to the folk-ideas. To understand Bastian on this point, we need to quote some of the numerous analogies from the natural sciences he uses to explain the concept. He states:

> The physical unity of the species man has been anthropologically established, and as a consequence we now look for the psychic unity of mankind. The psychic unity of social thought underlies the basic elements of the body social. The world over we will find a monotonous sub-stratum of identical elementary ideas.
>
> (Bastian in Koepping 1983:176; originally 1877:183ff.)

Scientific metaphors concerning elementary ideas abound in Bastian's work: they are compared with a nucleus, with a cell, with the simple plant forms of cryptogams (which, as he puts it, are not to be neglected or looked down upon, as all complex plants develop from the simple ferns and mosses), and so on. To put it simply, we can say that elementary ideas possess an innate propensity to change. grow and react to environmental stimuli and historical changes.

As these elementary ideas are reductive analytical categories, it may not surprise us to have so few mentioned in Bastian's whole *œuvre* of over a hundred books. Yet there are striking examples, such as the notion of propulsion instruments by artificial extension of the body, which finds its folk-idea realization in such diverse forms as the bow, spear and spear-thrower. That the bow did not develop among Australian Aborigines does not indicate for Bastian the inferior mental equipment of these ethnic groups. Rather it is the result of environmental factors. Australia has no trees which are flexible enough

to develop the bow. So the spear-thrower and the boomerang represent alternative folk-ideas.

This leads to a further question: where do we find elementary ideas most pristinely, if at all? Here his answer is clear: to find the simplest, least complex elaborations of elementary ideas we must turn to people who apparently had the least historical contact due to geographical isolation. As Bastian puts it:

> Like the physical habitus, so does the psychic habitus carry the imprint of the climatical agents, and thus we find the elementary ideas embedded in their specific milieu. Only when the elementary ideas of the savage tribe come into contact with outside stimuli do they develop their inherent potential through a growth process in historical forms of cultural development.
>
> (Bastian in Koepping 1983:167; originally Bastian 1871:172)

The study of so-called simple, primitive or *Naturvölker* is a way to recapture *elementary ideas* in their pristine state. The terms evolution and development should not be taken for notions from classical evolutionism, as Bastian categorically states: ‘No factual evidence exists for the postulate of an uninterrupted and constant progression in the evolution of culture, a regularly ascending line from lower to higher stages’ (Bastian in Koepping 1983:167; originally Bastian 1871), or again: ‘the idea of a process of evolution to higher forms in which mankind progresses to ultimate perfection can be no more than a hypothesis’. (Bastian in Koepping: 166; originally Bastian 1871). While the *Naturvölker* show a closer affinity to original elementary ideas, this does not imply for Bastian a lower state of mental development:

> Europeans were for so long deluding themselves in the conviction that they represent the ideal of all mankind, and in so doing despised all other ages and nations which dared to derive different ideals from their unique variations of social life…maybe the question about the nature of man is to be decided by the majority; in that case Europeans would be the eccentric ones when compared with average man.
>
> (Bastian 1860, I:230)

Here we find a clear indication of the Herderian influence in twofold form: the reliance on the average man, reminding us of the disdain of elaborate philosophies expressed by Herder, and the relativistic gaze on

other cultures. This is strongly expressed in the following quote, which clearly contains the spirit of Montaigne: 'in our own European civilization…we would most certainly find a form of mental barbarism that not only equals that of the African or American Indian, but surpasses in stupidity any savage society' (Bastian in Koepping: 169; originally Bastian 1871).

I think the uncanny mixture of the adulation of science mixed with a genuine appreciation of cultural diversity is proved by these quotes from different periods of Bastian's writings. One might ask in the end how this untiring traveller, who spent more than thirty years of his life overseas, repeating the Asian as well as the American crossings of his teacher Humboldt and much more, put his programme into action: how did he collect his data? He never makes this clear, though we do know that he always tried to learn local languages, as he did quickly when retained in Burma for more than two years as personal physician to the king. Instead of a method book, we find him appreciatively quoting an adage from another Romantic follower of Herder, Jacob Grimm, whose method of collecting folktales Bastian rendered applicable to anthropological collecting of data: 'rare flowers have to be picked with chaste hands' (Grimm 1844, 'Vorrede zur Deutschen Mythologie', in 1981:xi, quoted by Bastian 1885:39).

Bastian expressed the combination of this sensitivity and sensibility of a Romantic with the Enlightenment programme for a future culture in the following lines: 'The power of the mind of man must break the chains imposed upon it by mythical fantasies and self-forged delusions' (Bastian 1860 vol. 1:126).

CONCLUSION

Bastian may have failed to fulfil his aims. His view of the applicability of scientific principles to human affairs may have been overly naive and optimistic, while his restless collecting activities, instead of steady and limited fieldwork, might strike us as amateurish. Yet his passionate enthusiasm to put the ideas of Alexander von Humboldt and Herder into practice, and his equally passionate aim to unite thereby Enlightenment and Romanticism (paralleled by some natural scientists such as Fechner), gives him a secure place among the *côterie* of crucial creators of our discipline.

The epistemologies he pursued may for ever remain logically contradictory. Yet, without the universal message of the Enlightenment with its belief in reason, and without the mitigating caution against overgeneralizations and facile categorization as proposed by Herder, a

genuine science of culture is scarcely imaginable. We may put the aims of anthropology and the description of our results into a different language today. We may use a different discourse to appear neither corny nor naive, neither inhumanly objectifying nor navelgazing, by stating, as Michael Jackson did in his *Paths toward a Clearing,* that our ethnographies are co-productions between researcher and research subjects, or that they are creations from within a dialogue (Jackson 1989).

Yet we have to believe in the universality of the human potential behind the relativity of diversity, which in turn we can only grasp through the specificity of a particular formation. Without this double aim, we sink into the cynicism of inauthenticity, of which Stanley Diamond warned in his critique of civilization (1974) and to which we are prone if we consider anthropology and ethnography as mere rhetorical ruses to convince others about our authority (a warning also voiced by Geertz 1988:142). If credibility becomes a market strategy instead of a willingness to be open to an encounter which carries the risk of losing ourselves, then anthropology has lost its heritage.

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Part II

Contributions to European anthropology

5 *Orang Outang* and the definition *of Man*

The legacy of Lord Monboddo

Alan Barnard

> But, if I had called man an ape, or vice versa, I should have fallen under the ban of all the ecclesiastics. It may be that as a naturalist I ought to have done so.
>
> Linnaeus[1]

SEVENTEENTH- AND EIGHTEENTH-CENTURY IMAGES OF HUMAN NATURE

The definitions of the human species and human nature were major preoccupations among European thinkers of the seventeenth and eighteenth centuries. If *nature* became the goddess of the Enlightenment, then the *bon sauvage* became her human, and conceptually male, counterpart. The *Orang Outang,* in turn, became one form of his pure embodiment—a creature whose existence (or imagined existence) was to test the limits of the human species.[2] The very definition of *Man* depended on whether or not the *Orang Outang* was a member of this species.

Of related significance was the notion of the 'noble savage' (cf. Tinland 1968). The phrase itself originated in the seventeenth century—its first appearance being in Dryden's popular play *The Conquest of Granada*. It caught on, no doubt partly because of its inherent contradiction. 'Nobility' is an aspect of culture, while 'savagery' is an aspect of nature. The idea of 'noble savagery' therefore challenges the opposition between culture and nature. It also heightens consciousness of that exemplary anthropological opposition as a foundation of European conceptions of Man. The question of the 'nobility' of the Orang Outang, as much as that of the 'Savages' of North and South America, was an obvious focus of debate.

The common perception of the Savage State which prevailed in Dryden's time was, of course, rather different from what his catch-

phrase suggested. Hobbes's (1973 [1651]:65) image of 'the life of man, solitary, poore, nasty, brutish, and short' is best known. Later, Locke (1988 [1690]:269–78) equated man in the State of Nature with a life of freedom and equality, and in the eighteenth century many took up his position—most notably Rousseau. Meanwhile Monboddo, among other eighteenth-century thinkers, explored yet a different seventeenth-century image of human nature. This was the idea of the human being as a 'social animal', implicit in the works of a plethora of earlier writers, including Herbert, Grotius, Pufendorf, Cumberland and Leibniz. The notion of Man as a 'social animal' influenced later Enlightenment political philosophers, including Diderot, and it challenged others, especially Rousseau, to disagree. Some of these writers anticipated many of the theoretical ideas of twentieth-century anthropologists. Pufendorf's (1991 [1673]) development of the concept *socialitas* or 'sociality', for example, anticipates both the theories of exchange of Marcel Mauss and Claude Lévi-Strauss, and the notion of *communitas* propagated by Victor Turner.

Thus three images of natural man were emerging in the seventeenth and eighteen centuries: a benighted solitude, with war of all against all as the natural result of individual contact; a noble solitude, splendid and innocent; and a sociality from the earliest or most primitive stages of human existence. Perhaps more by chance than by direct influence, the last is the image which has stood the test of time. It is nevertheless worthwhile to recall these early and, to anthropologists, little-known debates, because they have much to tell of the *nature* of our discipline itself, if not its *history* in the narrowest sense of the word. The focus of this chapter is on one such debate, and the position in it of one key protagonist, Lord Monboddo.

LORD MONBODDO AND HIS ORANG OUTANG

James Burnet (or Burnett), Lord Monboddo, was a Scottish lawyer and judge, born in 1714 at Monboddo in Kincardineshire. He studied at King's College, Aberdeen, and took a general degree there in 1732. He then moved to Edinburgh to begin his legal training, and—as was then the Scottish custom—left for Holland soon after in order to obtain a grounding in the Dutch tradition upon which Scots law was based. From 1733 to 1736 he studied at Groningen. It was there that he wrote his first, still unpublished, paper on the origins of language (Monboddo n.d.[a]).

He returned to Edinburgh in 1736 and prepared a short dissertation

for admission to the Faculty of Advocates. He had a short and successful career at the bar, including visits to Paris on legal business in 1764 and 1765. There he saw an *Orang Outang* (a stuffed chimpanzee) and met a feral child named Memmie Le Blanc. An English translation of her life story was published in 1768 with an anonymous preface (actually written by Monboddo) on the state of nature as one without language (Monboddo 1768).[3]

Burnet had been appointed a sheriff in 1760 and was elevated to the Court of Session, as Lord Monboddo, in 1767.[4] At that time, the court met for only six months a year. Monboddo used his spare time in writing, travelling, amateur dramatics and taking 'air baths' (exercising naked before his open window). His dinner parties were famous; in his rose-petal-strewn room, he entertained all the major figures of the Scottish Enlightenment. He corresponded with scholars throughout Europe, especially on linguistic matters. Throughout his life he steadfastly held to his belief in speechless, human races in Africa and Asia, and in the genetic relationship between all the known languages of the world. He died in 1799.

Twelve volumes of Monboddo's writings were published in his lifetime. These include six volumes under the general title *Of the Origin and Progress of Language* (published between 1773 and 1792), and six volumes under the general title *Antient Metaphysics* (published between 1779 and 1799). The volumes of greatest anthropological relevance are *Origin and Progress of Language,* vol. I (1773) and *Antient Metaphysics,* vols. III (1784a), IV (1795) and V (1797). The later volumes of *Origin and Progress* deal not with the origin of language, but rather with universal grammar and the history and comparison of European languages since classical times. The remaining volumes of the *Antient Metaphysics* consist mainly of a glorification of Greek idealism and attacks on Newton, Locke, Berkeley, Hume and others.

Most of Monboddo's unpublished papers are today housed in the National Library of Scotland, where they have recently been recatalogued (rendering pre-1990 catalogue citations out of date). His papers include a number of short, roughly chapter-length works partly incorporated into the major volumes, plus various notebooks on a variety of subjects. Among the more interesting are Monboddo (n.d.[b]), (n.d.[c]), (n.d.[d]), and a letter to Monboddo from a Bristol merchant who describes an Orang Outang of his acquaintance (Monboddo n.d.[e.]).

While Monboddo's writings were voluminous, copies of his published works are now fairly rare. The source easiest to locate is *Of*

the Origin and Progress of Language, vol. I. This exists in the first edition (1773), the second edition (1774a), and a contemporary German translation with a foreword by Herder (Monboddo 1784b), as well as in modern English and Italian versions (respectively a reprint and a translation of the first edition). Significantly, the second edition contains revisions made after Monboddo learned of the implications of Tyson's (1699) anatomical treatise on the chimpanzee in 1773 (see especially Monboddo 1774a: 270–361).[5] This edition has come to be regarded by some modern scholars as the definitive Monboddo. Other modern scholars prefer *Antient Metaphysics,* vol. IV, which repeats and develops much of the same material.

Origin and Progress, vol. I, consists of three books. Book I argues 'That Language is not natural to man', because, in the first instance, *Ideas* are formed prior to their expression through language, and because, in the second instance, *Articulation* is 'not natural to man'. The argument for the latter is based, among other things, on such notions as the muteness of 'savages caught in Europe' and of 'the Orang Outang'. Book II argues 'That the Political State was necessary for the Invention of Language', and further, 'That such state is not natural to man, any more than Language, to which it gave birth'. Book III speculates 'Of the first beginning of Language', which, Monboddo argues, was in Egypt. He believed that this Egyptian language had spread across the world and changed gradually as it did so.

Monboddo's notion that society predates language depends on his assumption that Orang Outangs belong to the same *species* as humanity in general. As we shall see, this is not so far-fetched a notion as it may seem, but it did cause him some problems in his time (cf. Cloyd 1972:57–63). Certainly, it was considered preposterous by most of Monboddo's Scottish and English contemporaries, and he was ridiculed for his views, not least by James Boswell and Samuel Johnson. While it is doubtful that Monboddo ever really held the view attributed to him that humans in general are born with tails, he did assert that a few humans have been known to have tails and suggested this as evidence for a close relationship between humankind and Orang Outangs (see, e.g., Monboddo 1784a:250–1). He believed that Orang Outangs also have vestiges of tails, and that the common ancestors of Orang Outangs and 'Ourselves' were tailed beings, like baboons and monkeys.

For Monboddo the term 'Ourselves' included humans of European and non-European origin alike. It often refers to the black inhabitants of the Portuguese colony of Angola who formed the branch of 'our'

(non-simian hominid) population most in contact with the Orang Outang. In Angola folk belief and travellers' accounts had provided uniquely corroborative evidence that Orang Outang males would steal young non-Orang women as their brides. Indeed, an instance was graphically recorded in a famous woodcut, which is published in the later editions of Linnaeus's *Systema Naturae* and frequently referred to by Monboddo. From this and other evidence, Monboddo deduced that Orang males copulate with 'females of our species', who in turn have children who 'likewise produce' (see, e.g., Monboddo 1795:26–7; 1774a:188, 274–5, 334).

Following James Harris (1751), Monboddo believed in universal grammar (see, e.g., 1774b:5–221 *passim,* 337–63). Given his premise that this grammar emerged *after* the development of human society, the pre-linguistic hominids were ripe for inclusion into the human species. The evidence Monboddo had available did suggest that Orang Outangs, particularly the chimpanzees of Angola, might well be human. These Angolan Orang Outangs were reported to be gregarious, and Monboddo accepted this. Rousseau, who receives much praise in Monboddo's works, also seems to have accepted that Orang Outangs were essentially human (cf. Rousseau 1984 [1755]: 154–61), though he thought of them, along with other humans, as naturally solitary beings. Today, we know that orangs in South East Asia are indeed solitary, but chimps in Africa are gregarious. In these senses, both Rousseau and Monboddo were right—albeit fortuitously.

Monboddo's writings can be seen as part of a great European debate in both biology and political philosophy. His views touched on those of a number of Continental writers. Linnaeus and Rousseau are the obvious examples (cf. Lovejoy 1933; Verri 1975:37–59); and Charles Bonnet (1764), the first person to use the word 'evolution' in a biological context, was influenced by Monboddo in his assessment of the similarities between the Orang Outang and Man (Lovejoy 1936:235, 361). Monboddo also built upon the work of the Comte de Buffon, whose forty-four-volume compendium, *Histoire naturelle* (published between 1749 and 1804), includes much speculation on the relation of the animal and the human. Monboddo drew greatly on vol. XIV (Buffon 1766); indeed the copy of this volume which Monboddo used (the Advocates' Library copy, now in the National Library of Scotland) is covered in his pen markings (Hammett 1985:212–14). Buffon considered the Orang Outang to be of the 'animal' category and was conservative in his rejection of a direct relation between ape and man. Monboddo's discussions tended to be critical of Buffon's denials of 'reason' among the Orang Outang, though not of his denials of

'speech'. Speech, for Monboddo (though not Buffon), was held to be quite distinct from reason and indeed also from language. According to Buffon, linguistic ability developed rapidly. According to Monboddo, language evolved very slowly as the need for reason grew with society's gradually increasing complexity (see, e.g., Monboddo n.d.[b]; 1774a:270–314).

In the Scottish context, it is also pertinent to see Monboddo's theory on the Orang Outang as part of his lifelong quarrel with his senior judge and rival, Henry Home, Lord Kames (1696–1782). Kames, in stark contrast to Monboddo, argued for a narrow definition of humanity. He believed that the differences between cultures are so great that different populations around the world could reasonably be regarded as separate species. He even suggested, on the basis of *cultural* evidence, that American Indians were not *biologically* of the same species as Europeans (1774, vol. I: 24–5, 70–98). They were therefore, in his view, incapable of ever attaining European culture and were 'naturally' inferior to Europeans. A champion of James Macpherson's 'translations' of the poetry of Ossian, Kames preferred his Noble Savage closer to home—in the 'Caledonian' (Scottish) Ireland which Macpherson claimed for his bogus bard. Monboddo (1774a:586–91) duly went to the other extreme in arguing that some of the aboriginal languages of North America were mutually intelligible with Gaelic. Thus, not only were North American aborigines fully human; they even spoke the same language as some of his countrymen!

Monboddo's theory of social evolution, as presented in the *Antient Metaphysics,* vol. III (1784a), was that the most primitive hominoids were Brutes and Tailed Men. Next came the Orang Outangs and Speechless Savages, and among these early 'civilization' or 'social life' began. Then came the Savages proper, followed by the 'Antients' *(sic)*. Monboddo regarded Greek civilization as humankind's highest achievement. Roman times marked a decline, and society subsequently continued to degenerate to produce ultimately the Moderns of the eighteenth century. Monboddo continues his analysis of human history, with more depth, in vol. IV. The 'progress to civility' (1795:60–70) involved an early stage of living in 'herds'; Men (and Orang Outangs) may have lived 'as brutes', but they were political and gregarious. Only later did language become necessary, for purposes of 'propositions and reasoning' (1795:71–81). Then followed, first among Savages and then among the Antients, 'the invention of arts and sciences' (1795:104–279), and their subsequent transmission across the globe (1795:280–402). An important part of

his argument hinges on the relation of Orang Outangs of distant realms to the feral children of Europe:

> Nothing but Vanity can hinder us from being convinced of the Orang Outang being a Man.—If the Orang Outang be not a Man, Peter the Wild Boy is not one.
>
> (Monboddo 1784a:336–7)

> [F]or, if Mr Buffon's Orang Outang was not a man, because he had not learned to speak at the age of two, it is impossible to believe that Peter, who, at the age of seventy, and, after having been above fifty years in England, has learned to articulate but a few words, is a man…
>
> (Monboddo 1784a:367)

The Orang Outang, Monboddo (e.g., 1774a:287–8; 1795:27) claimed, had 'a sense of honour' and skills which surpassed those of some savage islanders. The Orang Outang used sticks to defend himself, built huts to dwell in, knew the use of fire, and could either walk erect or go on all fours. To Monboddo (1795:35–6), this indicated a development from first standing erect, to then using the hands to make tools, and to hunting and fishing. Monboddo argues (1795:39–40) that these modes of subsistence, at first 'unnatural' habits for Man, became necessary as human populations increased prior to the invention of agriculture.

In the late 1770s and 1780s, Blumenbach, Camper, Soemmering and others were to show that the great apes lack anatomical features, most importantly the organs of speech, which were presumed to be held in common between Orang Outang and Man (see also Wokler 1988:162–3; 1993:130–1). Indeed Blumenbach adds that his inducement to comment on the differences between apes and men stemmed from 'the opinions lately expressed by some famous men, who however are ill-instructed in natural history and anatomy, but who are not ashamed to say that this ape is very nearly allied, and indeed of the same species with themselves' (Blumenbach 1865 [1775]:94–5). Included among the four 'famous men' Blumenbach cites is, of course, the author of *The Origin and Progress of Language*.

Oddly, one of Camper's key papers on the subject appeared 'in a letter to Sir John Pringle' (Camper 1779)—for Monboddo had himself written to the same military physician some years before:

> If ever my book comes to a second edition, which you seem to think it deserves, I shall certainly correct that too strong expression about the exact conformity of the anatomy of the OrangOutang with that of man. I had my information upon that point from M.Jussieu at Paris, who either did not know, or did not think it worth his while to inform me, of those differences which Dr Tisson *[sic]* has observed betwixt the anatomy of the OrangOutang and ours.
>
> (Monboddo to Pringle, 16 June 1773; full letter published in Knight 1900:82–8)

The second edition of *Origin and Progress* did appear the following year, and Monboddo expanded his commentary on the Orang Outang. While he may have taken account there of Tyson's findings on both the similarities and the differences between Orang Outangs and Ourselves, he concentrated instead on the debate between his own contemporaries Linnaeus and Buffon (Monboddo 1774a: 270–361).

Monboddo (1784a:44) later glossed over Camper's data in an interesting way; he argued that Camper's Orang Outang was from Borneo, whereas Tyson's, quite a different 'species', was from Angola and represented a creature more similar to Ourselves. Yet to concentrate too much on such issues would be to miss the very essence of Monboddo's argument. Monboddo was not trying to define 'Orang Outang'; he was trying to define 'Man'.

THE DEFINITION OF MAN

Crudely, Monboddo's definition of the species rests on two things: Aristotle's philosophy and Horace's verse. 'What is *Man,'* says Monboddo, 'is a question which I believe no person can answer who has not studied the antient Philosophy' (1795:12). The definition of a bee is based on its ability to make honey. The definition of a man might be made on the basis of its ability to speak; but this would be erroneous, in Monboddo's view, because not all men speak. Speech is an art which Man has invented; therefore it is not natural to Man. 'The question then is, What is Man by nature, without any of the arts or sciences which he has invented?' (1795:12). Only one person has answered this question, Monboddo goes on to say: Aristotle, who defines Man as 'an animal, capable of intellect…and also of science'; or more fully, 'a Comparative Animal…who has also the capacity of acquiring Intellect and Science, and who is Mortal' (1795: 12; cf.

1774a: 338; apparently from Aristotle's *Topica* [1908–52, vol.I]: 128b 36–7).

More fully still, *Man,* says Monboddo (1784a:6), is composed of four substances: (1) the four Elements (Fire, Air, Earth and Water) animated by the Elemental Mind; (2) the vegetable substance (that relating to growth and nourishment); (3) the animal substance (pleasures, pains, desires and sensations); and (4) the intellectual substance. The intellectual substance, or reason (Greek *nous*), is what distinguishes Man from other animals. The use of this Aristotelian definition enables Monboddo to argue, almost simultaneously, two related but nevertheless different theses (see, e.g., 1784a: 359–67; 1797:164–80). One is a central thesis of the *Antient Metaphysics,* that Locke's (1975 [1690]) understanding of the human mind is flawed because he fails to take into account innate faculties in his theory of ideas. The other is more tangential to the *Antient Metaphysics* as a whole, but is central to the argument of the first volume of *Origin and Progress*—that speech is not a necessary, defining aspect of Mankind.

Furthermore, both Men and Brutes have the 'comparative faculty', i.e., the ability to distinguish one thing from another. (By 'Brutes' here, he is thinking of horses and dogs.) But only Man has the capacity to form ideas of 'species' and 'genus', in Aristotle's senses of those terms—in other words, the ability not just to compare, but to compare within a hierarchy of knowledge (cf. Aristotle's *Categoriae* [1908–52, vol. I]: 1a 1–3b 23; Monboddo 1774a:338–9; 1795:15–16).

Monboddo asserts several times (e.g., 1795:32, 63) that Man is initially a quadruped. This is true in two senses. In the first place, men more primitive than the likes of Monboddo are more quadrupedal: the Orang Outang, Wild Peter (whom Monboddo had met), and members of certain 'primitive' races. In the second place, all humans are 'quadruminous' at birth, and only learn to walk after a few years. The same is true of language; the Orang Outang is a speechless race of Man, as the infant is a speechless form of Ourselves (1774a:677–8). Or, in more general terms, 'man must first have cultivated himself, before he could have cultivated anything else. It is then a wonder, that this man of nature, the Orang Outang, should be so different from us?' (1774a:356).

A temptation today might be to read racial slurs into any such pronouncements on 'primitive' peoples, but this would be misleading. Rather, Monboddo's apparent concern with political correctness was to avoid the charge of insulting the intelligensia who might read his work. His example of the infant in 'polished' society was chosen to counter such potential charges (see, e.g., 1795:32). There is a further

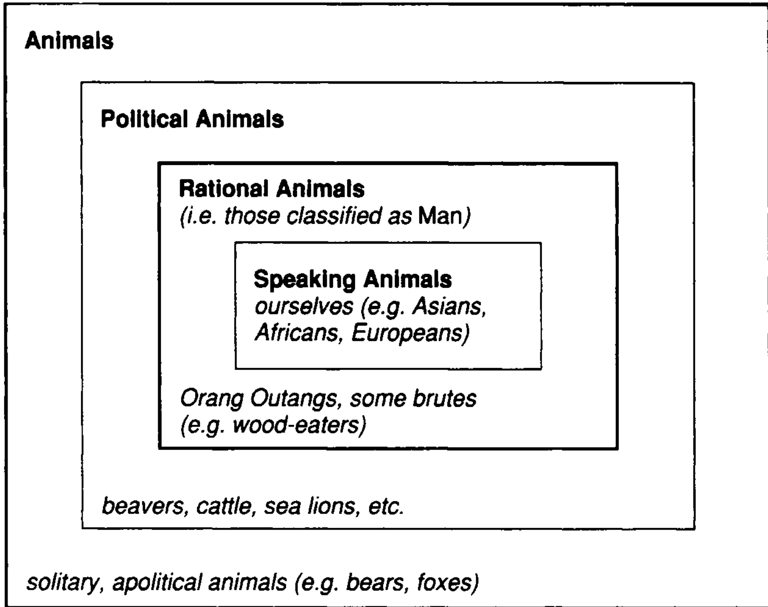

Figure 5.1 The relation between political, rational and speaking animals, according to Monboddo

irony here in that Tyson believed his Orang Outang, who he said was not human, should walk upright, and so gave him a walking-stick or a rope to hold on to in his drawings. Twentieth-century biologists, commenting on Tyson, have pointed out that infant chimpanzees resemble humans, especially facially, much more than adult chimpanzees do. From a twentieth-century point of view (or indeed from a chimpanzee point of view), humans are paedomorphic apes, not the other way around.

In fact, there is a sense in which the Orang Outang is more human than the Wild Boy—in Monboddo's view. The Wild Boy might be more human in appearance, but he lacks sociality and culture. The Orang Outang was reported to be gregarious, though his society was not developed enough for him yet to need language. I will not dwell on it here, but Figure 5.1 shows a hierarchy of classification as Monboddo appears to see it, between politics (or sociality), rationality and language.

In addition to Aristotle, Horace is also frequently invoked in Monboddo's definition of Man. The key passage is invariably quoted

only in Latin, but a twentieth-century prose translation which captures its flavour reads:

> When [rude] animals, they crawled forth upon the first-formed earth, the mute and dirty herd fought with their nails and fists for their acorn and caves, afterwards with clubs, and finally with arms which experience had forged: till they found out words and names, by which they ascertained their language and sensations: thenceforward they began to abstain from war…
>
> (Horace 1911:159; translator's gloss)

This, Monboddo says,

> applies so exactly to the Ourang Outang, that it may be said to be a description of him; for man is said first to creep, that is, to go upon all four, and then he is very properly denominated *mutum ac turpe pecus* [the mute and dirty herd]. After that, he is erected, and gets the use of an artificial weapon, such as the Ourang Outang uses. Next, he invents rude and barbarous cries…. And, last of all, they formed ideas and invented words to express them, which Horace calls *nomina* [names]. But this is a step in the progress towards the civilized life, which the Ourang Outang has not yet made.
>
> (Monboddo 1795:31–2)

In short, Horace's speculation on human progress encompasses Man's development from Orang Outang to Ourselves. As for language, it is, in Monboddo's words, 'the parent of all arts and sciences, and to be the first step of the ladder' (1795:70).

Finally, for Monboddo, Aristotle is vindicated in yet another way. Animals are classified in the *Historia Animalium* (1908–52, vol. IV: 488a 7, 9) as 'solitary' and 'gregarious', with Man alone being naturally both solitary and gregarious. Feral children perform a function for Rousseau as solitary, natural humans—as humans supposedly were at the beginning of society. Feral children perform the same function for Monboddo, but his vision of the Orang Outang is different from Rousseau's. For Monboddo (e.g., 1774a:277–99; 1797:323), the Orang Outang is the gregarious aspect of natural man. The fact that he is the mute version of social man is equally important for the development of his theory of the origin of language. The existence of the Orang Outang, and the classification of the Orang Outang as human is therefore virtually a necessary part both of Monboddo's theory of language and of his understanding of humanity.

MONBODDO'S ORANG OUTANG TODAY

Monboddo's theory is often regarded by his apologists as an anticipation of nineteenth-century ideas, especially those of Charles Darwin. It is generally regarded by his detractors as an incoherent copy of Rousseau or other contemporaries, and unconnected to the ideas of later thinkers who probably never read him. Certainly, the significance of Monboddo cannot lie merely in his anticipation of the isolated discoveries of his chronological successors. Rather, what is interesting is the structural relation between Monboddo's own milieu and that of twentieth-century science.

For Monboddo, the ape is but an untutored man. For Raymond Dart, Phillip Tobias or the Leakeys the same is true, though the means of expression are reversed. Man is an ape, though clothed and enculturated in the ways of one civilization or another. The means of expression do create different images, but there is a peculiar similarity between Monboddo's and our present-day notions in that each entails a subsumation of one category with another. Figure 5.2 is

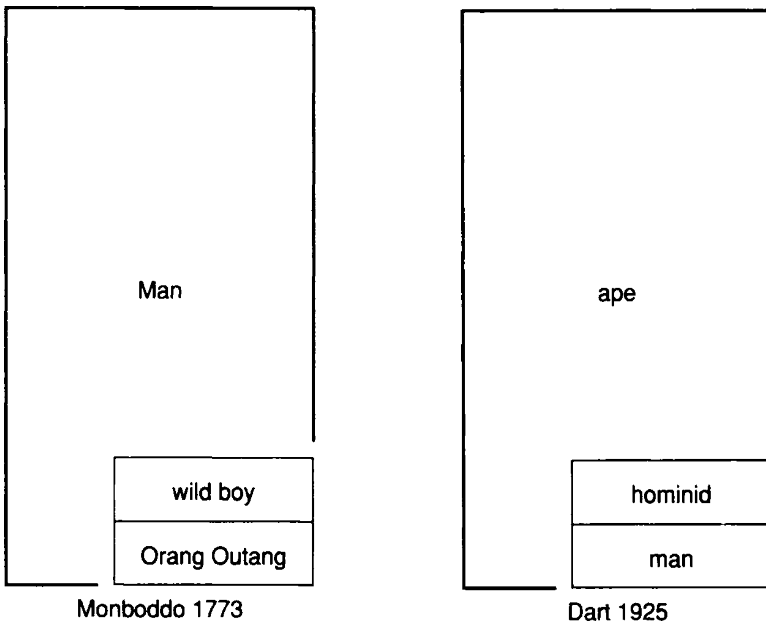

Figure 5.2 Eighteenth- and twentieth-century relations between human and simian races

an attempt to capture these images in diagrammatic form. I give our modern image the name of 'Dart 1925' in recognition of his discovery of the fossil African 'Orang Outang'.

In the twentieth century, we have learned to reverse Monboddo's relation between human and simian, but the whole history of twentieth-century physical anthropology is caught up in the changing status of the ape/man divide which bears a strange relation to points raised in the debate between Monboddo and Kames. When the 'discovery' of Piltdown Man was announced in December 1912 (Smith Woodward 1913), Great Britain could lay claim to the missing link. Those who were soon to comment so favourably on Piltdown knew perfectly well that a human-like skull had been found in association with an ape-like jaw, but it took decades before anyone suggested that they were not from the same animal. The early debate was not on forgery, but on the significance of the Piltdown bones for human prehistory.

The challenge to Piltdown was not from within British archaeology but from an alien camp. In 1925, Raymond Dart, an Australian-South African anatomist, announced the discovery of *Australopithecus africanus*. The British archaeological establishment had hailed Piltdown as overthrowing the ancestral claims of Neanderthal Man and Java Man, and they denounced the new foreign rival. 'Dart's child' was simply a juvenile ape and not a human ancestor at all (Keith, Elliot Smith, Smith Woodward and Duckworth 1925). The British archaeological establishment, like Lord Kames, had found an 'Ossian' in their own back yard. They did not want his place usurped by an African ape.

The term 'ape', which in the eighteenth century variously meant 'ape', 'baboon' or 'monkey', has long since come to be applied only to higher primates. Its primary designants today, chimpanzees and gorillas, are rapidly coming to resemble Monboddo's 'Orang Outang' in the scientific literature. This is even more true of fossil hominids, and the extension of the definition of 'humankind' is occurring among primatologists of otherwise opposed theoretical positions. Richard Leakey (e.g., 1981), like his father, has consistently argued for the great antiquity of *Homo*. For L.S.B.Leakey this meant redefining *Homo* when *Homo habilis* was found in order to allow for a smaller brain case. For Richard Leakey it has meant sticking to early and controversial dates when most other specialists have accepted much more recent ones. In contrast, Donald Johanson (e.g., Johanson and Edey 1981) argues for the recent genesis of *Homo,* and one which is much more in line with the findings of biochemistry and molecular biology. Yet a recent genesis places humankind much closer to our

more ‘distant’ ancestors by bringing the time depth between the species nearer.

The classification of higher primates is today as problematic as ever. Biochemists and anatomists have very different ideas about what constitutes a relevant degree of similarity. Within the last decade, new biochemical evidence has suggested that a much closer relationship exists between humans and chimpanzees than was previously thought. Chimps and humans share over 97 per cent of their genetic substance, which is more than that shared by some other pairs of species occupying the same given genus. This has caused some to argue that humans, chimps and gorillas alike (but not orang-utans) could be reclassified as members of the subfamily Homininae, and even that chimpanzees properly belong to the genus *Homo* (Diamond 1992:25).[6]

Today, there are anthropologists and psychologists who study chimps as if they were human, and there are linguists who work with them to try to understand the origins of human speech. None of this would have seemed odd in eighteenth-century Scotland. Monboddo’s notion of ‘Man’ is explicitly not based on language, but depends rather on the ability and practice of making weapons and tools. He would no doubt have taken pleasure in the fact that twentieth-century studies have attributed ‘culture’ to the wild chimpanzees (e.g., McGrew 1991), and the fact that at least a few anatomists, no doubt pushed by the new biochemical evidence, have been finding systematic links between chimpanzees and humans which have not been seen since Tyson (e.g., Groves 1986).

CONCLUSION

The eighteenth-century concern with ‘Ourselves’ has in late twentieth-century social anthropology been transposed to an egocentric preoccupation with ‘the self’, but the image of the ‘other’ remains as a touchstone of our discipline. In the twentieth century, we have reversed the relationship between human and simian categories, though the basis for our discourse remains the same. Within the last decade biochemical evidence has suggested that a much closer relationship exists between humans and chimpanzees than was previously thought, and Monboddo’s image of the Orang Outang seems closer to that of biological science now than at any time since the eighteenth century. Yet it is not the fact that Monboddo accidentally anticipated this development which is noteworthy. Rather, the true significance of

Monboddo lies in his unsurpassed, if eccentric, exploration of the relation between categories Man and Orang Outang in terms of language, political organization, material culture and capacity for learning.

Now that modern biology has turned our attention once more to the definition of the species, social anthropology can and should reclaim Monboddo as one of its founders. His legacy is a paradigm for probing the common humanity at the root of all cultures.

NOTES

I am grateful to Klaus Keuthmann and Han Vermeulen for their many insightful comments on earlier versions of this chapter.

1. C.Linnaeus to J.G.Gmelin, 14 February 1747 (quoted in Slotkin 1965:179–80).
2. I employ eighteenth-century rules of capitalization, spelling, and italicization, where relevant, to represent eighteenth-century concepts, and twentieth-century initial lower-case letters to represent twentieth-century concepts. Thus the *Orang Outang* is the ape-man of eighteenth-century imagination. This creature is quite different from the orang-utan known to twentieth-century science as a species of ape *(Pongo pygmaeus)* which dwells in South East Asia.
3. Anonymous publication was quite usual in the eighteenth century. Monboddo did not include the name Burnet or Monboddo on any of his works, though their authorship was widely known, and extant copies almost invariably have Monboddo's name added to the title page. Monboddo's works are here cited as Monboddo, but they are today sometimes catalogued under either Burnet or Burnett.
4. The Scottish legal system is somewhat different from both English and Continental systems. Advocates in the Scottish system are roughly the equivalent of barristers in the English system. Sheriffs are judges of middle rank, and in Monboddo's day it was possible to practise as one part time. Senior judges are called 'law lords' or, more formally, either Lords of Council and Session or Senators of the College of Justice. They always bear the title 'Lord' but are not normally members of the House of Lords.
5. Tyson, in fact, was somewhat equivocal on the exact status of the juvenile 'Orang Outang' or 'Pygmie' he had dissected: 'our *Pygmie* [chimpanzee] is no *Man,* nor yet the *Common Ape* [monkey]; but a sort *of Animal* between both; and tho' a *Biped,* yet of the *Quadrumanus-kind*' (1699:91).
6. What is yet more ironic is that Diamond's suggested species name for the chimpanzee is *Homo troglodytes*—Linnaeus's (1956 [1758]) term for 'Orang Outang'! Humans cannot be renamed *Pan sapiens* because the rules of Linnaean nomenclature dictate that the older genus name takes precedence.

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6 Beyond evolutionism

The work of H.J.Nieboer on slavery, 1900–1910

Jan J. de Wolf

INTRODUCTION

At the end of the nineteenth century the dominance of evolutionism in anthropology started to break down. Even though the principle of evolutionism in general was seldom rejected, research priorities shifted when anthropological practice was confronted with problems which had to be solved before further progress could be made. An example is the rise of modern methods of fieldwork through participant observation associated with the work of Rivers and Malinowski. A new mode of ethnographic reporting stressing the interconnections between institutions was the result.

While it is commonplace knowledge that the emphasis on the collection of primary data contributed to the newly emerging functionalist paradigm early in this century, it is less well known that secondary analysis through systematic comparison of many different societies could have a similar anti-evolutionist effect. In this chapter I should like to demonstrate this through the work of the Dutch scholar H.J.Nieboer (1873–1920) on slavery. At the same time I shall try to contextualize this work in relation to his mentor Steinmetz as well as to some broader political and economic issues and their social policy implications.

THE INFLUENCE OF STEINMETZ

The origins of Dutch anthropology as an academic discipline in the nineteenth century are twofold. On the one hand, there was the interest in the description and comparative study of the peoples of the Dutch East Indies. This resulted in the establishment of a chair of Indonesian studies at Leiden University in 1877. One of its most influential occupants was the ethnologist G.A.Wilken (1847–91). He wrote

extensively on kinship and customary law and applied current evolutionary notions to disparate ethnographic facts from many parts of the archipelago, thus furnishing these data with a unified framework. E.B.Tylor's influence is especially apparent in Wilken's work on animism in Indonesia (Jaarsma and de Wolf 1991b).

The other source contributing to the development of anthropology in Holland was the growing theoretical concern with the position of the so-called savages as a specific category of human beings, the study of which could throw light on the development of one's own society. This required a change in perspective from a regional focus to a worldwide vista. S.R.Steinmetz (1862–1940) was the first Dutch scholar who followed this path. Originally he studied law at Leiden University. After his graduation in 1886 Steinmetz continued his studies at Leipzig, where W.Wundt and F.Ratzel were among his teachers. Having returned to Leiden in 1888 he presented the second volume of his studies on the origins and early development of legal punishment as his PhD thesis in 1892. He had been dissatisfied with the first volume and rewrote it completely to have it published together with the second volume in 1894. Steinmetz's choice of his subject had been influenced by Wilken who had demonstrated the usefulness of ethnology for the comparative study of jurisprudence. Wilken's untimely death in 1891 prevented him from examining Steinmetz's thesis. In the event, Tylor was asked to be external referee. His report was so favourable that the degree was awarded *cum laude*. Then as well as now this indicates a rare achievement in the Dutch academic world (Jaarsma and de Wolf 1991a).

Steinmetz saw as the aim of ethnology the explanation of the ways of life of primitive peoples and the discovery of the laws, i.e. the descriptive regularities pertaining to the incidence, spatial diffusion and development in time of these ways of life. However, such laws were empirical generalizations which could be reduced to physiological, psycho-physical or psychological laws, which were enduring exceptionless principles (1894, I:xxii). Similarity in customs, institutions, ideas etc. of different peoples had to be explained by reference to the sameness of their developmental stage or the similarity of their external circumstances (1894, I:xxxvii–xxxviii). Although present-day savage peoples could not be considered to be wholly identical with the earliest stage of the development of mankind, they presented nevertheless the closest approximation. Therefore it was perfectly legitimate to base conclusions about the first forms of current institutions on the study of such primitives.

Steinmetz also wanted to improve the methods of ethnology. He

thought that many ethnologists failed to consider counter-examples and to explain how such negative instances could occur without invalidating their current hypothesis. He also stressed the importance of stating the argument as exactly as possible, giving numbers of positive and negative cases. Finally, explanations and hypotheses should be based on statements which themselves had been proved to be true. Steinmetz acknowledged the value of Tylor's article 'On a method of investigating the development of institutions' of 1889 as an important step in the right direction. However, he regretted that Tylor did not supply the facts on which his numerical presentation was based, nor consider other explanations (1894, I:xxxi–xxxii).

For Steinmetz the origin of punishment was vengeance and the psychological condition which caused vengeance was cruelty. He did not think it necessary to prove that savages are cruel. The examples simply abound (1894, I:300). He was only worried whether this sentiment could be sufficiently enduring for the motivation of vengeance. He found fifty cases where this was the case, against twenty cases of only feeble and transient feelings of vengeance (1894, I:306, 309). The original motive for vengeance was the cult of the dead. As punishment is universal, so the cult of the dead, more particularly the fear of the dead, has to be universal among savages. The exhaustive demonstration required a large part of volume I (1894, I:141–296). Next, the evolution of vengeance as blood feud was considered and the development of compensatory payments. In the second volume Steinmetz dealt with duelling and the position of women, before proceeding to consider punishments within the family and the domination of men over women as leading eventually to punishment by the state. He concluded his work with an overview of beliefs concerning divine punishments on earth and in the hereafter.

In the years following the publication of his study on punishment, Steinmetz continued to elaborate and revise his methodological ideas. In 1899 he published a review article on recent studies of the history of the human family (1930:61–95) which he ends with a rejection of the hypothesis that everywhere development has followed and will follow the same laws and that differences are only due to relative progress along the same path. Like the denial of regression it is an unwarranted exaggeration of the evolutionary principle to entertain such ideas (1930:95).

Steinmetz considered proper classification as the key to any progress in sociology. In a long article published in the *Année Sociologique* in 1900 he argues that the need for classification arises when enough peoples become known which are sufficiently different

from us to recognize their variety and yet too numerous to be dealt with individually. But in order to see their apparent similarities one has to know them so well that it becomes difficult not to feel that each case is in fact quite unique. It is only because political economy, comparative ethnology and anthropo-sociology have shown how the way of gaining a livelihood, how social structure and how biological characteristics influence all social manifestations that we have become sensitive to the underlying resemblances. Here scientific analysis had to precede and prepare the way for classification (1930:117–8).

Steinmetz then reviews various attempts at classification. He believes that it is possible to improve the classification based on the successive stages of economic evolution by changing this series of consecutive terms into co-ordinated classes. In this way the fundamental error of assuming that all mankind must have passed through such stages in a uniform way is avoided (1930:165). Another way of classifying peoples which Steinmetz finds useful is Vierkandt's attempt to distinguish between primitive and civilized peoples on the basis of psychological characteristics such as individual liberty, a critical attitude and freedom of investigation. After all, psychic processes are in the end the cause of all aspects of civilization. The degree of civilization and the psychic capacity of a people determine each other (1930:189–90).

Steinmetz firmly believed that mental as well as physical characteristics were hereditary. This was true of individuals but also of peoples. Consequently he was very much in favour of eugenic measures (Noordman 1989:64–9). He considered warfare as one of the most important mechanisms by which the relative strength of different peoples could influence the general progress of the human race (1899). The hereditary constitution of all peoples was probably the same to start with but they followed different developmental trajectories and as a consequence their genetic dispositions also became different (Steinmetz 1907:44–9). Instead of using the primitives to investigate our own early history Steinmetz came to believe that the study of savages should establish why they had remained backward, and what caused them to differ amongst themselves and set them apart from civilized peoples (Steinmetz 1907:54).

NIEBOER'S COMPARATIVE STUDY OF SLAVERY

Many of the changes in the aims and methods of ethnological investigations advocated by Steinmetz found their exemplary

application in the work of Nieboer. In 1896 he started with his research for his doctoral dissertation on the origins of slavery under the guidance of Steinmetz. In 1898 Steinmetz was able to report the preliminary findings at the first meeting of the Dutch Anthropological Association, and in 1900 the results were submitted as a PhD thesis at Utrecht University. According to the academic regulations the work appeared in print and was also sold through a commercial publisher. A second edition was published in 1910. The chapter on the geographical distribution of slavery was much enlarged and the theoretical part was also revised. Some passages were altered and others were added, without however changing the main substance of the argument.[1]

Nieboer's aim is to investigate the conditions which caused slavery to be successful as an industrial system as well as the circumstances under which slavery had to give way to free labour. Although Nieboer limits his research to savage tribes, he is not interested in a reconstruction of the early history of mankind. 'It is sociological laws that we want in the first place.' He wants to know the circumstances on which the existence of each social phenomenon depends (1910: xvi). Before surveying the existing literature Nieboer turns his attention to the definition of slavery. He follows Puchta who saw the function of slavery in terms of a system of compulsory labour to which end the personality of the forced labourer is completely absorbed. 'As this absorption is properly expressed by the word "property" or "possession", we may define the slave as *a man who is the property or possession of another man, and forced to work for him*' (1910:8). But because the fact that one man is the property of another implies compulsory labour, mere physical possession being of little consequence socially, the definition can be simplified as '*slavery is the fact that one man is the property or possession of another'* (1910:9).

Having defined slavery in this manner Nieboer goes some way to consider phenomena which resemble slavery and which are often called slavery, but which do not come under his definition. However abject their position may be, wives are not slaves. More generally, slavery can only be said to exist outside the boundaries of the family proper (1910:30). Nieboer also disagrees with the usage of calling the subjects of a despot his slaves. Slaves can only exist when there are also freemen (1910:31). Serfs are not slaves either, because the master of a serf may require from him only such services and tributes as the law allows him to require. In contrast the slave-owner may do with his slave whatever he is not forbidden by special laws to do (1910:38). Finally there are slaves who become only temporarily slaves when they are unable to pay their debts to their creditor.

Nieboer wants to treat them in the same way as other slaves, provided that there is no limit to the amount of work their master may exact from them (1910:39–40).

After defining slavery, Nieboer tried to make a comprehensive list of all savage peoples and to indicate whether or not they are reported to have slavery. In the first edition Nieboer mentioned 176 positive and 164 negative cases; in the second edition he had been able to enlarge his survey to include 210 tribes with slaves and 181 without slaves. Nieboer would have liked to divide the several tribes according to their general culture and then investigate at which stages of culture slavery is found. However, a good classification on a scientific basis which could serve his purpose was not available. Failing that, Nieboer proposed to take 'one prominent side of social life, that may reasonably be supposed to have much influence on the social structure' (1910:171). 'Here the economic side of life comes in the first place into consideration.... [W]e may suppose that the division of labour between the several groups within a tribe, and therefore also the existence or non-existence of slavery, largely depends on the manner in which the tribe gets its subsistence' (1910:171). He distinguishes five modes of subsistence production. These are hunting and fishing; pastoral nomadism; and three stages of agriculture. The agriculturalists are divided according to the relative importance of hunting and gathering apart from agriculture proper. Agriculturalists who live for a considerable part on the produce of their cattle but who have fixed habitations are classified under the agricultural groups. Nieboer emphasizes that the classification does not imply any evolutionary sequence as the order of development is not known (1910:174–9).

How then can presence or absence of slavery among peoples with the same mode of subsistence be explained? Nieboer starts with slavery among hunters and fishers. The absence of slavery among most of the hunters and fishers is explained in different ways according to whether they would be foreigners or belong to the same tribe. Foreign slaves could easily escape because of the nomadic way of life and the small size of the groups. The surplus generated by the employment of slaves would not sufficiently outweigh the loss of labour necessary for supervision. If slaves belonged to the same tribe it would be difficult to force them to be successful hunters. Moreover, as status among these hunters does not depend on accumulation of wealth, but on performance in hunting and warfare, public opinion would not tolerate the employment of outstanding individuals as slaves of less value than the ordinary tribesmen. Finally the status of women is not so high that

hunters would want to have female labour performed by male slaves (Nieboer 1910:192–201).

Of the eighteen cases where slavery is yet present among hunters and fishers fifteen belong to one geographical group; they all live along the northwest coast of North America (1910:203). The question then becomes in what way do the northwest coast tribes differ from the general run of hunters and fishers? One factor is that fishing, which provides the major part of subsistence among most of these tribes, allows permanent settlements and easier supervision. However, as there are also some hunting tribes among them a more general explanation is called for. The major cause is a much higher standard of living which is apparent in the abundance and variety of food, permanent settlements, larger groups, development of commerce and artisan production, prominence of material wealth and an emphasis on property. These various aspects are interrelated (Nieboer 1910:201–27).

Next Nieboer investigates whether the causes of slavery which he identified among the northwest coast tribes were absent among other groups; or, where they are present, which factors prevent them from having the same effect as elsewhere. He considers the Australian Aborigines, tribes of central North America and the Eskimos (1910:227–54). Nieboer comes to the provisional conclusion that *'slavery can only exist when subsistence is easy to procure without the aid of capital'* (1910:258). The two aspects which he considers here and which may vary independently are procurement of subsistence and necessity of capital. It is only when the produce of an unskilled labourer can exceed the primary wants of the labourer and everybody can provide for himself because no capital is needed, that slavery is the only means to get others to work for one. But even then slavery does not always exist (1910:256–8).

Clearly, there are also secondary factors which increase or diminish the use of slavery. Important is the status of women. Where the division of labour allocates most productive and domestic tasks to women, slaves are not needed. Nieboer compares this to industrial societies where the employment of women in factories decreases the need for male labour and tends to keep their wages low. Where food is preserved in large quantities there is much demand for unskilled labour when such food becomes available. Moreover this stored food also makes large permanent settlements possible in which it is easier to organize the prevention of escape. Where trade and craft production are important occupations, extra labour is needed for supplying the primary wants of the persons who engage in them. Certain commercial commodities also need unskilled labour which can be supplied by

slaves. Finally increase in wealth stimulates the demand for luxuries and to the extent that people's wants exceed their primary needs there is more room for slave labour. Slaves may also be useful as soldiers. But if the mode of warfare requires all available men, slavery is unlikely to exist, except where the military organization is so highly developed that the slaves' potential threat to their masters can be contained (Nieboer 1910:258–61).

Pastoral tribes have not much use for slave labour. There tend to be sufficient men without animals who willingly become servants of those who have. On the other hand there are no absolute reasons why they should not keep slaves. Among the secondary causes which were not met when the cases of hunters and fishers were reviewed we find the keeping of slaves as a luxury and the subjection of entire tribes, which makes slavery proper superfluous. Also important is the slave trade. In many areas where pastoralists keep slaves this trade has for a long time been carried out on a large scale. A purchased slave whose home is far away is much less likely to run away than a slave captured from a neighbouring tribe (1910:288–91).

At the lowest stage of agriculture slavery is much less common than at the higher stages, being most common at the highest stage. At the lowest stage hunting is still important and those factors which made slavery a rare occurrence among hunters are still prevalent. The positive cases which do occur will have to be accounted for by secondary causes. These are not investigated in detail (1910:293–6). Instead, Nieboer turns his attention to the applicability of the factor which he had identified as the main cause of slavery, that is, the easy procurement of subsistence without the aid of capital. It turns out that this general rule requires an important qualification, because when all land fit for cultivation has been appropriated a landless person has to apply to a landowner to be employed as a tenant or labourer. Free labour being available, slavery is unnecessary (1910:302–3). This hypothesis is tested by an examination of the situation in Oceania, excluding New Guinea.

Nieboer combines his earlier findings on the absence of slavery where subsistence is dependent on capital with the outcome of his research on the role of landed property into a general rule: *'slavery, as an industrial system, is not likely to exist where subsistence depends on material resources which are present in limited quantity'* (1910:384). Nieboer then proposes to divide all peoples of the earth independently of their means of subsistence as *'peoples with open resources* and *peoples with closed resources'*. He then comes to the important conclusion: '[O]nly among peoples with open resources can slavery or

serfdom exist, whereas free labourers dependent on wages are only found among peoples with closed resources' (1910:385). However, he immediately acknowledges that there are exceptions due to secondary causes, and he stresses that open resources do not necessarily lead to slavery or serfdom. There are many simple societies in which everybody, or nearly everybody, is working for his own wants, and in which there are no labouring as opposed to ruling classes.

LABOUR RELATIONS AS A SOCIAL ISSUE

Steinmetz wrote as early as 1893 that ethnology was of the utmost importance for practical issues of colonial policy. If one wanted to combat certain practices among exotic peoples which were too repulsive to be tolerated, one had to get to know them, to understand their character and customs, and the conditions which determined them (1893:271). Slavery in the Dutch East Indies was such an issue. Although it had been abolished officially as from 1 January 1860, this measure was by no means implemented generally. The *Encyclopaedie van Nederlandsch-Indië,* using sources published up until 1902, gave a caustic summary of the main reasons which were of a political and financial nature.

> One is afraid of generating discontent, because in a certain sense [abolishment] is an attack on customary law *[adat],* and because a monetary disembursement would in no way compensate for the drawbacks which abolishment would cause for the proprietors. The chiefs *[hoofden]* would only be persuaded to a gradual abolishment. The princes *[vorsten]* would even only with difficulty be persuaded to a gradual abolishment. One would be reluctant to diminish the livelihood of those princes and chiefs.
>
> (Slavernij n.d.: 627)

Significantly the anonymous critical second part of the article on slavery from which this quotation is taken was preceded by a signed contribution written by Nieboer on the original (i.e. pre-colonial) situation (n.d.).

In his definition of slavery Nieboer was at pains to distinguish slavery from other forms of coerced labour. His formal approach differs from the substantive treatment of this issue by Letourneau whose book *L'Évolution de l'esclavage* (1897) was the only study available when Nieboer started his research. However, his methods did not satisfy Steinmetz's exacting standards. Moreover, only a small

part of it dealt with the origins of slavery and its primitive forms (Verslagen van Vereenigingen 1899:480). Letourneau argues that each human group requires for its maintenance social labour: provision of food and shelter, care of children, protection against enemies. Much of the hard work this involves is almost always imposed on only a part of the population. At first it is the women who have to work for the men. War captives originally kept as reserve food were gradually also put to work. Slavery became common with the advent of agriculture. But under despotic regimes coercion of large numbers of primary producers was achieved more easily through serfdom. Craft specialization gave rise to a category of former slaves and serfs who had to earn a living by selling their labour to workshop owners. The industrial revolution increased this class of wage labourers, and manifold and relentless competition led to widespread employment of women and children who could be paid less. According to Letourneau improvement has to await political reform. Nation states should be broken up and war abolished. Instead of becoming soldiers young men could then serve in turn in large-scale publicly owned factories which should exist alongside privately owned small-scale workshops (cf. Letourneau 1894).

Nieboer's focus on slavery rather than on coerced labour prevented him from considering the relevance of his study for contemporary colonial labour relations in the Dutch East Indies. Although slavery was being abolished, that did not mean that shortage of people willing to work for others had also disappeared. It turned out to be very difficult to recruit labour on the plantations which were established outside Java, especially on Sumatra, where entrepreneurs had become active since the 1860s. The planters had to rely on contract labour recruited from elsewhere. But they complained that it was impossible to enforce the contract without legal sanctions. This led to state intervention. Labourers who deserted could be returned to their employers by the police, and refusal to work was punished by the state. Labourers had to pay a fine in money or to work for a period on public works. The system was open to many abuses. Although employers were officially required to provide decent working conditions, this remained a dead letter until the establishment of an official system of inspection in the period 1904–8 (Kloosterboer 1954:60–5; cf. Fahrenfort 1943:37–9). The position of indentured labourers did not differ materially from that of slaves, at least while the contract was in force.

Although his definition of slavery allowed Nieboer to discount the implications of his argument for other kinds of labour relations than

slavery and to avoid taking a stand on colonial labour recruitment policies, yet at the end of his book he refers to the theory of Loria. Loria maintained that when free land is available which can be cultivated without the aid of capital nobody wants to work for a capitalist. The only means by which a capitalist can make a profit in such circumstances is to reduce the labour force to slavery. When all land that can be cultivated without the aid of capital has been occupied slavery is no longer necessary (Loria 1893:2–6). Although Nieboer thought that many objections could be made to Loria's arguments, Nieboer could not but think that in the main he was right (1910:306). However, he did not support the political implications of this Marxist analysis. After concluding that 'in countries with closed resources the landlord or capitalist has a natural advantage over his labourers; he need neither use severity nor indulgence to maintain his position' (1900:423–4; 1910:420–2), Nieboer wrote in the 1900 edition that he did not agree with those who believe 'that mankind has lived up to the present under an unsound social system, which enables some men mercilessly to "exploit" their fellow men, but is destined soon to disappear', to make place for a socialist alternative, 'under which all men will combine to exploit the forces of nature, without exploiting one another'. Nieboer does not defend excesses, but 'the most conclusive evidence as to the reasonableness of the present system is its long continued existence'. He ends his diatribe by condemning socialism as 'an ugly system, which would sacrifice the ultimate welfare of the human race to a questionable increase of present comfort' (1900:424–5).

Nieboer must have been personally familiar with the practical problems of labour relations in a time of rapid industrialization.[2] His father was public notary at Hengelo, a town in Twente, the eastern region of the province of Overijssel. In the nineteenth century this area became the centre of the Dutch cotton industry. Although a few spinning factories using steam-power were established before 1865, the real growth of large factories took place after that year when Twente was connected by rail to coal-producing areas in Germany. Soon handlooms disappeared altogether. Hengelo itself was situated on a railway junction and for that reason the town was chosen as location for machine repair and production workshops. Around 1880 the labourers started to try to improve wages and working conditions through organized strikes. Although employers were on the whole extremely individualistic they soon decided to combine forces to break strikes. Lockouts were used relentlessly after 1890 (Kokhuis 1982:150–65).

Twente had become a vivid example of the situation described by Nieboer as one in which

> [t]he labourers are not held as property, because they are not valued. If a labourer leaves his service, the employer knows that there are many others ready to take his place. Here it is not the employer who prevents his labourers from escaping, but the employed who, by means of strikes and trade-unions, try to prevent the employer from dismissing his workmen.
>
> (1910:420)

The social and political views of Nieboer expressed in the 1900 edition were elaborated in an article published in 1904. Its general question was how one could achieve a situation in which the work people had to perform would be suited optimally to their natural aptitudes. Nieboer did not expect that the government could do much about it (1904:377). He also believed that the knowledge and character necessary for good government could generally be found only among the upper class. The common welfare would suffer if the numerically strongest group of voters would be able to dictate what should happen according to its own narrow interests (1904:397). Nieboer was to change his position soon afterwards. He became convinced of the necessity and reasonableness of the socialist alternative and even joined the Social Democratic Labour Party. Consequently in the 1910 edition of his book Nieboer left out the passages in which he criticized socialism. On the contrary, he quotes with approval Lange (1879:12–13) who wrote about ‘the semblance of liberty’ for the labourer in capitalist systems, whereas in reality ‘he is exploited and subjected’. Instead of defending the existing system Nieboer admits that ‘the condition of the working classes in modern Europe in many respects certainly is not better than that of slaves in countries of lower civilization’.

CONCLUSION

Although both Steinmetz’s study of the origin and development of penal law and Nieboer’s account of the occurrence of slavery were concerned with ‘savages’, there are some marked differences. Steinmetz traced within this category a definite evolutionary path. He made use of the method of ‘survivals’ (Steinmetz 1894, I:341–2) and accepted the ideas of Morgan and Tylor on the development of kinship. Nieboer distinguished various types of subsistence but he was quite

emphatic that these do not constitute evolutionary stages. As a matter of fact this classification turned out to have no theoretical importance. It was only heuristically useful. Empirical generalizations about the prevalence of slavery associated with different modes of subsistence led Nieboer to concentrate on exceptional cases. In this way underlying causes could be identified. Nieboer's aim was not primarily to investigate the bearing of laws established among savages on the 'social life among civilized and semi-civilized nations.' But with regard to the relation between land and population Nieboer explicitly referred to the work done by historians on the decline of serfdom in Western Europe. He was of the opinion that this factor had been 'of the utmost importance in shaping the lot of the labouring classes', but that it was commonly overlooked (1910: xvi).

Psychological principles, which provided the ultimate explanation in Steinmetz's own study were relegated to the background. The only assumption Nieboer made was that people prefer to leave heavy and disagreeable work to others (1910:420). Nieboer himself freely admitted that he had not investigated the historical development of slavery systematically (1910:438). Instead he formulated general laws concerning the relation between land, labour and capital. Changes in one factor of production had repercussions for the other factors. In this broad sense Nieboer's study is functional rather than evolutionary. But it is perhaps in his closely argued analyses of secondary causes that Nieboer shows himself most aware of how different aspects of economy and society hang together in concrete cases. Still, Nieboer did not adhere to the kind of functionalism which came to characterize British social anthropology after 1920. He did not share its preoccupation with social relations as causal in their own right, nor their assumption that the analysis of one empirical case would be a sufficient proof of a sociological hypothesis.

If Nieboer cannot be called a functionalist in the strict sense of the word, his work on slavery was not evolutionist either. As Nieboer defined slavery according to contemporary jural notions of individual property and saw its function in exclusively economic terms, his study simply could not support evolutionist premises. If slavery is everywhere and always the same phenomenon it becomes impossible to discern any developmental sequence. At the same time this approach allowed him to make use of notions which were commonly accepted in political economy with the result that savages and civilized people turned out to behave in identical ways. It is evident that Nieboer assumed that man acts rationally, choosing means which are best adapted to the situation in order to reach his goals. That is

why slavery is most likely to be found among higher agriculturalists with free access to land and few capital requirements (cf. Siegel 1945:358–9).

It is tempting to interpret this choice of a political economy perspective as eventually leading to Nieboer's change of political convictions. However that may be, Nieboer's politically orientated articles written while he was leader of the socialist members of the municipal council of The Hague, reveal a striking similarity with his more strictly scholarly work (1918; 1919; 1920). In both cases a carefully worked out and closely reasoned argument is linked with solid empirical evidence. It is these characteristics which in my opinion provide the enduring appeal of Nieboer.

NOTES

1 Most later writers on slavery referred to his study, either rejecting or modifying its thesis (e.g. MacLeod 1929; Ruibing 1937; Fahrenfort 1943; Siegel 1945; Kloosterboer 1954, 1960; Baks *et al.* 1966; Kopytoff 1982). Nieboer's contribution to the development of anthropological theory was considered by Köbben (1952), Harris (1968:612), Evans-Pritchard (1982:187–8) and Vincent (1990:9; 87–9). Renewed attention on the part of contemporary historians and anthropologists to the phenomenon of slavery also revived interest in the substantive issues raised by Nieboer (e.g. Domar 1970; Engerman 1973; Hoetink 1973; Patterson 1977; Miers and Kopytoff 1977; Watson 1980; Patterson 1982).

2 For details of Nieboer's biography I am indebted to A.F.J.Köbben, who corresponded with one of Nieboer's daughters in 1963.

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7 Bronislaw Kasper Malinowski and Stanislaw Ignacy Witkiewicz

Science versus art in the conceptualization of culture

Peter Skalník

There would be hardly any point in introducing Malinowski alone, at least to an anthropological reader. In the company of another, almost unpronounceable Polish name the introduction may acquire new, unexpected dimensions. My intention in this chapter is to introduce to the reader an unknown Malinowski—Malinowski together with his *alter ego,* namely Witkiewicz. My thesis is that without Witkiewicz there would be no Malinowski, at least as we know him: the man who revolutionized anthropology. I would like to show that it is hardly possible to understand Malinowski the anthropologist and man without an insight into his closest friend's work and life. Witkiewicz, as is increasingly recognized worldwide, has been one of the most seminal, though eccentric, creative personalities of the century. Here follows in brief the story of an unusual and ambivalent friendship the ramifications of which shaped Bronislaw Malinowski into what he is.

THE STORY OF FRIENDSHIP

At the turn of the nineteenth century, two young people met in Zakopane, a climatic spa south of Cracow. Malinowski, called Bronio by his intimates, was a year older than Witkiewicz or Stas as he was known to his family and circle of friends. Malinowski, born in 1884 in Cracow, an Austrian subject, was the only son of Lucjan Malinowski, a university professor, the father of Polish dialectology and a well-known folklorist. Witkiewicz was born one year later in Warsaw, a Russian subject, since Poland was divided between Russia, Prussia and Austria between 1795 and 1918. Witkiewicz's father, Stanislaw Witkiewicz, was a writer, architect and artist, very well known in Poland and virtually revered at the moment of his death in 1915. Stas was very attached to both his parents. He maintained an extensive and often

moving correspondence with his father, who was seriously ill for many years, (S.Witkiewicz 1969). To his mother, a practical and educated woman, he always turned in times of crisis, indeed he lived alternately with his wife in Warsaw and his mother in Zakopane during the latter years of his mother's life.

Malinowski's father died when he was fourteen years old. His mother was from the gentry, a cultivated and highly educated woman to whom he was very close. She became the source upon which he depended for his education and indeed his whole well-being. Soon after the death of his father, Malinowski's eyes and lungs were seriously afflicted and his mother took him three times to climatic spas in northern Africa. For a while she had to read to the young Bronio from his textbooks and most of his secondary education was done at home. This did not diminish the success with which Malinowski passed his matriculation examinations. In fact, the struggle with illness made Malinowski a very determined personality, able to face odds. Malinowski was marked by doggedness and ambition but less by outstanding talent, whereas Witkiewicz was a young prodigy who began to paint at six, wrote his first play at eight and first philosophical essay at seventeen. He was educated entirely by his father and private tutors.

Zakopane and especially Cracow were centres of the artistic and intellectual life of the then Austrian province of Galicia. The times were liberal in Galicia as they were elsewhere in Austria after 1867. Galicia enjoyed political autonomy, and the Polish language was dominant in all spheres of life. Polish culture flourished without being hijacked for the purpose of ethnic nationalism. By the turn of the century, an authentic modernist movement *Mloda Polska* or Young Poland developed in Cracow, known for its originality, audacity and high ideals (Segel 1960). Witkiewicz, who for a while enrolled as a student in the Cracow Academy of Fine Arts (in 1905–6), was an integral part of Young Poland; Malinowski, a student at Jagiellonian University in Cracow between 1902 and 1906, less so. None the less, through and with Witkiewicz and other young friends, Malinowski absorbed the ethos of the epoch which was full of literary and philosophical disputes, artistic experiments, eroticism. There was a whole circle of young friends who looked up to those who were already famous representatives of the movement and whom they could meet easily while spending vacations in Zakopane at their parents' homes. As Karol Estreicher, one of the students of the epoch put it: '[D]reams about success, power and fame were mixed with dreaming

about love, perceived somewhat naively as a sensual lunacy' (Estreicher 1971:8).

Malinowski's closest friends in the circle were young Witkiewicz and Leon Chwistek, later an artist and mathematician. Witkiewicz called Malinowski Lord Douglas, a reference to the real Douglas who was a close friend of Oscar Wilde. It seems that already in his late teens Malinowski was showing traits of character which set him in opposition to Witkiewicz and perhaps also to other members of the group, which led to conflicts. Estreicher, whose father knew the young Malinowski well, characterized Bronio as a diligent boy but not imaginative, a quick learner whose reserve about revealing his thoughts would suddenly transform into action which was decisive but devoid of responsibility. Witkiewicz's father was even more severe in judging the young Bronio's character and was concerned about the latter's influence on his son. He appreciated Bronio's exceptional intelligence and his independence of mind which, however, bordered on cynicism. Witkiewicz senior saw Malinowski as a total egoist, engaged in the development of 'antisocial theories' in which there were no feelings or spirituality.

Witkiewicz junior was dependent, to some degree, on his friendship with Malinowski. The relationship between the two, other links and the bohemian atmosphere of Young Poland are depicted with naturalist fervour in Witkiewicz's autobiographic novel *622 Downfalls of Bungo or the Demonic Woman*. It was written in 1910–11, further edited around 1920, but published only in 1972. The three main heroes are Bungo (Witkiewicz), Edgar, Duke of Nevermore (Malinowski) and Baron Brummel (Chwistek). Although the main theme of the novel is Witkiewicz's passionate romance with the demonic woman, the actress Irena Solska, the reader finds in its pages a description of a homosexual act between Nevermore and Bungo, who is apparently seduced by Nevermore. This was one of the more serious of Bungo's downfalls.

Nevermore (does this name stand for 'never more' homosexual liaisons or is it a play on Poe's refrain from *The Raven?*) is depicted as Bungo's opponent, a person with eyes as cold as a snake's, and a face full of terrible will-power, expressing a longing for the satisfaction of all life's appetites which his failing health prevented. The duke gave the impression of a tiger at rest; he was sceptical and had even learnt to master his consciousness, an ability which he used when he needed to change direction. The friendship between Bungo and Nevermore was strengthened by their conflicts: they complemented each other like contrasting colours. Nevermore had a substantial influence on Bungo. Bungo knew how to take the best from Nevermore because he really

loved him. For Bungo the duke was the most reliable drug in moments of depression, and he fully believed in the latter's life force and detached unexcited dialectics.

The novel, a kind of extravagant diary of Witkiewicz's, is believed by specialists such as Anna Micinska to reflect fairly, although in somewhat exalted form, the truth about Young Poland. We know that Malinowski, as was essential in the epoch of Young Poland, also kept his own intimate diary. However, until we can compare what Bronio noted,[1] felt and thought about Stas and their friendship, we have to rely on Witkiewicz's novel, various letter exchanges,[2] the memoirs of contemporaries, and Malinowski's own early writings which have finally been brought out by Cambridge University Press in English translation (Malinowski 1993).[3] The period of intimate friendship between Bronio and Stas stretches from 1900 to 1914, comprising a full fifteen years, the time of their adolescence and early manhood. Despite differences of character, talent and professional activity, the two friends followed a fairly similar pattern in their intellectual development. It was very Continental in both content and quality. Both, for example, studied mathematics and logic and learned foreign languages. They read Frazer's *The Golden Bough* at approximately the same time, and studied the works of Nietzsche, Schopenhauer, Mach and Avenarius. Witkiewicz wrote an essay on Schopenhauer's philosophy while Malinowski was producing his essay on Nietzsche's *The Birth of Tragedy*.[4]

OPPOSITION

Malinowski, as is well known,[5] very successfully concluded his university studies with a Doctor of Philosophy degree awarded *sub auspiciis Imperatoris*. Witkiewicz never finished any formal education beyond matriculation. His informal education was, however, of supreme quality. When Malinowski studied ethnology and sociology at the London School of Economics (1910–14), Witkiewicz not only visited him there several times, but followed his friend's work with great interest. For example, he commented extensively on Malinowski's manuscript *Primitive Beliefs and Forms of Social Structure,* which was published in Polish in Cracow in 1915 while Malinowski was already in New Guinea.[6] Although the author planned its translation, the book unfortunately remains untranslated to this day.

The friendship suffered, however, not only because of diminishing face-to-face contacts and differing life experiences, but also because of

the different world-views of the two protagonists. This can be exemplified by their dispute over the theory of religion and the role of magic and totemism in its early forms. Whereas Malinowski had a radical critique of Frazer, whom he chided for methodological inconsistency, and proposed a sociological theory of totemism and religion instead (see Malinowski 1911–13; 1915), Witkiewicz had the highest admiration for Frazer whom he perceived not as a failed scientist, but as an unsurpassed writer on past and exotic customs. This may sound like the naive reaction of a layman, but it was not. Witkiewicz, at the same time, was already an accomplished painter, photographer and writer, searching for the deeper meaning of existence which he sensed in artistic creation. He found Malinowski's theory reductionist, seeing culture as the mere functional satisfaction of psychological and physiological needs (perhaps this prompted Witkiewicz's father to consider Malinowski's theories antisocial). Witkiewicz's view of culture was much more individualistic. Culture, in his view, was autonomous, symbolic and metaphysical, expressed through the existential dilemma of individuals, implicit in the tension between the 'I' and 'non-I'.

This basic opposition between Malinowski and Witkiewicz can be translated into the opposition of science and art, the main directions the two friends and rivals took. In around 1910, Malinowski chose science and through a radical critique of ethnology embarked upon the arduous task of creating a scientific, sociological theory of culture. This remained the obsession of his life, until the very end.[7] Witkiewicz, through art and philosophy, was obsessed with the much broader question of human existence itself. He clearly perceived the limitations of the scientific method, especially when applied to social and human questions. It must be stressed that Malinowski was also deeply impressed by metaphysics, religion and especially art as powerful expressions of human essence. That he chose science as the vehicle of his ambitious career must be, in my opinion, ascribed to his relative lack of imagination, and the realization that he could never be a good artist.[8] Malinowski's and Witkiewicz's conflicts and interdependence continued throughout their mature lives while each of them took a different path.

BREAK

Before reaching the central part of this chapter, an important digression is in order. While Malinowski was abroad in order to throw himself single-mindedly into the study of social sciences—first psychology and

economics in Leipzig with Wilhelm Wundt and Karl Bücher, later ethnology and sociology in London with Edvard Westermarck and Carl Seligman, publishing one book in English[9] and writing extensively in Polish—Witkiewicz's oversensitive nature realized itself in passionate artistic creation, romances and foremost in existential torments which often bordered on despair and thoughts of suicide. When his fiancée killed herself early in 1914 in response to Witkiewicz's jealousy and his subsequent disappearance for several days, Stas turned to Bronio in total despair, on the brink of suicide himself. Witkiewicz's avalanche of letters to Malinowski (eleven of them between March and May 1914 have survived) illustrate vividly what led to their joint departure for Australia in June 1914 (cf. Witkiewicz 1981).

For Malinowski the trip to Australia and planned fieldwork in New Guinea was the first pinnacle on his well calculated journey to fame. Witkiewicz, however, joined the expedition at very short notice and out of despair, Malinowski apparently agreeing out of compassion. Unfortunately, only Witkiewicz's letters to Malinowski from this period have survived.[10] All we know is that Malinowski agreed to his friend's joining him even though he was well aware of Witkiewicz's poor psychological state and knew that he had to contribute substantially to Witkiewicz's travel expenses. The remarks of the relative Jan Stanislaw Witkiewicz (1982:70–1) regarding Malinowski's homosexual motivations in taking Witkiewicz along will have to remain speculations. At any rate, Witkiewicz's departure with Malinowski did not help his depression; instead he constantly thought of his late fiancée and almost committed suicide in Ceylon (Witkiewicz 1981:75–8).

News of the outbreak of the First World War reached the travelling party of the 34th meeting of the British Association for the Advancement of Science (to whose anthropology section Malinowski was a secretary) in Albany, Western Australia, on 3 August 1914. Witkiewicz could not hold out any more and decided to return to Europe and fight the Germans alongside the Russian army. Malinowski, an Austrian subject, was technically an enemy, but he was determined to stay and continue his fieldwork strategy. He and Witkiewicz became alienated and finally separated on 1 September 1914 at Toowoomba, Queensland. Neither Witkiewicz's exchange of letters with Malinowski nor Malinowski's diary reveal the exact cause of the break. Perhaps the letters of Malinowski to his mother or Witkiewicz's to his family would reveal more. The latter, however, most likely did not survive.

What is obvious from the known documents is that the break was

traumatic, though not equally so, for both friends. Malinowski compared it to the rupture between Nietzsche and Wagner, he being Nietzsche and Witkiewicz Wagner (Malinowski 1967:34). From my point of view the essence of the conflict was nationalism. Money or some public action of Witkiewicz in front of other people which was perceived by Malinowski as an insult (such as the proposal that Malinowski switch citizenship and become British) could have played a part, but I conclude from the later writings of both Malinowski and Witkiewicz that the main issue was loyalty to the state and nation. Malinowski was fairly relaxed about his loyalty to Austria and later Poland as states, stressing his personal and cultural bonds and values, basically happy about belonging to the liberal Austria (Skalník 1993). In 1915, however, he gave an interview to a Sydney newspaper, in which he stressed that he was a Pole and that Poles were opposed to Germany's war against the Entente (*Daily Telegraph,* 19 May 1915).

Witkiewicz felt otherwise. He was raised as a Polish patriot and wanted to fight the Germans as the greatest enemy of the Poles. Unlike Malinowski, he had a positive attitude to the Russians and their political system: he saw them as the only power capable of defeating the Germans. Moreover, he wished to redeem himself by doing something significant. Failing to find this in accompanying his friend on his field expedition, it was only logical for him to leave and join the Russian army.

AFTER THE PURGATORY: PROFESSOR MALINOWSKI VERSUS PANARTIST WITKACY

Thus, from 1 September 1914 on, the lives and careers of Malinowski and Witkiewicz developed separately, although their friendship and the themes of their disputes continued to exert a decisive influence on their actions. Wartime was for both a kind of purgatory.

Witkiewicz joined the Pavlov elite regiment based in St Petersburg, fought on the battlefield, was wounded, given a war distinction, lived through both phases of the Russian Revolution and finally returned to Poland in 1918. His biographers marvel at this change in Witkiewicz—an oversensitive and morally broken artist—into a soldier and man of decisive action. During the war, he never stopped painting, however, and made many portraits, sketches and drawings, even while on the battlefield. He also started to write down his views on the theory of art (Witkiewicz 1919), and became a great admirer of Picasso, in particular of the Shchukin collection of Picasso's paintings in Moscow.

Malinowski, in the meantime, worked hard in the field, struggling with his loneliness and hypochondria in Mailu and the Trobriand Islands (cf. Malinowski 1967). A Polish author, Paszkowski, says openly that given Malinowski's capricious nature he would never have managed to spend three years among the savages had there been no war to keep him in the field (Paszkowski 1972:5) This is of course exaggerated as Malinowski actually spent more time in the pleasant conditions of Adelaide and Melbourne in Australia than in the field. In 1920, Malinowski finally returned to Europe with a vast amount of very valuable field data—'the general theoretical "sauce" in which my concrete observations are to be dressed up' (as he put it in his diary, 2 November 1914, Malinowski 1967:158) and a young wife Elsie née Masson.

His determination to make a breakthrough in ethnology succeeded in an extraordinary book which he romantically and cryptically called *Argonauts of the Western Pacific*. Most of it was written while he was living on the Canary Islands in 1920–1. Malinowski dictated the text to his wife, an accomplished writer, who edited it before it was again revised by Malinowski. The famous methodological revelations in the introduction to the book, in which the author suggested a sociologically tuned transformation of ethnology, are followed by vivid descriptions of Trobriand society viewed through the prism of the particular, fascinating custom of the *Kula* or a ceremonial exchange of valuables. There is a distinct contrast between the scientific, indeed scientistic methodology of the introduction, and the novel-like style of the main text. Malinowski, apparently, did not give up his artistic ambitions: as Lady Frazer reported, he wished to become the Conrad (also a Pole!) of ethnology and thus surpass his immediate predecessor Rivers, whom Malinowski compared to Rider Haggard (Firth 1957:6).[11] He must have given up the idea of translating into English his severe criticism of another predecessor, Lady Frazer's husband,[12] and instead secured Frazer's introductory foreword for his *Argonauts*. It seemed that nothing stood in the way of fulfilment of the main tenet of Malinowski's life: to become famous in at least one scholarly discipline (cf. Malinowski 1967:160, 161, 253, 295, 297).

To ensure this he had to make two more decisions: first, to settle in England and not in Poland, now independent but impoverished and peripheral; and second, to use his theoretical and methodological equipment, including the seminar form of teaching, acquired on the Continent, without clearly revealing these sources.

Functionalism was presented by its author as a methodological revelation justifying the emergence of a new discipline—social

anthropology. Malinowski repeated that functionalism stemmed directly from his fieldwork in Melanesia, although we know today that it was exactly the other way around: Malinowski went to the field to find factual confirmation—the 'sauce' as he put it in his field diary—of his already-formed theories. The Malinowskian myth was created by Malinowski himself. In 1922 he rejected an offer to become Professor of Ethnology in his native Cracow and instead of buying a summer house in Zakopane, as proposed to him by Witkiewicz (whom he saw during his brief visit to Poland the same year), he bought a house in South Tyrol where he spent a substantial portion of each year ever since.

Most important, he secured an appointment to a lectureship at the London School of Economics, in the Department of Ethnology, which was headed by his teacher, sponsor and friend, C.G.Seligman. The Malinowskian revolution, the emergence of a new discipline, received a formal confirmation when he was first made Reader in Social Anthropology in 1924 and finally became Professor of Social Anthropology in 1927. This time also marks the change of name of the LSE department from 'ethnology' to 'anthropology'.

The rest is well known: a splendid career in England, marked by his very successful teaching and public lectures, publication of his Trobriand books and, above all, his *tour de force* of a functional theory of culture. This last quest, however, remained mostly an ambition. Malinowski is today, more than fifty years after his death, praised for his methodological innovation and his ethnographies more than for his theoretical input. He himself was not fully satisfied with his 'scientific theory of culture', and his book of the same name was eventually published only after his death (Malinowski 1944). I shall return to this topic later.

It should be mentioned that, at the height of his career, Malinowski remained in contact with Witkiewicz, although they saw each other only twice or three times between 1922 and 1938. There are many references to their friendship in the correspondence that has survived—so many that it sometimes looked as if the friendship was restored (Witkiewicz 1981). However, the time-span between letters is sometimes too long. Malinowski dedicated the 1930 German translation of his most popular work *The Sexual Life of Savages in Northwestern Melanesia* (1929) to Witkiewicz. Witkiewicz kept sending his works to Malinowski without receiving much response from the latter.

Witkiewicz's post-war years were filled with frantic activity in almost all forms of art. He began signing his avant-garde works

'Witkacy', combining Witkiewicz and Ignacy. He developed a highly original style of painting, combining realistic and naturalistic features. Witkacy became well known in Poland as a portraitist, which gave him a means of livelihood. However, his main effort, at least for a while, seemed to be his theatre plays. During the first seven years after his return to Poland, he wrote no fewer than twenty-six plays. Five of them have tropical themes, evidently inspired by his journey with Malinowski.

His plays, some of them set in perfectly described and concrete locales in Australia or Papua New Guinea, are a mixture of metaphysical deliberation, sexual intrigue and biting satire on, if not condemnation of, the idea of the white man's burden in the tropics. Here, Witkiewicz in practice shows his understanding of religion, totemism and art. In one of the plays, *The Metaphysics of the Two-Headed Calf* (1921), Witkiewicz directly attacks Malinowski's sociological concept of religion. Through the native king Aparura, Witkacy exclaims that totems are true and it does not matter what the scholars write about them. Malinowski, referred to by name in the play, is given the epithet of 'that damned, anglicized, immodest dreamer'.

At about the same time, Witkiewicz made some additions to his manuscript *622 Downfalls of Bungo*. One of them is probably a passage concerning Malinowski which summarizes his changed attitude to his former close friend:

> The duke [as punishment] for some unheard of crimes he committed in the lanes of Whitechapel with a couple of lords, was sentenced to deportation to New Guinea, where he wrote a work (*The Golden Bough of Pleasure*—Edgar, Duke of Nevermore, Cambridge University Press) so outstanding about the perversions of these seemingly savage people, who are called contemptuously the Papuans, that he returned after a couple of years to England as a Member of the British Association for [the] Advancement of Science and [a] Fellow [of the] Royal Society. His further life was only a series of wild, spurious triumphs.
>
> (Witkiewicz 1978:423)

Malinowski's effect on Witkiewicz emerges again in an article by Witkiewicz from 1927 called 'On polemics and enemies', in which he refers to Malinowski and Chwistek as people with whose viewpoints he cannot agree, indeed he sees them as enemies, but for whose early friendship and influence he is grateful.

Witkiewicz's life was a lonely struggle. His other plays directly

predict in a kind of visionary way the loss of individual freedom to collective, totalitarian machinery. Again and again, he explores the secret of power, which he views, obviously influenced by anthropology but critical of Malinowski, as stemming from magic and myth, which in the hands of a particular individual serve as a bridge between the people and their fear of the void, the mystery of their existence. The quest for understanding of the secret of power, the drives behind human actions, found expression in his anti-utopian novels such as *Farewell to Autumn* (1927), *Insatiability* (1930), his most famous anti-totalitarian play *The Shoemakers* (1932), theoretical works on art and theatre such as *New Forms in Painting* (1919) and *Theatre* (1922), and finally the quasi-sociological work *Unwashed Souls* (1935) along with the philosophical treatise *The Concepts and Principles Implied by the Concept of Existence* (1935) which he sent to Malinowski, who reacted positively (Witkiewicz 1981).

It is of course impossible to give here a comprehensive account of the views of Witkiewicz. I have to limit myself to pointing out his search for a 'Pure Form of Art', which he understood as the only worthwhile form of Existence. There was nothing more valuable in life than artistic activity. 'For Witkacy, man's mind is his highest faculty which he must never renounce, no matter what the pressures for social conformity', writes his biographer Daniel Gerould (1981:295). The anguish of existence, which so haunted Witkiewicz all his life, was the motor of his creativity. He constantly searched for *The Only Way Out*—the title of his fourth, unfinished novel—i.e. to realize oneself in three related metaphysical forms of existence: religion, the art of painting and philosophy. Against this stands Malinowski's dictum that there is no alternative to science.

FINAL COMPARISON

Witkiewicz was a pantagruelist who used sexual metaphors in practically all his plays and novels. Eroticism was a way of filling the void of existence, part of the art which overcomes the existential alienation. In Witkiewicz's understanding, the sober practice of science leaves a void whereas art fills it. While Malinowski interprets magic functionally, as fulfilment of one of the human needs, perhaps as a protoscience, Witkiewicz sees it, like religion in general, as a potency which lasts, a metaphysical attempt to answer questions about human loneliness in the universe. The leader of the clan, such as Aparura, would with the help of magic also master, at least in the eyes of his subjects, the world of 'non-I'. Witkiewicz's approach is asociological,

metaphysical. His concept of culture is autonomous, symbolic, metaphysical and individualistic. 'Pure Form' is created by the individual, the lonely creator of true and absolute art. Witkiewicz's gigantic output of work, with the exception of portraits and some productions of his plays, remained unconsumed, misunderstood and often rejected by his contemporaries. He had, however, some success as a modernist eccentric among the like-minded and would-be-like-minded and he was appreciated by some contemporary philosophers such as Hans Cornelius or Tadeusz Kotarbinski. Only now, in the postmodern era, does Witkiewicz's work receive the recognition it deserves.

Malinowski's career, on the other hand, was conscious, teleological, instrumental. He was highly successful in the conventional meaning of the term. Ernest Gellner, using the metaphor from *The Golden Bough,* wrote that whereas Malinowski had slain Frazer as the king of anthropology, nobody has yet slain Malinowski who, in spite of all subsequent developments within the discipline, remains the king (Gellner 1986). If true, this also says something about the loss of direction in a branch of knowledge which, under Malinowski's leadership, claimed scientific substance.

I believe we should at last realize that Malinowski through his main ambition in anthropology—the holistic scientific theory of culture—committed a reductionist *faux pas*. In contrast, Witkiewicz's unity of the practice of art and his theory of existence and pure form, expressed in his now more than ever appreciated art, achieved a truly holistic aim. Whereas Malinowski is perhaps the king of social anthropology, Witkiewicz is one of the geniuses of the century, who was far ahead of his time and remains so for many even today. We return, thus, to the initial theme: 'two wilful and demanding temperaments' and the friendship based on them, Bronio and Stas, Cracow and Zakopane, the beginning of the century. The circle closes, or never ends.

NOTES

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valuable comments and editorial improvements. Jeanne Hromník is to be thanked for her devoted editing.

1 Malinowski's diaries from the pre-fieldwork period are kept by his youngest daughter Helena Wayne. Their Polish, i.e. original, edition should be part of the ambitious complete works, *Dziela,* which was started by Panstwowe Wydawnictwo Naukowe in Warsaw in 1980. From thirteen planned volumes six have seen the light so far.
2 E.g. S.I.Witkiewicz (E.Martinek ed.), *Listy do Bronislawa Malinowskiego,* Warsaw, Panstwowy Institut Wydawniczy, 1981; S.Witkiewicz, *Listy do syna,* Warsaw: Panstwowy Institut Wydawniczy, 1969; S.I. Witkiewicz, 'Listy do Heleny Czerwijowskiej', *Twórczosc,* 1971, pp. 25–47. Malinowski's letters are kept among the Malinowski Papers in the Sterling Library at Yale University and in the British Library of Political and Economic Sciences in London.
3 Malinowski's early writings were translated from Polish by Ludwik Krzyzanowski and edited with an extensive introduction by Robert Thornton and Peter Skalník (see Malinowski 1993).
4 Malinowski's essay was found by myself in the Yale archives and makes part, in English translation, of the 1993 book (Malinowski 1993).
5 An excellent description of the ceremony by Andrzej Flis can be found in Ellen *et al* 1988. See also Borowska 1971 and Kubica-Klyszcz 1988.
6 Cf. Malinowski 1915. This book was intended to be Malinowski's 'Habilitationsschrift', enabling him to become a university professor in post-war Poland.
7 Malinowski postponed publication of his *A Scientific Theory of Culture* (Chapel Hill: University of North Carolina Press, 1944) until it was too late. He used to read and distribute various sections of the manuscript to his Yale students which the students perceived as a sign of uncertainty (Harold Finch, personal communication New York, July 1980).
8 His Nietzsche essay especially (cf. Malinowski 1993) testifies to his opinion about art. In Cracow and Zakopane he was surrounded by eminent established artists and great artistic talents from among his contemporaries.
9 This is his LSE Doctor of Science thesis *The Family among the Australian Aborigines: A Sociological Study,* London: University of London Press, 1913, in which he used the term 'function' but without explicit theoretical impact.
10 Witkiewicz 1981.
11 'Rivers is the Rider Haggard of anthropology; I shall be the Conrad.'
12 Cf. Malinowski (1911–13). The long review essay in *Lud* appeared in English only eighty years later and the 1915 book still remains untranslated.

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8 Malinowski and the origins of the ethnographic method

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Fieldwork by participant observation is recognized within and outside anthropology as a hallmark of the discipline. It is of no interest to continue arguing about who was the inventor of this research method, or where the invention first took place. What has some significance from a historical point of view is how it was discovered. Malinowski's ethnographic experience in Melanesia is still a relevant episode in the history of European anthropology when approaching this matter. In this chapter I shall compare Malinowski's ethnography in Mailu (Samarai) with his subsequent work during his first expedition to the Trobriand Islands. I shall attempt to show how Malinowski's invention of the ethnographic method was principally a result of his praxis in the field and not a natural outcome of his previous life or theoretical ideas.

Historians of anthropology have uncovered a great deal of detail about Malinowski's life, personality, historical background and theoretical training. Yet all this information does not explain how he came to revolutionize anthropological methods.[1] Why was Malinowski's ethnographic experience in Mailu so different from his work in Kiriwina (Trobriand Islands)? What changed it in just a few months? It was certainly not Malinowski's biography or his scholarly training. What really changed was his praxis in the field. Malinowski learned to do ethnography in a modern fashion while he was in Kiriwina. My thesis is not only historical but also epistemological. It is my view that a new knowledge, including methodological knowledge, is always a consequence of a particular kind of praxis.[2] Malinowski discovered a new way of doing ethnography in Kiriwina by modifying his behaviour in the field.

There were six crucial differences in Malinowski's ethnographic practice in the Trobriand Islands in comparison with his work in Mailu. (1) In Kiriwina he lived for a long time amongst the members of the

community that he studied; (2) he focused his research on a few specific subjects; (3) he studied the Trobriand people in their present existence and not their way of life in the past; (4) he learned the vernacular; (5) he increased the number of his own observations of native everyday life and institutions; and (6) he changed his style of reporting. It is my argument that these characteristics of Malinowski's Trobriand work lie at the core of the Malinowskian methodological revolution in ethnography.

'LIVING RIGHT AMONG THE NATIVES'

Malinowski finished his essay on 'the natives' of Mailu in Samarai at the beginning of June, 1915. From there he went to the Trobriand Islands. He expected to spend only 'one month' studying the 'material and artistic culture' of the Trobriand people, and then to continue his trip to the north-east coast (BM/CGS, 13/06/1915, in SP). Yet he stayed ten months in Kiriwina 'doing fieldwork…in the only way it is possible to do it' (BM/CGS, 30/07/1915, in SP).[3]

In the beginning he attempted to gather information from white settlers, in the same way as he had sometimes done before in Mailu. But soon he realized that 'it is quite futile to reckon on any one else but oneself (BM/CGS, 30/07/1915, in SP). Thus, at the beginning of July, he put up his tent in a village to live among the subjects that he was going to study. It was a completely new experience for him.

Throughout his stay in Mailu, Malinowski had lived outside the local village. According to his diary, Malinowski arrived at Mailu on 16 October 1914, and he stayed there until 25 November 1914. Then he took a three-week trip to the east coast. Afterwards he came back to Mailu and stayed there from 19 December until 23 January 1915. During that time he devoted himself to collecting ethnographic information about the Magi during less than forty-two working days. He gathered the ethnographic data by setting out to find his informants and visiting the village—many times accompanied by one of the locals.

Only in Kiriwina did Malinowski begin to put into practice his Mailu *dictum* 'the nearer one lives to a village and the more one sees actually of the natives the better' (Malinowski 1988:109). Such a close contact with Kiriwinans allowed him to witness their behaviour and check the informants' accuracy. Of course, a more intense relationship with them also increased the moral and emotional tension between Malinowski and them. Malinowski reveals this sentiment very clearly in a letter he wrote to Seligman on 19 October 1915:

> At times I feel damnably 'sick' (in the metaphorical sense) & I long to get away. Mind you, I am absolutely alone amongst niggers & at times they get on your nerves & add to it a bit of feverishness—any one would drink whisky under such circumstances. Now, I don't use neither whiskey nor the other 'white man's solace' *[sic]*—and such double abstinence makes life less merry.
>
> (BM/CGS, 19/10/1915, in SP)

When Malinowski pitched his tent in the village, he placed himself in the best circumstances to penetrate a form of life completely different from his own, but obviously he remained a member of his own culture. Malinowski's approach to the Trobrianders was cognitive, but not affective. Malinowski's virtue was not his capacity for empathy but his 'almost unbelievable capacity for work'—using Clifford Geertz's words (1967:12).

NARROWING THE FOCUS

Unlike Mailu, the Trobriand Islands had already been studied by Seligman when Malinowski visited them. He began his work checking the information on the islands that Seligman had published in his book *The Melanesians of British New Guinea* (Seligman 1910:660–735).

> I find your account of the Trobriands excellent, as far as it goes[.] (You express some misgivings in the Preface as to that part of your information; in fact, I don't think there is any essential inaccuracy in all you say about the Northern Massim).
>
> (BM/CGS, 30/07/1915, in SP)

After this quick review of Seligman's ethnographic account, Malinowski decided to continue his research on the Trobriand people, but in a different way. Instead of gathering more information on the same topics as Seligman—the table of cultural categories contained in *Notes and Queries*—Malinowski focused his investigations on a few specific topics. Later those research foci were the subjects of his principal monographs.

In Mailu, Malinowski had gathered information taking as a guide the *Notes and Queries*—the stereotyped system of cultural categories generated by the British anthropological tradition.[4] However, in the Trobriands, he left behind the traditional role of the ethnographer who went to the field to collect indiscriminately every kind of information

about the ancestral customs and beliefs of 'natives' in order to provide armchair anthropologists with facts that confirmed their theories. Instead, he attempted to investigate in the field several ethnological problems that he had found formulated in books by Frazer, Durkheim, Spencer, Westermarck, Rivers and others. Two issues had particularly attracted his attention: first, the relationship between religion, magic and economy in primitive societies; and second, the connection between native mentality and institutions. In the field, these theoretical interests drove Malinowski to study indigenous beliefs in the spirits of the dead and reincarnation, garden magic and the Kula ring.

> There are lots to be done yet—and things of extreme interest. There is their whole system of 'ceremonial gardening'—almost agricultural cult (in the Durkheimian sense); there are several beliefs and ceremonies about the spirits, BALO'M (even a belief in speedy reincarnation: a BALO'M goes to Tuma for another partial lifetime and then gets back into the first woman, it can get hold of; the annual harvest feast MILAMA'LA, connected and interwoven with the BALOM, a regular All souls day. The trading is much more peculiar and interesting, as it might appear at first sight.
>
> (BM/CGS, 30/07/1915, in SP)

Some could think that Malinowski's previous theoretical training played a definitive role in narrowing his research foci in the Trobriands. However, it should be remembered that he had carried the same theoretical ideas to Mailu, and there they did not have the same effect on his fieldwork. The reason for that change lies in the characteristics of Trobriand society and not in the theories that Malinowski had studied at home. Like a sculptor, in attempting to model his Trobriand ethnographic material, Malinowski was conditioned by the nature of the material itself. Even more, I think, Malinowski was anchored in the Trobriands by the kind of human material he found there: a matrilineal society politically organized into chieftainships, whose members were involved in a very complex system of rituals governing social life, and who, as it was phrased at the time, were ignorant of the physiological process of reproduction.

WHY NOT PRESENT FIRST?

At the same time as his research focus narrowed, Malinowski lost interest in the past life of Trobriand people. While he was reading Haddon, Seligman, Rivers and Spencer in the field, he asked himself:

why should we study the past of native institutions and not their present stage? Although Malinowski did not raise this question with his teachers in his correspondence, there is some evidence of his deep change of attitude in the notes he wrote in the margins of Rivers's book *Kinship and Social Organization*.[5] Rivers's aim in that book was:

> to show that *the terminology of relationship has been rigorously* determined by *social conditions* and that, if this position has been established and accepted, systems of relationship furnish us with *a most valuable instrument* in studying the *history of social institutions*.
>
> (Rivers 1914:1; underlined by Malinowski)

Malinowski marked this paragraph with two vertical lines and wrote next to it: 'Past[,] why not present first[?]' Until then Malinowski had been guided in his fieldwork by Seligman's directions. His work at Mailu had been 'a kind of practical training in [his mentor's] school' (BM/CGS, 20/09/1914, in SP). He had been looking for survivals of primitive society. His Mailu ethnography had been done from an evolutionist—or we might better say historicist—point of view. In the Trobriand Islands, on the contrary, he looked at native society from a synchronic, functionalist point of view. In my opinion, this change in the way of looking at things persuaded Malinowski to learn the vernacular language and to observe natives' behaviour.

TALKING AMONG NATIVES

During his stay in Kiriwina, Malinowski spent a long time learning Kiriwinian. Within three months he obtained a basic knowledge of that language. On 24 September 1915 he wrote to Seligman:

> I am beginning to talk Kiriwinian quite sufficiently to work in Kiriwinian, though I have an excellent interpreter at hand. By the way I am getting up a Grammar & Vocabulary.
>
> (BM/CGS, 24/09/1915, in SP)

Some weeks later he wrote to Seligman again, telling him that he had left his interpreter and that he was already talking to the Kiriwinians in their language.

> My work is going fairly smoothly, though I will have now a somewhat hard time, because I had to part with my interpreter,

> who was excellent in his 'professional capacity' but a bloody scoundrel in personal relations. But I have got so much Kiriwinian that for the last 3 weeks I hardly had to say a sentence or so per diem in Pidgin for the boy to interpret & having extremely intelligent natives to deal with, I am able always to get out of the difficulty.
>
> (BM/CGS, 19/10/1915, in SP)

Did Malinowski really need to learn Kiriwinian to study Trobriand people? What did he gain by learning their language? Was it just an instrument to get into touch with informants and nothing else? Could he have obtained the same information with an interpreter or by speaking a *lingua franca* such as Pidgin?

It seems that Malinowski did not consider it indispensable to learn the vernacular in order to conduct intensive ethnographic work. On 15 October 1915 he wrote to Rivers:

> I am doing 'intensive work' in the Trobriands and my linguistic facilities are of some use in it, though my experience has shown me that it is possible to do almost as good work with an interpreter, though one looses [sic] much time; when one begins to understand the natives talking among themselves, the old men discussing your question, or the people gossiping in the evening, lots of things crop up automatically.
>
> (BM/WHRR, 15/10/1915, in HC: 12055)

Does this mean that his learning of the vernacular did not play a crucial role in his work in the Trobriands? I do not think so. Malinowski writes in the Introduction to *Argonauts* that he realized the importance of speaking the language, when finally he was able to understand it. He discovered that, in order to understand the meaning of a native expression, it was not enough to find a verbal equivalence in other known languages. It was necessary to know the situation in which the expression had been pronounced. In 1923 Malinowski developed this idea in a famous paper, in which he explained his theory of meaning in primitive languages. Malinowski thought that learning a language and learning the culture in which that language is spoken are related tasks. Therefore, when he stated that he had learned the vernacular sufficiently, he did not mean to say that he had learned English or Polish verbal equivalences to native expressions. What he learned was how the native speakers used their language in their own culture. That is something he could do only by observing how the Trobrianders interact among one another, and by interacting with them himself—

linguistically or not, with or without interpreter, in their language or in a different one.

What does all this have to do with the fact that Malinowski took a presentist attitude in his research? We again find the answer to this question in the critical remarks that Malinowski wrote in the margins of Rivers's *Kinship and Social Organization*.

Rivers believed that kinship terminologies were a satisfactory instrument to study the history of social institutions. He thought that 'the greatest merit' of his genealogical method was that it made it possible to obtain records of marriage and descent and other features of social organization up to 150 years old (Rivers 1910:11). Malinowski considered Rivers's concepts of 'terminology of relationship' and 'social conditions' too restricted and partially wrong. He wrote on the first pages of Rivers's book:

> (A)
> — Terminology of rel[ationshi]p—is [a] too fragmentary concept, when [it is] referred to reality of living language
> — Terminology is a part within a determined whole—the 'indexed' language
> — Terminology is accidental as far as determination goes.
> (B)
> Social conditions, which determine k[inship], all its symptoms & manifestations are:
> The system of k[inship][,] i.e. arrangements, names, cognitive ideas & feelings which mould sentiments into definite patterns.

Malinowski accepted Rivers's idea that systems of kinship and marriage were determined by social conditions. For that reason he considered present conditions of primitive societies, and not their past, to be the cause of their kinship terminologies. From the horizon of the ethnographic present, learning the vernacular acquired new significance for Malinowski. Language was not only an instrument for collecting data from informants, but also the vehicle of native thought, and therefore a means to penetrate their mentality.[6] It was by learning the vernacular that Malinowski could gather the *corpus inscriptionum kiriwineiensium* he mentions in the famous Introduction to *Argonauts of the Western Pacific*.

INCREASING THE NUMBER OF OBSERVATIONS

Malinowski's interest in the present events of indigenous life also led him to put more emphasis on observation as a data-collection

technique. In Mailu he had tried to see as many things as he could,[7] but in fact he did not witness much, and he looked at the Magi 'from his verandah' very often—an attitude that he later criticized very harshly (see, for example, Malinowski 1926:92). For his Mailu ethnography he collected most of his information through interviews with informants. In those work sessions Malinowski used an interpreter and a Motu dialect to talk to the Magi.

> If not for the miserable shade of Motuan I have got I would have to close shop, for my Motu boy is far too stupid to be used as an unchequed [unchecked] interpreter. But I picked up a certain amount of Motu before & I am rapidly perfectioning it. Thus, slowly but at a definite rate I am working out the material.
>
> (BM/CGS, 03/11/1914, in SP)

In his diary he described his Mailu stay as 'short' and 'superficial', and he wrote that he conducted his fieldwork there under 'poor circumstances' (Malinowski 1967:72). He did not live among the Mailu people and he did not observe them enough.

Malinowski achieved both aims in the Trobriands. On 24 September 1915 he wrote a letter to Seligman in which he suggested several subjects on which he could write a paper for the *Journal of the Royal Anthropological Institute*. In that letter he also gave Seligman a clear idea of the kind of materials he had collected until then:

> I am not going to be in a hurry publishing this stuff, but I shall be able to send you an article for the Journal [of the] R[oyal] A[nthropological] I[nstitute] & publish some stuff in Australia. Let me know which would you think best for the J.R.A.I.: 1) Land tenure & gardening[,] inclusive garden magic (very good informa[tio]n; [I] have seen & watched 60% as an eye witness, [I have] got magic (esoteric) from [the] biggest man in the island[.] 2) Burial, afterlife, mourning. This stuff I have got also well done: [I have] seen 3 deaths, one almost inmediately after expiration [and] 2 in wailing stage; 1 burial & any amount (over a dozen Sàgali). But this information would encroach on your stuff (that Chapter in [on] the N[orthern] Massim is the only one that needs serious amplification, as far as my present knowledge goes). I have been through 2 Milamalas, too. 3) Short article about reincarnation; ideas about conception and pregnancy.
>
> (BM/CGS, 24/09/1915, in SP)

Malinowski became an observer only when he put himself into the proper situation for observation and opened his eyes to the ethnographic present. All this happened during his first period of fieldwork in the Trobriand Islands. On 25 May 1916, while he was analysing his field material in Sydney, he wrote a letter to Haddon to tell him of the success of his enterprise:

> I have spent over 8 months in one village in the Trobriand and this proved to me, how even a poor observer like myself can get a certain amount of reliable information, if he puts himself into the proper conditions for observation.
>
> (BM/ACH, 25/05/1916, in HC: 7)

A NEW ETHNOGRAPHIC DISCOURSE

After ten months of fieldwork in the Trobriands, Malinowski went back to Australia, where he wrote his first Trobriand ethnography: 'Baloma' (published in 1916). In this short monograph about Trobriand beliefs, Malinowski foreshadowed a new style of writing ethnography. It was the first time in British anthropology that an author, in order to anchor his discourse, included theoretical and practical contexts of his research in an ethnographic text.

In 'Natives of Mailu' (published in 1915) Malinowski had limited himself to presenting his facts, following too slavishly the format of the fourth edition of *Notes and Queries*. Contrary to his suggestion in the 'Introduction' to this monograph, he did not introduce a new order in his ethnographic discourse at that time. 'By adopting a fairly systematic division' (geography, sociology, economics, magico-religious matters, etc.) of chapters and sections, he did not deviate from 'a purely topical presentation of facts', as he claimed (Malinowski 1988:110). On the contrary, that division just covered the topical organization of *Notes and Queries* (Young 1988:26–7). In writing his Mailu ethnography, Malinowski remained loyal to the style of the Cambridge school.

Malinowski left that style behind only when he began to write 'Baloma'. This essay contains the basic lines of the new model of ethnography that he later improved in writing *Argonauts*. In 'Baloma', Malinowski attempted to construct an ethnographic argument which combined three elements: fieldwork data, information about the research process, and theories.

Some authors have tried to analyse ethnographic texts as if they were literary fictions (Marcus 1980, Clifford 1988, Van Maanen 1988, Atkinson 1990). In my opinion these interpretations are one-sided,

since both kinds of texts have different aims and, what is more important, different processes of construction. In order to write an ethnography, it is necessary to collect data in the field and to analyse the information afterwards. That is what Malinowski did. Reading Malinowski's fieldnotes[8] one realizes that the narrative structure of 'Baloma' was the result of a qualitative analysis of his data. The division of chapters and sections in the essay was the product of indexing, rearranging, checking, selecting, merging, etc. of his fieldnotes (see Alvarez Roldán 1992c). Details about the research process (descriptions of his informants, contrasts between observations and oral reports, distinctions between particular and general opinions of respondents, indications of the researcher's linguistic competence, etc.) and theoretical references (e.g. his critique of Durkheim's concept of *conscience collective,* the classification of social beliefs, etc.) that appear in the text should not be interpreted as mere rhetorical devices (Malinowski 1913, 1916). The description of conditions under which Malinowski gathered his information in the field and some of his theoretical ideas are essential elements for the assessment of the text's ethnographic validity (Sanjek 1990). After his Trobriand experience, Malinowski tried to ground his monographs by building ethnographic reasoning into them. Thus he opened a new ethnographic discourse. With 'Baloma' ethnography took the form of a continuous constructive process, involving the tasks of doing fieldwork and writing—two related phases of the ethnographic process. Thanks to his experience in the Trobriands Malinowski came to revolutionize ethnography not only as a fieldwork process but also as a written product.

NOTES

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1 For instance, Ernest Gellner has suggested in a thought-provoking paper (1985) that Malinowski's Polish background could have influenced the way he later approached fieldwork, as well as his interpretations of Trobriand society. Another good example of this kind of explanation is

Skalník's chapter in this book. For a panoramic view of Malinowski's theoretical influences, see Stocking 1983:93–5; 1986:19–22. An example of partial socio-political explanation can be found in the recent book *The Savage Within* by Henrika Kuklick:

> Not until after World War I, when colonial authority seemed secure in most parts of the Empire, did it become routine for anthropologists to go into the field to collect their own data for analysis, and the discipline's altered methodology was at least *a partial function* of political change, for anthropologists could be reasonably confident that peoples accustomed to defer to colonial rulers would be cooperative subjects. In the prewar era, scholarly anthropological analyses based on extensive field experience were written, but they were produced by men who were themselves colonial agents—missionaries and colonial administrators.
>
> (Kuklick 1993:287; my emphasis)

2 For further comments on this idea, see Alvarez Roldán 1992a, 1992b.
3 The phrase 'living right among the natives' is taken from a letter that Malinowski wrote to Haddon on 15 October 1915: 'Out here one feels sometimes lonely and isolated (I am living right among the natives in a village, since beginning of July) and it is a great help in work to receive such letters as yours and the letters I am receiving from Seligman.' (BM/ACH, 15.10.1915, in HC: 7).
4 Malinowski (1988). See Young (1988:24–7).
5 The copy of Rivers's book that Malinowski took with him to the field remains in the British Library of Political and Economic Science catalogued as R (Coll). Mis. 392 (1) M 695.
6 Raymond Firth has suggested a similar idea (1981:124).
7 On 24 November 1914 he wrote to Seligman:

> I think I have got now a fairly complete all round picture of the Mailu—but it is a rough sketch in black & white. So far as the touches of colour are much more difficult to get. I am trying to *see* as many things done as I can. I hope I shall be able to see a couple of feasts in the end of December or beginning January—the great annual feast.
>
> (BM/CGS, 24.11.1914, in SP)

8 Twelve of the field notebooks that Malinowski wrote during his first period of fieldwork in the Trobriand Islands survive in the Manuscripts Room of the British Library of Political and Economic Science, London. They contain entries from the end of June 1915 until the middle of February 1916. The notes on Baloma appear in all the notebooks.

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Part III

Anthropological traditions in Europe

9 Sweden: central ethnology, peripheral anthropology

Tomas Gerholm

The German distinction between *Volkskunde* and *Völkerkunde* has also been upheld in Sweden, although under different names. The labels currently in use are ethnology and social (or cultural) anthropology, but until quite recently ethnology was—and to some extent still is—called 'folk-life research' *(folklivsforskning)* and until the end of the 1960s anthropology had the name 'general and comparative ethnography' *(allmän och jämförande etnografi)*. Traditionally, the distinction between them has been clear-cut. As a common adage puts it, the ethnologists are the children of nationalism, the anthropologists of imperialism. In other words, while the ethnologists study Swedish (or possibly European) society, the anthropologists devote themselves to non-Western (or at least non-Swedish) society. Since the 1970s, however, there has been a slowly increasing number of Swedish anthropologists doing fieldwork in Sweden, initially among immigrant groups but later also among 'ordinary' Swedes.

Both ethnology and anthropology have enough practitioners internationally for us to be able to speak of a world-system of ethnology and a world-system of anthropology. These world-systems of academic disciplines, just like any world-system, have their centres and their peripheries. What is meant by this formulation may come across most easily if I use an example; I take that of anthropology, the world-system of which has been described in the following terms:

> It seems that the map of the discipline shows a prosperous mainland of British, American and French anthropologies, and outside it an archipelago of large and small islands—some of them connected to the mainland by sturdy bridges or frequent ferry traffic, others rather isolated.
>
> On the mainland, people can go through their professional lives more or less unaware of what happens on the islands. The reverse

> seems not so often to be the case. If international anthropology to a great extent equals American+British+French anthropology, in other words, then these national anthropologies need hardly take external influences into account to more than a very limited degree. To find a more intricate interplay between national and international anthropology, and perhaps also some tension between them, we have to go ashore on some of the islands… one of our interests is in the inequalities of international anthropology; in the ways the strong influence those relatively weaker. For such purposes, it is tempting to draw on recent thinking about 'center and periphery'…
>
> (Gerholm and Hannerz 1982:6)

In this chapter I want to compare the Swedish versions of ethnology and anthropology with regard to their position in their respective world-systems. Do they belong to the ethnological and anthropological mainland or are they only more or less isolated islands in the archipelago? What is the nature of the relationship between these centres and peripheries? Let me begin with two vignettes, which made me interested in these questions.

THE PILGRIMAGE OF HENRY GLASSIE

In a recent book on the renaissance of material culture as a subject worthy of ethnological research one comes across the following passage:

> On the Continent the status of ethnological research on artifacts is much higher. Apparently we need a Henry Glassie from Pennsylvania in order to start our rethinking in Sweden. In a 1988 issue of Doktorandbulletinen from the Institute for Folk Life Research in Stockholm, Ulf Palmenfeldt has written: 'It sounded as if several persons in the audience got the hiccups from pure surprise when Henry Glassie…started by calling Sigurd Erixon his theoretical guiding star, and then almost cried for joy when he told us about Skansen's rural houses and finally claimed that he experienced the journey to professor Erixon's institute at Lusthusporten as a pilgrimage.' According to the same referee, Åke Daun [the present professor] sounded 'quite regretful when he said after the lecture that we all belong to a generation of ethnologists who have been taught to see material culture as less important than human action. Åke promised that we shall redeem

> the error by observing the 100th anniversary of Sigurd Erixon's birth.'
>
> (Arvidsson *et al.* 1990:14)

The reason for the consternation among the audience was that at least two decades had already passed since Sigurd Erixon had been deemed required reading for budding ethnologists. How could he be so central and peripheral at one and the same time?

FIELDWORK AMONG ETHNOLOGISTS

In the mid-1980s Lena Gerholm and I made a study of six academic disciplines at Stockholm University viewed as 'cultural systems' in the Geertzian sense (Gerholm and Gerholm 1992). The study was carried out as if we had been dealing with exotic tribes: we read a great many of the relevant texts but, above all, we did participant observation by taking part in most activities that the PhD candidates of the disciplines devote themselves to. Ethnology and social anthropology were among those disciplines.

We were struck by the relation between these two subjects. Among the ethnologists, anthropological texts were very important indeed. Most theoretical texts that were discussed in the seminars were in fact anthropological and often texts that the local anthropologists a few kilometers away were also studying. They were taken from the very front line of international anthropology, mostly from American and sometimes British authors, rarely from Swedish anthropologists. The reflexive movement was eagerly studied and so was 'anthropology at home' which was treated as a novelty—as if ethnologists had ever done anything else but anthropology at home. We wondered at times whether the persistent focus on the developments in the neighbouring discipline was really relevant to ethnological concerns. We labelled this phenomenon the 'Big Brother Complex'.

At the same time, it was astounding to notice the relative disdain for the local anthropologists at the same university. They were not regarded as carriers of the anthropological visions that the ethnologists studied at their seminars. The ethnologists preferred to turn their gaze towards the centre of the international discipline, just as the local anthropologists themselves did.

The anthropologists did not return the ethnological gaze. Ethnological texts were *never* studied at the seminars (although, of course, an anthropologist dealing with Swedish society would at least read some of the relevant ethnological literature). In general,

ethnologists were not regarded as having their own theoretical perspective.

There were exceptions. Orvar Löfgren and Jonas Frykman of Lund University were respected for their clever and strikingly wellformulated applications of anthropological ideas (see, for example, Frykman and Löfgren 1979). But it was noticed, with some irritation, that these persons had their own international networks of anthropologists, and that on the international scene they were often regarded as Swedish anthropologists, perhaps even as the leading ones.

A few years later, around 1990, the ethnological infatuation with anthropology seemed to have subsided and there was a renewed interest in the traditional perspectives of the ethnological discipline. Theoretical novelties continued to be imported but not exclusively—perhaps not even predominantly—from anthropology. Particularly in Stockholm the folkloristic element in Swedish ethnology was strengthened, partially due to the efforts of Dr Barbro Klein who had returned after two decades of folkloristic work in the USA. Ethnology appeared to be well on its way along a new course and the distance from anthropology widened.

All this, however, was only one incident among many in the borderland between ethnology and anthropology, if looked at from a historical perspective. According to the common—but, as I hope to show, mistaken—view in the Stockholm ethnology department, the beginning of anthropological influence in the discipline should be set at the end of the 1960s. To be more specific, this was the time when Åke Daun, the future professor at the department, returned to Stockholm after having spent a semester at Bergen (Norway), where Fredrik Barth was laying the foundation of Norwegian anthropology as well as giving the most significant Scandinavian contribution to international anthropology (cf. the *Festschrift* for Barth, edited by Grønhaug, Haaland and Henriksen 1992). This may very well have been the beginning of the influence of *modern* social anthropology on ethnology in Sweden, but anthropological ideas had been of importance much earlier.

PERIPHERAL ANTHROPOLOGY

Throughout its history—and it is hardly more than a century old—Swedish anthropology has been securely anchored on the outskirts of the international discipline. In the beginning it existed only in connection with the ethnographic departments of a natural history museum (in Stockholm) and a general city museum (in Gothenburg). In

the 1930s and 1940s these departments were transformed into fully-fledged ethnographic museums. The first teachers of anthropology—Erland Nordenskiöld, Karl Gustav Izikowitz, Gerhard Lindblom and Sigvald Linné, for instance—divided their time between the museum and the university. They were very few, a fact that may sufficiently explain their modest place on the international scene. A report published in 1959 had the following to say about numbers: The sum total of scientific posts at the museums is no more than seven. Thus a professional anthropologist has few possibilities for making a living' (Izikowitz, Moberg and Eskeröd 1959:669).

Even though scholars such as Erland Nordenskiöld and Karl Gustav Izikowitz had international contacts and published their findings in one of the 'great' international languages, they were rightly regarded internationally as individual Swedes, not as exponents of a Swedish school of anthropology.

It was not until the late 1960s and early 1970s that the demographic basis of the discipline expanded so that it would have been reasonable to speak of a 'school'—if there had been one. It was also at this time that the previous name, 'general and comparative ethnography', was changed officially into the current names, 'social anthropology' and 'cultural anthropology'. The difference between them is, one should perhaps add, minute. The change of names is also indicative of another, greater and more important change. Between the wars Swedish academic life was dominated by German influences, but after the Second World War a radical shift took place. German disappeared as the first foreign language and was replaced by English. At the same time, German universities lost their previous importance and Anglo-Saxon ones took their place. As a consequence of this general realignment, it is not surprising that the cultural historical approach of German *Völkerkunde* gave way to fieldwork-orientated British social anthropology. There were hardly any spectacular conversions, however: the change was mainly generational. New names also meant the arrival of a discipline in which theoretical concerns were much more important than in the old, largely museum-based and material culture-orientated ethnography. The scene was set for anthropological contributions that might change the disciplinary setting from a location on the periphery into one at the centre.

This hardly happened, however. The growing community of Swedish anthropologists has become a more deeply integrated part of international anthropology than ever before—bridges and ferries abound. But this integration has not made Swedish anthropology stand out as an important landscape on the anthropological *mainland*. Rather,

it has become a holiday resort for international anthropology. Many Anglo-Saxon anthropologists have spent a semester or two in Sweden as guest professors. Gerald Berreman, Maurice Bloch, Sandra Wallman, Bruce Kapferer, Bill Arens, Steven Kemper and Roy Willis may serve as examples. And a few Swedish anthropologists, above all Ulf Hannerz, professor in Stockholm and the main architect behind this the largest of the Swedish departments, have become well-known participants at conferences on the international scene. But I think that they are still like Erland Nordenskiöld and Karl Gustav Izikowitz: Swedish individuals and cosmopolitan anthropologists, not real representatives of Swedish anthropology. The anthropological journal *Ethnos,* which is published in English (with an occasional article in French), is dominated by contributions from the international community. The very fact that Swedish *Ethnos* is far from primarily being an outlet for Swedish anthropology brings home the point that Swedish anthropology belongs at the periphery: it is peripheral even in its own journal!

CENTRAL ETHNOLOGY

What about ethnology, is it also peripheral? Probably not. For one thing, it would not be correct to say, as I did about anthropology, that ethnology throughout its history has been located on the periphery of the international discipline. Since the 1930s, at least, Swedish ethnology has been a highly respected member of the European ethnology family. Swedish textbooks on ethnology have been translated into German, while there has been no translation traffic in the other direction. But the main reason for placing Swedish ethnology near the centre of European ethnology can be summed up in one name: Sigurd Erixon.

Sigurd Erixon was born in 1888 and died in 1968, at the time when a number of leading European ethnologists were preparing a twovolume *Festschrift* to honour *'le Doyen des ethnologues européens'* (Bratanic *et al.* 1970:5). Erixon was in charge of ethnological field studies at the Nordic Museum in Stockholm from 1916 to 1934, when he was promoted to professor of 'Nordic and comparative folk-life research'. He remained in this position which made him share his time between the Nordic Museum and Stockholm University until 1955. Erixon was a prolific writer who had, towards the end of his life, more than 500 titles to his credit. He specialized in various manifestations of rural material culture as shown in works such as *Folklig möbelkultur i svenska bygder* (1938) [Popular furniture culture in the Swedish

countryside], which was reissued in 1970, and *Svensk byggnadskultur* (1947) [Swedish building culture]. His lifelong interest in the spatial distribution of artefacts led finally, in 1957, to the publication of an important atlas, *Atlas över svensk folkkultur* [Atlas of Swedish folk culture]. Erixon's main importance, however, may have lain in his activities as the founder and editor of ethnological journals intended to serve the ethnologists in Europe in whatever country they happened to live.

Folkliv (folk-life) was the first of these journals. It started appearing in 1937 and three thick volumes were produced before the war. English was the most important language followed by German and later French. The subtitle printed on the cover of the journal varied over the years in an interesting way. In 1937 it was 'Review of Nordic and European Ethnology'. This title was also given in German and French. The Swedish subtitle was 'Tidskrift for nordisk etnologi'. In 1938 the specification 'Nordic' disappeared, while both the English and the French subtitles now included the subject 'folklore': 'Journal for European Ethnology and Folklore', *'Journal d'Ethnologie et Folklore Européen'*. The inclusion of folklore was also indicated by a sudden hyphen in the middle of the main title: *Folk-Liv*, in which the first part seems to have been a reverential bow to the folkloristic interests while the second part signalled one of Erixon's own principal theoretical ideas, to which I shall return.

The reason for this change was that *Folk-Liv* had merged with *Folk,* an international journal of folklore studies which had been published by the Association for European Ethnology and Folklore and by the *Comité Exécutif de Folklore International* in two numbers in 1937. In 1939, finally, the subtitle read 'Acta ethnologica et folkloristica Europaea'. The reason for this change was another fusion or perhaps takeover, this time of *Acta Ethnologica* that had been published in Copenhagen for a couple of years (3 vols, 1936–8). So far the history of *Folk-Liv* was a history of expansion. During the war years, however, *Folk-Liv* was primarily published in Swedish. Its international character was gradually resumed after the war.

In the 1950s, Sigurd Erixon was asked by the Commission Internationale des Arts et Traditions Populaires (CIAP, established 1928), an organization which he had helped to revive after the paralysis inflicted by the war, to assume the editorship of a new annual publication financed by UNESCO and printed in Sweden. This annual, called *LAOS,* carried material in English, French and German and gathered contributions from all parts of Europe. Due to financial problems it had to cease publication after three volumes (1951–55).

Finally, in 1965–7, Erixon was at the head of a new European ethnological journal, *Ethnologia Europaea,* which he wanted to devote to that kind of comparative ethnology he had been championing during the greater part of his life.

The role played by Sigurd Erixon in these three important journals dedicated to European ethnology is an indication of an organizational talent that was also employed in several international organizations. It is not surprising that he was regarded, for several decades, as a leading European ethnologist. No doubt, he was also a strong personality. But when it comes to presenting what his *theoretical* contributions to the science of ethnology were, the picture is less clear.

In this context it is useful to consult *Erixoniana,* the alreadymentioned *Festschrift*. The editors of these two volumes, which also coincided with vols II–III and IV of *Ethnologia Europaea,* hailed him as the one who laid the 'scientific basis' for the journal, and in their invitation to prospective contributors they inquired about the influence of the 'Erixonian school' and asked for an evaluation of the concepts used by this 'school'. 'L'école erixonienne'—what was it? Some of its ideas emerge in various contributions to the *Festschrift*.

Erixon's Swedish colleague Gösta Berg (1970) mentions a couple of ideas that perhaps could be said to belong to Erixon's school:

> Sigurd Erixon never ceased to stress the significance of defining the spatial dimension of cultural phenomena, and in this respect he came to occupy an internationally recognized position as an expert and as a promotor of new enterprises in this field.
>
> (Berg 1970:8)

Berg also mentions Erixon's interest in methodological problems and his 'experiments in quantifying the human ways of living' (Berg 1970:10). A general consequence of this dealing with methodological issues was his concern for international co-operation:

> It was quite natural that Erixon's interest in methodological problems…made him take part in the international efforts to ameliorate the conditions of ethnological research work and to further a fruitful cooperation across all national and linguistic borders. He had long been the leading force in many associations with such aims.
>
> (Berg 1970:10)

One of the editors of the *Festschrift,* the Hungarian Geza de Rohan-Csermak (1970), singles out two specific theoretical contributions of

Erixon's school: his theory of the integration of ethnology and his concept of life.

De Rohan-Csermak starts by depicting the situation of ethnology in Europe before the impact of Sigurd Erixon. The intellectual interests of ethnology were dispersed over many different disciplines such as the history of literature and of art, cultural geography and agrarian history. This dispersion prevented many ethnologists from clearly realizing their relation to general ethnology or, as we would say today, to anthropology.

> Sigurd Erixon was the one who had the strength to go against this unfortunate tendency. He had the qualities and the dynamism necessary and, of course, also the fortune of belonging to a national school whose scientific traditions were closer to general ethnology than perhaps was the case in other countries. Erixon also had the capacity to organize and launch an international movement for European ethnological collaboration.
>
> (de Rohan-Csermak 1970:11)

According to Sigurd Erixon, the study of European ethnic cultures forms an integral part of anthropology. This is a position that one finds articulated throughout his work. Thus, there are not two ethnologies—ethnology proper and anthropology—but only one, general ethnology, i.e. anthropology, even through practical reasons may impose a division of labour according to the geographical area one is working in. The ethnological study of European cultures cannot at all be described as an isolated discipline independent of the branches of ethnology on other continents' (de Rohan-Csermak 1970:12). Although there is no evidence of it, Erixon must have been against the common division in German-speaking countries between *Volkskunde* and *Völkerkunde,* at least if it indicated different theoretical universes. Erixon's position met with approval above all in the countries of Central and Eastern Europe, where the dualist conception of ethnology and anthropology had not already been firmly established (de Rohan-Csermak 1970:13).

The second theoretical contribution by 'l'école erixonienne' was the concept of 'life'. According to de Rohan-Csermak, Erixon was also in this case fortunate to have had behind him the rich scientific traditions of his Scandinavian home country. In Scandinavia, a synonym for ethnology was from the very beginning of the century *folklivsforskning* in Sweden, *folkelivsgransking* in Norway *and folkelivsforskning* in Denmark. Erixon managed to give this term a theoretical interpretation so that 'folk-life' came to designate a special angle from which the

ethnologist could observe and analyse culture. When de Rohan-Csermak goes through Erixon's various writings on this theme he finds a certain development of the concept which reaches its end in his last contribution to the journal *Ethnologia Europaea*. The ethnological concept of life does not refer to life in its entirety, not its somatic foundation nor its purely psychological and personal side, but to that part of life which is due to social transmissions and contacts. In other words, says Erixon, 'folk-life is the social life' (quoted by de Rohan-Csermak 1970:15).

According to de Rohan-Csermak, the Erixonian interpretation of the concept of life is a humanist one, since it accentuates that part of life which is determined by culture and not by nature. Thus, the goal of folk-life research is to elucidate culture and its role in life as well as the influence of life on the development of culture.

> In this context, even the concept of culture receives an original interpretation by Erixon. This culture…becomes, in Erixon's perspective, real and dynamic, a culture that is lived and determined by vital factors that are human *par excellence*…. To bring together the concept of culture with that of life is above all to demonstrate that they are intimately united in reality. This joining of concepts allows us to see culture in permanent change, a culture which is not only lived but which is living and which does not live independently of but through man, through human life.
>
> (de Rohan-Csermak 1970:16)

To the anthropologist of the 1990s this concept of culture may not be very new. Perhaps it had more freshness in the times of Sigurd Erixon, not only among ethnologists but also among some anthropologists.

However, after having read this evaluation together with others in the two volumes of the *Erixoniana,* it is tempting to draw the conclusion that Erixon's greatness rested primarily on his ability to put ideas into practice. His own theoretical contributions were not always highly original. They often had much in common with ideas that had been around for some time, having been developed by different scholars but not having been put systematically into practice. And *this* was Sigurd Erixon's forte.

CONCLUSIONS

Scattered interviews with members of the two disciplines indicate that there is a high degree of consensus concerning the position of one's own discipline within the relevant world-system. Anthropologists agree

that the Swedish version of the discipline is a rather peripheral creature on the international scene. Ethnologists, on the other hand, are well aware of the importance of Sigurd Erixon in the past, while hoping that the leaders of the present generation have managed to keep Swedish ethnology in a reasonably central place.

For this purpose members of both disciplines seem happy with the view of the disciplines as independent of each other. But on other occasions it is obvious that there is a curious intermingling of them. Ethnologists have been very dependent on anthropological theory in recent decades and some of the leading figures act as anthropologists on the international scene. But few ethnologists seem to realize that the leader of Swedish ethnology—at a time when it was definitely a factor to be reckoned with on the international scene—considered ethnology to be part of 'general ethnology', i.e. anthropology. Swedish anthropologists recognize their peripheral status in the international discipline but seem to comfort themselves by assuming a haughty attitude to local ethnology which they consider to be, at best, second-rate anthropology. (A common complaint is that ethnologists settle for culture and seem unaware of social structure.) Perhaps they have not realized that the ethnologists extend the same kind of judgement to local anthropology: second-rate.

A curious twist to these centre/periphery relations is that while Swedish ethnology may still have a relatively strong position internationally, it seems that at times practitioners of it have opted for a peripheral position in the *anthropological* world-system. So, without knowing it, such ethnologists are following in Sigurd Erixon's footsteps: there is only one ethnology, namely anthropology.

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10 The anthropological tradition in Slovenia

Zmago Šmitek and Božidar Jezernik

Anthropology in Slovenia is one of the younger academic disciplines. In Slovenia cultural anthropology acquired its place at the Faculty of Philosophy only in 1991 when, following changes in the curricula, the Department of Ethnology *(Oddelek za etnologijo)* was formally renamed the Department of Ethnology and Cultural Anthropology *(Oddelek za etnologijo in kulturno antropologijo)*. Before that, i.e. since the end of the sixties, political and social anthropology had been one of the subjects studied at the Faculty of Sociology, Political Sciences and Journalism in Ljubljana.

Despite its unofficial status anthropological issues prior to this still aroused scientific interest. In spite of the prevailing ethnological tradition in Slovenia there has also existed a comparable active anthropological current of thought. In this chapter we will survey some of its main exponents from the sixteenth century onwards. Because of the vague demarcation between anthropology and ethnology (or ethnography, which dominates in Slovenia), our chapter focuses specifically on the viewpoints of Slovene authors with regard to the relation between the two fields.

Modern Slovenian experts advocate a spectrum of different views concerning the relation between anthropology and ethnology. For the majority, anthropology is the study of other, mostly non-Western, peoples and cultures, in contradistinction to ethnology which ranks as a 'national' discipline *par excellence*. According to some authors, ethnology in Slovenia deals with Slovenian folk culture (Novak 1986:367–8). All existing surveys of the history of Slovenian ethnology (except Kremenšek 1989) hold this view, where one seeks in vain for writers dealing with non-Western peoples and cultures (Kotnik 1944; Kremenšek 1978; Slavec 1983; Slavec 1988).

Among Slovenian ethnologists there exists a fairly widespread opinion that in contradistinction to the historically rooted ethnology,

anthropology is the study of peoples and their cultures from a nonhistorical angle, i.e. an unscientific undertaking: the belief that history is the only science did not die with Marx. Still others believe there is no distinction between anthropology and ethnology; these are two terms for the same discipline (Telban 1991). We hold that the anthropological and the ethnological traditions in Slovenia cannot be equated. These two traditions often interlink or overlay one another and sometimes develop independently of each other.

The principal characteristic of the anthropological tradition in this context is its aim to treat its subject holistically. Anthropology concerns itself with the questions: what is humankind, what is its origin and its process of cultural maturity, what is it that makes 'humans' different from animals? Ethnology in Slovenia does not explicate these questions as dilemmas and still continues to behave as if a nonproblematic approach or an ignorance of these questions is possible.

According to the latest research, the term anthropology was first used in Slovenia by Gian Rinaldo Carli, a member of the old Italian nobility from Koper, a coastal town in south-western Slovenia, in his poem *Andropologia, ossia della societá e della felicitá* (1786). The title indicates that by 'andropologia' he meant a philosophical cognition of general social and cultural laws. According to Carli, anthropology should have an entirely practical side: it should enable a happy life, full of contentment, even in the 'corrupt society' (Carli 1787:282).

In the second half of the nineteenth century, two terms were used in Slovenia: *anlropologija* and *narodoznanstvo* (knowledge of the nation), each with its own precise definition. The first meant 'a science of humankind' or 'a science of human nature', while the second term, which was equated with the terms *Ethnologie* and *Völkerkunde,* denoted a science of (foreign) ethnic groups. In the first case the emphasis was on the generally human and, in the second, on the national (Cigale 1860:83, 1807; 1880:5, 36). This 'second wave' of the development of the field offers no historical basis for equating anthropology with (non-Western) ethnology (Vermeulen in this volume).

Why did the term and the usage of ethnology prevail in Slovenia? Perhaps it gained popularity as a result of the Slovene feeling of being nationally imperilled in the nineteenth and the first half of the twentieth century. Within the contemporaneous European networks the nation as a collective entity was the relevant political factor, not the human as an individual. Nowadays, traces of the opinion still persist that Slovenia has never had an anthropological tradition and does not need one now.

Folk culture, ethnology's subject matter, was essentially the phenomena of the past epochs. It had a connection with prehistory. Folk culture should have been the product of irrational creativity and typical thinking. Support of tradition and community should have been its basis, but they had started to disintegrate under modern civilization. The citizens should not have been formed organically but should represent the pure mass and the product of mechanical laws of modern life. Therefore, as Rajko Ložar put it, the citizens are the subject of social ethnography and not of *narodopisje* (science of folk or nation) (Ložar 1944:20).

Due to great social changes, characteristic of the mid-twentieth century, traditional folk-life started to vanish rapidly. Therefore, thought Niko Kuret, the whole European East came to a turning point in the 1960s. In the industrial highly developed countries of Western Europe, where the traditional folk-life 'had almost died out' the call for the newly orientated *narodopisje* emerged, the call for something that would resemble cultural anthropology in the USA. In Eastern European countries, the movements towards cultural anthropology were still merely weak attempts (Kuret 1965).

Although the changes in Slovenian traditional life were 'quick and perfect', cultural anthropology was not very influential. Its advocates were silenced by those who, influenced by Soviet ethnography, claimed that cultural anthropology was a deviation from histor(iograph)y. This was a strict approach. The Soviet social sciences thought historical development was a process which followed certain objective laws. Therefore there was no room for the subjective factor, which also influenced the cultural phenomena and changes in the way of life: 'For the most part American cultural anthropologists or ethnologists do not see the historical process of development as the object of study which would make it possible to discover the nature of human beings and the laws shaping their way of life. They do not seek the answers to those questions in history, but in psychology…' (Kremenšek 1965b: 367). Such views were strictly rejected by Kremenšek, who wrote: 'There is no real connection between the historical process of development and its adequate interpretation and the cultural and social anthropological, biological and psychological elements' (Kremenšek 1965a: 106). In accordance with the so-called adequate interpretation of the social historical process of development, Kremenšek advocated that ethnology should investigate the townspeople as well. In his own research he was interested in workmen, that is, in the collective entity.

A lexicographical definition of anthropology postulates that it is the study of human beings in relation to the distribution, origin,

classification and relationship of races, their physical character and their social relations and culture. Anthropology as a word is first recorded in 1501 (Hundt 1501), although anthropological issues were considered earlier: 'Anthropological questions are timeless because they center around the universal concern to understand human existence and human behavior' (Malefijt 1974:vii).

The fundamental difference between anthropology and ethnology is that the subject of anthropology is humankind while the subject of ethnology is ethnic/national entities. According to currently available sources, the word 'ethnology' first appeared in the second half of the eighteenth century, specifically in the eastern part of Central Europe. At that time ethnology developed as a discipline which was to help cope with contemporary problems, particularly those faced by the Austrian court confronted with the ethnic variation of the population in the Danube basin, in the area liberated and taken from the Turkish empire. The term ethnology was used in this sense by Adam Franz Kollár in 1783. He regarded ethnology to be an intellectual activity in which the study of the language, customs and the institutions of individual peoples uncovered their origins and first settlements. Ethnology should thus be concerned with the ethnic history of individual communities (Kollár 1783:80–1; Belaj 1989:9). While the very term ethnology implies belonging to an *ethnos* is essential, for the human as an individual it is often more important to understand where s/he belongs according to sex, race, religion, age, vocation, class, etc. In nineteenth century Europe, belonging to an *ethnosi* nation obtained special significance during Romanticism, a movement intent upon discovering the power of national awareness to form states.

The beginnings of anthropology are linked to encounters with members of foreign cultures. Many mention Herodotus as a founder. For anthropology, the difference between cultures is essential. The rule that the anthropologist is a 'foreigner by profession' still holds today. Only the comparative method enables one to postulate and examine certain problems. Without comparison, anthropology as a science is not feasible. Because a comparison can only be made from a fixed cultural position, anthropology is in a permanently paradoxical position, doomed to be insufficient. The anthropologist may be dissatisfied with and critical about his/her own culture, s/he can be non-critical, or indifferent, but s/he cannot be without it. Therefore, his/her view is necessarily burdened by the cultural environment in which s/he was socialized.

According to Hodgen, in the sixteenth and seventeenth centuries the interest in the cultural characteristics of humankind in far-away lands and times was immeasurably sharpened:

> This sixteenth- and seventeenth-century literature, which laid the foundation of modern anthropology, comparative religions, anthropo-geography, and many other related studies, exhibits the emergence of what must now be regarded as the scientific method in the study of culture and society: first, in a definite transition from the motive of entertainment to that of inquiry; second, in the more or less clear statement of questions or problems of importance; and third, in the choice of organizing ideas to be employed in dealing with the problem of the origin of man, of diversity of cultures, the significance of similarities, the sequence of high civilizations, and of course the process of cultural change.
>
> (Hodgen 1971:8)

The first works of anthropological character arise from the pens of travellers who travelled to distant foreign places. This also applies to the Slovenian tradition, which starts in the sixteenth century with Benedikt Kuripecic (Benedicten Curipeschitz) and his travel book *Wegrayss Kö(niglicher) May(estät) potschafft gen Constantinopel zu dem Türkischen Kayser Soleyman,* published in 1531 anonymously and without stating the place of printing. The description of his journey through the Slovenian part is short: 'We went from Ljubljana, through Grosuplje, Novo mesto and Metlika.' He assumes that the reader is familiar with his trail and the places along it. The author describes in greater detail the more distant places, in particular Bosnia, at that time unknown to the European reader. This travel book is the oldest description of a journey through the Balkan Peninsula.

Among the noblemen from the Slovenian territory who (as Austrian or Venetian diplomatic emissaries) travelled either to Moscow or to Constantinople, Sigismund Herberstein stands out for his scientific reputation. On his two missions to Russia (in 1516–18 and 1526–7) he carefully recorded everything that might be of interest to the European intellectual of his time, who knew practically nothing about Russia. In Vienna in 1549, Herberstein published a full-scale description of Russia entitled *Rerum Moscoviticarum Comentarii*. It became a best-seller and was subsequently published in numerous translations and issues. Throughout his descriptions Herberstein tried to be rational and impartial and, at the same time, as exhaustive and vivid as possible. Although he relied predominantly on his own experiences and rational

evaluations, he collected, through his Russian acquaintances, information about the regions east of Moscow, which he himself had not visited. He thus recorded interesting information about Siberia and the Tatars.

In his *Letters in Verse* (1568), George Turberville, the English emissary to Moscow, praised Herberstein's work. And the celebrated *Theatrum orbis terrarum* by Ortelius, first issued in 1571, refers the reader to Sigismund Herberstein for further information (Hodgen 1971:153).

Although travel writers of Kuripecic's and Herberstein's type offered only unsystematic descriptions of anthropological phenomena, they greatly influenced the formation of European ideas about (foreign) places and peoples (Hazard 1959; Jezernik 1988). Interest in the differences between domestic and foreign culture became integrated in European endeavours to define themselves as civilized; and for this they needed an antithesis and an Other. They were discovering savages, noble and ignoble ones, everywhere.

In the sixteenth century a differentiation between theology and philosophy started in Slovenia, with the emergence of the question of the relation between belief and reason. The influence of Socinianism, a radical movement of reform, was significant. Socinianism was also familiar to Primož Trubar, the most important representative of the Slovenian reformation (Pogacnik 1990:221–3).

The philosophy of the period dealt with human issues because of the differentiation mentioned—and included the germs of anthropology and ethnology. Trubar took an active interest in the Slovenian cultural characteristics and those of other Southern Slavs (especially their language and customs) and reported on Islam's characteristics (Šmitek 1986:165).

Adam Bohoric, another reformer, in the Preface to the Slovenian grammar, shows a marked scepticism that philosophy will let it be possible to discover significant knowledge, 'because the human mind has been made blind through the sin of the parents' (Bohoric 1584). Yet a century and a half later, a few theorists understood philosophy, as Saint-Simon did, as a general science integrating the findings of other sciences (Saint-Simon 1979:54). The philosopher Jakob Štelin argued in his work *Specimen de ortu et progressu morum* (Venezia 1740) that the moral retrogression of humankind was parallel to humankind's estrangement from nature. Franz Xaver Gmeiner (*Literargeschichte des Ursprungs und Fortganges der Philosophie,* vols 1–2, Graz 1788–9) saw the satisfaction of humanity's constantly increasing needs as the motivating force of human progress. Martin Kuralt (1783) wrote about

the equality of various peoples with regard to the quality of reason and about their inequality as regards its quantity.

From the seventeenth century onwards a need for a systematic and methodological approach emerges from various instructions for collecting pertinent information. A particularly notable set of instructions was set forth by the English scientist Robert Boyle:

> Considering the Great Improvements, that have of late been made of Natural History (the only sure Foundation of Natural Philosophy), by the Travels of Gentlemen, Seamen, and others; And the great Disadvantage many Ingenious Men are at in their Travels, by reason they know not before hand, what things are they to inform themselves of in every Country they come to, or by what Method they may make Enquiries about things to be known there, I thought it would not be unacceptable to such, to have Direction in General, relating to all, and also in Particular, relating to Particular Countries, in as little Bounds as possible, presented in their View.
>
> (Boyle 1692:1–2)

Boyle's instructions appeared in book form posthumously but they had appeared in manuscript versions earlier.

A similar questionnaire, intended for travellers, was also used by Janez Vajkard Valvasor (Johann Weichard Valvasor). In his encyclopaedically conceived work *Die Ehre des Herzogthums Krain,* published in Ljubljana 1689, he offered, in addition to natural specifics, topographical and historical information, the first systematic presentation of the population of the Carniola (a western part of Slovenia) and Istria of his time. In this work he described the customs, food, clothes, dwellings and economy of these two regions, as well as outlining the ethnic affiliation of their peoples. In the chapter on the inhabitants of the Karst region, he paid considerable attention to the external appearance of men and women, 'and in doing that he reached a long way towards modern views (of a kind of anthropological or—with some exaggeration—"racial" aesthetics)' (Novak 1986:14).

Valvasor's approach may be called anthropological because of his orientation towards natural science and also because of his holistic pursuit in an area where the peasant element remains an inseparable part of nature. Valvasor collected information on the spot, primarily because of his scientific scepticism. He wanted to verify everything with his own eyes. His field method included observation, inquiry and

sketching. Since in his instructions Boyle explicitly mentioned Lake Cerknica in Carniola (Boyle 1692:6), a description of which made Valvasor famous in England, we may posit a certain connection between Valvasor and Boyle. We may conclude that Valvasor was familiar with Boyle's instructions when collecting the material for his book. His work is conceived on an exceptionally broad scale and represents one of the peaks of European encyclopaedic knowledge of the time. Among the learned men of Europe of the seventeenth and eighteenth centuries, Valvasor's description of Lake Cerknica evoked much response: 'About the latter Baron Valvasor brought forth a fairly extensive, exact, and curious description in his comprehensive work on the Glory of the Dukedom of Carniola, which one rarely finds in our libraries' (Fortis 1771:81).

Anton Tomaž Linhart published a historiographic work entitled *Versuch einer Geschichte von Krain und den übrigen Ländern der südlichen Slaven Oesterreichs* (vol. i–ii, Ljubljana 1789–91). In the initial chapters he tried to outline the Slovenians' way of life in their earliest period and the evolutionary course of their culture. Linhart believed that firm rules operate in this process, as in nature generally. He understood cultural development as a process of separation from nature and as a process of spiritual and intellectual growth. He distinguished four basic stages of cultural progress. Significantly August Ludwig Schlözer's work was not only well known to Linhart, but also one of his main ideals. Linhart doubtless also knew some of the works of the French theorists of his time which discussed the development of civilization. His text 'Ueber die Nutzbarkeit der natürlichen Philosophie' (contained in the almanach *Blumen aus Krain für das Jahr 1781,* Ljubljana 1780) is modelled upon Alexander Pope's *An Essay on Man*.

At the turn of the eighteenth century, a significant work *Abbildung und Beschreibung südwest- und östliche Wenden, Illyren und Slawen* (Leipzig 1801–8) by Breton Balthasar Hacquet appeared, relating his diverse interests in the natural sciences as well as the study of the way of life, economy and the physical characteristics and linguistic specialties of the people. Gian Rinaldo Carli produced in the 1780s a sizeable study 'Lettere Americane', published in *Magazzino Letterario* (Florence 1780), republished as a book in two volumes in the same year, and later in an expanded edition (Cremona 1781–3). There he wrote about the Aztec and Inca cultures. He was full of enthusiasm for their civic, social and economic organization, and rejected the American 'degeneration theory' advocated by Buffon, Pauw and others.

In the first book of his work *Darstellung der Philosophie ohne Beynamen* (Vienna 1802) Franz Samuel Karpe discussed the field of 'empirical psychology or anthropology'. Karpe was a proponent of cultural evolutionism. He was primarily interested in the mental differences between peoples and the reasons for them (climate, food, racial features, social order, and the like).

Friderik Baraga played a role in forming American cultural anthropology. He was a missionary of Slovenian origin, who, between 1831 and 1868, worked among the Ottawa and the Ojibwa (Chippewa). He produced a monograph on their cultures, based mostly on his fiveyear stay among them on Lake Michigan's eastern coast, although he occasionally drew upon other authors for his information. Baraga's book entitled *Geschichte, Character, Sitten und Gebräuche der nordamerikanischen Indier* was published in 1837 in Ljubljana simultaneously with the Slovenian version and a French edition, printed in Paris. Baraga's scientific work is anthropological above all because he drew upon American anthropological literature for his patterns and initiatives. Baraga provided Henry R.Schoolcraft, a founder of American anthropology, with answers to his comprehensive questionnaire about the Ojibwa Indians. Baraga also collected specimens of Indian culture and, in 1837, he donated to the Carniolan Provincial Museum a collection from the area around the Upper and Michigan Lakes.

Baraga's colleague Franc Pirc was another collector, and also wrote a book about Indians of the Ottawa and Ojibwa tribes (Pirc 1855). Ivan Benigar, who, in 1908, settled for good in Argentina, lived among the Patagonian Indians and Araucanians, establishing his family there. He studied the Araucanians' concepts of space, time and causality and wrote three accounts, which were printed in *Boletino de la Junta de Historia y Numismatica* in the 1920s. Essentially these accounts centre around the study of the way of thinking and perceiving the external world, and are based on linguistic analysis. Benigar was opposed to some of the conclusions of Lévy-Bruhl's theory about pre-logical thinking.

Benigar's largest work is *El problema del hombre americano* (Bahia Blanca 1921), in which he investigated the ethnogenesis of American Indians. He focused in particular upon the diffusionist Pacifico-American theory, as represented by Paul Rivet and José Imbelloni. Benigar opposed the conclusion reached by Imbelloni. He was likewise critical of evolutionism, although in principle he admitted its validity. His scientific interest spanned fields such as linguistics, history,

archeology, ethnology, sociology and philosophy; he attempted to integrate them and called them a 'science of the essence of Human'.

The theologian Lambert Ehrlich specialized in social anthropology with Professor R.R.Marett in Oxford between 1920 and 1921. He wrote *Origin of Australian Beliefs* (St Gabriel-Mödling 1922) which in the author's words is 'an attempt to apply the test of Australian facts to the various theories put forward concerning the origin of religion, and endeavours to give an explanation—as far as this may be possible—of Australian beliefs'. Ehrlich closely co-operated with Wilhelm Schmidt and other representatives of the Viennese cultural historical school. As Professor at the Faculty of Theology in Ljubljana he was also active in the field of comparative religions. He took a very broad view of ethnology, taking it to be almost equal with cultural or social anthropology.

Niko županic was the first Slovenian physical anthropologist of academic rank. Thanks to his endeavour, the Ethnographic Institute was founded in 1921 at the National Museum in Ljubljana. županic became its first principal custodian and after the founding of the Ethnographic Museum (1923), its director. He was also the editor of the journal *Etnolog* (from 1926 onwards). According to županic, the museum's task was to 'promote ethnography, anthropology, the history of folk art, to collect relevant material, and to keep it preserved in its exhibition collections' (županic 1934:235).

županic's work is extremely varied, with investigations ranging from linguistics and physical anthropology to the ethnic history of the Balkan Peninsula (cf., for instance, županic 1933). He resolutely rejected eugenics, despite its popularity in Europe. At the International Congress for the Scientific Study of Folk Problems in Berlin, on 29 August 1935, županic presented a paper 'On people's racial aesthetics among the Yugoslavs' and rejected racial legislature, which permitted the contraction of matrimonial union between Arians and non-Arians solely on the basis of already-performed sterilization, as 'indecent, too humiliating and too much directed against the elementary law of humanity and any nation' (županic 1936:62). In 1940 županic became the first professor at the Faculty of Philosophy holding the chair of ethnology, which in due course expanded into an independent department.

From 1933 to the end of the 1950s, Božo Škerlj taught physical anthropology at Ljubljana University (Škerlj 1946, 1948, 1959) and during the last decade of his activity he was involved in the cultural anthropological study of 'primitive cultures' (Škerlj 1962, 1963). He

was succeeded as physical anthropologist by Zlata Dolinar-Osole, Marija Štefancic, Anton Pogacnik and others.

Branislava Sušnik covers a very broad area of physical and culturalanthropological issues in her numerous books and papers. She is an expert on Paraguay Indians at the Andrés Barbero Museum in Asuncion (Telban 1993).

A greater degree of subject specialization in Slovenia occurred after 1968, when the political scientist Stane Južnic at the Faculty of Sociology, Political Sciences and Journalism, introduced, *inter alia,* lectures focusing on social, political and linguistic anthropology. He also wrote textbooks addressing these issues (Južnic 1978, 1983, 1987).

The purpose of this chapter has been to outline the existence of a (cultural) anthropological tradition in Slovenia from the sixteenth century onwards, although most of the authors mentioned did not characterize their work as anthropology. From the end of the eighteenth century the terms ethnography and ethnology appeared as a consequence of an awakening national self-consciousness. In the nineteenth and twentieth centuries this awareness resulted in the enthusiastic study of vernacular Slovene culture, of folklore in particular. An emphasis on the nation was regarded as important enough to result in a distinction between European and non-Western ethnology. Yet anthropological concepts and methods were older than the disciplines based on collective appurtenances to the nation, ethnic group or tribe. Besides, the content of anthropological studies included neither civics, nor only historical methodology, but kept its close relations to natural history. The Slovenian anthropologists mentioned above represent varicoloured currents of the European anthropological thought in a specific local framework.

NOTE

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11 Ethnography and anthropology

The case of Polish ethnology

Zbigniew Jasiewicz and David Slattery

> Polish ethnology, with its focus on Polish folk customs, has in some respects resembled Soviet ethnology, although it has probably had closer ties to Western scholarship, often depending on foreigners for its theoretical frameworks.
>
> (Winters 1991:x)

INTRODUCTION

The discipline of ethnology is highly sensitive to local and individual influences. Both the changing socio-political contexts in Poland and the activities of individuals who gave rise to schools of thought and research orientations have been very important in shaping the character of national ethnology (Gerholm and Hannerz 1982:13; Burszta 1973:5; Service 1985:vii). Because of these particularized influences, many orientations in ethnology can be observed co-existing in Poland. This map of ethnology is further complicated when we consider the dramatic social changes that mark Poland's history even in the present day. The forty-year-old totalitarian system, with its ethnocentricism and cultural isolationism, is rapidly going into decline in Poland and, on the other hand, a form of global consciousness that embodies a sense of the world community of anthropologists is taking its place.

Polish ethnology is made up of two types of activity: *etnografia* and *antropologia*. *Etnografia* (ethnography) is principally a descriptive form of folk studies, or the study of the self. This definition is only partially true; it will shortly be modified in our discussion of the study of non-European peoples. The term *etnografia* has taken over from the more arcane *ludoznawstwo* with its connotation of leisured amateurism. In Poland's history, *etnografia* has sometimes taken other cultures as its object but has remained

strongly tied to a descriptive method. Indeed, it has also been the name given to the study of ‘primitive societies’.

Antropologia (anthropology), on the other hand, is characterized both by a study of the other, as other cultures, and as a study which produces conclusions about the human condition in general. It is not just the study of extra-European peoples though, indeed, ‘exotic’ peoples are the traditional mainstay of anthropological investigation in Poland. Anthropology uses generalizations and sophisticated theoretical assumptions which are part of a global tradition. This term is used in Poland to refer to social and cultural anthropological traditions which were imported from outside. ‘Ethnology’ is the term which has persisted in Poland as the supposed name for all of the activities including ethnography, folk studies and anthropology. This term differs from ‘ethnology’ since ethnology was regarded as the study of ethnic groups until the late sixties and early seventies.

In this chapter, we shall outline the dynamics of the diverse trends in the history of Polish ethnology and follow it to its contemporary shape. We reject a history of ethnology which simply conceives of the tradition as moving from studies on folk culture, concentrated on local groups, towards global cultural concerns and anthropological generalizations on so called ‘exotic’ peoples. Instead we believe that these two traditions have sometimes co-existed in the Polish context and often shared their objects of study. There has been an interest in the articulation of holistic cultural formulations in Poland for a long time and this interest has co-existed with the recording of local folk culture. We contend that what really changed in Poland’s history was the different theoretical orientations within these two kinds of studies, the relations between them, the conception of their unity or separateness, and the names given to them. Perhaps, then, Poland’s intellectual history and the histories of other Central European countries, which are only now coming to light, will force us to reformulate our more traditional oppositions.

FOLK STUDIES (ETHNOGRAPHY), ANTHROPOLOGY AND ETHNOLOGY

By the term ‘folk studies’ is meant the kind of study which consists in the recording and interpretation of socio-cultural phenomena of a particular part of one’s own society called *lud* (folk). The object of this kind of study is people (folk) and folk culture. The method employed is mainly straightforward description and historical interpretation. While folk studies are descriptive in character, they are also able to yield

valuable information on partial cultures (subcultures). When they are interpretative, they yield useful elaborations on cultural situations in significant social strata. These interpretations are sometimes presented in the wider context of an ethnic group, or of a nation. Folk studies have had a distinct political function when they were associated with influential ideologies. Folk studies were used in the past in the creation of nationalist and communist idealized socio-cultural reality. These studies were ideological, descriptive, rejected comparison, created traditional culture as *'praesens ethnographicum',* and rejected the questioning of global culture.

Folk studies played a positive role in raising the self-esteem of the Polish common people (folk mass), especially that of the peasants. On the one hand, folk studies allowed certain elements of folk culture to be incorporated into Polish national culture which had been dominated by the culture of the nobility. On the other hand, during communist rule, folk studies were regarded as an integral part of the general conception of the primary role of workers and peasants in the cultural order of the state, the Polish People's Republic. The Polish term that was commonly used to denote these folk studies is *ludoznawstwo,* which is equivalent to the German term *Volkskunde*. However, this term has largely fallen out of use since the beginning of the twentieth century and has more or less been replaced by *etnografia*.

The Polish opposition between *etnografia,* as a form of folk studies, and *antropologia* is mediated to some degree by the fact that there is a Polish *etnografia* which takes extra-European peoples as its object. This ethnographic study of extra-European people had a very specific function within the discipline of ethnology in Poland. The study of extra-European people was not generally conceived of, in Polish ethnology, as a general and comparative *Völkerkunde* in opposition to a specific and descriptive *Volkskunde*. Studies of extra-European peoples were often based on description and repeated the pattern of folk studies of Polish people on a distant territory. In this way, they were a form of folk study since, in Poland, ethnography is strongly linked to mere description. Sometimes, however, they employed comparisons and used many assumptions from anthropology, and in that way they were anthropological. These studies of extra-European peoples had the role, in Polish ethnology, of forming a link between *etnografia* (folk studies) and *antropologia*. This is what gives it its particular importance.

The term 'ethnology' embodies a very strong sense of the unity and uniqueness of a discipline in Poland that contains ethnography, which comes under the various labels *etnografia, folklorystyka* and, earlier,

ludoznawstwo; ethnographic studies of extra-European peoples, which also employ the terms *etnografia* and *etnologia;* and the study of culture as an independent phenomenon which operates in Poland under the various names of *etnologia, antropologia kulturowa* (cultural anthropology) and *antropologia spoleczna* (social anthropology). Nowadays, the term *etnologia* is sometimes used in the collective form of 'ethnological sciences'. During the Stalinist period, the term *etnografia* had this function and was introduced as the single obligatory name for all of these activities. This is because social and cultural anthropology left ethnology during this period and is only now trying to reclaim its old place in Polish ethnology.

Since our treatment here is historical, it is necessary to briefly outline the main transformations that have occurred in Polish ethnology from its origins in the Enlightenment to the present day. We feel that a useful history of the subject would have to address at least five topics. First, it is necessary to elucidate the changes that have taken place in both the choice of objects of ethnological study and the theoretical orientations that were employed. Second, it is necessary to outline the main factors which influenced the choice of those objects of study and the conditions which made the emergence and maintenance of some aspects of ethnology possible. Third, it is also necessary to outline the different types of studies that have made up the discipline of Polish ethnology and the strategies that have been employed, and are presently being employed, to unify those studies under one name. Fourth, contacts with foreign ethnology and anthropology can be traced and, fifth, the challenges of the contemporary transformational period must be articulated. The most interesting and stimulating period for us in this Polish history is that which followed the upheavals of 1989. All of these themes together would form a programme of inquiry that is far too broad and detailed for our present purposes. We have to confine ourselves here to providing an overview. While our analysis is connected with the past we are only concerned with how that history can help us to understand and deal with the challenges of the present.

THE ORIGINS OF POLISH ETHNOLOGY

The received view is that Polish ethnography, as folk studies, has its origin in the late Enlightenment period. Its beginnings may be traced to a letter written by Kollataj to the publisher and bookseller, Jan Maj, in 1802. Kollataj was a well-known thinker of the Polish Enlightenment, interested in the history, nature and culture of

humankind. Kollataj conceived of this culture in terms of a 'philosophical history' (Szacki 1981:149). His letter outlines a broad programme of historical research and included a developed approach for folk studies (Kutrzebianka 1948:7; Posern-Zielinski 1973:31). He listed problems that he thought worthy of interest, advocated the use of direct observations and questionnaires, and he appreciated the value of accurate recordings which he believed should include drawings.

The philosopher, economist and statesman, and sometimes poet, Staszic was also a prominent member of the Polish Enlightenment. His comprehensive verse of 1820, *Rod ludzki* (The Human Kind), developed the theme of humankind and its culture. Staszic joined his broader interests to the particular description of groups of Polish villagers. In this way, these two figures particularized the generalized abstract notion of 'Mankind' and promoted programmes of research on concrete groups of people.

The Romantic movement in Poland generated interest in folk literature and customs. Numerous collectors, at that time, recorded and published folk songs and legends. The most famous among them was Dolega-Chodakowski who was the author of *O Slowianszczyznie przed chrzescijanstwem* (On Slavs before Christianity) in 1818. In this Romantic tradition, folklore was treated both as a source for the reconstruction of old Slavic culture and as an aesthetic value in itself. However, Burszta has accused Polish Romanticism of being antiethnographical and believed that it renounced broader ethnographic orientations (Burszta 1973:30).

According to the majority of Polish writers, the first really informative work on folk culture in Poland, *Lud polski* (Polish People), was written by Golebiowski in 1830 and this work belongs more easily to the Enlightenment than to the Romantic tradition because it does not romanticize the peasantry.

As early as the end of the eighteenth century, the first Polish travellers with a scientific interest were studying extra-European peoples. These included Potocki's travels in Morocco, the Caucasus and Central Asia. After the Kosciuszko Insurrection of 1794, and the November Uprising of 1830, these travellers were mainly insurgents escaping to the West, or were more often deported to Siberia and Kazakhstan. Strzelecki went to Australia, while Kopec and Januszkiewicz were deported. However, the term *etnografia* was not used at that time to describe this kind of work. Indeed, the terms *ludoznawstwo, etnologia* and *etnografia* did not yet appear at all in Poland in the first half of the nineteenth century. At that time ethnology

was simply a form of folk studies. Any other form of ethnology may be said not to have appeared in Poland until the second half of the nineteenth century.

EVOLUTIONISM

In the second half of the nineteenth century the main object of study was still folk culture and *'lud',* both within Poland and in other ethnic contexts. The former Romantic writer, Berwinski, strongly criticized the Romantic tradition in *Studia o literaturze ludowej* (Studies of Folk Literature) in 1854 by pointing out the secondary and unoriginal character of folk culture (Kutrzebianka 1948:11). However, his criticisms were not widely accepted and folk studies continued to have a Romantic tone, and, in fact, they were renewed in the intellectual-artistic movement called 'Young Poland' which had an influence on Malinowski.

The most notable recorder of folk culture at this time was Kolberg (Linette 1991). Besides the recording of culture there were historical interpretations of particular regions and subjects. The main subjects were folk literature and art followed in popularity by technology and economy. The beginning of the twentieth century saw the publication of books which attempted to synthesize all these aspects of Polish folk culture; for example, Karlowicz's *Lud. Rys ludoznawstwa polskiego* (Folk. An Outline of Polish Folk Studies) which appeared in 1904.

Political deportees to Siberia and the far east region of Russia continued to sustain the study of extra-European peoples. These exiles were the mainstay of Polish extra-European ethnographic studies. The most notable are Sieroszewski, for his study of Yakut traditional culture and acculturation processes, which was published as a monograph in Russia in 1896, and Pilsudski who specialized in Ainu culture (Armon 1977:85). In the nineteenth and at the beginning of the twentieth century, Poles were participating in Russian, German, American and English ethnographic expeditions. Among these were Czekanowski, Poniatowski and Czaplicka (Sokolewicz 1973:176).

In the middle of the nineteenth century, ethnographic studies were regarded as part of the discipline of geography (Kutrzeba-Pojnarowa 1977:39). However, evolutionism began to dominate Polish folk studies from the 1870s onwards (Moszynski 1958:162). Many of the classical evolutionist writers, such as Tylor, Morgan, Spencer, Lubbock and others were translated into Polish. Evolutionism gave Polish ethnographers a theoretical base for their studies and helped to initiate

the study of 'primitive societies'. One of the most notable Polish evolutionists was Krzywicki who was the author of *Ludy. Zarys antropologii etnicznej* (People. An Outline of Ethnic Anthropology) in 1893 and *Primitive Society and its Vital Statistics* in 1934 (Holda-Roziewicz 1976:41). There are forty years between these publications and this reflects Krzywicki's very long and active career.

The first ethnological institutions were primarily associations that had many amateur members and journals. We do not have space here to outline the development of these associations and journals but we can say a bit about the terminology they employed. The term *etnografia* was used by Pol as the name for the subject matter of the lectures he presented at Jagiellonian University in Cracow in 1851 (Wroblewski 1969:20). This term was also used in the *Encyklopedia Powszechna* (Universal Encyclopaedia) from 1861 in connection with the description and collection of information on exotic peoples (Wronska 1992:9). Shortly afterwards, this term appeared in the subtitle of the journal *Wisla* (1880s) and in the name of a museum in Warsaw (1880s). Under the influence of eighteenth-century scientific classifications, *etnografia* was treated as the activity of description and was opposed to *etnologia,* which was comparative and theoretical (Wroblewski 1967:13, 17). *Etnografia,* which took Polish, European and extra-European peoples as its object, was a descriptive form of folk studies. Whenever *etnografia* took a group of Polish people as its object, it was the same as *ludoznawstwo* (first used in 1854) for all practical and theoretical considerations, but came under the name *etnografia*. This means that identical studies were functioning under the labels of *ludoznawstwo* and *etnografia*. This is not very clear but the ambiguity may have arisen from the presence of two sources: tradition and contemporary encyclopaedias. The names used were sometimes the result of traditional practice, or habit, and sometimes entries in encyclopaedias had a normative function. However, from this time on the term *etnografia* began to replace *ludoznawstwo* as the generic name for this type of study.

Ludoznawstwo was the term used to describe the naming and recording of items of folk culture from one's own country. It has its equivalents in other countries in Europe at that time: for example *Volkskunde* in Germany, *narodovedenie* in Russia, *narodopis* in Slovakia, *folklore* in England, *folk-liv-forskning* in Scandinavia, etc. The Polish term *ludoznawstwo* was first used by Berwinski in 1854. However, while it was included in the names of museums and associations, and appeared in the titles of books, it was never used in the name of any academic institution. It connotes the activities of

amateur collectors and recorders. However, very often the three terms, *etnografia, ludoznawstwo* and *etnologia* were used interchangeably (Damrosz 1988:69). The unity of these three areas emerged from the activities of the associations and journals. While this unity is often questioned, it has not yet been rejected in Poland.

The term *etnologia* (ethnology) was used by a group of the Anthropological Committee in Cracow in 1873 to describe their activities. This was also the name given to a chair in Lwow University in 1910.

Antropologia (anthropology), on the other hand, was defined in the *Encyklopedia powszechna* (Universal Encyclopaedia) of 1859 as a science of the mental and physical nature of the human species. This formulation was influenced by Broca's conception of anthropology (Wronska 1992:10; Wroblewski 1967:14). Later, however, there was a distinct tendency to reduce the meaning of this term to include only the study of physical features. By the 1950s, *antropologia* came to mean exclusively physical anthropology.

BETWEEN THE WARS

New universities and museums were created after Poland achieved independence in 1918. A chair of ethnology and anthropology had already existed in Lwow University since 1913 and new chairs of ethnology and ethnography were created after 1918: a new chair of ethnology was created in Poznan, later two chairs in Cracow, one in Vilna and two in Warsaw (Kutrzebianka 1948:33; Posern-Zielinski 1973:105).[1] These chairs had various titles: ethnology and ethnography, ethnology and sociology, ethnology of Slavs and ethnology. There were no chairs of *ludoznawstwo* because this term was only used to describe amateur research and was not associated with academia. All the professors that were appointed to these chairs received their education abroad: in France, Austria, Russia, and especially, Germany. Ethnology, which had the primary function of recording the culture of a divided nation before the First World War, now had the function of integrating the cultures and ethnic groups that had emerged from partition. Where ethnology maintained contact with the wider scientific community, it regarded itself more as an abstract academic activity. Where it was more closely related to folk studies it regarded itself as the practical means for the implementation of a national culture. Polish ethnology has always regarded itself as a practical science and this pragmatism was developed especially after

the Second World War as a project of developing an idealized proletariat and peasant folk culture.

The study of folk culture had a strong emphasis on the idea of the 'nation'. However, in the Polish context, folk studies never managed to become an important element in Polish nationalism. Polish folk studies tended to serve more for the establishment of a multi-ethnic rather than a uni-ethnic concept of Polish nationhood. This was because, at that time, the Polish borders encompassed Slavonic ethnic groups in the East which were strenuously represented politically by the famous Pilsudski.

While folk culture remained the principal object of study, Polish ethnologists began to look at neighbouring cultures outside of Poland, especially that of the Slavs. At this time there were numerous handbooks on Polish culture published, as well as the monumental *Kultura ludowa Slowian* (Folk Culture of Slavs) which appeared in three volumes between 1929 and 1939 and was written by Moszynski.

There were some ethnographic studies on extra-European peoples during this period but these were much more limited than the previous studies carried out by the exiles and others before independence. Polish and European ethnographic material was also used in studies on the theory of culture by Czarnowski, Bystron, Dobrowolski and others. The influence of evolutionism was dramatically lessened during this time, following developments elsewhere. However, Moszynski formulated a 'critical evolutionism'. The culture-historical school was very popular at this time with such adherents as Fiszer, Poniatowski and Czekanowski. However, the most dynamic group were those who were influenced by the French sociological school but at the same time remained historically orientated. Among these were Czarnowski, Bystron and Dobrowolski. Functionalist and phenomenological themes also appeared in Polish ethnology and links were established between ethnology and sociology. Znaniecki worked as a sociologist, and together with Thomas wrote the famous *The Polish Peasant in Europe and America* between 1918 and 1920, and developed the important anthropological concept of human co-efficient (Szacki 1981:740).

At this time in Poland, the term *antropologia* was used mainly in the study of the biology of mankind and only exceptionally in a wider sense, for example in the name of the Anthropological Institute of the Warsaw Scientific Society which was established as a workshop in 1902 and became a department in 1911. The most common terms were *etnologia* and *etnografia* which were frequently used together to

describe the names of chairs in university departments. On the one hand, ethnography was conceived of as a separate discipline which dealt mainly with European folk culture *(Volkskunde);* a more professional, or sophisticated, form of *ludoznawstwo*. On the other, ethnography was thought of as a part of ethnology: descriptive or regional ethnology. At the same time, ethnology was also conceived of as a separate discipline taking extra-European peoples as its object and as having a higher level heuristic nature consisting of comparative analysis and generalizations. At this time, *ludoznawstwo* established its contemporary meaning as the activity and name of an association of professionals and friends who had a shared interest in regional folk culture.

AFTER THE SECOND WORLD WAR

The end of hostilities in 1945 saw many Polish institutions, libraries and museum collections destroyed and many eminent ethnologists dead or in permanent exile. The territory of the Polish state was pushed to the West and the Polish nation became mono-ethnic in principle.

The period of communist 'ideologization' in Poland, which followed the Yalta Convention, resulted in ethnology losing its theoretical independence and its entire transformation into historical and (mainly) descriptive ethnography. In 1950, university studies were reorganized and established within the framework of 'history of material culture studies'. Ethnography, along with Classical and European archaeology, became part of the activities of the 'Institute of the History of Material Culture' of the Polish Academy of Sciences (or PAN, established respectively in 1953 and 1951). Sociology was virtually eliminated as a discipline in Poland and many sociologists became ethnographers. These sociologists were very influential as to policy in Polish ethnography (Burszta and Kopczynska-Jaworska 1982:55). Social and cultural anthropology left the realm of ethnology and virtually disappeared.

There were some opportunities for ethnographic research in the new so-called 'Regained Territories' where the new settlers from different regions made it possible to study contemporary social processes and, to a lesser extent, in the Soviet Union and other socialist countries. However, ethnology as anthropology and sociology was abolished and the Polish ethnological community was isolated by the end of the fifties within the 'socialist camp' without any significant access to Western literature or ideas. This isolation strengthened a pattern of ethnography that focused more and more on local culture and was, with very few

exceptions, ignorant of themes and topics in world ethnology. Polish ethnology was made up almost exclusively of ethnographic activity until well into the late sixties and early seventies when university chairs and other institutions began to use the term *etnologia* again. In the more liberal political climate following Stalin's death, there was a return to the use of terms that was more in keeping with the Polish and European traditions. However, in our conclusions we shall discuss how much theoretical independence and support this revived term could have had.

Unlike Soviet ethnography, Polish ethnography never became a history of primitive peoples. The object of Polish ethnography was 'traditional' folk culture, where folk culture was treated as a survival from earlier times. However, at the end of the fifties, contemporary culture began to be recorded and urban populations were included in the studies. Processes of socio-cultural changes were part of joint studies involving ethnographers and sociologists. The integration of the north-west region of Poland, which was German before the war, provided a strong impulse for these kinds of studies which had a distinct nationalist and ideological function. This broadening of the base of ethnographic studies was accompanied by a feeling of 'crisis in ethnography' (Burszta 1965:92).

The study of folk culture, which formed the backbone of Polish ethnography from the beginning, evolved in the direction of the study of the possibility for change of this same culture (Biernacka 1976–81). There was also a move towards the investigation of the principles of mythologization in folk culture. This elevated folk culture from a local phenomenon to a folk-type with universal characteristics (Robotycki 1992:12). In contemporary Polish ethnology, we will shortly show, this latter trend in ethnographic studies is presently being used by some ethnographers (folklorists) to justify the term ethnology being applied to what they do. However, it was not until the seventies that ethnography included the new study of extra-European peoples of Central Asia and the Near East, Latin America and Africa (Frankowska 1973:258).

The 'integral method' was elaborated by Dobrowolski. This approach combined ethnological, historical and sociological methods and sources. Since the late fifties works of Western anthropologists were translated into Polish. Amongst these were the writings of Malinowski, Benedict, Durkheim, Eliade, Firth, Gellner, Halbwachs, Hall, Harris, Kuper, Lewis, Lévi-Strauss, and many more. Burszta and Kopczynska-Jaworska list the many theoretical orientations that animated the research of young Polish ethnographers in the seventies:

functionalism, structuralism, sociologism, the Russian Tartu school of semiotics, phenomenology and American cognitive anthropology (Burszta and Kopczynska-Jaworska 1982:61). However, they also note the extremely modest success of these theoretical approaches in producing worthwhile results and stress that they were merely attempts. We contend that this lack of success is due neither to any inability of the participants, nor to any particular inadequacy of the theories, but rather to the absence of any significant and fully independent intellectual context for such work at that time, despite Burszta and Jaworska's enthusiasm.

By the middle of the seventies, the monopoly on the study of culture, held by ethnography, was broken and the term *etnologia* was restored. In 1975 a Committee of Ethnological Sciences was established in the Polish Academy of Sciences and the journal *Ethnologia Polona* was founded which published many articles in English. At the same time, the term *antropologia* became popular even in the titles of Polish ethnographies. An example is Stomma's (1986) *Antropologia kultury wsi polskiej XIX wieku* (The Anthropology of Nineteenth-Century Polish Village Culture).

One of the more important of Polish philosophical theories to be developed in ethnology was that of Kmita's socio-pragmatic theory of culture which was especially important in Poznan. Also in Poznan, an ethnological dictionary was compiled, *Slownik etnologiczny,* under the editorship of Staszczak, which tried to mark the boundaries of the discipline.

CONTEMPORARY POLISH ETHNOLOGY

At the moment there are seven departments of ethnology in Polish universities. There is also a Department of Archaeology and Ethnology in the Polish Academy of Sciences. Six ethnological journals are published.[2] The Polish Ethnological Society *(Polskie Towarystwo Ludoznawcze)* has more than a thousand members and is the most important publisher of ethnological publications. Other ethnologists are members of the Social Anthropology section of the Polish Sociological Society.

In the late thirties there were nine professors of ethnology, some of whom were also sociologists and physical anthropologists.[3] Five of them combined an interest in Polish and European folk culture with an interest in extra-European culture. However, none of them was solely engaged in extra-European problems or in methodological problems. The other four were only interested in Polish and Slavonic folk culture.

The present situation repeats, in principle, this pattern from the thirties. Among forty-five professors and habilitated doctors who call themselves ethnologists, ten have an interest in both European and extra-European topics; four specialize in extra-European studies and four others specialize in methodological problems. These figures show a trend towards specialization.

Because of the ideological restriction placed on critical theorizing during the forty years of communist rule, it would be naive to imagine that ethnology can simply take up again where it was before the Second World War. Despite the return of social and cultural anthropology to ethnology in the seventies, Polish ethnology maintained a nature that was almost exclusively ethnographic despite notable exceptions. These exceptions included moderate theories which were always created on an empirical base: for instance, the concept of cultural adaptation and integration; cultural changes in folk culture or traditional society under the influence of industrialization or modernization; the role of tradition in culture; the relation between folk and national culture; the distance between family culture and the culture of Polish public life. However, ethnology remained dominated by a descriptive ethnography that was low on theoretical assumptions. A material history,[4] that was not in opposition to the communist ideology, prevailed. Indeed, Marxist analyses from the West, that were critical of the Eastern orthodoxy, were suppressed more than any other theory. However, structuralism and functionalism, including Malinowski's writings, were not regarded by the authorities as having the same potential for disrupting the new order and these texts were often translated into Polish. If something was allowed, it is probably an indication of the perceived political and social insignificance of those works and sometimes a sign of the presence of a liberal attitude on the part of the authorities. Indeed, in the case of Malinowski, there was some attempt to lay claim to his anthropological origins.

The whole intellectual context which makes independent anthropological thinking possible must now be put back in place in order to restore the traditional balance of ethnological elements. This does not mean that there is anything wrong with ethnography as 'folkstudies', since these studies have a very important value in themselves. Nor does it mean that nothing is happening in ethnology at the moment. On the contrary, the last few years have probably been the most dynamic in Polish ethnology. The most recent situation is characterized by the initiation of discussions on the relationship between ethnology and anthropology; the publication of a dictionary of

ethnology (Staszczak 1987) and three handbooks on social and cultural anthropology (Olszewska-Dyoniziak 1991; Nowicka 1991; Brozi 1992–3); efforts to change the syllabus of ethnological studies in the universities; more meaningful and intensive contacts between Polish ethnologists and international anthropological associations and centres; and the appearance of the first university lecturers from Western countries.

The contemporary scene is marked by several trends which are sometimes related to each other. First, some ethnographers (folklorists) imagine that they can simply use the term 'anthropology' to describe what they have been doing in the past, and continue to do at present. They base their arguments on the assumption that Polish ethnology was always identical to anthropology in other countries. This strategy seems to be the result of a growing pressure on the relatively large numbers of ethnographers to rewrite the history of their own activities as something other than, at best, passive description and at worst, ideological fabrication. In this context, the term 'cultural anthropology' is used without reference to either extra-European peoples or any real theoretical generalizations. However, we have already seen that the notion of a 'folk-type' was invoked as a feature of global culture and this trend to anthropologize ethnography has adopted this strategy. These ethnologists argue that there has been no significant transformation in ethnology in Poland and, therefore, the contemporary situation offers them no new challenges. Perhaps it would be less ambiguous if this group were content to adopt the label of ethnographers (or folklorists) to describe their activities. The reluctance to do this lies in the general contemporary perception in Poland that 'anthropology' is a more valuable activity than ethnography.

Another attitude amongst ethnologists assumes the necessity of the reconstruction and redevelopment of anthropology in Poland. This is supported by ethnologists, sociologists and philosophers. One small but quite visible group seems to promote an extreme form of methodological relativism that is inspired by the philosophical writings of Rorty. This group can usefully be described as postmodernists. These include Wojciech Burszta, Stomma, Piatkowski and, to an extent, Buchowski; and sociologists with an interest in anthropology include Jawlowska and Wyka amongst others. This movement has enabled lively and critical discussion and provides a kind of shocktherapy for a discipline long removed from significant critical reflection. They look to the rethinking of their own tradition and to the re-establishment of

intellectual ties with the West as being the most pragmatic way forward.

It is hardly surprising that a discipline deprived of any significant theoretical possibilities other than historical materialism, and subjected along with the other humanities to the pressure of official doctrine, would embrace the most extreme versions of theoretical relativism available in contemporary philosophical thinking. It is argued that since some contemporary philosophers, and Rorty is most frequently cited, believe that today there is no possibility for general theory, there should be no theory in anthropology. Giedymin in his essay 'Remarks on the philosophical meaning of Postmodernism' (1993) tries to account for the recent popularity of postmodern ideas in a broad spectrum of Polish disciplines in terms of their perceived intellectual liberalism. Constraint on space here means that we are only offering a simplification of this trend. This kind of thinking is also driven by a reaction to the bleak positivism of Marxism here in Poland and a contemporary crisis of self-awareness and political correctness in anthropology, in general, about its valid objects of study and techniques. This leap from early modernist ideological confidence to postmodern despair of any truth is reflected, not just in ethnology, but in many aspects of Poland today.

There also exists amongst many ethnologists a desire to promote opportunities to critically rethink the results which have been obtained hitherto and to renew contact with Western institutions. This seems to offer one of the most constructive possibilities for Polish ethnology. Some members of the ethnological community in Poland, at every level, are availing themselves of opportunities both to study in the West and to carry out fieldwork in non-Western regions.[5]

In any event, the present situation is extremely fluid and the choices are many and exciting. It seems obvious that ethnology will embrace independent theoretical orientations from the field of social and cultural anthropology and ethnography will be elevated from its present, almost anti-intellectual descriptive condition.

NOTES

1 These chairs were occupied by Professor J.S.Bystron in Poznan in 1919; Professor Bystron again in Cracow in 1925 and Professor K.Moszynski also in Cracow in 1926; Professor C.Baudouin de Courtenay-Ehrenkreutz-Jedrzewiczowa in 1927 in Vilna and also in Warsaw in 1935; Professor S. Poniatowski in 1935 in Warsaw.

2 These ethnological journals are: *Lud* founded in 1895, Polish Ethnological Society; *Polska Sztuka Ludowa* founded in 1947, Institute of Art, Polish Academy of Sciences (PAN); *Literatura Ludowa* founded in 1957, Polish

Ethnological Society; *Etnografia Polska* founded in 1957, Institute of Archaeology and Ethnology, PAN; *Lodzkie Studia Etnograficzne* founded in 1959, Polish Ethnological Society; *Ethnologia Polonia* founded in 1975 and published in foreign languages, Institute of Archaeology and Ethnology, PAN.

3 These professors were: Baudouin de Courtenay-Ehrenkreutz-Jedrzewiczowa, Bystron, Czekanowski, Dobrowolski, Fiszer, Frankowski, Krzywicki, Moszynski and Poniatowski.

4 This term *historia kultury materialnej* denotes the historical study of cultural objects. For example, dress, machinery, tools, buildings, etc. This field of studies was of joint interest to ethnographers, historians and archaeologists.

5 Details of current research activities in Poznan are outlined in the Institute of Ethnology and Cultural Anthropology Handbook (see bibliography).

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12 Historical anthropology and the history of anthropology in Germany

Nikola Susanne Bock

Historical anthropology is a relatively new element in the landscape of German science.[1] In France and England this approach has a long tradition and it plays a central role in historical and anthropological research at universities and other institutions. In Germany, however, historical anthropology has been situated on the borderline of 'official' science for the past ten years—similar to so-called *Alltagsgeschichte* and oral history in the 1970s. In contrast to the situation in France and England, historical anthropology in Germany had to develop without the support of social history and without the support of ethnology and anthropology. To me, this seems to have its origin in a particular aspect of the history of science in Germany.

The question of the (non-)acceptance of historical anthropology in the domain of history in Germany is closely connected with the acceptance of social, cultural and symbolic anthropology within the field of German *Völkerkunde* (ethnology) and historiography in general. The vocabulary used by various scholars describing historical anthropology is instructive: it is repeatedly referred to as an 'irrational', 'subjectivist' or 'total' approach.[2] This reminds the (German) reader immediately of the vocabulary with which ethnology (more particularly *Volkskunde*) in post-war Germany has been criticized and blamed for its misuse by National Socialism.

Although justly done,[3] terms such as 'mentality', 'unconscious', 'imaginary' or 'irrational', seem to be connected to this experience of misuse to such an extent that they cannot be employed without invoking this experience in the history of sciences in Germany. So much so, that these terms still seem to be frightening to German scholars working in the humanities today, dedicated, as they are, to preventing new 'irrational' fascism by reinforcing rational and structural explanations of human behaviour and social life. This same fear may be one of the reasons why the empirical approach within

German ethnology *(Völkerkunde), Volkskunde* and historiography predominated in post-war Germany until the seventies and still exists in some schools of thought.

In this chapter I shall discuss whether this hypothesis clarifies why historical anthropology had to overcome a great number of difficulties and had to respond to the resentment of the 1968 generation of German social historians. Among the least pliable were the so-called *Bielefelder Schule,* which stands for a historiography orientated on sociological methods and structural models ('Historische Sozialwissenschaften') connected above all with the names ofJürgen Kocka and Hans-Jürgen Wehler. I shall also consider the importance of this process in the wider European context in countries where social history has been supportive to the development of historical anthropology.

I shall concentrate, first, on the creation and development of historical anthropology in France, its history and concepts, focusing on the *Annales* school. Second, I shall examine the situation of German folklore studies *(Volkskunde)* and ethnology *(Völkerkunde)* during the National Socialist period and the attempts to create a 'clearing' discussion after 1945. Finally, I shall turn to the emerging lines of thought within current German discussions about historical anthropology and its scope.

HISTORICAL ANTHROPOLOGY IN ITS BEGINNINGS

Historical anthropology originated in France in the early 1930s as a special branch of historiography. In his book *The French Historical Revolution* (1990), Peter Burke describes the history of the *Annales* school, recounting the conditions under which the 'histoire de mentalités' was created and its aim and importance for current historical and anthropological research. Only since the seventies has historical anthropology, which gradually developed out of the history of *mentality,* crossed the border into Germany. Edward Thompson and Eric Hobsbawm are two pioneers of historical anthropology in England, where it gained more attention since the 1960s (Burke 1990). In his book *The Making of the English Working Class* (1964), Thompson analyses the development, self-perception and self-awareness of a heterogeneous group of day-labourers, homeworkers and small craftsmen over a period of a hundred years. Their protests cannot be explained by means of Marxist or materialistic analysis as a mere reaction to their life conditions. While these men were shaped by social and economic structures, it was their perception of these life conditions which made these conditions significant. This discovery was

important within British social history, which up to then had been structurally orientated. Rebekka Habermas, a German scholar within the field of historical anthropology, underlines the importance of Thompson's analysis: Thompson together with large parts of social and cultural anthropology defines social processes as an alternate play between structures on the one hand and perceptions, interpretations and actions on the other' (Habermas and Minkmar 1992:9). Later on, Thompson's approach was reinforced by two well-known studies of witchcraft in Tudor and Stuart England (Macfarlane 1970; Thomas 1971). Both works were influenced by anthropology and by the increase of micro-historical research in the 1970s. The tension between the structural level and the individual experience was a central element of this new historical anthropology.

The history of mentality, as historical anthropology is still known in Germany, 'belongs to those renegade sciences which as such indulge in penetrating the basis of generally accepted cultural norms taken for granted by singular societies' (Raulff 1987:9). With these words Ulrich Raulff, the editor of one of the first collections of French articles on historical anthropology in Germany, describes a basic concept within the history of mentality, influenced by the *Annales:* turning to the subconscious or the imaginary through reflection. Raulff describes as 'mentality' the silent acceptance of norms in a given culture.

As a term, mentality was not created by the *Annales* or its founders Lucien Fevbre and Marc Bloch. They gave mentality a new dimension, although a unanimous definition has never been agreed upon. Thus, Lucien Fevbre describes mentality as the 'mental framework' which a certain society can utilize at a certain time (Bruguière 1987:33). Braudel, belonging to the second generation of the *Annales* in the 1950s, saw mentality mainly as a 'mental prison' which withstands changes. According to his concept of different historical times (the division of historical time into geographical, social and individual time), the *longue durée* was the equivalent of 'mentality' (LeGoff 1990:39).

The main goal of the founders, namely a renewal of history as a scientific discipline, can be characterized as the movement from the traditional 'story-telling' of events towards an analytic historical science focusing on research problems. The main domain of historiography until then—politics—was to be replaced by a more general view of all human activity, thereby enlarging the scope of historiography. An integrative approach, including other scientific

disciplines such as geography, sociology, psychology, economics, linguistics and social anthropology was aimed at, and achieved.

This created a new relationship between the concepts of society and culture, the individual and the collective within history. This enabled an escape from the static event and a move towards new concepts of time and space. In this context, the term mentality, corresponding to Durkheim's 'les réprésentations collectives', obtained its central importance for historiography. The term, mentality, made it possible to avoid an ideological interpretation of history, or to concentrate only on the economic and structural aspects.

The history of the *Annales* has been described thoroughly by LeGoff (1990 and 1992) and others. In this context, it is relevant to mention the so-called third generation, the generation of today, which Burke characterizes as supporting: (1) a preference for cultural anthropology; (2) a return to politics; (3) a renaissance of narration in historiography (Burke 1990:79). These new elements enable differentiation between current historical anthropology and the history of mentality as defined by the first generation of the *Annales* school.

Focusing on cultural and symbolic anthropology is no longer understood as adopting concepts and methods of anthropology to tackle the term mentality within historical research. This was the case with Fevbre or Bloch. Historical anthropology today, represented by the work of the 'new cultural historians' such as Roger Chattier, Robert Darnton, Pierre Nora, Arlette Farge, Carlo Ginzburg, Natalie Zemon Davis and others, considers the growth of history and culture, not merely its outcome *(Das Werden, nicht das Gewordensein),* to be the object of historical research. Therefore, one can often find Erving Goffmann and Victor Turner, both of whom advocate studying the dramaturgy of everyday life, as well as Pierre Bourdieu, Michel de Certeau, Clifford Geertz, James Clifford and Michel Foucault mentioned in the publications of anthropological historians.

In general terms, historical anthropology interprets history as 'foreign perception' *(Fremdwahrnehmung).* Today's anthropological historians practise cultural interpretation. They perceive the world as a field of discourse, a network of images and viewpoints which constitute and create its reality. Their goal is to neutralize the assumed existence of total units, described by terms such as 'event', 'society', and so on (Roger Chattier 1989). Instead—in contrast to general assumptions—anthropological historians concentrate on a single subject. They try to avoid 'the whole' by focusing on the 'inside of the actors' using 'thick description' (Geertz 1983a).

By employing this semiotic definition of culture, historical anthropology relates to the discussions within cultural anthropology in the last decades, especially those of a new hermeneutical approach in its search for new criteria for Writing Culture (Clifford & Marcus 1986). Diachronic and synchronic processes enter into what can no longer be called reconstruction but rather the re-creation of the past. The past is no longer understood as a state of being but as a relationship. The categories of time and space, common in historiography, can be questioned by reflecting on one's own position as an 'observer'. Thus, perception and observation must not be objectified in a *tour-de-force*.

More important than the authors' idea of history is their interpretation of the past and the 'other' and how this is organized in time and space. The perception of the 'other' becomes a 'constructive deal' between perceiver and perception-giver on the basis of 'dialogic and polyphonic processes' (Clifford and Marcus 1986). The process of meeting and perceiving is no longer considered an annoying, subjective factor in the frame of objective historical perception. Rather it is recognized as a useful basis and a necessary requirement that perception be integrated in the depictive process. This also implies filling a gap in the history of science: the reintegration of aesthetics in science. The aesthetic dimension gains significance as a means of communication.

Thus, the self-ordained task of historical anthropology is to understand the past as a process and not as a result or a static fact. One does not want to limit the past to reconstructions, based on results only obtained by looking back from the present future. It consequently involves relinquishing the concept of reconstruction itself. According to Raulff, editor of the *Kleine Kulturwissenschaftliche Reihe* at the publishing house Wagenbach (Berlin), historical anthropology is the 'historical phenomenology of the humanly possible' (Raulff 1987:11).

The concept of creating history is reflected in the author's choice of subject matter. For instance, Natalie Zemon Davis's research on the value attached to truth or the ability of telling lies in early modern France concentrates on the analysis of petitions addressed to the king by peasants accused of murder (Davis 1988). Another example is Roger Chartier's research on reading behaviour, which not only questions what was read in a statistical manner, but also how it was read: silent or aloud, in a familiar circle, etc. (Chartier 1989). Peter Burke's social history of memory and his exploration of the development of collective memory and its media in modern societies

(Burke 1989) also is an important example of using new sources in historical anthropology. Another important contribution is Pierre Nora's reframing the old term 'source' into the term 'memory-place' (Nora 1990), thereby clarifying the process of creating history and enlarging the theoretical or cognitive potential of historical anthropology.

HISTORICAL ANTHROPOLOGY AND ETHNOLOGICAL SCIENCES IN POST-WAR GERMANY

Against this background I now propose to discuss the situation of historical anthropology in Germany. Historical anthropology as it developed in Germany in the 1970s is connected with the work of historians such as Richard van Dülmen (1988, 1990), Hans Medick (Medick and Sabean 1984), Norbert Schindler (1984), Thomas Nipperdey (1976), Alf Lüdtke (1989), Claudia Honegger (1977; Honegger and Heintze 1981), Rebekka Habermas (1991), the sociologist Wolf Lepenies (1976, 1985) and others. Although they have been quite productive, historical anthropologists had to face two main difficulties. On the one hand, there was a remarkable lack of interest in cultural anthropology among German ethnologists and historians (which only began to change in the last ten years). On the other hand, there was a strong feeling of resentment on the part of social historians (as already mentioned above), which partly exists until today, as I shall show later.

However, one can find traces of a cultural historical tradition in German historiography as early as the eighteenth century. In that period, cultural history involved the description of the multiplicity of cultures, focusing on the origin and the development of cultures. The concept of cultural history from the very beginning was based on the idea of the completion of universal history through the human intellect (Voltaire). The 'universal' approach remained central in the eighteenth and early nineteenth century. Jakob Burckhardt's *Kulturgeschichte,* in his qualitative understanding of culture following Voltaire, provides one of the first examples for not defining culture as a final process. Instead, Burckhardt understood cultural history as the presentation of circumstances and habits in images. In his opinion cultural history is a revision of reality. Burckhardt can therefore be seen as a pioneer of cultural sociology in Germany (Wunder 1990:69). However, as Wunder points out, 'German cultural historiography in the nineteenth century holds an exceptional position in that it was oriented towards *Volk,* for

lack of a social identity', as we see with Herder and Grimm (Wunder 1990:69).

These different views of cultural historiography culminated in the very first *Methodenstreit* between Schäfer, a student of Treitschke, and Gothein, who followed Burckhardt's teaching. Schäfer insisted on the importance of state and national history, whereas Gothein considered cultural history to be the one and true source of universal historiography.

During the second confrontation, the so-called *Lamprechtstreit* (1893–8), the primacy of the 'individual' or the 'collective' approach in history was questioned. A gap was created between the methodology of an 'objective' science and an idealistic understanding of history which did not explain human actions through causal means. In Karl Lamprecht's approach, cultural history was based on social psychology. Therefore, one can understand this debate to be 'at the very beginning of social history' (Oestreich 1969:70) or, even better, as one of the first attempts to combine cultural and social history. Wunder describes the consequences of this debate for a German history of science: 'With the defeat of Lamprecht cultural history as universal social history disappeared from historiography in Germany. The following *Enthistorisierung* is significantly expressed in the change of the term "cultural history", into "cultural science". Afterwards, official historiography in Germany ignored these cultural and psychological aspects' (Wunder 1990:71).

A period of nearly a hundred years passed before new impulses influencing historical anthropology occurred with the work of Norbert Elias, Pierre Bourdieu and others mentioned above. One school of thought within historical anthropology depends upon the philosophical anthropology of the eighteenth century. This is concentrated around the *Institut für historische Anthropologie* in Freiburg founded in 1975 (Martin 1982) and the *Institut für empirische Kulturwissenschaften* in Tübingen, 'which has again taken up the subject of cultural history and supplied the old folklore studies *(Volkskunde)* as an empirical science of culture again with its historical basis (Rehistorisierung der *Volkskunde* als empirische Kulturwissenschaft)' (Wunder 1990:72).[4] The historian Nipperdey convincingly demonstrates that most approaches of cultural anthropology in the nineteenth century were a reduction of the relationship between culture and individual, following only a 'scheme of expression' (Nipperdey 1976:33–58). The question of the genesis of culture and mentality and their relationship was not a thematic impulse of German *Kulturgeschichte* in the nineteenth century. In his monograph on cultural history, social history and historical

anthropology (1967), Nipperdey discussed these scientific traditions in a new way. He argued in favour of an anthropological dimension to social history. The sociologist Wolf Lepenies focused on the idea of an instrumentalization of anthropology for historical purposes (Wunder 1990:81). The scholars of the Freiburger Institute for Historical Anthropology just mentioned are near the centre of this universalistic understanding. We can sum up these approaches as an anthropology with utility and adaptability for history. In contrast, historical anthropology, as understood by the 'Göttinger Kreis' around Hans Medick, Alf Lüdtke and Rebekka Habermas can be characterized as 'historical hermeneutic of symbolic acting' (Wunder 1990:83; Habermas 1992:19).

Turning now to ethnology *(Völkerkunde)*, it has been observed by Kohl (1989) that in post-war Germany only the empirical concept prevailed in both *Völkerkunde* and historiography (Kohl 1989). One possible explanation for why none of the traditions of cultural history were resumed after the war is probably fear—the fear of using ideas and terms which had been part of Nazi terminology and ideology. This fear functions as an informal censorship of social memory (see Burke 1989). This partly results from the allegations made in post-war Germany against the traditional schools of thought. They are accused of being exploited and misused by National Socialism.

In the last years a new focus on the history of science of both *Völkerkunde* and *Volkskunde* during National Socialism has begun. Recently, quite a lot of literature about ethnology in the National Socialist period has appeared (Fischer 1990; Linimayr 1994; Hauschild 1987; Jell-Bahlsen 1985, and others). This literature rejects the longlived assumption that, in contrast to *Volkskunde* and prehistory, *Völkerkunde* as a discipline suffered repression because it did not fit Nazi ideology. Fischer, for instance, describes the opportunism of the majority of ethnologists and their readiness to adapt their work to Nazi ideology (Fischer 1990). He does not blame German ethnologists for their personal responsibility, but analyses the influence of Nazism as a political system on ethnology as an academic discipline. Thomas Hauschild and Lothar Pützstück, who organized the first colloquium on ethnology and National Socialism at the *Institut für Völkerkunde* (in Cologne) in November 1990, also stress the importance of a deeper analysis into the history of *Völkerkunde* during National Socialism, claiming it is necessary for the identity of the discipline today. They indicate several problems in the discussion of the disciplinary identity for which a deeper engagement with the history of *Völkerkunde* during

the National Socialist period could be useful. There is, first, the problem of an anthropology in Germany. In Hauschild's and Pützstück's understanding, a nationalist tradition can be reflected in the names of *Volks-* and *Völkerkunde,* which sometimes prevents a fruitful exchange with colleagues from other countries. Second, they mention the problematic relationship between ethnology and the *Zeitgeist*. As Hauschild and Pützstück point out in their account of the colloquium, *Völkerkunde* in Germany has displayed an understandable but exaggerated scepticism towards *Ideologisierungen* because of the National Socialist experience. This precluded the possibility of coming to terms with substantial theoretical developments in the social and cultural sciences during the 1960s and 1970s (Hauschild and Pützstück 1991:566).

Just like the discussions on ethnology and National Socialism, the discussions on the history of German *Volkskunde* from 1933 onwards also began with a forty years' delay, excluding the work of a very few scholars, such as Hermann Bausinger and Wolfgang Emmerich (cf. Gerndt 1987). Bausinger and Maus, in their analysis of *Volkskunde* after 1945, argued that the discipline contained long-lived affinities which made exploitation by National Socialists possible (Maus 1946/47; Bausinger 1965). I shall sum up Bausinger's analysis shortly.

Nazi ideology misused the term 'people' both in the discipline of history and in *Volkskunde* and *Völkerkunde*. The term 'people' had not been a social category within the context of *Volkskunde* since Romanticism. It was only used as an ideological term. In Germany, according to Bausinger, this ideological understanding of *Volk* led to a readiness and willingness to propagandize within an 'irrational *Weltanschauung*'. Fischer analysed the same phenomenon in his book on *Völkerkunde* during National Socialism, discussing 'the irrationality of *Lebensphilosophie* and the anti-democratic attitude it implied as harbingers of the Nazi ideology' (De Wolf, in a review of Fischer, 1992:473).

In his fundamental article on 'Volksideologie und Volksforschung' (1965), Bausinger described the history of *Volkskunde* during National Socialism. He placed the roots of *Volkskunde* in Romanticism and cultural history and demonstrated 'elements of *Volksideologie*' that characterized the German *Sonderweg*. He described the national accent of German *Volkskunde,* followed by the racist accent, the equation of Nordic and Germanic, the esteem of the peasantry and, decisive for our argument, the 'organic construction of a closed national personality' *(Volkspersönlichkeit)* (Bausinger 1965:191). He concluded his characterization of elements of 'Nazi-

Volkskunde' with a description of tradition and the quasi-religious character of popular culture, as well as describing the absolute priority of *Volkstumspraxis* in Nazi ideology.

The second concept misused by National Socialism was that of continuity, which was used to support the theory of the superiority of the Germans. The continuity of traditions was stylized as German *Volkstum,* thus creating an allegedly German mentality. In his analysis of *Volkskunde im Nazismus,* Utz Jeggle (1988) underlines three aspects as the most relevant. These correspond to the three terms (people/continuity/German mentality) described above: The myth of the origin; the longing for sense and its representations; and finally the idea of race as a scientific principle.' All these aspects are part of a strategy which aimed at a total depreciation of history: 'a denial of the historical world which is altogether fundamental for NS-Volkskunde' (Jeggle 1988:62). A decisive element of how *Volkskunde* functioned during the National Socialist period in post-war German *Volkskunde* in Jeggle's point of view was the historical approach of the so-called Munich school. In this school *Volkskunde* re-entered history by getting rid of this mystical longing for sense and establishing a serious critique of sources.

After 1945, another attempt to clear *Volkskunde* of its Nazi past was made by the Tübinger Ludwig-Uhland Institute. According to its approach, *Volkskunde* is understood as an empirical science of everyday life *(Alltagswissenschaft)* with an emphasis on present interests. In Jeggle's view this is 'also a basis to learn the difference between ideological statements and social reality' (Jeggle 1988:67). Jeggle considers the student revolt of 1968 to be the third important element for coming to terms with Nazi history in *Volkskunde*. Describing the discussions at a congress of the Deutsche Gesellschaft für Volkskunde (DGV) in Detmold in 1969, he points out: The tensions present themselves in the so-called *Namensdebatte,* which started from the assumption that history and especially the Nazi-period extremely misused the category "Volk" or "nation" and thus, it should be cast off (Jeggle 1988:68). The muddle of today's confusing multiplicity of terms, such as *Völkerkunde, empirische Kulturwissenschaft, Ethnologie, europäische Ethnologie,* and others, was not resolved at the 1969 congress, where it was impossible to reach agreement. This dilemma, as Jeggle points out, is a countermovement to National Socialist history: 'After having yearned for unity in the Nazi period, afterwards there was a mistrust of endeavours longing for unity' (Jeggle 1988:68).

Another consequence of the Nazi period is a fear of long periods

and continuity in history. This can be observed in the social historical orientation on micro-structures over short time-spans, which was explicitly meant to oppose the Nazi idea of mythical continuity. It might be that the descredited search and longing for meaning in history, essential in the Nazi ideology of Germanic continuity, has caused scholars to overlook unbroken traditions and slowly changing movements in history. Jeggle concludes: The reduction of history into surveyable time units cuts off the fearless consideration of human potential, which has seen the light of day in the NS-period in a murderous way. Denying the irrational in history and life is no solution' (Jeggle 1988:69).

The need for a new clarifying exploration of continuity as a variant in history and historical process has thus emerged, as well as the need to find a new way of dealing with 'the other', 'the irrational' and 'the unknown' to escape a narrow empiricistic viewpoint.

CONCLUSIONS

This contextualization also provides an explanation for the ideological confrontations in certain areas of German social historical research during the sixties and seventies. These initially centred around oral history and then focused upon historical anthropology. Historical anthropology, based on the 'irrational', 'imaginary', 'subjectivity' and 'the insight of the actors', was not allowed to develop from social history: in the German context it seemed to be its opponent. The 'irrationally' interpreted 'understanding' (as it was named by Wehler, 1979) of cultural anthropology, as well as of historical anthropology was perceived as an attack on the sociohistorical explanatory possibilities of the so-called *Bielefelder Schule*. On the one hand, this can be explained by the rather late development of social history's legitimacy but, on the other hand, this seems to be caused by the reputation the term 'understanding' has in the context of German social sciences. That this 'understanding' was not a historicist one but rather of the epistemological kind was not recognized by social historians for a long time (Wunder 1990).

Though institutionally still on the border of official university programmes, historical anthropology in Germany continues to free itself of the reputation of being 'irrational' and 'total'. Not only the work of the already-mentioned historians working in the field of historical anthropology proves this, but so does the establishment of a new graduate school in Berlin for the historical, anthropological and sociological comparison of societies, founded by, among others, the

main representatives of the *historischen Sozialwissenschaften (Bielefelder Schule)* such as Jürgen Kocka. However, in this context, accepting anthropological concepts seems to be a kind of compromise, a power struggle. 'Yet as a matter of principle it is recommended to counterbalance: historicism is no more convincing when coming from the left and draped as oral history *(Alltagsgeschichte)*. The public functions of historiography are not absorbed by the formation of identity. Finally, it is worthwhile to stick to the rational possibilities of scientific history…and these have gathered considerable strength by an opening up towards the social sciences', concludes Kocka in his short survey of history and its tasks today (1991:358). At the same time, the first official chair for *Mentalitätsgeschichte* has been established at the historical faculty of the Humboldt University in (East) Berlin, the 'evacuation' of which after 1989 is being utilized to raise the human disciplines to European standards. Significantly, the chair was not occupied by one of the few anthropological historians working in Germany, but by a social historian (Wolfgang Hardtwig). Today, historical anthropology in Germany mainly deals with research on the family and women, protest behaviour and research into festivals and festivities. Like the research associated with the *Annales* for a long time, it deals only with the early modern period.

Historical anthropology should have its scope thematically enlarged to include modern and contemporary history, as has been done in France during the past ten years, exemplified by the work of Michelle Vovelle, Pierre Nora, Arlette Farge and others. The development of historical anthropology in Germany as an accepted discipline offers an opportunity to integrate the 'irrational' and 'subjective' into an understanding of science based on rationality. According to Burke's definition of historical anthropology, as a mediator between social history and the history of ideas, it might help resolve the opposition between 'the irrational' and 'the rational' which has dominated post-war history of science in Germany.

NOTES

1 This discipline is sometimes also called anthropological history. 'Historical anthropology', however, is the common term in the English-speaking world, in German translation 'historische Anthropologie', in French: 'l'anthropologie historique'. I use 'historical anthropology' in this chapter in order not to create confusion. Nevertheless there are some authors, such as Peter Burke, Marshall Sahlins, Kirsten Hastrup, Natalie Zemon Davis, to whom I mainly refer here, who also use the term 'anthropological

history'. In their understanding, the term 'anthropological history' indicates that this kind of research is not to be taken as the history of anthropology, but tries to define an anthropology of history, a new understanding of history and culture, structure and event. See Habermas and Minkmar (1992:8–18, Introduction by Habermas); also Sahlins (1991:37–99); Burke (1990:79–89 and 112–16); Hastrup (1990:1–11 and 296–304).

2 About general prejudices towards historical anthropology see Rebekka Habermas (1992: Introduction). A good example of these kind of attributes is also the discussion between Volker Reinhardt and Rebekka Habermas in the *Frankfurter Rundschau, Forum Humanwissenschaften,* June 1992.

3 See Hans Fischer (1990, ch. 4) on irrationality and its significance to National Socialist ideology.

4 The terms *Volkskunde, Völkerkunde, Europäische Ethnologie* and *Ethnologie* are the most commonly used in German academic language. *Volkskunde* in general occupies itself mainly with popular culture or folklore within German history. See Hermann Bausinger, '*Volkskunde* im Wandel' in Bausinger *et al* (1978:1–11).

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13 Spanish social anthropologists in Mexico

Anthropology in exile and anthropology of exiles

Enrique Hugo García Valencia

Reflections on the development of anthropology have been an important issue among Mexican and Spanish anthropologists for a long time. Recently, in the seventies and eighties, a whole range of authors have dedicated their writings to reflecting on the role of anthropology in the concert of the social sciences and its political implications.[1]

My intention is to connect two recent papers dedicated to giving an overview of the development of anthropology in Spain and in Mexico, respectively by Joan Prat (1990) and Esteban Krotz (1991). The reason why I want to connect these papers is that, in spite of a long-lasting mutual influence between Mexican and Spanish intellectuals, one feels that anthropological reflections still underestimate the strong links that existed and still exist between anthropologists from these two countries.[2] In a more general context I am interested in studying the strong influence that European anthropologists (German, Austrian, Spanish and Italian) have had in founding and promoting centres of anthropological teaching and research in Latin America.

Academic relations between Latin America and Spain became especially strong during two historical periods: one in which most countries of Latin America and Spain were members of the same empire; the other, when Latin American countries, and in particular Mexico, served as host to Spanish exiles many of whom were political refugees with ideas akin to those of the revolutionary Mexican elites.[3] Some of those migrants decided to study anthropology and the work of some of them became extremely important in Spain and in Mexico.

The modern flow of Spanish, German, Austrian and Italian anthropologists to Mexico, and Central and South America is a direct consequence of the European wars: the Spanish Civil War in one case and the First and Second World Wars in both cases. The case of those

emigrants illustrates the strong links between the complexities of the world's political scene and the development of anthropology on the one hand, with the complexities of anthropology itself on the other.

In 1910 the first school of anthropology was founded in Mexico as a joint venture of the German government and the American government, under the leadership of Franz Boas (Krotz 1991:183). Such an initiative was supported by the Mexican government of Porfirio Díaz, precisely in the last year of his thirty years of dictatorship, before being overthrown as a consequence of the Mexican Revolution of 1910. Since then, the schemes of anthropological institutional development in Latin America are more directly related to Spanish, German, Austrian or Italian scientists than with any other theoretical trends, in spite of the great theoretical influence of British, American or French anthropology in Latin America.

In spite of the commonly accepted understanding that it is under American theoretical and managerial influence, anthropology in Latin America is the result of rather complicated processes in which one can distinguish certain theoretical influences on a superficial level. However, the manner of doing anthropology in those countries is the result of rather complex and often divergent influences.[4]

One of the papers to which I shall make reference, is Joan Prat Carós's account of how social anthropology was developed in Spain (Prat 1990:117–24). The fact that this author contributes to a book with a very broad scope, explains perhaps why his account is rather brief and not very detailed.[5] The other is by Esteban Krotz: *A Panoramic View of Recent Mexican Anthropology* (Krotz 1991:183–8). This is a rather rapid overview of the development of anthropology in Mexico and in particular of the development of social anthropology.[6]

COLONIAL SPANISH CONQUERORS

From a historical point of view, anthropological work has been done in Mexico from the 'first contacts between the European invaders and the Americans' onwards (Krotz 1991:183; Aguirre Beltrán 1990:271).[7] The colonial anthropological works to which Krotz and Aguirre Beltrán refer can also be considered as the pioneer works of Spanish anthropology, because in the sixteenth century Mexico and Spain were part of a single empire, and because the early anthropological works on America were mostly written by Spaniards. From that time onwards the intellectual relationship between Spain and Mexico has been uninterrupted.

The fact that Krotz relates the origins of Mexican anthropology to the writings of friars and conquerors, who documented Indian customs in the early years of the conquest, indicates his readiness to accept the Spanish presence in Mexico. That presence is still felt in this country due to a continually renewed migratory flow from Spain.

The writings of Spanish conquerors, friars and travellers did not have a similar influence in Spain where anthropology is traced back in Prat's account to 'the second half of the nineteenth century onwards' and is more connected with folklore (Prat 1990:117) than with Spanish writers from overseas.[8]

Over the centuries the bases for the development of anthropology in Spain and in Mexico followed different paths. In Mexico a great interest was manifested by the study and collection of Indian antiquities,[9] in Spain anthropology and folklore have been closely related. In Mexico, history and the Indian past were used to legitimize nationalistic and independentist aspirations of the national creoles *(criollos nacionales)*.[10] In Spain a discourse based on folklore or a discourse based on a reflexion on a generic man have formed the bases of the 'two main theoretical discourses about men in the last century' (Prat 1991a:14).

In spite of their differences, an independent museological tradition, in Spain and in Mexico, constituted a common link where a holistic approach to mankind was the main theme. Therefore, when Franz Boas founded the International School of Archaeology and Ethnology in Mexico (1910) with a holistic approach, that approach was already traditionally followed in the National Museum of Mexico. It is possible to think that Boas's holistic approach was also a consequence of his museum work and the conception of culture prevalent in the museums of his time.[11]

Spanish museums, in the last century, also led to what Prat calls a proper anthropological discourse. Following such discourse several anthropological institutions were founded in the last century: the Spanish Anthropological Society (1865), the Seville Anthropological Society (1870), the Madrid Museum of Anthropology (1875), the Scientific Bureau of Tenerife (1877) and the Canary Museum (1879 at Las Palmas).

But then The great tradition of anthropology and folklore which had developed in Spain from the second half of the nineteenth century onwards was cut short by the Civil War' (1936–9) (Prat 1990:117).

MODERN SPANISH EXILES

Again Spain unwillingly found in its by then ex-colonies a fertile ground where to mix with a great anthropological tradition such as the Mexican one. Only this time, unlike the sixteenth century, their relations were established on more equal bases.

Only in this century have the paths of Mexican and Spanish anthropology converged. Due to the Spanish Civil War (1936–9) there was a flow of Spanish exiles who migrated to Mexico. Some migrants were young people who did not already have a professional training, many of whom chose to study social anthropology in that country. For example Angel Palerm and Claudio Esteva, quoted by Prat (Prat 1990:118), and many others such as Carmen Viqueira and Pedro Carrasco studied anthropology. Some of them, such as Pedro Armillas and Juan Comas, had already had some professional training in Spain.

It is probable that some of the Spanish exiles in Mexico chose to study social anthropology in order to make sense of the new cultural and social conditions in which they were forced to live. They were also confronted, through the strong Indianist Mexican tradition, with the Mexican Indians, a Spanish-made reality largely ignored in Spain. Finally, most of them, if not all, who had strong socialist points of view, saw in the aims of the Mexican Revolution a fertile ground for intellectual and practical action.[12]

Carmen Viqueira, one of those exiles, acknowledges that she did not even know where Mexico was. Others, like Angel Palerm, were forced by the circumstances to change their intellectual interests and to choose to study anthropology. The fact that some of the Spanish anthropologists mentioned by Prat were not social anthropologists in a formal sense would explain, perhaps, why their work is unknown to some Spanish anthropologists. Of Santiago Genovés, Juan Comas and Angel Palerm, only Palerm was professionally trained as a social anthropologist.

Rightly, however, Prat lists Santiago Genovés and Juan Comas among the names of other social anthropologists, because their work has produced an outstanding and long-lasting impact on Latin American social anthropology. They, together with 'exiles, immigrants, participants in temporary projects' (Krotz 1991:186) have contributed to the development of the social sciences in general and of anthropology in particular.

The admiration that the Mexican anthropologist Aguirre Beltran feels for the work of the Spanish anthropologist Juan Comas and the

recognition of its transcendence only describes an attitude towards his work in Mexico and in Latin America in general (Aguirre 1990:314). Moreover, his recognized merits as indigenist qualified him to contribute in the UNESCO declaration about race and racial differences *(Declaration sobre la raza y las diferencias raciales que preparó la UNESCO)* (Aguirre 1990:314).

The range of contributions of the Spanish anthropologists Comas, Palerm, Esteva, Genovés, Viqueira and Carrasco to the development of social anthropology in Mexico and in Latin America is too broad to attempt to describe it here. I want to centre the rest of this chapter on the contributions made by Palerm and Esteva to the development of academic programmes of anthropology in Mexico and Spain.

The background to further developments of professional teaching of anthropology in Mexico was laid by the foundation of the International School of Archaeology and Ethnology founded by Boas in Mexico City in 1910 (1910–17) on the eve of the Mexican Revolution (Krotz 1991:183; Castro-Leal 1988:522).

The International School was organized according to a holistic conception of anthropology.[13] According to the holistic approach, as Prat calls it, anthropology is thought to be a discipline constituted by several subdisciplines, such as physical anthropology, archaeology, linguistics and ethnology (Medina 1993:44). Later on, at least in Mexico, others have been added such as: social anthropology and ethno-history. Hereafter I shall refer to this as the holistic model.

Although the old Mexican School of Anthropology did not survive the effects of the Revolution, Boas and his influence was felt long after that period. Boas influenced Mexican and Spanish anthropologists who founded anthropological institutions, all with a holistic perspective.

Once the experiment of an International School of Anthropology had failed in Mexico, several attempts were made to teach anthropology. However, it was not until 1937 that the National School of Anthropology and the Mexican Association of Anthropologists were founded. In 1939 the National Institute of Anthropology and History was founded, and the *Instituto National Indigenista* a few years later (1948).

Different from what actually happened in Spain, and perhaps in most countries, modern anthropology developed in Mexico as a political initiative (Aguirre 1990:323; Gamio 1993:27–8) strongly supported by the government.

The interests of the national creole *(criollos nativos)* for the study of the Indian past, prevalent before the Mexican Revolution, passed to a second plane after the Mexican Revolution ended. Anthropology was

used, by the post-revolutionary political class, as a means to understand the modern Indian population and its destiny once the Revolution had ended.[14]

Aguirre Beltrán thinks that anthropology in Mexico started with the publication of Gamio's *Forjando Patria* (1916) (Forging a Nation)—a title suggesting the strong political bias that Mexican anthropology would adopt from then on. According to this new way of thinking, the Mexican nation was thought of as comprising an Indian population, not only as a political entity independent from Spain as it was thought of before the Revolution.

The first academic programmes of anthropology designed in the post-revolutionary period were also set up along the lines of a political project. The National School of Anthropology was founded (1937) as an organ of the Organization of American States to produce native Latin American anthropologists. Within internal Mexican politics, it was founded according to a policy orientated to create schools outside the university where new tendencies could develop, as opposed to the ones represented within the university in which some faculties were strongholds of intellectuals representing prerevolutionary interests.

This was the National School of Anthropology of Mexico which several exiles entered to study anthropology in the forties and later. Two of them, Angel Palerm and Claudio Esteva, became rather successful in promoting and setting up programmes of anthropology both in Spain and in Mexico.

SOCIAL ANTHROPOLOGY VERSUS HOLISTIC ANTHROPOLOGY

Esteva's and Palerm's theoretical interests are expressed in the titles of their theses. Angel Palerm presented a Master's thesis entitled *El regadío en Mesoamérica y la revolución urbana* (Irrigation in Mesoamerica and the Urban revolution).

Three years later, in 1955, Claudio Esteva presented a thesis entitled *Dinámica del caracter social (bases para la interpretación del obrero Mexicano)* (Dynamics of social character. Bases for the interpretation of Mexican labourers) (Avila *et al.,* 1988; García, 1990).

Both theses challenged the accepted theories of the times. Angel Palerm challenged Marxist assumptions prevalent in those times, and Esteva challenged the idea that in Mexico anthropology was concerned only with the rural population, particularly the Indians.

Their theoretical interests, especially those of Palerm, were the source of inspiration for many anthropologists of younger generations. Esteva was a pioneer in studies concerned with the Mexican working class. However, their theoretical studies were not the only way in which they contributed to the development of anthropology in their respective countries. They promoted, with extraordinary success, the development of centres of research and of the teaching of anthropology.

Prat considers that the second important event 'which would later prove to be significant' for the development of anthropology in Spain, was 'the return of those who had gone into exile'. One of them was Claudio Esteva himself, 'who had studied anthropology while in exile in Mexico' (1990:119).[15] Esteva returned to Spain in 1956, only a year after his graduation. There he formed scholars who eventually would take teaching posts in Madrid, Seville and Barcelona.

Also, more than fifty years after the International School of Archaeology and Ethnology was founded in Mexico (1910), and almost thirty years after the National School of Anthropology had been founded in Mexico (1937), Esteva established the School of Anthropological Studies in Madrid in 1965.

The National School of Anthropology where Esteva obtained his Master's degree was, then, shaped according to the canons of what Prat considers to be holistic anthropology. It is not surprising, then, that when Esteva founded the School of Anthropological Studies in Madrid in 1965 'it was the first institution which had completely adopted the holistic postulates of American cultural anthropology as its own'(Prat 1990:119).

The inclination of Esteva for American cultural anthropology developed because of his education in the Mexican School of Anthropology and because of his personal involvement with a research group led by Erich Fromm, another exile in Mexico. I assume that the acceptance of a holistic approach was also based on the long Spanish museological tradition, in which, as we saw before, a holistic approach was prevalent.

The holistic approach was challenged later on by Carmelo Lisón Tolosana, a Spanish anthropologist educated at Oxford. This 'model broke down…in 1977 during the *1er Congreso de Antropólogos Españoles* (First Congress of Spanish Anthropologists)' (Prat 1990:119).

In Mexico, as we have seen, the holistic approach had been a consequence of the immense influence of Boas and the *Escuela International de Arqueología y Etnología Americana,* plus the Mexican

museological tradition. Although the School could not resist the effects of the Mexican Revolution, the influence of Boas was still the leading force in other attempts made to create new centres of teaching of anthropology. The new National School of Anthropology and History created in 1937 and the National Institute of Anthropology and History created in 1939 gave a renewed impulse to the holistic model in the country.

The holistic model was not the only mode of disciplinary organization, however, and it is dubious whether it operated in the same way in the National School of Anthropology of Mexico as in the School of Anthropological Studies of Madrid.

Even in Mexico, the National Indigenist Institute did not follow a holistic approach. In spite of the tendencies of the Mexican anthropologist Gamio towards archaeology, the Mexican Caso and Aguirre Beltrán towards ethno-history and the Spanish Comas's specialization in physical anthropology,[16] indigenist practice has been more inclined towards the exclusion of archaeology and physical anthropology and concentrated in studies of social anthropology, ethnohistory and ethno-linguistics. In general, Mexican anthropology, holistic or not, has always been closely related to history (Krotz 1991:186).[17] Academically, however, the holistic approach was predominant until the late sixties. All programmes of anthropology, which were opened during the forties, followed the Boasian model.

Meanwhile, since the mid-fifties, Palerm, Aguirre Beltrán and many other Latin American anthropologists were creating conditions to establish new programmes of anthropology which would, eventually, conform to a different model (Aguirre 1990:342).

It was in 1967, when Angel Palerm entered the Department of Social Anthropology and the Institute of Social Sciences of the Jesuit Ibero-American University, that the holistic model, which had remained unchallenged for more than fifty years, gave way to a more specific one, namely, social anthropology.

The breakdown of the holistic model in Mexico, in favour of social anthropology, coincided in time with a similar phenomenon in Spain, although completely unrelated.

It is paradoxical that two anthropologists educated at the National School of Anthropology of Mexico established two educational models each of which excludes the other: the holistic model in Spain by Esteva, and the model of social anthropology in Mexico by Palerm.

I assume that political considerations were determinant in both cases. In the case of Esteva I suppose that a holistic approach was more

acceptable during the Franco regime. Palerm's inclination for a non-holistic model was the result of his confrontation with the establishment of the National School of Anthropology and the Department of Anthropology of the National University (Medina 1993:45) rather than of any theoretical position.[18]

Since then, all new Mexican programmes of anthropology have been programmes of social anthropology, with one exception which is the programme of anthropology of the University of Guadalajara, Mexico, where social anthropology and archaeology combine in one single department.

Later on, in 1975, Palerm founded the *Centro de Investigaciones Superiores del Instituto Nacional de Antropología e Historia* (CISINAH) (Centre of Higher Research of the National Institute of Anthropology and History), a research institution dedicated to studies of social anthropology, ethno-linguistics, history and ethnohistory. The activities of CISINAH, although biased by the mere fact of being set up under the umbrella of the National Institute of Anthropology and History, which is a holistic institution, became more and more specialized until it was set up as an independent entity under the name of *Centro de Investigaciones Superiores en Antropología Social* (CIESAS) (Centre for Higher Research in Social Anthropology). This centre was detached from the National Institute of Anthropology and History, and from its holistic approach, although ethno-history, history and ethno-linguistics still played an important role.

After Angel Palerm presented a successful alternative to the holistic approach, his influence on Mexican anthropology acquired still greater importance. His model has been followed by the creation of most programmes of anthropology founded in the seventies and afterwards. Moreover, he is, perhaps, one of the few Mexican social anthropologists who have created a school, or a distinct group of followers. Rightly, therefore, Aguirre Beltrán considers that one of the most important aspects of the life of Angel Palerm was the transcendent role that he had in the professional formation of generations of social anthropologists promoted in the last fifteen years (Aguirre 1990:340–1).

An example of Palerm's extraordinary influence on Mexican academic circles is that, once the programmes of anthropology at the Ibero-American University and CIESAS were opened, at least nine new programmes of social anthropology opened subsequently, and one can observe that followers of Angel Palerm have been directly involved in the organization and management of at least five of them.[19]

A new era of anthropological relations between Spain and Latin America seems to be developing. Nowadays one notices a renewed interest in Spain by Latin American anthropologists and a renewed interest in Latin America by Spanish anthropologists.[20]

CONTRIBUTIONS TO SOCIAL ANTHROPOLOGY

The examples of exiled Spanish anthropologists described above show, I think, their influence on the development of Mexican and Latin American anthropology. They promoted the creation of anthropological education and research centres; they contributed to the development of Latin American indigenist theory, and they made important contributions towards challenging old trends of doing anthropology and promoting new ones.

One notices in the process of development of anthropology in Spain and Mexico a phenomenon of reciprocity. On the one hand, Spain provided Mexican academic life with highly trained scholars. On the other hand, some Spanish anthropologists exiled in Mexico returned to Spain after having been educated in Mexico. Among them were pioneers who, together with many other national and foreign scholars, contributed to laying the foundations of anthropology in both countries.

While Spain suffered from the Civil War and a long period of dictatorship, the people in the governments resulting from the Mexican Revolution attracted and protected many scientists exiled from Spain. The strong political bias of Mexican anthropology is, therefore, a direct result of the need for national reconstruction after the revolutionary period, which led the government to promote anthropology, as well as of the experiences of war and the political commitments of some of its most conspicuous founders.

FINAL OBSERVATIONS

Spanish anthropologists who arrived in Mexico and those who became anthropologists in Mexico were, in many cases, politically committed to different Spanish causes which they supported while in exile (they were mostly socialist according to Dr Carmen Viqueira). In the same way one can predict that other European exiles and anthropologists who emigrated to different Latin American countries were also politically committed to different European causes.[21] The political tendencies of these migrants mixed with the political tendencies of local politicians and anthropologists. Therefore, American political and

theoretical domination in Latin America is only one of the several forces which have shaped anthropology there.

Certainly neither American, French nor British anthropologists have had such a strong influence in shaping institutions in Mexico as the influence of Palerm, studied in this chapter, or the influence of the German anthropologist Kirchhoff and of the Spanish anthropologist Comas, not studied here.

European and Latin American anthropology at large are more interwoven than one might be prepared to admit. Certainly, their relations are not relations of dependency but rather of mutual, and quite often unacknowledged, collaboration. The same applies to relations 'with the anthropology produced in the rest of Latin America and the Caribbean' (Krotz 1991:187).

Admirable efforts have been made recently in Spain and in Mexico to reflect on the development of anthropology. However, renewed efforts need to be made in order properly to understand and evaluate the interrelations and influences of the development of anthropology in Latin America and in Spain. In a more general sense, anthropologists have to rescue and value the nature of anthropology done in exile by exiles, a phenomenon perhaps not that infrequent, and bring that into a more general discussion of the complexities of anthropological thought and the production of knowledge.

NOTES

Several people helped me with useful comments and advice while writing this chapter: Han Vermeulen and Arturo Alvarez Roldán, Luiz Vazquez, and Joaquin Garcia Barcenas. Dr Carmen Viqueira allowed me to use her library and lent me some books. Dr Ricardo Avila gave me information on the international congress held in Guadalajara. Ann Mary Gerrard de Navarro helped me to correct the English version.

1 For example we have a recent book entitled *Antropología de los Pueblos de España* (Anthropology of Spanish Peoples) published in Spain by Joan Prat, Ubaldo Martinez, Jesus Contreras and Isidoro Moreno. Also in Mexico we have the monumental *Historia de la Antropologia de Mexico* (History of Mexican Anthropology) in fifteen volumes edited by Carlos García Mora and María de la Luz del Valle.

2 These articles triggered my interest in this issue, and since I presented an early version of this chapter in Prague new articles have appeared. For example, Joan Prat published an article on the history of Spanish Anthropology (Prat 1991a), and Carlos García Mora edited a new book entitled *Dos aportaciones a la historia de la antropologia en Mexico* (Two contributions to the history of anthropology in Mexico) (García 1993).

3 In the forties, when most exiles arrived in Mexico, there was a government

with socialist ideas. Lazaro Cardenas, the then President of Mexico, gave facilities to the exiles to join some Mexican institutions, and to further their studies in the subject that they wanted.

4 In the forties, when Palerm arrived in Mexico (1940), Mexican anthropology received new influences. This was due to the Second World War when many European refugees came to America and when American anthropologists could not do fieldwork in many of their traditional places, paying more attention to Latin America, particularly Mexico and Peru (A.Palerm 1970b:85).

5 His contributions to the anthropology of the Spanish people are more detailed and extensive (Prat 1991a:13–32 and 1991b:113–40).

6 The book edited by García Mora (1993) contains two articles from very different perspectives in time and in theory. One of those articles is a document written by the Mexican pioneer anthropologist Manuel Gamio in 1917; the other was written by Andres Medina in 1993. Both complement Krotz's points of view about Mexican anthropology.

7 This is an opinion widely accepted by Mexican anthropologists. Gamio in 1917 said that research with an anthropological character has been done in Mexico for 400 years up to now (Gamio 1993:12).

8 The rich Spanish expeditionary tradition lasted until the nineteenth century. In that century, thirteen persons travelled in or wrote about Africa, twenty-one about America, four about Asia and twenty-two about Oceania (Lisón 1991:41).

9 Medina holds a well-accepted opinion that before and after Mexican Independence (around 1810), the national creoles *(criollos nacionales),* the offspring of Spaniards already born in America, legitimized their national aspirations and justified Mexican Independence from Spain because of sharing with American Indians the fact of having been born in this part of the world.

10 These national creoles *(criollos nacionales)* were the leading forces in the Mexican Independence from Spain, which started in 1810 and finished in 1821.

11 This observation is part of a comment that the Mexican anthropologist Luis Vazquez made on a draft of this chapter.

12 Not surprisingly most of them fitted within a non-holistic anthropological trend in Mexico, a sociological one, developed after the end of the Mexican Revolution (1910), when responses were intended to solve Mexican economic and social problems (Medina 1993:49). This trend was present in the National Indigenist Institute.

13 Comas refers to integral anthropology and integral studies (Gamio 1993:16); Medina calls it global culturalist conception *(concepcion culturalista global)* (Medina 1993:44), which corresponds with Prat's holistic anthropology.

14 The Mexican anthropologist Sáenz was the first one, in Mexico, to use the term social anthropology, as the applied branch of anthropology to solve social problems. Descriptive linguistics, problems of bilingual education, physical anthropology applied to the study of living people and even museography belonged to the field of social anthropology (Medina 1993:50). Aguirre also says that the Mexican anthropologist Moisés Sáenz (in 1936) described social anthropology as a science with normative ends,

that studies reality, catalogues the facts and describes phenomena (Aguirre 1988:482). In Spain, an academic programme of social anthropology existed since 1877, some thirty years before Sir James Frazer gave this title to his lectures in Liverpool in 1908 (Lisón 1991:48–9).

15 The first of these was the interest of the American anthropologist George G.Foster and the British anthropologist Julian Pitt-Rivers in doing field research in Spain' (Prat 1990:118).

16 The Mexican anthropologists Gamio, Caso, Aguirre Beltrán and the Spanish Comas are some of the founders of Mexican indigenism.

17 Indigenism in Mexico still uses social anthropology as the applied branch of anthropology (Medina 1993:50).

18 See also a selected bibliography published by his son Juan Vicente Palerm (1970b:105–107). Palerm became largely involved in archaeological issues precisely because of his interests in the Asiatic mode of production (A.Palerm 1970:33–104). Moreover, due to his evolutionist theoretical interests, Palerm declares himself a follower of another Spanish exile in the United States, the archaeologist Pedro Armillas (A.Palerm 1970:87).

19 The Department of Anthropology of the Metropolitan University, the Department of Anthropology of the University of Querétaro, the Department of Anthropology of the *Colegio de Michoacán,* the new PhD degree in anthropology by CIESAS-Occidente in Guadalajara City, the old PhD degree in anthropology by CIESAS in Mexico City.

20 In 1992 Dr Ricardo Avila Palafox organized an 'International colloquium on regions and identity' where there were eighteen participants, eight Spanish and seven Mexican, with one Canadian, one Frenchman and one American.

The Iberoamerican University has became a channel through which several Spanish anthropologists do fieldwork in Mexico. Also Spanish anthropologists have been attached to the Colegio de Michoacán, CIESAS-Golfo and CIESAS-Occidente.

There is an agreement of scientific co-operation between the National Institute of Anthropology and History (Institute Nacional de Antropologia e Historia) (INAH) and the Beau Arts General Directorate of the Culture Ministry of the Spanish Government (Direccion General de Bellas Artes del Ministerio de la Cultura del Gobierno Español) to create an archive of oral history about Spanish refugees in Mexico (Civil War and exile). There is also an academic agreement between INAH and the University of Seville.

Several Mexican anthropologists have spent sabbatical leave at Spanish universities.

An example of the new sort of relations between Spain and Latin America is the Carlos Giménez Romero's recent PhD thesis presented to the America's Anthropology and Ethnology Department of the Faculty of Geography and History of the Complutense University of Madrid. In this thesis the author compares the agrarian communal regime of the Valley of Valdelaguna (Burgos, Spain) with the one existing in rural communities of San Nicolas Coatepec (State of Mexico) (Giménez 1991:19). The thesis was supervised and directed by Carmen Viqueira, a Spanish exile who decided to stay in Mexico. The background of the thesis is the work of the

Peruvian anthropologist Arguedas, who compares Andean zones with Spanish ones (Giménez 1991:13, 19).

21 It is my intention to carry on this line of research by studying the influence of German, Austrian and Italian anthropologists in Latin America, particularly from the point of view of their contributions to the teaching of anthropology and the shaping of national anthropological institutions.

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14 A history of paradoxes

Anthropologies of Europe

Thomas K.Schippers

INTRODUCTION

If it has become a commonplace nowadays to speak of 'unity through diversity' when the cultural and political complexity of Europe is discussed, this dialectically coloured slogan seems also appropriate to apprehend the history of European anthropology. Especially in the field of the human and social sciences, and in contrast to the exact sciences which possess culturally neutral languages of description and analysis, the history of both the theoretical and the institutional development of anthropology is marked by variety and diversity. In Europe, the development of anthropology since its academic institutionalization in the second half of the nineteenth century seems to have been very closely related to national geopolitical contexts such as the presence or the absence of overseas colonies or the different degrees of cultural diversity and homogeneity of the national states. But European anthropology also emerged as a more specialized branch of pre-existing scientific fields of interest, which differ from one country to another. Since the end of the nineteenth century, this process of segmentation, combined with international filiation, has led to a patchwork of disciplines, scientific interests, methods of investigation and theoretical schools, as a result of which it is very difficult to speak of European anthropology as a monolithic whole.

Like other professional groups, European anthropologists have known changing centres of gravity according to theoretical choices, methodological options, fields of interests or more simply according to professional opportunities. For example, until the First World War, while the European anthropological community was still quite small, its geographical centre was formed by a triangle between Berlin, London and Paris which still had close relations with emigrant

colleagues in the USA such as Franz Boas. Very interesting in regard to the later developments is that most of the scholars of this first 'centre of gravity', such as Tylor and Frazer in Great Britain, Mauss, Durkheim, Van Gennep and Herz in France, and Alexander von Humboldt and Friedrich Ratzel to a lesser degree in Germany, included Western and non-Western societies in their studies, although often within an evolutionist framework (or against it). During this period, the history of European anthropology saw its first important segmentation, which divided, often until the present day, physical anthropology on the one hand and social or cultural anthropology on the other. This bipartition, generally initiated by social or cultural anthropologists in disagreement with the racial—and often racist—evolutionism proclaimed by physical anthropologists such as Paul Broca and Arthur de Gobineau, had appeared by the 1880s in France, and soon afterwards in Great Britain, but only emerged after the Second World War in Germany, and even later in Franco-ruled Spain.

THE YEARS OF SEGMENTATION, 1920–45

After 1918 different orientations within the anthropological world appear more clearly, often corresponding both to different geographical and methodological options. The theoretical downfall of anthropological evolutionism after the First World War curiously parallels the institutionalization of one of its hypotheses: the establishment in many countries of two distinct anthropological disciplines, one exclusively studying non-European societies, the other studying exclusively European and mainly national rural societies. The causes of this institutional 'divorce' and its consequences vary from one European country to another as we shall see further on, but it is interesting to observe that in many countries during the *interbellum* separate anthropological institutions such as museums or university departments were created for the study of European and non-Western societies, although in many cases a dialogue was maintained between the specialists of non-Western regions and those studying aspects of European cultures.

Another more perverse consequence of this 'divorce' concerns the unequal sharing of what had been anthropology's double vocation when founded at the end of the nineteenth century: the accumulation and classification of ethnographic materials on the one hand, and theoretical analysis, often through comparison, on the other. While the practitioners of overseas anthropology innovated anthropological theory by doing fieldwork, the specialists of European societies

maintained a tradition of armchair anthropology with a rather low theoretical profile (often influenced by German and, in the 1930s, English diffusionism) and with most of their efforts concentrated on the accumulation of knowledge in the form of (huge) archives. This split of anthropology into a more nomothetic tendency combined with overseas fieldwork on the one hand and a more learned and cumulative study of European societies on the other,[1] has also had its academic, geographic and linguistic consequences: the first group was formed by a scientific universe using mainly English and French as a language of communication, while the second group discussed mainly in German. During the *interbellum* the different geopolitical situations, mainly the presence or absence of colonial empires, also led to an unequal development of what were becoming two types of anthropological practice. In countries with vast colonial empires, such as Great Britain, France or The Netherlands, overseas anthropology became predominant, while in countries with few or no colonies studies in national and regional cultures of Europe became institutionally more important.[2]

It has to be stressed, however, that these differences in institutional weight never prevented the existence and development of the *two* heir-disciplines even in those countries where one or the other prevailed, as is sometimes made out.

In this perspective, the history of anthropology in Great Britain offers a good example. In the nineteenth century the directors of the Folk-Lore Society,[3] such as Jane E.Harrison and R.R.Marrett,[4] were very close to Tylorian comparativism. In the period after the First World War, while the development of overseas anthropology was important in Oxford, Cambridge and London, one also saw the creation of the Scottish Anthropological and Folklore Society and its Scottish Archive for Ethnology in Edinburgh, as well as the Irish Folklore Institute in Belfast. The directors of these institutions were influenced by Herbert J.Fleure, who taught anthropology and geography in Aberystwyth (Wales), and by William J.Perry who taught in London. Although these two forms of anthropological practice increasingly had a tendency to move away from each other, scholars of both fields attended the meetings of the Royal Anthropological Institute. While on the one hand scholars were involved in intensive fieldwork overseas and in elaborating basic frameworks for social anthropology, on the other research and collecting was done throughout the United Kingdom (inspired by Scandinavian, French and German scholars), which resulted, for instance, in the eight-volume *British Calendar Customs,*

published between 1936 and 1946 by Arthur R.Wright, Mary Banks, Thomas E.Lones and Cyril Patton.

In France we can observe a similar development after the First World War, although on a more modest scale. The scholarly career of Arnold Van Gennep is a good illustration of the anthropological bipartition after 1920. While most of Van Gennep's early publications reflect the general, often overseas, anthropological discussions of his time, such as totemism in his *L'État actuel du problème totémique* (1920), from 1924, when he published *Le Folklore, croyances et coutumes populaires françaises,* Van Gennep devoted himself to the systematic collecting, partly through personal fieldwork, of data on rural France which were published in his *opus magnum 'Manuel de folklore français contemporain'* (Paris, 1938–58).[5] The hostility between the ethnological members of the Durkheimian *Année sociologique* school (such as Mauss) and Van Gennep, based on theoretical divergence and personal grievance, played an important role in the exclusion of an anthropology studying Europe from the local universities. As in Great Britain, the anthropology of Europe found its institutional base in France in a museum, the Musée national des Arts et Traditions populaires, created in 1937 (Chiva and Jeggle, 1987). During the interwar years French overseas anthropology did not witness the same spectacular development, based on fieldwork, as social anthropology in Great Britain. Marcel Mauss continued to teach and practice armchair anthropology in Paris, sending a few students abroad, such as Marcel Griaule to Africa and André Haudricourt to the Soviet Union. Particularly the empirical tradition of intensive fieldwork did not really develop in France before 1950. Only a few expeditions were organized, such as the 'Mission Dakar-Djibouti' in 1932–3, and only a few anthropologists had done fieldwork, most notably Maurice Leenhardt in New Caledonia from 1902 to 1926.

In Germany, the schism is perhaps the most evident during the period immediately preceding the Second World War. But already in the nineteenth century anthropology was born in Germany out of rather different academic traditions in comparison with Great Britain and France: as a child of geography, German overseas anthropology (called *Völkerkunde*) on the one hand, and as a child of *Statistik*[6] and philology, German national ethnology (called *Volkskunde*) on the other hand, had their methodological approaches very strongly marked by their academic origins in their ulterior developments. Without entering into too much detail, it is possible to say that both disciplines shared the following characteristics: first, a preference for an areal and a geographical instead of a social differentiation,[7] which expressed itself

through the theoretical development of diffusionism and the *Kulturkreis* (culture area) concept;[8] second, a clear distinction between the material and non-material aspects of culture; third, a certain interest in the psychological dimension of culture *(Volksgeist);*[9] and finally, a clear differentiation between non-Western societies ascribed as *'Naturvölker'* and the inhabitants of Western nations regarded as *'Kulturvölker'*—which also geographically distanced both disciplines. As the postfix '-*kunde'* (to be translated as 'knowledge of) indicates, the descriptive and cumulative dimensions have often dominated both disciplines (including physical anthropology), while the theoretical development was limited to (historical) diffusionism—with a few exceptions, such as Richard Thurnwald.[10] The moral collapse of Germany after the First World War indirectly gave a great stimulus to the institutional development of a Europe-orientated *Volkskunde,* of which the huge enterprise of the *Atlas der Deutschen Volkskunde,* started in 1928 as a sort of regional inventory of German-speaking peoples of Europe, has been one of the most evident aspects.[11] This institutionally powerful *Volkskunde* of the period 1920–44 has been a (positive or negative) centre of reference for many anthropologists studying European cultures in the 1930s. Prominent scholars such as Sigurd Erixon (Sweden), A.Jorge Dias (Portugal), Richard Weiss (Switzerland) and others from Central, Eastern and Northern Europe received part of their academic training in Berlin during the 1920s and 1930s.

At the same time extra-European *Völkerkunde* remained a rather small discipline, often closely linked with ethnographic museums. As Lutz (1982:41–2) has emphasized, the fact that German *Völkerkundler* also used the name *Ethnologie* to designate their discipline,[12] has contributed to a widening schism between them and the often nationally or regionally embedded *Volkskundler,* who seem to have had a kind of epidermic allergy towards a discipline indifferently studying European and non-European cultures within a single conceptual framework. The strong financial and institutional support by the Nazis to *Volkskunde* as a discipline after 1933 contributed both to its international influence until 1944 and its equally international disgrace after the Second World War until the end of the 1960s.

To summarize the development of anthropology in Europe during the 1920s and 1930s, one may say that in countries with important overseas interests such as Great Britain, France or The Netherlands, the discipline consolidated itself through the elaboration of (new) theoretical frameworks. This was often but not always based on empirical fieldwork by anthropologists themselves, within a context of international discussions in which scholars from other countries, such

as Germany and Italy, who were interested both in anthropological theory and in overseas cultures, participated. But during this same period of intensive post-First World War internationalism the Swedish professor Sigurd Erixon elaborated a project to federate the *nebula* of disciplines and scholars studying the people of their own country or region. These disciplines had emerged in the nineteenth century in many European countries—in the context of Romanticism and nationalism or regionalism—as the studies of national or regional specificity and identity, through the collection of oral traditions and material culture of fellow, mainly rural, countrymen. Some of the founders of these national disciplines, such as the Scot William Thoms in Great Britain (see note 3) or the Andalusian Antonio Machado y Alvarez, had been influenced by anthropologists such as E.B.Tylor and more generally by evolutionism. Others, such as the Swede Nils Lithberg and his successor Sigurd Erixon were inspired by (mostly) German anthropo-geography and the analysis of the geographical variation of different aspects of material culture (Stoklund 1990). Still others, such as the German *Volkskundler* Wilhelm Riehl[13] and the brothers Grimm, were influenced by both (linguistic) philology and national *'Statistik'*, i.e. national ethnography. After 1920, and often even before, these different forms of national studies were given names by their practitioners in which the root 'people' or 'popular' was of central concern: *Volkskunde* in the Germanspeaking countries, *folklivsforskning* and *folkeminnevidenskab* in Scandinavia,[14] *demologia* in Italy, *laographia* in Greece, *ludoznawstwo* in Poland, *narodopis* or *lidopis* in Czechoslovakia, *neprazj* in Hungary, *arts et traditions populaires* or *folklore* in France, *artes y costumbres populares* in Spain, *folklore, ethnology* or *Folk-Life* studies in Great Britain, *etnografija* in Russia, and so forth.

In 1928 the 'Commission internationale des Arts et Traditions populaires' (CIAP) was founded in Prague with the aim of providing a forum for international discussions between the practitioners of the above-mentioned national studies. In July 1935 a group of Scandinavian, German, Scottish and Dutch scholars, gathering in Edinburgh on the invitation of the Scottish Anthropological and Folklore Society, decided to found a new network to promote 'European Folklore and Ethnology' as an autonomous field within the anthropological discipline. This initiative was taken in protest at the absence of a special section for European studies during the International Congress for Anthropology held in London in 1934. This newly founded 'International Association for Folklore and Ethnology' published two volumes of a journal called *Folk* in 1937,

and merged one year later with the Paris-based 'Comité exécutif de Folklore international' (also founded in 1937) into the 'Association for European Ethnology and Folklore' during the (second) International Congress for Anthropological and Ethnological Sciences (ICAES) held in Copenhagen in August 1938. The journal of this unified association of specialists of Europe which became *Folk-Liv,* founded in 1937 by Sigurd Erixon, was, from 1938 onwards, significantly subtitled 'Journal for European Ethnology and Folklore/Zeitschrift für Europäische Volkskunde/Journal d'Ethnologie et Folklore Européen' (cf. *Folk* 1937, *Folk-Liv* 1938; see Bringéus 1983 and Gerholm in this volume, pp. 163–4). The choice of the name *ethnology* can be understood as a reference to the mid-nineteenth century's (German) sense of the word, as including the study of both material and spiritual elements of (popular) culture around the world. But by the time Erixon's group chose the name 'European ethnology', the disciplines they wanted to federate had, as already mentioned, very different frames of reference as well as different research methods in comparison to (overseas) anthropology in the same period.[15]

ANTHROPOLOGIES OF EUROPE, 1950–80

After the Second World War, the European anthropological landscape continued its process of transformation, through segmentation and affiliation, which had begun earlier. The geopolitical situation of the different countries in which it was practised seems to have maintained its influence on this development. In geographical terms, the centre of the international anthropological world had become *circum*-atlantic, gathering Great Britain, France, The Netherlands, Scandinavia and the United States of America in a network of academic exchange and discussions, using mainly English as language for communication. On the other hand, Soviet Russian *etnografija,* in some aspects continuing the tradition of German anthropogeography, influenced research in Eastern Europe by its idiographic methods and Marxist analysis. The influence of German scholars, especially *Volkskundler,* dwindled, due to their past affinities with National Socialism and considerable sectarianism. In 1955, German *Volkskundler* left the network formed by Erixon and others around the concept of European ethnology (Lutz 1982:43), whereas the few German-speaking *Völkerkundler* studying non-Western societies linked up with Anglo-American anthropology, which represented a dominant paradigm in the 1960s.

The post-war period, which witnessed the independence of many former European colonies in Africa and Asia and accordingly less facilities to do empirical fieldwork overseas, was characterized by intense theoretical discussions about the analysis of ethnographic data often collected earlier, but also by a growing interest in European societies on the part of anthropologists schooled in non-Western anthropology. Armed with the concepts and theories but also the necessity to do personal fieldwork as inherent within social anthropology as it had developed in the 1920s and 1930s, young European anthropologists started to do research in Europe in the 1950s, at the same time as some young American (cultural) anthropologists.

The importance accorded to both personal fieldwork and the *mise en œuvre* of concepts elaborated by overseas general anthropology inaugurated a new anthropological genre: (holistic) community studies. From the 1950s onwards, series of these studies were made by British or American anthropologists in Europe, such as Julian Pitt-Rivers or George Foster in Spain, Lawrence Wylie in France, or by students of Conrad M.Arensberg and Solon T.Kimball who had done pioneering research in Ireland in the 1930s. But some (young) European anthropologists also carried out monographic community studies within their own country often directed by (well-known) professors who had done their main research overseas: in Great Britain Kelvin Little directed the research of Erving Goffman and John Littlejohn in Scotland, Daryll Forde directed the research of Alwynn Rees in Wales, Max Gluckman directed the research of Ronald Frankenberg, also in Wales, Edmund Leach directed students studying local communities in England (Elmdon) and on the Shetland Isles, while Raymond Firth himself studied kinship relations in urban London.[16] In France pioneering research was done in a rural community by Lucien Bernot and René Blanchard in 1948 (Bernot and Blancard 1953), as well as by Louis Dumont in 1950 (Dumont 1951), while Marcel Maget published a first fieldwork manual significantly subtitled *Ethnographie métropolitaine* in 1953. Curiously, many of the British scholars who conducted fieldwork in Great Britain during the 1950s, and who were tutored by social anthropologists, continued their career overseas (such as Marilyn Strathern) or called themselves 'sociologists' afterwards (like Erving Goffman). In France, where specific training in anthropology started only in 1947 after the foundation of the 'Centre de Formation de la Recherche Ethnologique', directed by Roger Bastide and André Leroi-Gourhan, the differentiation between national and overseas anthropology was only seen as a geographic difference and not as a

difference of disciplines. The death of Van Gennep in 1957 and the often marginal position of folklorists, partly due to their relations with the Vichy regime during the war, probably facilitated the hatching of an *Ethnologie de la France* as a geographic subfield of general anthropology.

The post-war period was also a period of changing perspectives of the different subdisciplines of national or regional studies participating in the international network organized by Erixon and others. Especially Swedish researchers introduced concepts of American cultural anthropology into these disciplines, while British scholars such as Estyn Evans were also influenced by French human geography as developed by Jean Brunhes and others. This led to a rupture, in the mid-1950s, with the German *Volkskundler* (who left during an international congress held at Arnhem). But most of the efforts of this European Ethnology group remained concentrated on a terminological—and conceptual—harmonization of the different national academic traditions. The publication by the Swedish folklorist Åke Hultkrantz of an *International Dictionary of Regional Ethnology and Folklore* in 1960 is an example of the efforts to integrate anthropological concepts into the circle of national and regional studies. In 1964 the network of European scholars was formally organized in the Société internationale d'Ethnologie et de Folklore (SIEF) (recognized by UNESCO) and in 1967 some individual members of this group created the journal, *Ethnologia Europaea,* as an international forum (and in protest against the bureaucratic habits of the SIEF congresses). The heterogeneity—both theoretical and methodological—of the members of this network, partly due to national academic traditions, partly to the strong personality of some of its leading scholars such as Nils Bringéus or Branimir Bratanic, kept many of the members relatively isolated from the methods and theories of contemporary social and cultural anthropology until quite recently.[17] As the Hungarian scholar Tamás Hofer wrote in 1968, the study of European societies had in many countries become the subject of two 'professional personalities': (social) anthropologists 'at home' on the one hand, regionally specialized scholars[18] on the other (Hofer 1968).

In some European countries these different 'professional personalities' had a certain influence on the attractiveness of their subjects to the students, especially during the social and intellectual turmoil of the sixties: in Scandinavia, Great Britain or The Netherlands many students were charmed by the 'exoticism' and social criticism of the anthropological milieu, while in Germany the sometimes radical

critique of society by a new generation of *Volkskundler* such as Hermann Bausinger in Tübingen or Anna-Maria Greverus in Frankfurt helped to make the discipline very popular among the younger generations (Dow and Lixfeld 1986). These contextual aspects played a certain role in the 'national' orientations of anthropology in the various European countries which did not always contribute in favour of international communication. For example, a series of encounters during the 1980s of French *'ethnologues de la France'* (close to social anthropologists) and German practitioners of a renewed *Volkskunde* showed the differences of wave-length between the two groups of participants (Chiva and Jeggle 1987).[19]

CONCLUSION

It has been impossible in this chapter to review all approaches and disciplines converging into the rather abstract whole one could call the anthropology of European societies. At least three different types of anthropology, which often have common origins in the nineteenth century, but which have also known their own specific orientations especially after the First World War, can be distinguished:

- a (social) anthropological orientation, mainly French and British, based on personal fieldwork combined with the elaboration of theories and concepts to which the study of European society has often appeared as a marginal phenomenon or even as a 'secondbest choice', although increasingly popular since the 1970s;
- an American (cultural) anthropological orientation, in which Europe offers possibilities for case-studies on specific topics, often in a worldwide perspective;[20]
- the variety of disciplines more or less federated within the European Ethnology project founded by Sigurd Erixon.

The degree of interaction between these types of anthropological practice has known both qualitative and quantitative variations during the past sixty years in almost every European country. In some countries such as Sweden or especially in Switzerland there have been very interesting exchanges between some scholars—but not all—belonging to these three types of anthropology (see, e.g., Niederer 1980; Centlivres 1980), without a loss of professional identity. In other countries, where overseas anthropology was academically predominant, such as Great Britain or The Netherlands, the few anthropologists working in their home country have often lived in

mutual ignorance with the scholars of the third type of anthropology and vice versa. In countries where national and regional studies are well developed and anthropology is exclusively interested in non-Western societies, such as in Germany, until recently there has been very little exchange or mutual interest. These are only a few examples of the diversity one encounters when surveying the situation of the anthropology of Europe today (Schippers 1991).

If one takes into consideration the links between the European subcontinent as a whole and the history of a scientific discipline called anthropology its scholars gave birth to in the nineteenth century, it clearly appears as if this discipline is specifically European and by scientific layering North American. The fact that during the early stages of this discipline many of its scholars predominantly studied the still-existing particular forms of society which were threatened by Western industrial, political and cultural expansionism, has often *de facto* assimilated anthropology to the study of exclusively non-Western societies. But in many countries this non-scientific *coupure,* which was partly based on evolutionistic prejudices, has been abandoned, such as in the United States and in some European anthropological traditions such as in France and Scandinavia. If it is undeniable that most of the theoretical and conceptual progress in anthropology has emerged from the study of non-Western societies, there seems to be no scientific argument to consider the study of Western societies above or outside the range of this discipline, which belongs to the few social sciences that produce their own data through empirical ethnography. Considering the anthropological study of Western societies as a paradox seems to belong more to some outdated anthropological folklore than to solid scientific argumentation. If anthropology is not destined to become a purely historical discipline based on data collected overseas during the first half of the twentieth century, as Claude Lévi-Strauss has suggested in his earlier work, it is probably bound to reintegrate the different geographically specialized subdisciplines into a common field of discussion on theory as well as on methods. A certain familiarity with the history of the different anthropological traditions which have been developed in Europe and North America seems to be a first step in this direction.

NOTES

1 This study has sometimes been qualified as mainly *idiographic*.
2 I shall present the subtle, often confused taxonomies used in different national academic contexts, further on in this chapter.

3 It was founded in 1878 in London by the Scot William Thoms, who seems to have been the inventor of the concept ‘folklore’ (see Fenton 1985, I:45).
4 Marett was with William J.Perry and George Elliot-Smith one of the British hyper-diffusionists of this period.
5 Van Gennep’s well-known book *Les Rites de passage,* published in Paris in 1909, belongs to the earlier period.
6 In the nineteenth-century sense of this word, i.e., an ethnographic description of the national territory.
7 Until 1960 both disciplines identified themselves as not belonging to the social sciences.
8 These concepts had an important influence on American anthropology: Boas and Kroeber were highly familiar with German anthropological theories.
9 Which also influenced American culturalism (Benedict, Linton and others).
10 Richard Thurnwald (1869–1954) was called by Lowie the liaison officer of the social sciences’. Influenced by Franco-British social anthropology Thurnwald rejected German diffusionism.
11 The recently published book by Gansohr-Meinel (1993) clearly shows the development of the social and political context of this enterprise.
12 The first German anthropological journal founded in 1869 by Adolf Bastian was significantly called *Zeitschrift für Ethnologie*.
13 Riehl is often considered as the father of German *Volkskunde*.
14 *Folklivsforskning* very often studies the material aspects of popular culture, while *Folkeminnevidenskap* mainly collects oral traditions.
15 It is interesting to note that the academic development of anthropology in the United States and especially the schism between literary folklore and anthropological folklore also took place during the late 1930s. When W.W.Newell founded the American Folklore Society and its *Journal of American Folklore* in 1888, Boas, who established himself definitively in America after several expeditions among Canadian Eskimos in 1886, was among its first members. Although this society had been inspired by the British Folklore Society founded in 1878, it differed from European folklore studies by including the study of native Indians and Eskimos in its field of interest. Up to 1940 American anthropologists from the Boasian school played a dominant role in the Folklore Society and its journal, while its annual meeting often coincided with that of the American Anthropological Association (Lévy-Zumwalt 1988:39). The split between American anthropologists and folklorists was not based, as in Europe, on a difference between the study of Western and non-Western societies, but on a disagreement concerning sources and methods. For more details see Lévy-Zumwalt (1988).
16 For a detailed presentation of British social anthropologists who conducted research in Great Britain see Cohen (1982 and 1989).
17 Only in the beginning of the 1980s (social) anthropologists studying European societies started to publish in *Ethnologia Europaea* and to participate in SIEF congresses. A diachronic analysis of themes and authorship of *Ethnologia Europaea* is illuminating to observe this development.
18 Sometimes sarcastically referred to as ‘footnote matadors’ or as ‘museum moles’ by the more theoretically interested social anthropologists.
19 Similar situations can be observed during international meetings of the

SIEF or more recently of the European Association of Social Anthropologists (EASA).

20 The Society for the Anthropology of Europe, part of the American Anthropological Association, whose membership exceeded 700 in 1993 (see 2nd Directory) is one of the fastest growing units of the AAA.

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