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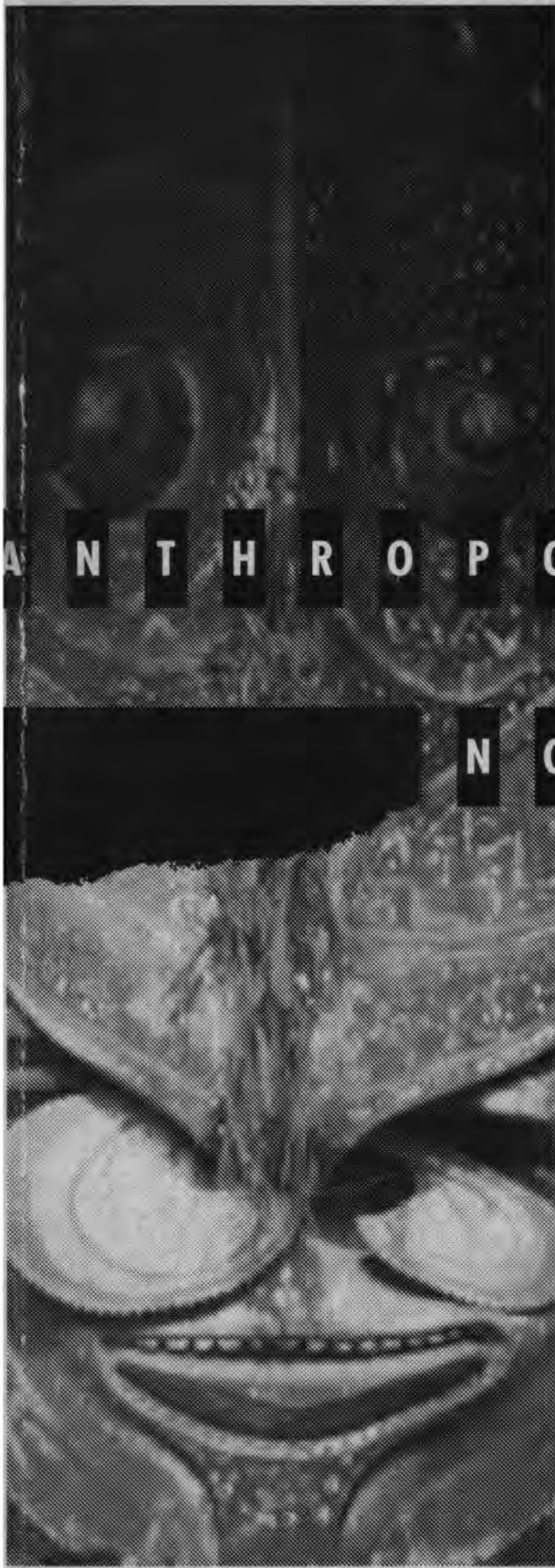

ANTHROPOLOGICAL NOTEBOOKS

year V, no. 1

ARCHAEOLOGY
Ivan Šprajc

SOCIAL AND CULTURAL ANTHROPOLOGY
Michael Young
linus s. digim'Rina
Tatiana Bajuk Senčar
Gabriele Weichart

SOCIOLOGY
Frane Adam, Darka Podmenik, Dijana Krajina

SELECTED BIBLIOGRAPHY
Borut Telban

LJUBLJANA 1999

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ANTHROPOLOGICAL NOTEBOOKS V/1

FIELDWORK AND QUALITATIVE RESEARCH IN ANTHROPOLOGY AND BEYOND

Edited by Borut Telban

DRUŠTVO ANTROPOLOGOV SLOVENIJE
SLOVENE ANTHROPOLOGICAL SOCIETY

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Front page: *wunduma*, the carved mask of a female spirit on the men's house in Ambonwari village, Papua New Guinea.
Photo: Borut Telban.

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Contents

Anthropological Notebooks V/1, 1999

Introduction

The idea of intensive, long-term fieldwork among a community of people remains in many ways the essence of anthropological endeavour seen as a distinctive craft within the human sciences. Qualitative research methods, of which participant observation became the genuine basis for everything else, were for an anthropologist always equally important as quantitative demographic, survey, textual, and other types of analysis. Anthropological fieldwork has its own specificity, including its history, epistemology, and socialisation practice.[1] *Since Malinowski, and until recently, the "real" fieldwork — long-term and intense — was perceived as being the one conducted among "exotic", far away people, where the anthropologist would undergo personal transformation and achieve "a profound, multidimensional knowledge not available to someone who visits a community for a few days or weeks".*[2] *His knowledge would be based more on lived experience — participant observation — than on information gained from the paid informants.*

The times, however, have changed. During the last hundred years rapid globalisation has influenced different localities and people, be these either Trobrianders from Papua New Guinea, the Australian Aborigines, or the Slovenes who gained independence from Yugoslavia less than ten years ago. Rapid globalisation — when things are growing larger, faster, and multifarious — has influenced not only different populations all over the world but has also changed people's views about fieldwork and the whole subject of anthropology. There is one peculiarity, however, which has been characteristic of human life in general, and to which anthropologists have always paid great attention: intimacy. Intimacy has always been the main characteristic of both socialisation practice and a deep and thorough ethnography.

In his "Fieldwork in the Era of Globalisation", a short discussion of articles published under the common title Fieldwork Revisited *in a special issue of* Anthropology and Humanism *(1997), Arjun Appadurai enlightens the problem of intimacy in a world of post-localities, in other words, "how intimacy is produced and reproduced under the conditions of globalization".*[3] *Anthropological fieldwork and ethnography have been criticised in every sense: some have said that anthropologists present their research subjects as being frozen in time, others yet, as being frozen in space. I think that Appadurai's summary of the many critical arguments regarding the web of intimate relations and experiences during fieldwork are worth quoting at length:*

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1 Jackson, Jean 1997. Fieldwork. In: Thomas Barfield (ed.), *The Dictionary of Anthropology*. Oxford: Blackwell Publishers, pp. 188-190.
2 *Ibid.*: 189
3 Appadurai, Arjun 1997. Discussion: Fieldwork in the Era of Globalization. *Anthropology and Humanism* 22(1): 115-118, pp. 115.

> *Much doubt has been cast on the politics of this intimacy in recent decades. Some have argued that it depended on the implicit brutalities of colonialism. Others have bemoaned the banishment of fieldwork to a sort of structural nontime. Yet others have exposed the partial nature of ethnographic intimacy, showing that it was often about men, or elites, or some other necessarily partial source of knowledge. In the heated debates about voice and representation, even these partialities and complicities were argued to be relatively innocent, compared to the crimes of representation alleged to have been committed by anthropologists, crimes against the subtleties of voice, genre, the sensorium, speech, and translation. The combination of Foucault and Said has been hard on the authority of ethnography.*[4]

Appadurai says that the best fieldworkers gained deep knowledge "because they entered the web of intimate relations in a world not previously known to them"; these intimate relations were necessary for the humanist anthropology.

> *For humanistic anthropology the central issues of interpretation have to do with sensibility, with particularity, with embodiment, with lived worlds and real lives. In this sense, all good ethnography is humanist in impulse, insofar as it operates at the intersection of the intimate and the everyday.*[5]

This issue of Anthropological Notebooks *with its focus on ethnography and research methodology, endeavours to uncover some of these intimacies which anthropologists experience while being in the field.*

The first article is by ***Ivan Šprajc****, a Senior Research Fellow from the Scientific Research Centre of the Slovene Academy of Sciences and Arts, an archaeologist who spent 12 years studying and researching in Mexico. The paper presents some specific methods and techniques employed in a systematic archaeoastronomical study of architectural orientations and other alignments at a number of Preclassic, Classic and Postclassic archaeological sites in central Mexico. While the results of this research are briefly presented in the Introduction, the article focuses on technical and methodological issues relevant to archaeoastronomical inquiries in general: the criteria for the selection of alignments considered in the study are discussed, as well as some technical questions concerning the collection and analysis of the alignment data.*

After introducing the research methods in archaeoastronomy, we move into the field of social and cultural anthropology. For the editor of a volume which is focused on anthropological fieldwork and ethnography it is a great pleasure to publish side by side an article on Bronislaw Malinowski — who after his research on the Trobriand Islands became the "father" of modern fieldwork — by Malinowski's authorised biographer ***Michael Young****, and an article by a Trobriander,* ***linus digim'Rina*** *who after his PhD at The Australian National University, and after being supervised by Michael Young, took the Chair at the Department of Anthropology and Sociology at the University of Papua New Guinea.*

4 *Ibid.*: 115-116.
5 *Ibid.*: 116.

In the article entitled "The making of an anthropologist" Michael Young remarks that it is much easier to answer the question of "how" Bronislaw Malinowski became an anthropologist than "why", and argues that the gradual settling into a career is a far more common experience than the sudden conversion. I am pleased that Young, whose biography of Malinowski should appear on the book shelves quite soon, had chosen Anthropological Notebooks *to publish for the first time a biographical note discovered in an immigration file in the South African state archives in Pretoria. Malinowski's "ethnographer's magic" was based on lengthy immersion in the field, fluency in the vernacular, and participant observation. Young restates his old argument that Malinowski was not "interned" in the Trobriand Islands at all, as some had claimed, but went and later returned to the Trobriands entirely by his own choice. As the main reason for becoming an anthropologist, Young rejects both Malinowski's familiarity with Frazer's* The Golden Bough *and the romantic allure of the unknown and the exotic as providing a sufficient explanation. He asserts that Malinowski was "neither an intrepid explorer nor a discoverer of untouched tribes". In the archive of Malinowski's papers at the London School of Economics, Young found a seven-page synopsis of the introduction to a textbook that Malinowski had been invited to write. The text was never published. One can notice from this manuscript, however, his earliest experiences of cultural diversity on the one hand and the omission of any mention of his father on the other. Young argues that both the early travels and the Oedipal relationship with his father — a professor of Slavonic philology — were those determinants which prompted the young Malinowski to transcend his father and make a new challenge out of anthropological fieldwork.*

After critically reflecting upon some of Malinowski's conclusions about a sound scientific study of people, digim'Rina argues that the field of informants among one's own people should be constituted of at least "four concentric circles": firstly friends, secondly relatives, thirdly knowledgeable persons, big men, and chiefs, and fourthly collaborating colleagues in the field. He goes on to discuss the difficulties of different kinds of social relationships (political, religious, gender, age, etc.), and his emotional life while he was involved in long-term ethnographic research in his home area, the Massim region of Papua New Guinea.

***Tatiana Bajuk Senčar**, who recently received her PhD from Rice University, Houston, Texas, presents a reflective and critical article about anthropological fieldwork. Emphasising the gap between theory and practice in fieldwork, she argues that the concept of "research imaginary" illustrates this gap which is maintained by the presumption that fieldwork is an unquestioned, emblematic anthropological research paradigm. She additionally emphasises the importance of remembering "the historical development of fieldwork as a methodology not in order to essentialize fieldwork's origins but to counteract the naturalisation of fieldwork as well as to understand the research imaginary to which it once belonged".*

***Gabriele Weichart** from the University of Heidelberg tells us her own personal story about her fieldwork beginnings and those reasons which were crucial for her decision to conduct fieldwork among the Australian Aborigines. Weichart — fascinated by the idea of "crossing borders" — decided to go to live in Central Australia after she received an offer to work as a volunteer for an Aboriginal organisation in Alice Springs. There she was supposed to look — from the perspective of a feminist — at the rapidly increasing market of Aboriginal art and artefact production. Early in her fieldwork she had to learn that anthropology and anthropologists already had some kind of bad 'fame' among both the*

Aboriginal and non-Aboriginal people, and were suspiciously looked upon. Therefore, she needed to establish her own position as a serious fieldworker, sometimes struggling against mistrust, other times unwillingly changing sides.

Sociologists ***Frane Adam, Darka Podmenik*** *and* ***Dijana Krajina*** *(Adam and Krajina are from the Faculty of Social Sciences, University of Ljubljana, while Podmenik is an independent researcher) summarise the history of qualitative methodology used in sociological and social-psychological research in Slovenia over the last twenty-five years. These methods have nonetheless remained marginal mainly, the authors argue, because of the unsystematic use of these methods and lack of epistemological (self)reflection.*

The last article in this issue by ***Borut Telban*** *is intended to provide a selected bibliography on fieldwork, research methods and ethnography. It does not encompass everything published in this broad field. It could, however, be seen as a guidance for all those who are just beginning to set foot in social and cultural anthropology, as well as a useful aid to those who are eager to penetrate deeper into the field of research methods and the tradition of anthropological fieldwork.*

Editor-In-Chief
Borut Telban

STUDY OF ASTRONOMICAL ALIGNMENTS IN ARCHAEOLOGICAL SITES OF CENTRAL MEXICO: SOME METHODOLOGICAL CONSIDERATIONS

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INTRODUCTION

The alignment studies are the most typical aspect of *archaeoastronomy*, a relatively new anthropological discipline whose endeavors are focused upon those segments of the archaeologically documented societies that have some relationship to the observation of the sky. The object of archaeoastronomical research is not only exact knowledge about celestial phenomena but rather all astronomically-derived concepts and related cultural manifestations. Taking into account concrete environmental peculiarities and geographical location, as well as subsistence strategies, sociopolitical structure and historical antecedents of the society studied, archaeoastronomy searches for responses to a number of questions: What were the social functions of astronomical knowledge? Why did certain astronomical phenomena acquire a prevailing importance? Which were the observational bases of the concepts embedded in myths, iconography, attributes of gods, etc.? What is the nature of the interrelationship between astronomical concepts, natural environment and cultural context? In its attempts to solve such problems, archaeoastronomy participates in the common efforts of anthropological disciplines and contributes to a more comprehensive understanding of ancient societies, as well as of the general processes of cultural evolution.[1]

Important information on past astronomical practices and concepts may be provided by architectural orientations and other alignments recognizable in the spatial distribution of certain archaeological features. It is thus understandable that most archaeoastronomical alignment studies have been so far accomplished in the areas where the remains of this kind

1 Archaeoastronomy thus differs from the *history of astronomy*, which is based primarily on written sources and focused on the development of exact astronomical knowledge, without paying much attention to the natural and cultural circumstances that conditioned particular developments, and to non-scientific concepts. The latter, however, are no less interesting for archaeoastronomy, considering its holistic approach and anthropologically relevant goals: both correct and false ideas are, in a particular social group, normally intertwined, composing a relatively coherent world view, which can be properly understood only if examined as a whole and in the light of the natural, social and historical context. Both "scientific", or exact, and "non-scientific" concepts may thus shed light on a number of aspects of the society being studied (*cf.* Ruggles 1999: 80f, 155). In fact, any attempt to distinguish the two classes of ideas is, to a certain extent, arbitrary and depends on the knowledge and/or beliefs of whoever tries to make such a distinction. It would be illusory to think that our modern scientific criteria are entirely objective: describing the scientific world view, astronomer and historian of science Owen Gingerich (1989: 38f) says: "It is an interlocked and coherent picture, a most workable explanation, but it is not ultimate truth."
General information on the history and theoretical and methodological bases of archaeoastronomy and some related fields of research can be found, for example, in Baity (1973), Aveni (1981; 1989; 1991), Šprajc (1991), Iwaniszewski (1994a; 1995a; 1995b), Ruggles (1999) and several articles in Macey (1994).

are particularly abundant. One of such regions is Mesoamerica, a culturally defined geographical area corresponding to the central and southern parts of modern Mexico and the northern part of Central America, where civilizations with a number of common cultural traits flourished since the 2nd millenium B.C., when the first complex, state-organized societies emerged, until the Spanish conquest in the early 16th century A.D.

Systematic archaeoastronomical research carried out during the last few decades has revealed that Mesoamerican architectural orientations exhibit a clearly non-random distribution and that civic and ceremonial buildings were oriented largely on the basis of astronomical considerations, particularly to the Sun's positions on the horizon on certain dates of the tropical year (Aveni 1975; 1991; Aveni and Gibbs 1976; Aveni and Hartung 1986; Tichy 1991; Šprajc 1997a). According to various hypotheses forwarded thus far, the dates recorded by the orientations can be interpreted in terms of their relevance in the agricultural cycle and in computations related to the calendrical system. It has been suggested, for example, that the dates indicated by the alignments are separated by calendrically significant intervals. The most elaborate model of this type has been proposed by Tichy (1981; 1991), who contends that these dates mark intervals of 13 and 20 days and multiples thereof. Some authors have reconstructed possible horizon calendars for particular sites, on the assumption that prominent peaks of the local horizon served as natural markers of sunrises and sunsets on relevant dates (*e.g.*, Ponce de León 1982; Aveni *et al.* 1988; Tichy 1991; Broda 1993; Morante 1993; 1996; Galindo 1994; Iwaniszewski 1994b).

Since both the accumulated fieldwork experiences and the feedback information generated by interpretational attempts revealed that the available alignment data were neither sufficient nor accurate enough for testing such specific hypotheses, I undertook precise measurements of alignments at 37 Preclassic, Classic and Postclassic archaeological sites in central Mexico,[2] taking into account a variety of facts and circumstances whose relevance had not been recognized before. Not only the orientations of civic-ceremonial structures but also the alignments to prominent peaks on the local horizon, placed within the angle of annual movement of the Sun, were measured. The results of my research agree with some general ideas formerly expressed by other authors, but differ in important details which concern the principles underlying orientational patterns and the observational use of alignments. The general conclusions based on my analyses of the alignment data from central Mexican archaeological sites can be summarized as follows:

(1) The dates of sunrises and sunsets both along architectural orientations and above prominent hills on the local horizon exhibit consistent patterns; at any particular site they are separated by intervals that are predominantly multiples of 13 and 20 days and are, therefore, significant in terms of the Mesoamerican calendrical system.[3]

(2) Since the horizon prominences were measured from the main temple of every site, the patterns of dates and intervals based on these alignment data indicate that the important ceremonial structures were not only oriented towards but also located on astronomical grounds: the places selected for their construction allowed certain surrounding peaks to be

2 The sites included in the study date to the period from about 500 B.C. to A.D. 1519.

3 One of the important Mesoamerican calendrical cycles was the so-called sacred count of 260 days: since any date of this cycle was a combination of a number from 1 to 13 and a sign in the series of 20, the dates at intervals of 13/20 days had the same numeral/sign. The importance of intervalic time reckoning based on multiples of 13 and 20 days is attested both in the central Mexican (Siarkiewicz 1995) and in the Maya codices (Aveni *et al.* 1995; 1996).

employed as natural markers of sunrises and sunsets on culturally significant dates. Furthermore, various structures have been found to be oriented towards prominent peaks on the local horizon.

(3) The relevance of the most recurrent dates, recorded at a number of sites, can be interpreted in terms of their approximate coincidence with important seasonal changes in the natural environment and the corresponding stages of the agricultural cycle. However, the fact that certain series of *exactly* the same dates, separated by multiples of 13 and 20 days, are marked by alignments at a number of sites, even in ecologically different zones, suggests the existence of a *ritual* or *canonical* agricultural cycle: the dates involved must have been canonized precisely because the intervals separating them were easy to handle by means of the sacred 260-day calendar count.[4]

(4) Both the orientations embodied in the monumental architecture of a particular site and the prominent local horizon features allowed the use of an *observational calendar* which, in view of the lack of permanent concordance of the calendrical and tropical years,[5] was necessary for predicting important seasonal changes and for efficient scheduling of the corresponding agricultural activities.

While the results of my study in central Mexico, including the interpretations of the alignment data for particular sites and the supporting evidence, are exhaustively presented in my Ph.D. dissertation (Šprajc 1997b), the purpose of this paper is to focus on some specific methods and techniques that I developed and applied in this research and which, I believe, may be useful in further archaeoastronomical inquiries, both in Mesoamerica and elsewhere.

SELECTION OF ALIGNMENTS

An objective data selection is of foremost importance in the alignment studies: the results of an analysis of a number of alignments will be valid and meaningful only if the sightlines considered have been selected fairly in the first place (Ruggles 1999: 51).

Architectural orientations

The purpose of my research in central Mexico was to explore the orientational rules that reflect astronomical concepts and related aspects of world view and religion. Assuming that such principles were involved particularly in the construction of ceremonial and important civic buildings, only the latter's orientations were included in the study. It is very likely that not only temples but also high rank residences and administrative buildings were oriented in accordance with astronomical principles, because in this way they reproduced and underscored the existing earthly and celestial order, of which the protagonists of the ruling class, normally considered as man-gods, claimed to be responsible (*cf.* Broda 1982: 104ff; 1991: 491; López Austin 1973).[6] The structures whose function cannot be undoubtedly linked to

4 *V. supra*: note 3.
5 The Mesoamerican calendrical year had invariably 365 days and thus did not preserve a fixed correlation with the tropical year of 365.2422 days (Šprajc 1998).
6 A convenient example is the Palace of the Governor at Uxmal; this residence of a ruler called Chac was arguably oriented to the

ritual practices and mechanisms of power were not considered in my analyses, because they were probably oriented at random or on essentially different grounds, related to environmental characteristics (topographic and geomorphological features, climatic peculiarities), military considerations or other, more practical motives.[7]

It can be affirmed that the orientations in the Mesoamerican civic and ceremonial architecture were astronomically functional, as a rule, in the east-west direction, referring to the Sun's positions on the horizon on certain dates of the tropical year, because most of the known east-west orientation azimuths[8] fall within the angle of annual movement of the Sun along the horizon (*cf.* Aveni and Hartung 1986: 59-60; Tichy 1991: 117; Šprajc 1997a; 1997b: 9f). Though it is quite likely that *some* structures were oriented to stars or planets (*cf.* Aveni 1991; Šprajc 1993a; 1993b; 1996a; Galindo 1994), the practice could not have been very common: by postulating that stars were primary orientational references, we would be forced to accept that only those rising or setting at azimuths within the angle of annual movement of the Sun, or in perpendicular northerly and southerly directions, were of interest. In view of these facts I have not explored the eventual astronomical potential of the north-south orientation azimuths. Neither have I considered the structures facing north and south, because it is difficult to imagine they facilitated observations of astronomical phenomena on the eastern or western horizon. It is also unlikely, however, that such buildings recorded astronomical events on the northern and southern horizon: since their orientations normally conform to those of the adjacent buildings facing east or west, it was probably the latter (related to the Sun) that dictated the orientations of entire architectural complexes (Šprajc 1997b: 9f, 13ff).

At any site with relatively well preserved architecture we can often find a number of sightlines with possible astronomical significance, connecting diverse architectural elements or even separate buildings and running at different angles with respect to the horizontal (*cf.* Hartung 1975). However, according to the evidence available so far (see above: Introduction), the astronomical basis seems to be indisputable only for the orientations of the main axes of buildings, which can be associated with the phenomena observable on the horizon; therefore, and with the purpose of having a homogenous data sample (*cf.* Hawkins 1968: 49; Ruggles 1999: 51ff), I only considered the alignments indicated by walls, wall faces and other construction elements that make patent the orientation of a structure in the horizontal plane.

Local horizon features

At every site the alignments to prominent horizon features, situated within the angle of the annual movement of the Sun, were also measured, with the purpose of testing the hypothe-

maximum northerly extreme of Venus on the western horizon (Šprajc 1993a: 47; 1993b: 272f; 1996a: 75ff; 1996b: 173ff; for different views on the significance of this orientation see Bricker and Bricker 1996, with comments).

7 The selection of structures considered in my study can be justified also by the fact that the hypotheses I intended to verify were all based on orientations of civic and ceremonial buildings. According to the available data (which are admittedly meager), domestic and other structures with secular functions do not seem to have been oriented on astronomical grounds. However, where a single orientation dominates the whole urban layout, as is evident at a few sites, it must have been dictated by the (astronomically functional) orientation of the main temple; this assumption is supported by comparative data from other cultures (*cf.* Wheatley 1971) and also, in the case of Teotihuacan, by internal evidence (Šprajc 1997b: 157ff).

8 The *azimuth* is the angle in the horizontal plane measured clockwise from the north.

ses about the use of horizon calendars. Though the possibility that prominences beyond this angle also served as markers of certain astronomical phenomena cannot be discarded, the distribution of architectural orientations suggests that the alignments related to the Sun were particularly important.

The readings of the horizon features were taken from the main buildings of every site, mostly temples, assuming they were the most important observing points: if astronomical function is attributed to the orientations of civic and ceremonial structures, it seems logical to suppose that other phenomena on the horizon were also observed from the same buildings.

Although not only peaks but also notches, cuts and similar features (even little prominent) of the local horizon may have served as astronomical markers (*cf.* McCluskey 1990; Zeilik 1985; 1991; Morante 1996: 82), I only took into account prominent and clearly defined hilltops lying on the horizon line. This selection is based on the data about architectural orientations: while a number of buildings are oriented to mountain peaks, none has been found to align with a landmark of any other type. Considering the astronomical basis of architectural orientations, the prominences on the eastern and western horizon to which the buildings are oriented served as exact markers of the phenomena recorded by orientations and, thus, facilitated observations; since the horizon features are in these cases exclusively mountain tops, it can be assumed that other astronomical horizon markers were also of the same kind.

The selection of the mountain peaks considered to be "prominent" was necessarily, to a certain extent, subjective, as Ruggles and Martlew (1992: S4) also admit in a similar case. However, if all conceivably usable prominences had been taken into account, their large number would have introduced too much "noise", making impossible any objective evaluation of their eventual astronomical potential (*cf.* Ruggles 1999: 232, note 83).[9] Though my selection was not biased by pre-existent astronomical hypotheses, the results I obtained do seem meaningful. Similarly, Ruggles and Martlew (1992; 1993), in their study of prehistoric sites on the Isle of Mull in Scotland, identified prominent summits on the local horizon of each site and, plotting their declinations,[10] obtained astronomically significant groups (related to characteristic lunar positions).

MEASUREMENT OF ALIGNMENTS

I employed the alignment data based on my own measurements in field and calculations, because the published information was, for various reasons discussed below, neither sufficient nor reliable enough for the purposes of my research.

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9 The situation would have been similar to the one described by Hawkins (1968: 49) in relation with alignments at megalithic sites of western Europe: "as the number of markers increases, the problem rapidly degenerates to the insoluble level".

10 Any object in space (not only celestial bodies but also, for instance, points on the horizon) can be considered as located (or projected) on an imaginary celestial sphere. The *declination* of a point on this sphere is its angular distance from the celestial equator, which can be imagined as a projection of the Earth's equator on the celestial sphere. Declinations are measured perpendicularly to the celestial equator to the north and south (positive and negative declinations). By determining the declination of a horizon point, having its azimuth and altitude measured from a particular spot, one can find out which heavenly bodies rise or set behind it (or did so in the past) and (in the case of the Sun, Moon and the planets) on which dates, because the declinations of celestial objects for particular epochs and dates are known and can be found in astronomical sources (ephemerides, star atlases etc.) (*cf.* Aveni 1981; 1991; Ruggles 1999).

As Reyman (1975: 210) pointed out, the available archaeological site maps are "notoriously inaccurate". The true north is frequently laid out erroneously or confused with the magnetic north, and in many cases it is not even clear which of the two is indicated (Aveni 1975: 164; 1991: 250). It is noteworthy, Hartung (1980: 165) observes, that the first scientific explorers of Maya ruins showed a concern for measuring exact orientations. In 1913, Alfred Tozzer included a chapter on orientation in his study of Nakum and mentioned that this important topic should not be ignored by future researchers working in Central America (*ibid.*: 167, note 11). Indeed, various orientation studies were accomplished in the twenties and thirties of this century, particularly notable being those by Blom, Ricketson and Ruppert on Group E of Uaxactún and the Caracol of Chichén Itzá (*ibid.*: 165; Aveni 1991: 292ff, 314ff). The site maps elaborated in this period are regularly oriented to the astronomical north, indicating the angle of magnetic declination. In the following decades, however, the interest in architectural orientations declined (Hartung 1980: 165); even if the topic regained popularity with the appearance of archaeoastronomy in the sixties, the achievements within the newly consolidated anthropological discipline have not had adequate repercussions in the main-stream archaeological literature. In spite of the evident importance and intentionality of orientations in the civic and ceremonial architecture, the site plans marking true north are still extremely rare.

According to Reyman (1975: 210), even when site maps are very accurate (*e.g.*, Millon *et al.* 1973), they are still unsuitable for archaeoastronomical purposes, because they lack critical data such as the heights of the horizon along the orientation axes. It should be noted, however, that horizon altitudes, indispensable for calculating astronomical declinations and, therefore, for identifying with precision the celestial phenomena the alignments may have referred to, can often be determined on the basis of topographic maps. Accurate site plans can thus be of considerable help, suggesting possible astronomical references of orientations, but there is another problem for which field measurements seem to be inevitable.

Prehispanic buildings in Mesoamerica normally exhibit an orientation (either deliberate or fortuitous) that can be determined, because their ground plans are in most cases roughly rectangular or composed of rectangular elements; in other words, the directions in which the axes of a structure are laid out can be established. Even circular structures or those with a combined ground plan (composed of rectangular and circular elements, *e.g.*, the temples of the wind god Ehécatl) generally possess an orientation, indicated by the stairway of access and other architectural elements. However, the problem consists in the *determination of the exact orientation* of a structure. In the available archaeoastronomical bibliography concerned with Mesoamerican architectural orientations, the azimuth of a line measured at a building is commonly given as representing the orientation of the whole structure. Since the ground plans of most buildings incorporate lines that are roughly parallel and perpendicular to each other, these data have been highly revealing as to the determination of *approximate* orientations, and sufficiently exact to allow the discovery of azimuth distribution patterns and *orientation groups* (*cf.* Macgowan 1945; Aveni 1975; 1991; Aveni and Gibbs 1976; Aveni and Hartung 1986; Tichy 1981; 1982; 1991). However, the azimuths so determined cannot be considered as sufficiently precise for more detailed archaeoastronomical studies, because they do not represent the original and intended architectural orientations with the accuracy required for testing diverse hypotheses that have been forwarded on the basis of these data.

The walls of a structure may appear parallel and perpendicular to each other, but precise measurements reveal that this is frequently not the case (*cf.* Hartung 1980: 155; Ponce de León 1982: 9). Such irregularities are relevant, obviously, only if elements manifesting them are evidently original and *in situ.* Ideally, all reliable lines incorporated in a structure should be measured: if the azimuths of roughly parallel lines do not exhibit systematic variations that can be associated with particular construction phases or architectural elements, such divergences can be attributed to sloppy construction or to the fact that high precision was not aimed at by the builders; the mean value of the measured azimuths is likely to represent the originally intended orientation with reasonable accuracy, since the errors in the orientation of individual lines can be expected to cancel out. In several cases I noted that the walls and wall faces near the upper part of the building tended to be more parallel to each other than in lower sections, which seems understandable: if the orientation of a temple was intentional, it must have been laid out with particular attention in the area of *sancta sanctorum, i.e.* in the upper parts of the building. I considered such consistent alignments, if found, as particularly relevant for the determination of a structure's intended orientation.

On the other hand, the lines appearing to be perpendicular to each other often do not intersect at right angles. Ground plans of some buildings are patently rhomboidal (*e.g.*, of the Acropolis and the Pyramid of the Stelae at Xochicalco, or of Structure I at Teopanzolco: Šprajc 1997b: 202ff, 268ff). It is obvious that the orientation of such a structure cannot be described with a single azimuth. I do not believe that north-south lines of a building can be considered as indicative of its orientation in the east-west direction, and vice versa. If, for example, the base of a stairway in the north-south direction is measured, the perpendicular to this line should not be considered as corresponding to the structure's east-west axis, because the latter could be laid out rather by columns, pillars, wall faces or other construction elements that marked the desired astronomical direction with much greater precision than the imaginary perpendicular to the stairway. It thus seems much more natural to relate astronomical events on the eastern or western horizon to architectural east-west lines than to non-existent perpendiculars, whose relationship with these phenomena is not directly manifest or easily observable. The argument is additionally supported by the fact that the mountains to which many buildings are oriented are located along the physically existent architectural lines — as one can verify visually — and not along the imaginary perpendiculars.

As already noted (Reyman 1975: 207; Hartung 1980: 145; Aveni and Hartung 1986: 7, 12), not all of the lines actually incorporated in a building are equally reliable. A number of archaeological structures have been altered during recent excavation, restoration or reconstruction works. In these cases, it is necessary to examine the corresponding reports, in order to determine which of the actually manifest lines are original and *in situ* (Hartung 1980: 155); pertinent indications in the field should also be sought (*e.g.*, remains of original stucco or certain characteristics of the construction system). If there is no evidence to this effect, it is recommendable to measure every possible alignment: by averaging various readings, the azimuthal errors originated by recent alterations are likely to be canceled out. However, structures suspected to have undergone drastic modifications should be excluded from considerations. When measurable lines (walls, wall faces etc.) have been considerably altered by deterioration processes, readings should be taken along the lines based on the most reliable elements, *e.g.* corners. Measurements of slanted faces (*taludes*) require particular caution: readings must be taken horizontally, because the azimuths of the lines sighted along the *taludes* at different angles obviously do not reproduce the orientation of the building in the horizontal plane.

Recalling that the Sun disk has a diameter of merely 32 arc minutes, it is clear that measurements must be carried out quite precisely; even if the accuracy which the alignments were intended to have is commonly unknown, reliable evaluations of various hypotheses are possible only if the precision of our data equals or, better, exceeds the one achievable by the builders (*cf.* Aveni and Hartung 1986: 7; Ruggles 1999: 165). In order to determine sufficiently exact azimuths of alignments, it is indispensable to take readings with a theodolite or surveyor's transit, using an astronomical reference, normally the Sun. The techniques that can be employed have been described, for example, by Thom (1971: 119f), Aveni (1981; 1991: 148ff), Šprajc (1991: 45ff) and Ruggles (1999: 164ff), as well as in textbooks on topography and geodesy, and therefore need not be repeated here.

A compass can also be used, but only as an auxiliary instrument and with extreme caution. Directions in the horizontal plane are normally expressed in azimuths, which are angles from 0° to 360° measured from north to the right or, viewed from above, in the clockwise direction. Observing at whatever spot on the Earth, the direction to the astronomical (true) north/south is determined by the vertical plane that contains geographic poles, *i.e.* two points where the Earth's axis of rotation intersects the surface of the globe. Since the apparent rotatory motion of the celestial sphere is centered on the rotation axis of the Earth, it is obvious that the directions in which celestial bodies rise and set depend on the position of this axis in the space. However, the direction to which the magnetic needle points is determined by the position of the Earth's magnetic field, whose poles do not coincide with the geographic poles. Since magnetic azimuths, therefore, differ from astronomical ones, they have no relation to the apparent motion of celestial bodies and, consequently, to their rising and setting points that may have been aimed at by orientations. The angle between the directions to the astronomical and the magnetic north, termed the *magnetic declination*, depends on the location of magnetic poles and thus on the place of observation. Considering that magnetic poles move continuously and unpredictably, any magnetic declination varies irregularly, as a function of both place and time; furthermore, seasonal and daily fluctuations and local anomalies are not uncommon and may result in considerable variations in short time-spans and small areas (*cf.* Aveni 1975: 164; 1991: 62f (note 1), 139f; Ruggles 1999: 165).

In view of these facts, the magnetic compass can be used in archaeoastronomical work only if the local magnetic declination is determined for each site where the measurements are carried out. In order to determine this declination, an observation point must be selected from where both magnetic and astronomical azimuths of several well defined points (*e.g.*, prominent and distant peaks of the local horizon) can be measured. The points located at a short distance are not suitable for such purposes, because even small movements by the observer result in variations of the measured azimuth; walls are even less appropriate, because the sighted point usually is not clearly defined.

Before choosing the spot for these measurements, it is advisable to measure the magnetic azimuth of one and the same reference point on the horizon from various points several metres apart. If the sighted point is sufficiently far away (a few kilometers), its azimuth measured at any one of the observation points should be practically the same; if this is not the case, local magnetic anomalies exist and the use of the compass should be avoided, because it will not render reliable results. The phenomenon is very common in the vicinity of iron elements, though it can also be due to natural properties of the soil or the material employed in ancient constructions.

By taking about ten astronomical and magnetic readings, sufficient pairs of azimuths are obtained for determining the local magnetic declination, if the compass employed allows for distinguishing azimuthal differences of about 1/4°; in my own measurements, a prismatic compass was employed, which is particularly suitable for archaeoastronomical work, both for its size and precision (*cf.* Ruggles 1999: 165). For every alignment measured we determine the astronomical azimuth and its difference with respect to the magnetic azimuth; the mean value of these differences is then calculated. The more readings we have, the more exact the magnetic declination established will be, since the inevitable errors of individual magnetic readings will tend to cancel out.

In sum, only if the local magnetic declination has been determined with sufficient accuracy, can our compass readings be used confidently for determining astronomical azimuths. Measuring architectural alignments, magnetic readings of a single line are recommended to be taken in both directions because, by changing the observation point, possible local anomalies can be detected (Ruggles 1999: 165); if they are absent, the azimuths of one and the same line measured in opposite directions will differ exactly by 180°.[11]

A hand-held compass, if used scrupulously, can speed up field measurements considerably. It is particularly useful if a building preserves a large number of reliable walls that can be measured, since in the mean value based on various azimuths the errors of individual readings will tend to cancel out.

However, the most important and reliable alignments, particularly those that can be determined with high accuracy (*e.g.*, long and straight walls or horizon prominences) should always be measured with a theodolite and astronomical reference. The theodolite is necessary also for measuring altitudes of relevant points of the horizon, albeit a pocket-sized clinometer can also be used for these purposes (Ruggles 1999: 165). Since the orientation of a structure is normally determined by calculating the mean azimuth of readings along various lines, the exact point of the horizon to which the orientation corresponds is often not evident in the moment of measurements. It is thus recommendable to measure azimuths and altitudes of various points along the section of the horizon within which, according to visual estimation, the orientation azimuth of the structure will be located. It is always advisable to sketch relevant sections of the horizon and include the measurement data in these drawings. If the horizon is not visible at present (because of vegetation, modern constructions etc.), the necessary data can be obtained by calculations (*v. infra*).

CALCULATION PROCEDURES

The formulae for calculating azimuths of alignments measured with a theodolite and astronomical fix, and for converting readings of azimuth and altitude into declinations, are given in books on topography and geodesy (*e.g.*, Mueller 1969: 401ff), as well as in specialized archaeoastronomical publications (*e.g.*, Hawkins 1968: 50ff; Thom 1971: 120ff; Aveni 1981; 1991: 140ff; Šprajc 1991: 45ff; Ruggles 1999), and thus will not be repeated here.

11 It may be pointed out that, instead of azimuths, some compasses mark *bearings*, *i.e.* angles from 0° to 90° reckoned from magnetic north and magnetic south towards east and west (*cf.* Somerville 1927: 31, note 1). The values expressed in either system can be easily converted. For example, the bearing of N15°E equals the azimuth of 15°; the bearing of N15°W corresponds to the angle of 15° west of north and, therefore, to the azimuth of 345°, whereas the azimuth of 172° can be expressed as the bearing of S8°E.

The geographic coordinates of each site necessary for these calculations can be taken from sufficiently accurate maps. In my case topographic maps of the Mexican *Instituto Nacional de Estadística, Geografía e Informática* (INEGI; scale 1:50,000) were employed. The values of declination of the Sun and equation of time, necessary for azimuth calculations, were determined for the moment of measurement by interpolation of the values tabulated in ephemerides. Horizon altitudes used when calculating declinations of alignments were corrected for atmospheric refraction factors given by Hawkins (1968: 52, Table 1), Thom (1971: 28ff, Table 3.1) and Aveni (1991: 148). The values tabulated in the quoted works are approximately valid for sea level, the atmospheric pressure of 1002 millibars and the temperature of 10°C. The refraction factors were corrected for altitudes above sea level (taken from topographic maps), employing the formula (7) of Hawkins (1968: 53), whereas corrections for different atmospheric pressures and temperatures (*ibid.*: formula (6)), which are unpredictable variables, were not applied.[12]

At every site I tried to measure all relevant points of the horizon, *i.e.* the horizon altitudes corresponding to architectural orientations, and the azimuths and altitudes of prominent mountains located within the angle of annual movement of the Sun. Occasionally, however, such measurements were not possible, because the view to the horizon is nowadays blocked by modern constructions or trees in the immediate neighborhood, or because there was haze or smog on the days of measurements. The missing data were calculated on the basis of topographic maps: locating the site (observation point) and the point of interest of the horizon on the map, geographic coordinates (longitude λ and latitude φ) and altitudes above sea level of both points were determined. For calculating the azimuth of the visual line from the site, or point 1, to the point on the horizon, or point 2, the following formulae were employed, derived from the relations valid in the spherical triangle (*cf.* Woolard and Clemence 1966: 53ff; Mueller 1969: 37ff):

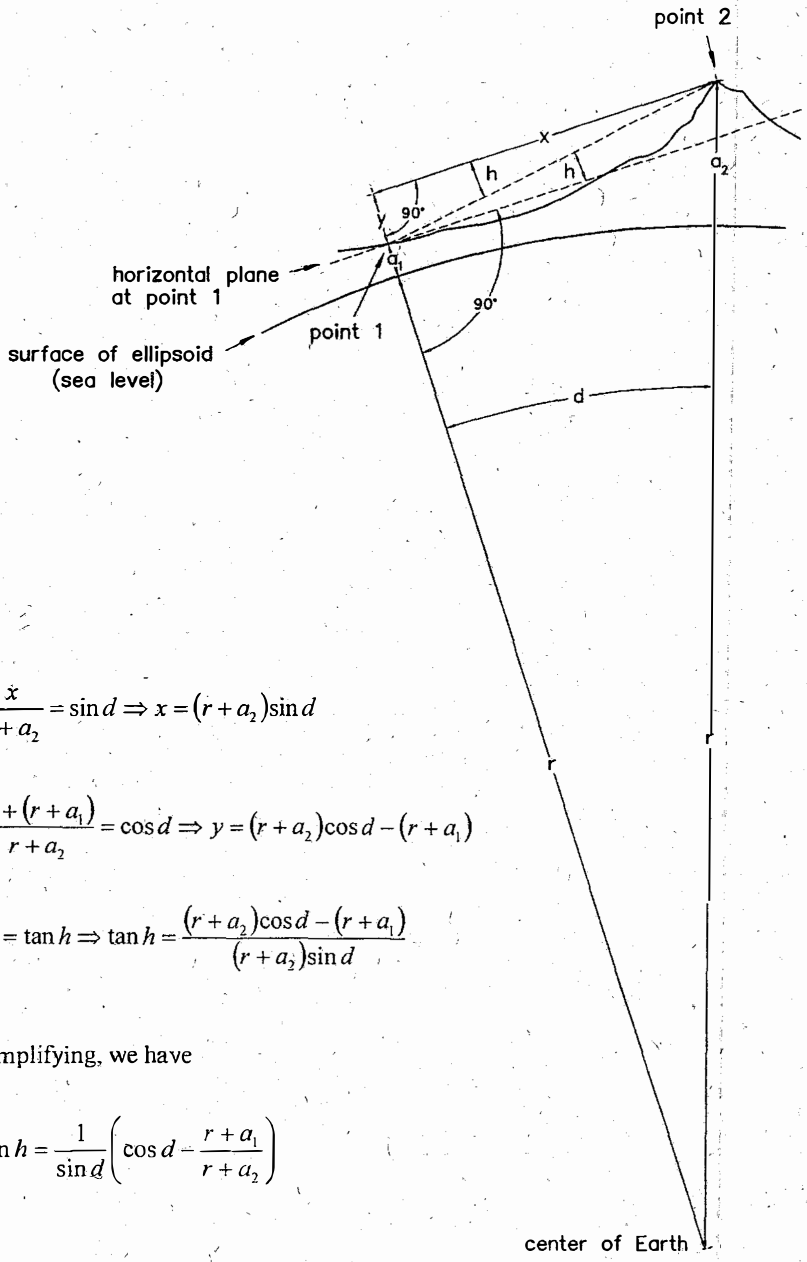
$$\cos d = \sin\varphi_1 \sin\varphi_2 + \cos\varphi_1 \cos\varphi_2 \cos(\lambda_1 - \lambda_2) \qquad (1)$$

$$\cos A' = \frac{\sin\varphi_2 - \sin\varphi_1 \cos d}{\cos\varphi_1 \sin d}$$

$$\lambda_1 - \lambda_2 > 0 \Rightarrow A = A'$$
$$\lambda_1 - \lambda_2 < 0 \Rightarrow A = 360° - A'$$

In these formulae λ_1 and φ_1 are the coordinates of point 1, λ_2 and φ_2 are those of point 2, d is the *angular* distance between the two points, and A is the azimuth of alignment, observing at point 1. The formulae are valid for any place on the Earth, if north/south latitudes and longitudes west/east of Greenwich are given positive/negative algebraic signs.

12 For details about refraction near the horizon and the problems relevant to archaeoastronomy, see Schaefer and Liller 1990.



$$\frac{x}{r+a_2}=\sin d \Rightarrow x=(r+a_2)\sin d$$

$$\frac{y+(r+a_1)}{r+a_2}=\cos d \Rightarrow y=(r+a_2)\cos d-(r+a_1)$$

$$\frac{y}{x}=\tan h \Rightarrow \tan h=\frac{(r+a_2)\cos d-(r+a_1)}{(r+a_2)\sin d}$$

Simplifying, we have

$$\tan h=\frac{1}{\sin d}\left(\cos d-\frac{r+a_1}{r+a_2}\right)$$

Figure 1. *Derivation of the formula for calculating the altitude above the horizontal plane of point 2 observed from point 1.*

As a following step, the angle of altitude of point 2 above the horizontal plane, observing at point 1, was calculated. Taking into account the curvature of the Earth's surface, I derived the expression

$$\tan h = \frac{1}{\sin d} \left(\cos d - \frac{r + a_1}{r + a_2} \right) \qquad \textbf{(2)}$$

where h is the altitude of point 2 above the horizontal plane, a_1 and a_2 are the altitudes above sea level of points 1 and 2, respectively, d is the angular distance between both points, and r is the mean radius of the Earth (see derivation of the formula in Figure 1). The approximate value of 6,370,000 m was taken for r in all calculations. Since the shape of the Earth is not spherical but rather ellipsoidal, its dimensions are, in fact, described in terms of its major and minor semiaxes (or equatorial and polar radii); the values assigned to each of the two dimensions vary in accordance with different ellipsoids that have been proposed for defining the Earth's shape (as approximations to the geoid, which is its real form). Consequently, the distance between the center of the Earth and a point at sea level (surface of ellipsoid) varies as a function of geographic latitude and depends, moreover, on the dimensions of the ellipsoid chosen for this calculation. However, the approximate value of r given above is sufficiently exact for our purposes; employing more accurate values of r for each site, the resultant variations in calculated altitudes would be negligible (up to about 5 arc seconds).

It is worth stressing that horizon altitudes should not be calculated without taking into consideration the curvature of the Earth's surface. For small distances, satisfactory results can be obtained by the formula (derived from the relations in the plane triangle)

$$\tan h = \frac{a_2 - a_1}{d} \qquad \textbf{(3)}$$

where h is the altitude of point 2, observing at point 1, a_1 and a_2 are the altitudes above sea level of points 1 and 2, respectively, and d is the distance between both points, *in metres*. However, expression **(3)** will render erroneous results in great many cases, because the relevant points of the horizon are often far away: for example, if the point of the horizon for which the altitude is being calculated is situated at a distance of about 37 km (equal to 20 nautical miles or 20' of angular distance) from the observation point, the difference between the altitudes calculated by formulae **(2)** and **(3)** will be of about 10'. Therefore, the error in altitude calculated by **(3)**, increasing proportionally with the distance, may affect notably the calculation of the declination corresponding to an alignment.

Attention should also be called to the fact that the azimuth and altitude calculations are not reliable when the relevant points of the horizon are situated at a relatively short distance, because the precision of the results depends on the accuracy with which the geographic coordinates and altitudes above sea level can be determined, both for the observation point and the one on the horizon. As the distance between the two points increases, the probable margin of error in the calculated azimuth and altitude diminishes. For example, if the azimuth of a mountain peak located east or west of the site is calculated, an error of 1" (arc second; *ca.* 30 m) in the latitude determined for the site will result in an error of 21' (arc minutes) in the calculated azimuth, if the mountain is 5 km away, and of 5', if it is situated at a distance of 20 km.

For the alignments within the angle of annual movement of the Sun along the horizon I also determined the corresponding sunrise and sunset dates, valid for the epoch of foundation of the site or construction of the buildings in question. Due to precessional variations in the obliquity of the ecliptic and in the heliocentric longitude of the perihelion of the Earth's orbit (the latter element determining the length of astronomical seasons), one and the same solar declination does not necessarily correspond in any time span to exactly the same date of the tropical year (*cf.* Woolard and Clemence 1966: 235ff; Mueller 1969: 59ff; Meeus 1983: 3-1f). In order to determine the exact dates corresponding to the declinations of the Sun in relevant epochs, I employed Tuckerman's (1962; 1964) tables, which give the Sun's positions for the period from 601 B.C. to A.D. 1649; since the positions are given in ecliptic coordinates, the corresponding declinations were obtained by the formula

$$\sin\delta_{\odot} = \sin\varepsilon \sin\lambda \qquad \textbf{(4)}$$

where $\delta_{\odot}$ is the declination of the Sun, ε is the obliquity of the ecliptic and λ is the ecliptic longitude of the Sun.[13] Tuckerman's tables present the Sun's longitudes at 10-day intervals, always for 16:00 hours of Universal Time, taking into consideration the movement of the perihelion and, therefore, the secular variations in the duration of the seasons; the value of the obliquity of the ecliptic was determined, for the epoch corresponding to every particular case, by means of the formula developed by de Sitter (Thom 1971: 15).[14]

Obviously, reliable computer programs can also be employed for calculating positions of the Sun on particular dates in the past.

ANALYSIS AND INTERPRETATION OF THE ALIGNMENT DATA

In order to analyze the alignment data I elaborated a number of histograms which show the distribution of azimuths, declinations and solar dates, and intervals between these dates (Šprajc 1997b: Figures 4.1-4.12). The most important general conclusions of my research in central Mexico (summarized above in Introduction) are supported by the evidently non-random distribution of the plotted values, particularly by the clustering of declinations (dates) and intervals around certain values (Figures 2 and 3).[15] The fact that the data on architectural orientations are combined with those corresponding to horizon features might provoke methodological objections in the sense that heterogeneous elements are compared,

13 In order to evaluate the astronomical significance of alignments, it is necessary to identify the positions on the celestial sphere to which they correspond. Any alignment is defined by its azimuth and altitude above the horizontal plane, which are coordinates of the horizon system, whereas the positions on the celestial sphere can be expressed either by the ecliptic or the equatorial system of coordinates. The astronomical reference of an alignment is indicated by the corresponding declination, which is a celestial coordinate in the equatorial system. If alignments are to be related to celestial positions expressed in ecliptic coordinates, the latter must first be converted into the equatorial system; formula (4) derives from the one used for calculating declination (Mueller 1969: 40), considering that ecliptic latitude β is, in the case of the Sun, always 0°:

$$\sin\delta = \sin\beta \cos\varepsilon + \cos\beta \sin\varepsilon \sin\lambda$$

14 The procedure is described in detail in Šprajc 1997b: 30f.

15 While Figures 2 and 3 show these data for all sites included in my study, histograms presenting them separately for the Preclassic, Classic and Postclassic periods can be found in Šprajc 1997b: Figures 4.6-4.12. A statistical analysis of the alignment data is still planned to be done.

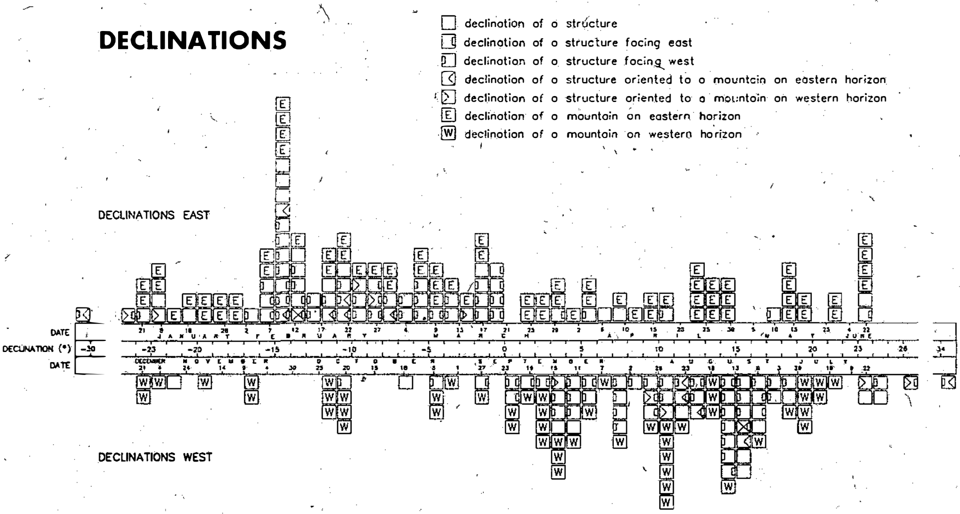


Figure 2. *Distribution of declinations recorded by alignments at central Mexican archaeological sites. Each quadrate represents one declination, corresponding either to a structure or to a horizon prominence; the meaning of signs and letters in the quadrates is explained in the figure. Declination values on the horizontal scale are spaced at 1° intervals; for example, all declinations greater than 15° and smaller than or equal to 16° appear in a single column. The declinations recorded on the eastern/western horizon are plotted upward/downward. For the range of declinations attained by the Sun, the corresponding dates of the year are also shown; winter and spring dates appear above the declination scale and summer and autumn dates below it.*

whose significance was not necessarily comparable (*cf.* Hawkins 1968: 49). It should be pointed out, however, that the alignment data of each type were first plotted separately; only after similar patterns had been obtained in both cases, I proceeded to elaborate histograms combining the two series of data. In fact, the lack of homogeneity is more apparent than real: in both cases we are dealing with alignments associated with phenomena observable on the horizon; furthermore, a generic relationship between the functions of architectural orientations and horizon features is indicated by the buildings oriented to prominent peaks on the local horizon (*cf.* Figure 2). Indeed, the results obtained suggest that architectural orientations served in combination with natural horizon markers, allowing for the use of observational calendars based on calendrically significant intervals.

I hope that the methodological approach employed in my study is free from prejudices that might distort the objectivity of the research results and prevent a global comprehension of the complexity of astronomical factors involved in the orientation and location of civic and ceremonial buildings. As an example of such prejudices, the significance assigned or denied *a priori* to certain dates of the tropical year can be mentioned. In his critique of the hypothesis (first proposed by Morley and later by Aveni) that the alignment from Stela 12 to Stela 10 at Copán was intended to mark sunsets on April 12 and September 1, Köhler (1991: 132) contends that these dates have no particular astronomical

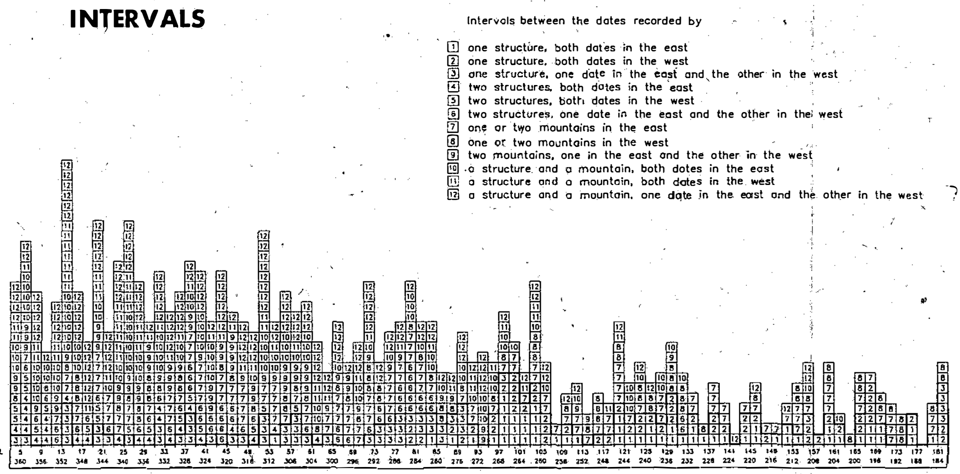


Figure 3. *Distribution of intervals between the dates recorded by alignments at central Mexican archaeological sites. The intervals, each represented by a quadrate, are distances, in days, from any one to any one of the dates recorded at any particular site, both by architectural orientations and by prominences on the local horizon. The meaning of numbers in the quadrates is explained in the figure. Since any (except a solstitial) alignment registers in one and the same direction two dates in a year and, therefore, two intervals whose sum is 365 days, both are represented by a single quadrate: in the upper line of the horizontal scale the shorter intervals are listed and in the lower one their complements to the 365-day year. The columns of quadrates are spaced at 2-day intervals: for example, all intervals in excess of 103 and smaller than or equal to 105 days (greater than or equal to 260 and smaller than 262 days) are included in a single column. The thick line divides the intervals produced by a single architectural orientation (quadrates 1, 2 and 3) from others. If an interval separates the dates recorded by* two *alignments, the latter actually mark* four *dates which, consequently, delimit* two *short intervals; since both are necessarily coexistent and similar (though not always exactly equal, due to the variable speed of the Sun's apparent movement), they are represented by a single quadrate, its location in the histogram being determined by their mean value. Since any (except a solstitial) architectural orientation that is functional in both directions records two dates on the eastern and two on the western horizon, two of the six intervals produced necessarily approach 182 days (half a year); such intervals, even if some of them may have been achieved intentionally (their exact lengths depend on horizon altitudes), are not represented in the histogram, because their high frequency would not reflect their real importance (for details see Šprajc 1997b: 63, 122-124).*

significance. Our still deficient knowledge about the importance of certain dates in the Mesoamerican world view certainly does not warrant such an assertion which, one can suspect, reflects an "application of European concepts", *i.e.* precisely the attitude the same author aptly criticizes in another context (*ibid.*: 133ff). Nor can the requisite be accepted that, for demonstrating the astronomical nature of architectural alignments in a culture it is indispensable, in the first place, to prove with independent data that celestial phenomena were "deified or at least considered to be powerful entities which directly influence the fate of mankind" (*ibid.*: 131); Köhler (*ibid.*) adds that, in the absence of archaeological evidences, the ethnohistoric and ethnographic data from the same cultural area may at least provide some hints. As Aveni (1995: S79) points out, this proposition, refuted by the very emergence and development of archaeoastronomy and its achievements in the second half of this century, "declares that one can discover nothing by beginning with alignments". In terms of general archaeological methodology we could say that, by implementing Köhler's postulate in practice, we would run the risk of neglecting the intrinsic value of archaeological remains and of approaching the attitude criticized by Binford (1972: 86) in relation to inadequate use of ethnographic analogies in archaeology: "Fitting archaeological remains into ethnographically known patterns of life adds nothing to our knowledge of the past." Curiously, however, referring to stellar orientations, Köhler (1991: 131) affirms:

> [...] there is also the possibility of obtaining information on the basis of purely archaeological sources. For example, if the orientation towards the point of rising or setting of a particular star is consistently found in a great number of sites of one area, there is a high probability that this orientation was a deliberate one and quite probably had the aim of being aligned with that particular star. — However, all conclusions based on a single site must remain highly speculative!

This argument, somehow contradictory in comparison with the formerly quoted claims of the same author, is much closer to what I consider to be the proper guidelines for the study of astronomical alignments in general, not only of those related to the stars.

Another example of a methodologically deficient approach is Morante's (1996: 83ss) attempt at identifying horizon markers at Teotihuacan for the dates he assumes to have been relevant. Even if, for the various dates he mentions, we know that they were, indeed, important, this method prevents us from finding other possibly significant dates, *i.e.* it does not allow us to discover anything new, limiting our explanatory endeavors to corroborations of what we already know or suppose (*cf.* Ruggles 1994: 498). These critical remarks notwithstanding, the studies of Xochicalco and Teotihuacan accomplished by Morante (1993; 1996) are important contributions to Mesoamerican archaeoastronomy. It is obvious that a "case study" focused on a single site can hardly detect patterns that would confirm the hypotheses proposed in relation to the alignments at that site. Regularities of this type can only be revealed by comparative research based on a number of sites that manifest some degree of cultural homogeneity, but this approach also involves deficiencies that seem inevitable: when studying diverse sites, it is impossible to pay sufficient attention to the whole complexity of each of them; clearly a detailed research at one site can detect more elements of potential astronomical significance and generate important new hypotheses, but these will have to be verified by comparative investigations. It can be concluded that both approaches are necessary and complementary, each of them having its advantages and limitations.

CONCLUDING REMARKS

The methodological guidelines presented above were developed in the course of my study of alignments in central Mexican archaeological sites. If, as I hope, the results of my research (summarized in Introduction) contribute to a deeper understanding of the significance of architectural orientations and other alignments incorporated in the cultural landscape of prehispanic central Mexico, the methods and techniques described should be helpful in further archaeoastronomical investigations in Mesoamerica, as well as in other areas with comparable types of archaeological remains.

Since the appearance of the sky and the characteristics of recurrent celestial events can be confidently reconstructed for any place on Earth and any time during the last several millennia, archaeoastronomy largely relies on mathematically exact data and thus has a significant advantage over studies of other aspects of the past (Ruggles and Saunders 1993: 9f; Ruggles 1999: 145). This characteristic of archaeoastronomical research is in keeping with the evident tendency in modern archaeology to employ as many as possible of the techniques, methods and procedures developed by exact sciences, in order to achieve accurate, testable and reliable results. Curiously, however, very little has been done within the main-stream archaeology to include measurements and study of alignments in the excavation process. Architectural orientations represent "attributes of material objects" (Iwaniszewski 1995a: 192) and should be considered as important as any other piece of archaeological evidence:

> Even if the surveyor of a prehistoric structure should be of opinion that there is "nothing in" Orientation, still the direction in which the structure is laid out on the ground should be accurately reproduced in the resulting plan, if only in the interests of scientific completeness. Until this is done, the matter will never be settled as to whether, in fact, there is, or is not Orientation in these structures of antiquity; and if there is, wherein it is expressed. (Somerville 1927: 37)

Unfortunately, this methodological advice, expressed more than seven decades ago, has not had much impact among archaeologists, whose general attitude has not changed substantially even in recent decades, in spite of the indisputable achievements made within the specialized field of archaeoastronomy (*cf.* Ruggles 1999: 1ff). Ideally, architectural orientations and other archaeologically documented alignments with conceivable astronomical significance should be measured in the course of excavation, when the construction elements are still *in situ*, considering that, as a result of later interventions, they are often moved off their original positions or disappear completely (Hartung 1980: 145). Studies of alignments should represent an *integral part* of archaeological research. However, if the place archaeoastronomy deserves within anthropological disciplines, specifically archaeology, is to be secured, the application and continuous development of rigorous and objective methodological procedures is obviously of foremost importance.

POVZETEK

Članek predstavlja nekatere specifične metode in tehnike, ki so bile uporabljene v sistematični arheoastronomski raziskavi orientacij v predšpanski arhitekturi na vrsti predklasičnih, klasičnih in postklasičnih arheoloških najdišč v osrednji Mehiki. Rezultati te raziskave so na kratko podani v uvodu, sicer pa se članek osredotoča na tehnična in metodološka vprašanja, ki so relevantna za arheoastronomska preučevanja nasploh: obravnavajo se kriteriji za izbor linij, ki so bile upoštevane v študiji, pa tudi nekatera tehnična vprašanja, ki zadevajo zbiranje in analizo podatkov o orientacijah.

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THE MAKING OF AN ANTHROPOLOGIST: FROM FRAZER TO FREUD IN THE LIFE OF THE YOUNG MALINOWSKI[1]

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Why did Bronislaw Malinowski, the Polish-born founder of British social anthropology between the wars, become an anthropologist? While this is a legitimate, indeed, an essential question for a biographer to pose, it would be ingenuous to expect a straightforward answer (it is far easier to answer the question "how?"). If I pause to ask myself why I became an anthropologist I am unable to give an entirely satisfactory answer. Simple reflection yields a complex tangle of reasons — emotional, intellectual, social, academic and institutional — of which curiosity concerning the ways of life of what used to be called "primitive societies" was among the least compelling. While there is certainly a sense in which I *chose* to become an anthropologist, there were also many contingent and fortuitous reasons for my decision, not to mention the kind of step-by-step shuffling towards a final commitment that is probably characteristic of the adoption of most professional careers. One wakes up one morning to find oneself practising a vocation, a calling that one had heard but faintly, if at all, during one's youth. This gradual settling into a career is a far more common experience than the sudden, illuminating conversion or the peremptory command from the sky that make for dramatic biography. I believe that it was Malinowski's experience, too, despite claims to the contrary.

In his case, why he became an anthropologist is complicated by the fact that there was no such academic niche in early twentieth century Poland. Insofar as he set out to pursue an anthropological career he was obliged to seek one abroad. Yet one of the surprising facts about his early manhood (in view of the revolutionary contribution he was to make) is how long he equivocated once he had begun the study of ethnology and primitive sociology (as social anthropology was generally referred to in those days). And although his retrospective self-mythologizing claimed otherwise, his commitment to the subject grew incrementally over a period of years. As in all mythopoeia, historical time is collapsed and annulled in Malinowski's inspirational image of the Ethnographer as Hero.[2]

Before proceeding, as a matter of courtesy I should introduce Bronislaw Kasper Malinowski to those readers who are not anthropologists. The main facts of his life are well known and have been rehearsed in scores of publications — obituaries, encyclopedias, bio-

1 This article bears a family resemblance to the talk on "Writing Malinowski's Biography" which I gave to the Historical Seminar of the Scientific Research Centre of the Slovene Academy of Sciences and Arts in November 1997. I am most grateful to this institution for inviting me to Slovenia and making me feel welcome. I thank especially Borut Telban for meeting me in Venice, for accommodating me in Ljubljana and introducing me to some of the best food in Europe, and not least, for showing me the Postojna caves, which the young Malinowski had visited with his mother on a hot September day in 1901 or 1902. I cannot claim, however, that his visit to the caves had anything to do with his becoming an anthropologist.

2 For an authoritative account of Malinowski's mythical charter see George W. Stocking Jr., "The ethnographer's magic: fieldwork in British anthropology from Tylor to Malinowski," *History of Anthropology*, 1983, vol.1, pp.70-120.

graphical dictionaries, and textbooks. But because I want to canvass Malinowski's own views concerning why he became an anthropologist it is best if I allow him to introduce himself. The following text has not previously been published. The original is a single page of typescript discovered in an immigration file in the South African state archives in Pretoria; presumably it had been elicited by Malinowski's sponsors in preparation for his attendance at an international conference on African education held in June 1934. This biographical note bears the hallmark of Malinowski's provocatively jaunty style so characteristic of his confident (some would say arrogant) maturity, and he had probably dictated it to his personal secretary in early 1934. He was then fifty years old and at the very pinnacle of his influence and renown.

> Born April 7, 1884, in Cracow, Poland. Parentage on both sides Polish gentry and nobility. Educated in Poland, where graduated PhD at old Polish Univerity of Cracow. Owing to breakdown in health, travelled subsequently in Mediterranean, North Africa and Canary Islands for two years, and decided to take up the study of exotic cultures and peoples. Like his friend and countryman, Joseph Conrad, acting on the principle that if [one is to be] an anthropologist, then [be] a British anthropologist. Came to London a few years before the War, and attached himself to the London School of Economics, where he worked with Professors Westermarck, Graham Wallas, and Hobhouse.
>
> Soon started publications in English in **Man, Journal of the Royal Anthropological Institute, Folklore**, and later on, in **Nature**. First book in English, **The Family among the Australian Aborigines**, published in 1913. A few weeks before the war, started an expedition to Australia and New Guinea, intending to remain in the Antipodes about two years. Owing to the outbreak of hostilities, and an Austrian subject, B.M. was technically prevented from returning to Europe – remained for six years, carrying out three expeditions to New Guinea. Difficulties of scientific work during the war compelled him to live among the natives, like a native, speaking the language, and he thus got into closer contact with them than if he had done his work from a little yacht or with numerous staff on a well-equipped expedition.
>
> Returning to Europe in 1920, he started lecturing at the LSE, was appointed lecturer on the permanent staff in 1922, Reader in Social Anthropology in 1924, and Professor of Anthropology in the University of London in 1927. In fact this was the first full-time chair in Anthropology in Great Britain.
>
> To the specialist, B.M. is best known as the founder of the Functional School, the new tendency in anthropology which abandons antiquarian explanations of savage customs, institutions, and ideas in terms of evolution and history and tries to account for them by showing what part they fulfil within the scheme of primitive culture. To the general reader, such books as **Argonauts of the Western Pacific**, and **The Sexual Life of Savages**, (which has also appeared in French, Spanish, Italian and German), **Crime and Custom in Savage Society, Myth in Primitive Psychology**, etc. represent in scientific anthropology what the works of Joseph Conrad represent in the realm of the novel. B.M. reveals to us the savage as essentially human, yet more mysterious and problematic, and in a way thrilling, than he generally appears. To those interested in practical applications of anthropology, B.M.'s contributions on primitive law, primitive economics, and his championship of the

> essential identity in the mental constitution of all races, will be of interest. The whole tendency presented by the Functional School is in the realm of theory what Lord Lugard's policy of Indirect Rule has been in the realm of practice.[3]

To complete this semi-official biographical summary, I might add that Malinowski lived for only eight more years. During that time he published a few more books (most notably his two-volume ethnography of Trobriand horticulture and the language of magic, *Coral Gardens and their Magic*), and received many distinguished awards. He mourned the death of his Scottish-Australian wife, Elsie; intermittently worried about what to do with his three daughters; took sabbatical leave from London in 1938 to visit the U.S.A. where he found himself once again exiled by an outbreak of war in Europe; took a teaching post at Yale University where he was ultimately granted a tenured professorship; campaigned tirelessly against Nazi Germany and in support of the United States' joining the war; married a young English painter, Valetta Swann, with whom he made two fieldwork trips to Oaxaca in Mexico. He died at his home in New Haven, Connecticut, on 15 May 1942, the day after delivering his inaugural presidential address to the New York-based Polish Academy of Arts and Sciences.

Malinowski's cryptic self-presentation as given above contains several unexpected, even whimsical comments. Twice he invokes Conrad's name to invite comparison with his own life and works, and in a rather far-fetched analogy he compares Functionalism with Lugard's Indirect Rule. Concerning his Trobriands fieldwork, in the simplest terms he alludes to the essential ingredients of what he once called his "ethnographer's magic": that is, lengthy immersion in the field, command of the vernacular, and participant observation. Surprisingly, he denies his own agency by blaming the war for compelling him to live as he did "among the natives". (Did he now really believe that his exemplary fieldwork was a matter of happenstance, dictated by external circumstances?) At the very least, he suggests, his enforced sojourn in the Trobriands precluded superficial, hit-and-run fieldwork from "a little yacht": a shaft surely directed at his British patrons and mentors A. C. Haddon, W. H. R. Rivers and C. G. Seligman, who had all conducted ethnological surveys in the Western Pacific from small boats. The "well-equipped expedition" that he probably had in mind was the Cambridge expedition to the Torres Strait led by Haddon in 1898-99.

The fact is, however, as I have amply demonstrated elsewhere, Malinowski was not "interned" in the Trobriand Islands at all, and intially at least, he was given *carte blanche* by the Australian colonial authorities to work for as long as he wanted wherever he wished. It was entirely his own choice to go to the Trobriands in 1915, to stay there much longer than everyone expected, and then to return to the same island in 1917 for a second lengthy period.[4] He placed himself in the odd position of having to apologise to Seligman, his academic supervisor in London who ten years previously had himself done a brief spell of fieldwork in the Trobriands, for staying so long in one place, thereby neglecting the opportunity to explore ethnographically unknown peoples in neighbouring districts. Note that Seligman's name is missing from the list of Malinowski's early academic mentors at the London School of Economics. This is a puzzling omission, for Seligman's influence on

3 South African State Archives, Pretoria. Ref.: UOD 1086 (E46/58/16).

4 Michael W. Young, "The intensive study of a restricted area, or, Why did Malinowski go to the Trobriand Islands?" *Oceania* vol.55, 1984, pp.1-26.

Malinowski's career from 1910 onwards greatly exceeded the combined influence of Wallas and Hobhouse. It perhaps reflects the low point to which Malinowski's relationship with his senior colleague had sunk during the early thirties.

For the purpose of this article it is Malinowski's explanation for his turn to anthropology that I want to address. He omits to mention in this sketch that his initial training at the Jagiellonian University in Cracow during 1902-1906 was in mathematics, physics an philosophy, and that his doctoral thesis (awarded with Imperial honours in 1908) was a critique of Ernst Mach's philosophy of science. When he enrolled at the University of Leipzig in 1909 it was principally to continue laboratory studies in physical chemistry, and it was only later that year that he begin to dabble seriously in ethnology under the somewhat remote tutledge of Wilhelm Wundt, the polymath founder of *Völkerpsychologie*. Moreover, it was not simply the pursuit of this new intellectual interest that took him to London the following year. It was also the indulgent pursuit of a double-sided romantic infatuation: one with the English and their culture (he confessed to "Anglomania"), the other with an English-South African woman with whom he had begun an affair in Leipzig and who preceded him to London. (This timely love affair is an excellent example of those fortuitous career-deciding factors I mentioned at the beginning of this essay.) The English Channel proved to be Malinowski's Rubicon. His studies in London did indeed finally shape his decision to become an anthropologist, though it was as late as 1913 that he felt the commitment to be irrevocable. By then, in addition to the anthropologists I mentioned above, he had befriended Joseph Conrad.

In his biographical sketch Malinowski pushes back the date of his interest in anthropology to the period when, "owing to [a] breakdown in health" he travelled widely throughout the Mediterranean, to North Africa and the Canary Islands. He implicitly lays at the door of his two years' travel experience his decision to study anthropology (conceived here in broad, Orientalist terms as "the study of exotic cultures and peoples"). It is true that he suffered poor health as a youth, and that following the death of his father when he was fourteen, his mother took him on extended journeys. They sought dry and sunny climes for what was suspected to be tuberculosis. Malinowski was wracked by frequent illness throughout his life, and his perennial quest for bodily health emerges as a major theme in his biography. Like his hero Conrad, however, he learned to use his illness creatively.

The most oft-cited pronouncement Malinowski made concerning his reasons for becoming an anthropologist does not mention his early travels at all, though it does refer to his illness. This particular disclosure has been the cause of some contention because Malinowski nominated a rather unlikely intellectual mentor: Sir James Frazer, the Scottish classicist, Biblical scholar and comparative anthropologist, successor to Sir Edward Tylor, and for two or three decades the doyen of the evolutionary school of anthropology in Britain. The following passage has been cited frequently, sometimes to prove — taking Malinowski's words at face value — that his turn to anthropology had a British, not Polish, impetus. Latterly it has been cited with more critical intent, for it continues to puzzle the skeptics.

On the occasion of his delivery of the first Frazer Lecture at the University of Liverpool in November 1925, Malinowski began with a Dedication. His opening words were patently, plangently autobiographical, claiming a conversion to anthropology in the manner of Paul on the road to Damascus.

> If I had the power of evoking the past, I should like to lead you back some twenty years to an old Slavonic university town — I mean the town of Cracow, the ancient capital of Poland and the seat of the oldest university in eastern Europe. I could then show you a student leaving the medieval college buildings, obviously in some distress of mind, hugging, however, under his arm, as the only solace of his troubles, three green volumes with the well-known golden imprint, a beautiful conventionalized design of mistletoe — the symbol of *The Golden Bough*.
>
> I had just then been ordered to abandon for a time my physical and chemical research because of ill-health, but I was allowed to follow up a favourite side line of study, and I decided to make my first attempt to read an English masterpiece in the original. Perhaps my mental distress would have been lessened, had I been allowed to look into the future and see the present occasion, on which I have the great privilege of delivering an address in honour of Sir James Frazer to a distinguished audience, in the language of *The Golden Bough* itself.
>
> For no sooner had I begun to read this great work, than I became immersed in it and enslaved by it. I realized then that anthropology, as presented by Sir James Frazer, is a great science, worthy of as much devotion as any of her elder and more exact sister studies, and I became bound to the service of Frazerian anthropology.[5]

Although this is a compelling conversion myth it is not very good history. Malinowski's audience was English; he was still in the process of making his name in his adopted country. Not yet British in 1925, and only some months later to make another gesture of withdrawal from Poland by removing his remaining effects from Cracow, he was still marginal, an *emigre* Pole. The title of his lecture, ironically enough, was "Myth in Primitive Psychology", and it presented for the first time his innovative theory of myth-as-charter. Myths, legends and all stories about the past—including history—serve a legitimating function in the present. The idea doubtless sprang from his roots in Poland (as Ernest Gellner has effectively argued),[6] but on this occasion he introduced a lecture about myth in primitive psychology (primitive sociology would have been more accurate) with a charter myth about himself and his intellectual affiliation with the British academic establishment.

Myth notwithstanding, there is a kernel of historical truth in this account of his early brush with Frazerian anthropology. There is no reason to doubt that he was introduced to *The Golden Bough* for the reasons and in the circumstances he states. By whom we do not know, but we do know that his mother read to him a great deal when his sight was threatened by a series of eye infections, and a small, undated notebook survives into which she had copied passages from Frazer's work, presumably on her son's behalf. The incident dramatized by Malinowski cannot be dated with complete certainty, but if it was indeed a time

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5 Dedication to Sir James Frazer, "Myth in Primitive Psychology," in R. Redfield (ed.) *Magic, Science and Religion*, Doubleday, New York, 1954, pp.93-148.

6 Ernest Gellner, '"Zeno of Cracow" or "Revolution at Nemi" or "The Polish revenge: a drama in three acts," in R. Ellen, E. Gellner, G. Kubica and J. Mucha (eds), *Malinowski Between Two Worlds*, Cambridge University Press, Cambridge, 1988, pp.164-94. Gellner's principal thesis in this essay — that Malinowski succeeded Frazer as the "priest-king" of British anthropology by symbolically slaying him—is a secondary myth which does not stand up to historical scrutiny (see M.W. Young, "Young Malinowski — a Review Article," *Canberra Anthropology*, 1994, vol.17(2), pp.103-122).

when he was unable to work on physics and chemistry then it was probably in the academic year of 1905-06, for it was only in this final year of what we would now call undergraduate studies that he began to study physical chemistry. The desire to read English "in the most beautiful variety of that tongue" suggests an aesthetic motivation rather than a curriculum-driven one, and there is no evidence that he was to be examined in English at his *rigorosum*.[7]

In any event, Malinowski's retrospective claim to have become "enslaved" by *The Golden Bough* does not ring true for the years of his university studies in Cracow. Surrounded by living mentors (the list is extensive) who spoke his own language and addressed his immediate intellectual preoccupations, he would have had no practical need of any dialogue—even if such were possible — with a reclusive Scottish scholar who dwelt beyond the English Channel. But this is not to rule out the possibility that a youthful Malinowski was enraptured by sonorous sentences evoking colourful customs and was wafted on imaginative flights to enchanted savage worlds.

There is another, less well-known account of how Malinowski might have been called to become an anthropologist. Feliks Gross, a fellow Cracovian of a younger generation, was a pupil of Malinowski's at Yale. Malinowski had helped Gross and his brother, a medical doctor, to enter the United States after they had fled Poland in 1938. In the months preceding Malinowski's death in 1942, Gross assisted him with the preparation of the book that was posthumously published as *Freedom and Civilization*. It was Gross, moreover, who "discovered" Malinowski's personal diaries after his death. Among them were the revealing documents which, to the dismay of his pupils, Malinowski's widow published in 1967 under the title *A Diary in a Strict Sense of the Term.*

In his memoir of Malinowski's youth, Feliks Gross asks the rhetorical question:

> How did it happen that in this northern city [i.e.Cracow]—so far from the tropics, distant in history, interest, and space from what was falsely considered at that time 'a romance of the colonies,' that here, in a rather medieval urban setting, you find a young man who dedicated his life primarily to the anthropology of the Pacific?8

The question itself is ambiguously posed, for Malinowski did not dedicate his life to anthropology (let alone to that of the Pacific) while he was actually in Poland. As I have suggested, we can safely assume that his career took shape gradually and finally came to fruition in London. To the extent that ethnology interested him during his student years in Cracow it was, as he says "a favourite side line of study"— not necessarily even *the* favourite. But Gross might have a point when he tries to answer the question from his own experience of being a pupil at the Jan Sobieski Gymnasium (which he attended some 16 years after Malinowski). He writes that "there was a passionate interest—more than an interest — a fascination with the unknown, with undiscovered lands and their inhabitants":

7 Ashley Montagu, one of Malinowski's very first pupils at the LSE, states in his obituary that Malinowski had to pass an examination in the English language. I can find no evidence for this. ("Bronislaw Malinowski, 1884-1942," *Isis* vol.34, 1942, pp.146-150.)

8 Feliks Gross, "Young Malinowski and his later years," *American Ethnologist*, 1986, vol.13(3), pp.557-8.

> When we were 11 or 12 years old, we got our school atlases for our first geography classes... On some maps we found white spots. This was the mark to indicate that the region was still unknown, untraveled. That was what we were looking for. It made us think that perhaps there was a place where we would be the first visitors... And I think Malinowski, like many others, was fascinated by the romance of the unknown, not solely by anthropology itself.[9]

Feliks Gross is here perhaps subconsciously recalling a passage from Conrad's *A Personal Record* in which the novelist, as a child of nine or ten stares at a map of Africa and, putting his finger on a blank space at the heart of the continent, declares "When I grow up I shall go *there*".[10] As indeed he did. Or perhaps Gross was recalling *Heart of Darkness* (that subtext of Malinowski's first New Guinea diary), in which Conrad — in the voice of his narrator Marlowe — says:

> "Now when I was a little chap I had a passion for maps. I would look for hours at South America, or Africa, or Australia, and lose myself in all the glories of exploration. At that time there were many blank spaces on the earth, and when I saw one that looked particularly inviting on a map... I would put my finger on it and say, When I grow up I will go there."

Today this has the banal ring of a cliche. I confess that I, too, was entranced as a child by maps of distant lands with their seductive promise of the unknown, and that I, too, heard the first faint call of anthropology in the appeal of — the phrase is Conrad's —"a militant geography".

The romance of travel, exploration and discovery on the edges of Empire — whether or not Malinowski imbided it, as Gross did, from reading Alexander Humboldt, Joseph Conrad, Colonel Przewalski or Sven Hedin — was in the very air of Modernism that Polish intellectuals breathed at the turn of the century and beyond. But we must recall that Malinowski had already begun his foreign travels at an early age. With his mother he had travelled the Mediterranean in search of healing sunshine: the Dalmatian coast, Italy, Spain, Malta, Algeria, Egypt, Turkey... he had visited them all, it seems, by the time he was sixteen or seventeen. On these wearying travels he might well have quenched some of his youthful thirst for the exotic.

The romantic allure of the unknown and the exotic was something he was later to confess to his diaries on occasion, but he controlled it by intellectual analysis—as he controlled most of his impulses — and there is no sense in which he might be said to have been impelled or *driven* by it. The call of the wild was an aesthetic experience he could cultivate and mildly indulge; but it could not be the motive force of his ambition, and to suggest that it somehow led him to anthropology and the search for the primitive it to go beyond the biographical facts. Exoticism, in short, is not a sufficient explanation for Malinowski's choice of career.

9 Gross, *ibid.*, p.558.
10 Joseph Conrad, *A Personal Record*. Dent & Co. London, 1946, p.13.

He was neither an intrepid explorer nor a discoverer of untouched tribes. Whilst in New Guinea he had the opportunity to venture, if not directly into those tantalizing white spots on the map of which the hinterland was largely composed, then into the ethnographically uncharted areas adjacent to them. But he declined (for perfectly good reasons, let it be said) even this adventurous option. With one exception — the Amphlett Islands, to the south of the Trobriands — his field sites in New Guinea were places which had already been lightly scratched by previous ethnographers, and all had been more or less thoroughly missionized. In short, nervously restive throughout his life though he was, and rarely content to remain in one place for long, it was an inner impatience that drove him — not at all, one would think, the kind of psychological equipment conducive to contented travel. His ambitions not only lay well to one side, so to speak, of those blank spots on the map, but they also sprang from other imaginative sources.

Let me now reach back even further into Malinowski's life for any evidence of the existence of a disposition or an enticement to pursue anthropology. First we must pause to ponder what biographers are to make of childhood. How much interpretative weight can early experiences be made to bear, and to what extent do such events shape or determine the career patterns, the sexual and familial configurations of adult life? While it is impossible to discuss these questions fully here, let us observe that in the construction of a biography (which is, after all, a literary artifact that posits the internal coherence of any given life) a telelogical element is inescapable: *this* childhood led to *that* adulthood. This need not mean that there is a single luminous thread joining infancy, childhood, youth, adulthood and full maturity. A subject's sense of personal destiny notwithstanding, it would be an overly simplistic biography which presented his or her life in terms of an inexorable, unilinear development from cradle to grave. But in each particular case, we have every right to ask just how significant was the subject's childhood in giving shape and direction to the adult life. There is a minimalist position which accepts that childhood events and experiences *may* be significant, but are unlikely to be wholly determinative. Then there is the extreme position associated with the teachings of Freud and his followers, to whom infancy and childhood are the *fons et origo* of all adult dispositions and behaviour. In this philosophy, the child is indeed father to the man. While inclined to take a miminalist position, I am uneasily confronted by the fact that Bronislaw Malinowski himself flirted seriously with Freudian interpretations. He had first become acquainted with Freudian theories in about 1912, but it was not until the early 1920's that he began a serious study of Freud's writings — particularly those concerning human sexuality. He then wrote a polemical and highly influential book — *Sex and Repression in Savage Society*—challenging Freud's doctrinaire tenet that the oedipal complex was universal. Like many intellectuals of his generation, Malinowski found Freud's theories wonderfully illuminating, and he applied many of his insights to himself: including the proposition that he had been passionately devoted to his mother in direct proportion to the degree of jealous hatred he felt for his father. In other words, he tested by keen introspection what he might or might not have repressed. I shall return to this theme below.

There is a dearth of materials on Malinowski's childhood. He wrote no autobiography. His earliest companions left no memoirs — or if they did, they are buried in crumbling attics of the long-deceased Hapsburg province of Galicia. The silence of the sources is almost total. In such impoverished circumstances one is grateful for the most meagre of scraps. Quite recently, while mining (for the ninth or tenth time) the massive archive of Malinowski's papers at the London School of Economics, I struck a nugget of pure gold.

It was embedded in a seven-page synopsis of the introduction to an anthropology textbook that Malinowski had been invited to write by an American publisher in about 1932. Although he never found time to write this book (it being just one of many abortive projects), his methodically devised, handwritten synopsis is testimony to the conceptual scope of his plan. What concerns us here, however, is the autobiographical content of this document, for he had intended to include in the general introduction to the book several sections on his own experiences. The first of these was to be entitled "Culture as Personal Experience". (One may recall in this connection the prescriptive Introduction to *Argonauts of the Western Pacific*, perhaps the most influential account of fieldwork method ever written.) Under this heading, Malinowski jotted down in characteristic telegraphic form an outline of his childhood experiences, or those of them he judged to be relevant to his career as an anthropologist. Within the space of a couple of dense and cryptic foolscap pages there are more clues to his childhood experiences than are to be found in any other single source. At last, it seems, we can now begin to know what Malinowski, when already in his late forties, understood to be the determining youthful experiences that set him on the course to becoming an anthropologist.[11]

In what follows, I amplify a selection of his terse and telegraphic notes into a more coherent account. Although I have retained his key phrases, I cannot pretend that he would have written up his notes in precisely this way. "Ponice idyll", he begins, referring to a village in the extensive Podhale region to the south of Cracow which spreads into the foothills of the Tatra Mountains.

> As a child, between the ages of four and eight, I lived intermittently in a secluded Carpathian village among the peasantry. It was an Arcadian valley. My memories are vague, but my mother helped me to recover them. We made subsequent visits there too. I remember my contemporaries telling 'fairy tales' about stone houses and stone churches with stone steeples; there was a mythology about paved roads and carriages.... That urban world was familiar to me, but unknown to my friends. It was my first experience of duality, of the multiplicity of the world of culture. In the village there were wooden houses without chimneys, large stoves; and one slept in on cold days. The hamlets comprised a dozen houses and a wooden church; the priest led his flock in the worship of a local saint. Family life was simple, honest, rude. On Sundays family councils convened at which difficult matters—rows and problems to do with sex—were solved by patriarchal deliberations under the old pear tree. There was a public house kept by a memorable Jew, the only villager in touch with the outer world. Gendarmes visited periodically, but their appearance was regarded as a calamity. We ate *bryndza* [sheep's cheese] and gruel. The local economy was potatoes, oats and sheep, and the shepherds practised alpine transhumance. The dialect was entirely different; so was the local costume. How strange it is now that there seemed to be no money! It is a vanished world!

11 The synopsis is variously titled "What is Culture?", "The ABC of Culture: A Textbook of Comparative Anthropology and Sociology" or "An Introduction to the Study of Social Sciences from the Anthropological Point of View". (Malinowski Papers, LSE. Box 1, "Culture".)

Malinowski's notes continue on another page:

> When I was eight we returned more or less permanently to the town, though I also stayed on the country estates owned by one or two of my mother's father, brothers and sisters. In Cracow we lived in an old stone building, a property of the University. It was a shabby-genteel existence, withal a truly cultured world not without dignity and heroism (see Joseph Conrad's recollections [probably *A Personal Record*]). We belonged to the dispossessed, impoverished minor Polish nobility, shading into the *inteligencja.* It was a professional world that developed after being squeezed off the land and excluded from political life. Family tradition linked us with Warsaw and Paris; we spoke and read French, sang *chansons*, and hankered after things French. This was a third cultural medium for me. France seemed a Promised Land, but it was a false and unreal Gallicism. So by the time I was eight I had lived in two fully distinct cultural worlds, speaking two languages, eating two different kinds of food, using two sets of table manners, observing two sets of reticencies and delicacies, enjoying two sets of amusements. I also learned two sets of religious views, beliefs and practices, and was exposed to two sets of morality and sexual mores (see *Sex and Repression*).[12]

Malinowski then begins to generalize his earliest experiences of cultural diversity. He is evidently seeking to formulate (for the didactic purposes of his textbook) how he acquired a predilection for the study of anthropology:

> As a child I was surrounded by racial and cultural differences. They formed part of the background of my earliest experiences. There were the lowland peasants of the plains (the Podhale), an inferior 'caste' of *chlopi* described in the works of [Władysław] Reymont, and there were the Carpathian mountaineers, the Górale. There were Jews and Russians and Austrian Germans (the swaggering Austrian officers I remember were not admired by the Russians). The Jews were always on the social horizon: their different religious and occupational character. The Jews looked different. They wore "corkscrews" and long gabardines. They also smelled differently, of garlic, onions, goose and goat, and they were afflicted with scabies. They were untouchables, infinitely more so than the Negroes of the southern United States.
>
> But every child brought up within a national minority in the USA must have had experiences similar to mine: living at home within a transported, migrant culture and at school in the American culture. Anglo-Indians too (see Kipling), and African-born Whites, and Southern states whites who played with Negroes as children, and bilingual European children born in Melanesia. Again, every Jew brought up in an Eastern European ghetto, or in Whitechapel or the Bowry, who is then assimilated to the host culture, must also experience duality. Though perhaps a

12 In *Sex and Repression in Savage Society* (Routledge, London, 1927), Malinowski had contrasted the child-rearing practices of Eastern European peasants with those of the educated classes, and both of them with Trobriand child-rearing.

> Polish child (more so than an English, New Englander, French, German or Spanish child) would have experienced the reality of culture more sharply, at times being tragically aware of the differences. After all, Poland was itself a nation of minorities dominated by more powerful neighbours. In my case there was the additional duality of assumed foreign culture deriving from France.

Malinowski then sketched another section of his autobiographical introduction, entitling it "Living a Culture versus Studying it". His notes refer to the travelling he did with his mother while still a youth. Again I amplify these notes into a coherent form, while retaining his essential phrasing.

> No language, no penetration! I discovered when very young that certain things could not be expressed in the dialect of the Gorale. There was no vocabulary for refinements, abstractions and sophistications; the rude, clipped grammar was unsuited to clear and cogent reasoning. On the other hand, it permitted strong, direct speech. It was best for swearing and expressing the crudities of life.
>
> I spent almost a year in Mohammedan countries: nine months in North Africa and two months in Turkey. Then there were my Mediterranean experiences and my two years in the Canary Islands where I experienced considerable assimilation and a mode of life Arcadian indeed (*paranda* and the *manana* complex), enjoyed through the medium of the beautiful Spanish language. I already spoke German and French, some Italian and English. I was confronted with the various local forms of Roman Catholicism: of Spain, France, Poland and the Canary Islands.
>
> Almost everyone has travelled to some extent and even lived abroad. Some throw themselves into their surroundings with a special passion: that is the making of an Anthropologist.

An extraordinary thing about this revealing evocation of childhood is the omission of any mention of Malinowski's father (that his mother is mentioned only once is a less perplexing problem). Yet it must have been in the company of both parents that Malinowski found himself, between the ages of about four and eight, among the peasants and Górale of the Podhale and the Tatras.

Lucjan Malinowski was already a professor of Slavonic philology at the Jagiellonian University at the time his only son was born. He achieved national eminence as the founder of Polish dialect studies and as an indefatigable collector of folklore. As an early ethnographer he conducted village-based fieldwork, perhaps throughout his career, and it must have been some such expeditions of the late 1880s and early 1890s that Malinowski remembered. His father had academic duties in Cracow, however, and it is unlikely that he would have remained in the villages for more than a few weeks at a time. We can only conjecture whether the little boy (with or without his mother) was left behind for longer periods — whether, in short, he experienced peasant life without the mediating presence of his father.

If, as I suspect, Malinowski was deliberately supressing any mention of his father, then the implication is clear. Dare one say what Malinowski appeared to deny by his striking silence on the matter — that he followed in his father's footsteps by becoming a linguistically astute, fieldworking ethnographer? His genius lay precisely in his remarkable linguistic aptitude allied to the equally remarkable range and acuity of his ethnographic observation, talents that he creatively combined into in a fruitful theoretical vision for a scientific anthropology. It must surely have occurred to Malinowski that he had inherited some of his father's abilities.

Lucjan's biographers are as silent about his son as his son was to become about his father, for it is not only in this document that Malinowski neglects to mention him. In all of the diaries that he kept intermittently between 1908 and 1918 (amounting to several hundreds of pages) there is only one passing reference to Father — given in the context of a painful memory of his dead Mother — on the very last page of the very last diary.[13] Similarly, in the enormous correspondence between Malinowski and Elsie Masson only twice does he refer to his father — once in relation to his mother's love, and once to make an invidious comparision: "At times I catch myself in moments of paternal gaucherie which remind me of my father's rather unfortunate treatment of myself...".[14] All the initimate sources testify to Malinowski's great love for his mother; of his father they are either silent, evasive or blunt about his dislike of a dimly perceived "stern and distant" figure who "did not try to understand his son".[15]

This poses another set of general questions for the biographer: how important are parents for the understanding of one's subject? Suffice it to say here that the life and works of Bronislaw Malinowski do reflect the positive influence of his devoted mother and also the more ambivalent and shadowy influence of his emotionally distant father. Lucjan had perhaps even done Bronislaw a favour by dying when his son was still a boy. This apparently cruel judgement is prompted by Malinowski's own reading of his Oedipal impulses. In an unpublished footnote on the variable phenomenon of repression which he drafted for *Sex and Repression* Malinowski "confessed" to his notepad:

> Incest dreams very frequent & very distressing... Father hatred. Strong attachment to M.—inferiority to father. Death wishes. F. speaks about his death. Desire. Dreams of his death. Aft[er] death very strong conscience. Further back. Attachment to mother. Desire to go to bed. Same dreadful feelings when [I] leave [her] as when violently in love etc. Not repressed. Composed out of elements very distinctly in memory.[16]

Could any biographer, even one skeptical of Freud's brand of psychological determinism, ask for more? It is almost too good to be true: incestuous desire for mother, feeling of inferiority with respect to hated father, guilty conscience over father's early death... These troubled emotions surely stirred and spurred the man who made of anthopological fieldwork a new art (and a new fetish) and who contributed so significantly to linguistic theory. In both endeavours the son brilliantly eclipsed the father's achievements. On the evidence of at least one salient strand of his anthropology, Malinowski appears to have direct-

13 *A Diary in the Strict Sense of the Term*, Routledge, London, 1967, p.298.
14 Helena Wayne (ed.) *The Story of a Marriage*, Routledge, London, 1995, vol.2, p.129.
15 Helena Wayne, "Bronislaw Malinowski: the influence of various women on his life and works," *Journal of the Anthropological Society of Oxford*, vol.15(3), 1984, p.190.
16 Malinowski Papers, Yale University Library, Box 17, Folder 218.

ed his attention to paternity in the abstract. Having made strident claims that "savages" were ignorant of biological paternity, he built social fatherhood into his kinship theory. Here, too, was a man who became an internationally renowned authority on sex, marriage and the family, who wrote the articles on Kinship and Marriage for the *Encyclopaedia Britannica* (1929 edition), and whose contributions to the cross-cultural study of the family were summarized in an influential article entitled "Parenthood — the basis of social structure".[17] It is significant that Meyer Fortes (one of Malinowski's pupils and one of my own teachers), without being privy to Malinowski's Freudian musings on his parents, could write:

> Malinowski's debt to psycho-analysis is obvious in much of his work... Indeed I would maintain that it was the notion of the Oedipus Complex that gave Malinowski the main inspiration for the main thesis of his kinship theory.[18]

In this essay I have sifted through a few of the myths in which the unique career of Bronislaw Malinowski is clothed and I have tried to identify elements of historical truth in them. I have also suggested that because Malinowski himself took some of Sigmund Freud's theories seriously his biographer must do likewise, if only to the extent that they influenced Malinowski's self-understanding. At the very least, they warrant a thoughtful consideration of his memories of childhood.

The answer to the question with which I began has, predictably enough, proved elusive, but we have cleared away some of the dead branches (*The Golden Bough* among them) that hitherto have obscured the biographical view. We may venture the provisional conclusion (pending the discovery of more biographical information) that Malinowski was attracted to the study of anthropology by his very early confrontation with marked cultural differences, by his multi-lingual experience of "duality" (which recalls Conrad's self-designation Homo Duplex), and not least (implicitly deny it as he might) by the compelling model of the Ethnographer set by his own father. Finally, there was the finishing school of his extensive travels throughout southern Europe and North Africa with his mother (edited though she is out of his notes). It begins to appear as though young Malinowski's call to anthropology was over-determined. Whatever inspirational role James Frazer (and later Joseph Conrad) may have played in the process it was not a direct cause of his becoming an anthropologist.

I shall give Malinowski the last word. In the personal diary he kept during a lengthy stay in Zakopane in the Autumn of 1912, he reminded himself: "... my most important, my first creation is the creation of myself, of a life in depth, in the deepest current. Other forms of creativity are only secondary products." At the age of 28, he was thus declaring that his principal task was to create himself, a task which preceded any question of what particular career he should follow. Indeed, it seems that he never stopped making and re-making himself. As in the widespread folktale of Earth-Diver (and incidentally in the teachings of Nietzsche's *Zarathustra*, a youthful influence on Malinowski), the key to outstanding achievement in any superhuman enterprise is self-creation. In this archetypical endeavour fathers must be not merely superseded but transcended.[19]

17 First published in 1930, reprinted in Malinowski's *Sex, Culture and Myth*, Rupert Hart-Davis, London, 1963, pp.42-88.
18 Meyer Fortes, "Malinowski and the study of kinship," in R. Firth (ed), *Man and Culture*, Routledge, London, 1957, p.169.
19 Alan Dundes, "Earth-diver: creation of the mythopoeic male," *American Anthropologist* vol.64, 1962, pp.1032-51.

POVZETEK

OBLIKOVANJE ANTROPOLOGA: OD FRAZERJA DO FREUDA V ŽIVLJENJU MLADEGA MALINOWSKEGA

Po mnenju avtorja je precej lažje odgovoriti na vprašanje "kako" je Bronislaw Malinowski postal antropolog kot pa "zakaj". Young pravi, da je postopno vključevanje v kariero bolj običajno izkustvo kot pa nenadna preobrazba. Young tu prvič predstavi doslej neobjavljeno biografsko notico, ki je bila odkrita med imigracijskimi dokumenti v državnih arhivih v Pretoriji v Južni Afriki. Malinovskijevá "etnografova magija" je bila zasnovana na poglabljanju na terenu, obvladanju lokalnega jezika in metodi opazovanja z udeležbo. Young povzame svojo staro trditev, da Malinowski ni bil nikdar "interniran" na Trobrijandskih otokih kot so trdili nekateri, pač pa je odšel tja in se je kasneje tudi vrnil po svoji lastni želji. Kot glavni razlog zakaj naj bi Malinowski postal antropolog, Young zavrne tako Malinowskijevo poznavanje Frazerjevega obsežnega dela zbranega v več zvezkih pod naslovom The Golden Bough, *kot tudi njegovo romantično željo po neznanem in eksotičnem. Young trdi, da Malinowski ni bil niti neustrašen raziskovalec niti odkrivalec nedotakljivih plemen. V Malinowskijevih arhivih na* London School of Economics *v Londonu je Young odkril sedem strani sinopsisa za uvod v učbenik, ki naj bi ga Malinowski na povabilo iz ZDA napisal. Besedilo ni bilo nikoli objavljeno. Iz tega rokopisa lahko opazimo dve značilnosti, ki sta zaznamovali življenje mladega Malinowskega. Najprej so to zgodnje izkušnje z različnimi kulturami, potem pa — kar je precej bolj pomembno — njegov Ojdipov odnos do očeta, profesorja slovanske filologije. To zadnje je Malinowskega gonilo, da se je trudil preseči svojega očeta — zato tudi tak poudarek na jeziku — ter da je iz antropološkega terenskega dela naredil poseben, že kar fetišističen izziv.*

WANTOK KAIKAI WANTOK[1]: THE IRONY OF PARTICIPANT OBSERVATION OR, PERSONAL OBSERVATIONS

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INTRODUCTION

This article is the result of my own experiences in the various anthropological fieldwork exercises carried out amongst my own people. I have so far conducted several fieldworks in my own island within the Massim region and elsewhere in Papua New Guinea.[2]

It is from such fieldwork exercises that I have gained and derived many of my experiences outlined below. As exemplified in Malinowski's introduction (1922), this discussion centres around my own predicaments and dilemmas on how to confront local issues with the ever-overbearing objective scientific approach lurking at the back of my mind. It is the difficulty of having to deal with the uncomfortable fieldwork position of etic versus emic views that is at issue herein.

A word on Malinowski's (1922) instructive exegesis on fieldwork methods is necessary before I proceed with my own fieldwork experiences. The burning issue, it seems for me, is Malinowski's emphasis that:

> We have to study man, and we must study what concerns him most intimately, that is, the hold which life has on him...To study institutions, customs, and codes or to study the behaviour and mentality without the subjective desire of feeling by what this people live, of realising the substance of their happiness — is, in my view, to miss the greatest reward which we can hope to obtain from the study of man (1922:25).

Firstly, despite his passionate attempt to prescribe for the toolkit of the anthropologist as to what is required, I personally find his three conditions of fieldwork requirements

1 This is a PNG Tok Pisin phrase that I have chosen to represent the moot point for discussion about my own fieldwork experiences. The literal translation is something like "eating one's own kind"; however, in its proper context, it represents a competitive situation whereby "one is pitched against one's own kind"; whether in sports, gardening, fishing and/or in most endeavours in life.
2 Such fieldwork exercises include, Gardens of Sabarl Islands, Louisiade archipelagoes (1985), a social profile of the Trobriand Islanders in Port Moresby (1986), a social impact study of the Wapolu gold mine, Fergusson Island (1987), a survey of the Misima Material Culture (1988), a fourteen-month PhD fieldwork on Fergusson Island (1988-1990), a survey of the Mweuya burial caves in the Trobriand Islands (1989), a reappraisal of the operations of the Milne Bay Fishing Authority (1990), several occasional social surveys in the Trobriand Islands (1984, 1994-1996, 1997, 1998), social profile of some coastal villages of Central and Gulf provinces (1995-1999) and most recently, a preliminary archaeological excavation in the Trobriand Islands together with a Swedish Archaeological team (1999). Earlier on, I conducted archaeological fieldwork in the Kaironk region of Madang province (1985) and the renowned Huon Terraces of Morobe (1985).

pretentious and somewhat superficial. According to Malinowski (1922 : 24), the prescribed three avenues through which the goal of fieldwork is to be achieved are:

- *the organisation of the tribe, and the anatomy of its culture* involving the method of *concrete statistical documentation*,
- *inponderabilia of actual life* and *type of behaviour* and,
- *corpus inscriptionum.*

There is no doubt that these three avenues have thus far remained valuable guides as to how one might go about fieldwork. However, perhaps a closer examination of them is now due, as it is a duty for all fieldworkers to draw from their own experiences and test their applicability. Surely, when these guides were drawn then, many of the societies were not as exposed to capitalist and modern social systems as is the case today. My fieldwork experience in the last two decades poignantly suggests that Malinowski seemed to have missed one very important factor about fieldwork. Particular reference is made to the kind of fieldwork where the investigators are required to immerse themselves fully into the knowledge, attitudes and mentality of the subjects. Here I am especially referring to the *time factor*. Since, after all the trouble of providing instruction, the fieldworker is still left with much quandary as to how much time is really required to fully venture into the depths of a subject's mentality. I would personally prescribe a much longer period of time ranging between say five and ten years, or even more. Shorter than that, the fieldworker will necessarily miss out on the excitements and realities of anthropological fieldwork pointed out in the above quotation. Furthermore, fieldworkers will spend a good deal of time and resources in making numerous intermittent revisits so as to caulk the persisting gaps in their knowledge of the society. The question is, then: is it really necessary for us to remain stubborn with shorter fieldwork periods or, for once allow ourselves the luxury of providing more time for fieldwork?

Secondly, Malinowski's concluding statements about a sound scientific study of man are at best pretentious, and indeed the detailed description of the three conditions of fieldwork is nothing short of condescending to the subject. Malinowski was already convinced that the "native" was himself a non-agency to many of his actions — by extension, choices and decisions — and therefore must be studied in this manner. And wherever it is necessary for scientific adventure, the "native's" decorum and ethics ought to be violated. Why, because "The difference is that, in our society, every institution has its intelligent members, its historians and its archives and documents, whereas in a native society there are none of these" (ibid.: 12). He seemed to be carelessly swaying between the scientifically accepted Western forms of integration and systems and the local or indigenous (foreign to Western) systems. On the one hand, he accepts the natives' intellectual capacity and their social institutions (ibid.: 10-11) as organized and ordered entities in their own ways, and rejects them on the other (ibid.: 12). The upshot of his apparently uncomfortable intellectual position is that his concrete data could be easily subjected to an undue distortion when juxtaposed with the foreign categories, paradigms and forms of integration. Time will tell whether this was, in its entirety, scientifically objective, or whether it was tinged with the forces of some "refined" and politically-accepted culture.

> Having other forms of integration in our analytical toolkit is undoubtedly useful. Nevertheless, there remains the possibility that looking for forms of integration

> makes presumptions about the organization of these systems, that are to begin with, unwarranted (Damon 1999:6, cf. Persson 1999). [3]

It is now well over seventy years after Malinowski since a Trobriand Islander decided to disclose hidden secrets of learning in order to demonstrate the restricted spheres of knowledge.[4] In fact, there has been some mild controversy over John Kasaipwaplova's purported impropriety in revealing to all, what is considered as exclusive *dala* (matrilineage) property, as it were. This comment is not intended as a justification of my own failure to collect such long lists of magical formula, but is rather a reflection of my cultural sensitivity over these matters that has prevented me most from doing so. For I knew very well that collecting is one thing but, having the "heart" (faith for some), sincerity and preparedness to undergo the required rituals and procedures, including the displacement of the physical context of the whole act, will no doubt render my specimens lifeless! I decided not to pluck them out of their cultural context.

One wonders, then, whether it is best first to live through the "whole act" (that is, experience it fully), then, later on, study, reflect and critique upon it. For, until this is achieved, we are but fooling ourselves with superficial data in order to demonstrate the *fundamental natures* of man and societies.

My varied experience amongst my own people has placed me in so many dilemmas and frustrations over this very guilt as to whether I had really understood what I set out to understand and inscribe for a wider audience. These frustrations were brought home to me far more candidly whilst carrying out the fieldwork exercise for my doctoral thesis.

> There were also contradictions within the role of participant observer that I found frustrating and even stressful. It was uncomfortable to try to join wholeheartedly in people's activities while at the same time to try to remember to remain detached in order to observe. This was particularly the case during major ceremonies which might require my involvement. It was eas ier to be either a participant or an observer. Either I joined in and learned their way of gardening, feasting and so forth, or I stuck to my notebooks, cameras, and tape-recorder and became a lame chronicler of the proceedings (digim'Rina 1995: 18).

This frank admission was initially criticized by one of the three examiners of my PhD dissertation for "... being confused with the method of Participant Observation". I do

3 Using the Kula model of regional integration and other recent finds in the Massim, Damon argues that there are many other indigenous forms of regional integration and notions of interdependence in existence. So far however, most may have suffered their own fate emanating from the researchers' own perhaps unconscious mistake in not giving due recognition, particularly when they become juxtaposed with Euro/American forms of integration. He makes a valid point in that gobalism, as it is, could be yet another revelation of a current worldview not as it were and/or should be.

4 In Malnic's recent book titled Kula (1998:22-29), John Kasaipwalova unselfishly provides us an eloquent explanation of the symbolic meaning of the magical formulae of Monikiniki used for *mwasila* in Kula. It is this type of intellectual organisation and system of thought and learning that had been missed by many an anthropologist since the beginning of the last century. Malinowski and others that followed collected numerous magical formula but, like dead leaves, they all lacked the excitement he was preaching of for the *inponderabilia of actual life*. So how does one achieve such states of learning? The answer is again, *more time*. Like a degree or diploma program, there are requirements in which the possessor of the knowledge is the sole judge. Depending on one's loyalty, dedication and commitment, which are usually manifested in servitude, continuous major gifts and economic and political support rendered to the elder, one could, however, easily miss out. No doubt, birth rights gender and generational-status count where it matters.

acknowledge the presumed clarity of the method of Participant Observation as prescribed by Malinowski (1922). However, I cannot help but remain critical of the apparent open-endedness of the method's parameters and its inability to suggest to the fieldworker the strategies of choice between a lame chronicler and an active participant. That is, whether one's conscience is clear or warped during those instances while making the choice. Hence, could one participate fully whilst observing? If not, has one "immersed" oneself sufficiently or even fully in the cultural activities, so that one's recording of events and activities are not deprived of their cultural context? For want of better phrases, the method is not Participant Observation but, rather, Situated Observation. It is not too far from observing a column of ants or bees, even if one speaks the language and understands the semiotics of the culture concerned. Since, the aim of all these observations is to discover the *skeletal* common patterns devoid of juicy/facts and/or frills that however, provide *life* and *vitality* to the basic form. Otherwise, life is not worth living since it is about *how one travels, rather than the destination* one seeks.

Notwithstanding, Malinowski was not totally unaware of these problems and requirements. Thus, "... in this type of work, it is good for the Ethnographer sometimes to put aside camera, notebook and pencil, and to join in himself in what is going on" (1922: 21). Unfortunately, and again still suffering from the syndrome of "high culture verses low culture", he quipped as to whether it would at all be possible for Western Europeans to naturally fit into a "savage" act. Certainly, Malinowski did not find this a problem and attributes it to his "Slavonic nature" which he suggests as being "... more plastic and more naturally savage than that of the Western Europeans" (ibid).

The point is, there are a host of other contingencies involved, whereby all had a part to play in impeding the learning processes of qualitative data collection. In fact, at the end of eighteen or fewer months of supposedly living among the people, one finds that there was in actuality very little time devoted towards real learning in order to understand, let alone seriously engage in the various forms of social intercourse that are going on. What becomes obvious, however, is that more time is spent on having to "twist" the little data collected into some kind of relation with a fancied model. Failing that, leave it is a standard ethnography overloaded with bibliographies of similar ethnographies. We must admit that these models have never been totally free from cultural biases, prejudices and stereotypes. Let me leave Malinowski here and move on to the other issues this paper sets out to discuss.

QUALITATIVE METHODS

My own training as a social anthropologist began at the University of Papua New Guinea and continued at the Australian National University on a PhD program. In both areas, there was a strong touch of the British School tradition in social anthropology. My own avid preference for qualitative over quantitative methods, even if culturally ingrained, was quietly encouraged and even tolerated by those traditions. I often move around with a prejudice and even dislike of statistics or quantitative data. Although I do appreciate the values of meticulous recounting and tabulations of data, often times I found it constraining, even restrictive. It was as if the data were an impediment towards progress and greater face-to-face interaction for a deeper understanding of issues through total immersion into the community along their own learning processes.

It was therefore not so surprising that my base of *concrete data*, as it were, was characteristically confined to census reports on individuals, households, lineage groups, and items of produce from economic production. For my study, what was needed was what the people had, and needed to be enumerated and not the discourses on politics, cosmology and exchange, for instance. Diagrams illustrating basic themes together with discovery of patterns are quite simply unimportant, and their function towards acquisition of better knowledge — if I may say so — seemed to be a red herring. Certainly statistics do matter, but they should rather be values derived from and guided by the skills learnt with which rhetoric is used to achieve a final goal. However, the use and/or abuse of statistics is certainly derived from the sometimes-misguided notion of attempting to provide a democratic representation of everyone's opinion on any issue. This is regardless of whether one's opinion does carry any scientific value or even cultural significance with regard to the question under investigation.

For instance, from the perspective of the local landowner and gardener, I cannot comprehend why a researcher would go through the trouble of having to meticulously calculate the amount of potatoes, yams or taros cultivated, number of seeds planted, or the amount of kilojoules gained from a particular plot of land. Rather what is more important, and necessarily significant for the cultural logic, is to focus on how the produce is distributed and used, and what are the reasons behind this and the local cognition of determining the quality of the product. Since that cultural cognition certainly determines the destination of the produce along the existing nexuses of exchange. Ultimately, the social consequences of each act and decision taken by an individual will thus make a lot of sense. It is therefore the duty of the investigator to learn and uncover such rules and procedures of culturally logical choices, since that is what makes life interesting and liveable for the people. I accept that each research project has its own objectives, although some do require a greater amount of their data to be represented with statistics. And indeed statistics do clarify certain social issues. However, the point is that one cannot critique a cultural system on the basis of statistics alone; instead one should delve into the cultural logic in order to appreciate why certain practices are this way or that.

I suggest that the qualitative method is the avenue to begin with in order to understand the cultural logic and indigenous forms of integration. The conventional anthropological methods are — if I may say so — an excuse to eschew the apparent greater length of time required in studying the ways of a group of people. Priority should really be given to more time for greater familiarity with the social structures and relations entailed therein, rather than the cold statistics. As such, it is advisable that one might as well begin with learning how to behave (ethics, morals, etiquette, decorum, etc.). This entails acceptable relationships and proper attitudes towards issues of gender, generation, status and intergroup relations. While the knowledge may not necessarily open the Pandora's box, it does certainly lay down the foundation for a lasting and more sincere relationships between informants and the researcher.

RELATIONSHIPS WITH INFORMANTS

While reconnaissance trips are entirely necessary in order to establish rapport, my own experience amongst my own people tempts me to draw at least four concentric circles so as to illustrate one's entry into the field of informants in the process of establishing rapport.

1. "*Friends*" including childhood mates, ex-school mates as well as former working colleagues who happen to be within the social field of investigation. School teachers, medical orderlies, missionaries, and district officers initially fit into this category. These contacts often serve as very effective starting points in establishing the initial rapport.
2. *Relatives consisting of lineage and clansmen, which also extend to include adopting family members of the researcher*. This category of helpers to one's research subject invariably stand in a "love-hate" relationship. They can be very helpful when the chips are down, yet at the same time burdensome with their constant demand for one's time and resources. Simply because they believe it is their right to demand time and goods from the researcher who has a moral obligation to behave in such a way.
3. *Key informants include knowledgeable persons, big men and chiefs*. This category of informants operates at a level which is usually restrictive and sensitive, as it is based on the highest political platform. Particularly, those key informants whose political positions are overtly sanctioned by the society or the immediate community. As such, one's relationship with them could necessarily constrain one's ability to penetrate the core of knowledge. Sometimes, one is required to compromise one's ethics of research in order to gain better access to the information sought. However, what is so rewarding about this approach is that it almost inadvertently leads one to a greater understanding of the local politics and other related but essential issues. Many of the so-called fieldwork "mentors" come from within this category of informants.
4. *Collaborating colleagues out in the field*. Ideally, this category of helpers often proves to be the best combination in research endeavours, so long as there is no conflict of interest (practically or theoretically) involved.

The dilemma however is whether to immerse oneself into the social milieu of the community or not and, if one has already done s, whether to continue. This seems to be the enigma of every fieldworker. My experience reveals a lot of shortcomings resulting from the following situations.

A. *Political or Religious Discrimination*. Quite often I find that certain individuals are already distanced from me even before I could make an attempt to speak to them. Either they belong to a religious sect that I do not favour or, because the political position occupied by them is not in line with the people I have initially associated myself. Recently, while carrying out an archaeological survey in the Trobriands, I decided to quietly sneak into the research camp and gradually establish myself there. Such that I appeared more as a researcher than a Trobriand Islander. Weeks later the hosting village discovered that I was a Trobriand Islander from such and such a clan, matrlineage and son of Digim'Rina from Okeboma village. My own clansmen from the hosting village obligingly queried me frankly as to why I did not tell them so in the first place. My response was that, if I were to do so, I would not have the benefit of seeing the whole picture of the village's politics and social structure except the one presented to me by my own clansmen. Fortunately, they concurred with me that I needed to have a better understanding of the village social set up and not to have been prejudiced by clan views over the overall village affairs. And so my clan ties remained intact even if the local inhabitants felt let down initially.

B. *Gender Discomfort.* My very own discomfort in engaging in research contact with women, particularly younger women, for information elucidation affected the gender balance of the content of my data. However, the compensation is often justified by the common fact that most women seem to have very little to comment upon the issues I intend to pursue. This is largely attributed to the ever-present view that women remain lacking in knowledge, and in any case such information is easily extracted from their male counterparts. While I see this as no explanation or justification for the deficiencies of my own fieldwork tendencies, it is indeed true unless one is investigating an entirely women's domain, such as menstruation practices.

C. The same could said of the *younger generation* (men for mine), including those at High school, who invariably provide no more than unrealistic dreams about what they might like to do in future. While their reaction may have been largely influenced by the exogenous ideas of future professional aspirations it is, insofar as I am aware, a result of the convoluted and distorted presentations by people who know so little about life after school. This is especially true for most parts of Papua New Guinea rural areas.

It is also commonly experienced by fellow researchers that, as foreigners in a society, one's sensitivity of local customs is almost near to nothing. Not infrequently one's blunders are compared to those of a toddler, "bushman", "native" and even the "stoneaged". I personally find that this lack of sensitivity is detrimental to progress and effective communication, since it necessarily denies access to information in its proper context. It is also an uninvited guest to avoidable conflicts. Worse, however, are the deliberate intentions of researchers to totally disregard such issues, as if they were entirely insignificant.

Where possible, the choice of informants should be carefully considered in order for one to discriminate between sweeping misleading statements and the genuine ones. Again, the time factor is probably the only one that might be able to fill in the lacunae between personal and theoretical bias, and even status discrimination. Although relationships with informants will forever remain demanding, they are nevertheless an indispensable fieldwork condition which has to be maintained.

EMOTIONAL LIFE OF A FIELDWORKER

The emotional life of the fieldworker is, by and large, constantly moving along the planes of the relationships one has with the informants. Again, it can be either very advantageous, auspicious or, very demanding thus resulting in frustration and perhaps leading to the severance of potential sources of information (see for example Kuehling 1998: 21-28). Kuehling has indeed showed that certain characters of informants are clearly not conducive to proper conditions of fieldwork, and in the long run may prove to be pernicious to one's own research and the final outcome. On the other hand, there could spring to the fore certain characteristics of behaviour which may be judged as exceptional to the norm.

As anthropological fieldwork is based on social relationships, it seemed that the first area to begin with is to learn how to behave properly amidst a myriad of relationships between individuals, groups and/or categories. Most sensitive of all would be relationships with informants who could potentially drag one into very sensitive aspects of life, such as sanctioned sexual relations and politics, not least concerning land matters. I have had my own share in the field in which I can now thankfully claim good fortune. I was involved in a sensitive land matter which in the process taught me lessons I would not have learnt in the ordinary investigative ways (digim'Rina 1995: 208-209). Quite simply, the lesson learnt was not going to emerge were I to thread along the conventional methods of inquiry.

The greatest concerns for the fieldworker, however, are with the health and well-being the individual (or that of the family) and the time factor in relation to data collection and research schedule. Additionally, food and research requirement supplies — often exacerbated by transport problems — make research life terrible. These misfortunes are invariably mitigated by the very generous gestures of assistance from the most immediate informants. Inadequacy of resources has a further bearing in that, as a result of researcher-informant relationships, obligations necessarily go along with it. It is indeed the dilemma of having to provide enough in order to ensure that feasts and rituals are enacted. This can be rewarding for one's research but, depending on how well or badly one plays the game, one could drastically jeopardize the future of one's research project.

There have been societies I have worked among whose lifestyle got me carried away to such an extent that research was almost neglected. Playing football, gardening, fishing, hunting and travelling with the people, I found, were very rewarding both for my research as well as for getting to know the people better. Sometimes these engagements assumed a therapeutic function, in that they brought me to a reality away from the boredom of "asking, asking and asking" what may be trivial and sometimes silly questions to the people. While I came to understand a lot, at times I found that there was little time for me to sit back and think analytically over the events in relation to the outline I had set out to enrich.

So, the proper maintenance of relationships does matter, since these are usually realized through how well one meets one's own obligations. Having said that, however, I find that these are not rules, but rather principles of operation. Quite frequently people provide allowance for one's own shortcomings, and there are avenues for apologies and redress. One's own health however, remains the greatest threat to the progress of fieldwork.

While my relationships with my own informants do not always benefit my own research problems, I am nevertheless convinced that, for a deeper understanding of the people's mentality, it is worth investing the time and effort. Usually, the initial stages are very time-consuming, involving much wastage of one's own resources. In the long run, however, most people do appreciate the efforts and are most willing to come forth with a more accurate representation of their views about their culture. Seasoned fieldworkers will agree that things seemed to fall into place, and rather suddenly, at the point of departure. Could it be that, by about the fourteen month of fieldwork, it is really the moment to reconfigure the original outline of one's plans of study as well as the initial formulations of the outline of the society, et cetera?

COMMUNITY CONCERNS AND OTHER ISSUES

While dealing with the individuals, I also realize that the community must be addressed differently. This is partly due to the fact that I cannot reach out to every each member for information. Additionally, not everyone would be able to understand and thereby appreciate my research efforts — even less the potential benefits, if any, that might result from it. I find it useful to request from the village elders and leaders for about an hour or two of a public speech or even forum with them right in the middle of the village. This conveniently occurs after the usual evening prayer meetings or other. These meetings enabled me to explain, in the simplest of terms, what science is all about, who I was, what I intend to do, the implications of the study to their daily chores, forest, beaches and garden lands, and even local politics too. Not infrequently, I also stress the importance of modern education for them, that is if geographical distance is a disadvantage.

Virtually, the whole forum has an open-ended agenda whilst initially beginning with my own research purposes. Invariably people are curious and yet only a few questions are asked. It is nonetheless clear that people are happily satisfied that my work is perhaps necessary and should be assisted where required.

My own experience with research procedures in Papua New Guinea is that it is fraught with unnecessarily prolonged bureaucratic bungling. To make matters worse, the provincial governments invariably fail to inform the district administrators in time for the researchers to gain entry into the field; not to mention the village councillors and the concerned local land owners. Much to the bewilderment of the researcher, one is confronted with huge sums of Kina to pay for the duration of stay while desperately waiting for a good Samaritan to show up as a trusted host. This area certainly requires improvement. Misconception of the differences between scientific researchers, missionaries and tourists abound, having varied results and even some with very serious repercussions. As a result, research schedules are set back by weeks and months, together with the unwarranted inconvenience caused for the innocent researchers and hosts alike. Hence, the meetings conducted are intended to mitigate the misunderstanding of the local villagers in taking the researcher as a tourist, business person or a "filthy-rich miner". On the other hand, these initiatives usually bode well for the researcher bringing forth, in the open, his intentions whilst generating respect from the villagers. Not infrequently, misunderstanding of one's research intentions leads to envy and jealousy amongst the villagers and between the researcher and the community. Money and fees for residence and access to areas of research has become the talking point for negotiation, thus complicating what used to be very simple straight-forward agreements between any two parties, even at village levels.

As there are far too many levels of government to go through for research permits, it is therefore important to consider the research fees paid to the National Research Institute reduced and/or shared with the authorities at the provincial, district and the village level. Certainly, better communication between the various channels involved is urgently in need of attention. The National Research Institute (PNG), the National Museum and Art Gallery (PNG), the provincial, district and village councillors must work rather more efficiently in addressing the issues of FEES and PROTOCOL liaison. As is often mentioned, "too much politics" is creeping into scientific research, which indeed should remain politically neutral.

CONCLUSION

In my view there is an urgent need to seriously reappraise our methods but, more especially, the way we perceive the relationships we establish with our informants during and *after* fieldwork. It is indeed more to do with how seriously we take on the informants' views and whether we have really understood and translated the information, as it was given. It is very clear throughout the numerous acknowledgements contained in the books and publications that very little due credit is given to the informants. In its stead, a greater focus is placed on library research, colleagues support and the sponsors. I take nothing away from those support aids; however, the informants must be equally and satisfactorily acknowledged for their invariably unrewarded efforts. The best reward indeed lies in the greater recognition of their own cultural logic and knowledge.

We must also consider the wider scope of sourcing of information, perhaps at regional level, apart from library research for existing literature. There is much to be learnt from the field, and efforts must be given towards a deeper understanding of the people's forms ~~of integration not just~~ those from the literature. I understand that it may be very unrealistic to many, but I am inclined to suggest that a broader social field of fresh studies would be the way to go. Greater collaboration among researchers between cultures and research ~~institutions would certainly appeal to sponsors of any study project.~~ For the sake of today's and tomorrow's anthropology, more training efforts should be directed towards presently marginalised cultures who in turn have been the laboratories for ethnography studies in the last century. Collaboration with capable local researchers will no doubt enrich the data while reducing the project duration. And yet the knowledge and findings made might have spanned well over twenty years, taking into account the accumulated knowledge of the local researcher together with the immediate project's duration.

Returning to the subject of discussion, certainly the data collected from the field ought to be questioned in light of the politics surrounding it, the prejudices, biases and stereotypes affecting the formulation of models whose bases, however, are derived from the so-called loose facts from the field. The interplay of cultural prejudices between one's own and the subjects', on the one hand, and the global/Euro-American forms of integration on the other, must have great bearing upon the final outcome of our models. There is certainly room for indepth studies of cultures simply for a better and more varied knowledge of those marginalised forms of integration. Falling short of that, we stand to be accused yet again of providing merely superficial structures built upon very fragile bricks — as has been the experience in the last century.

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POVZETEK

WANTOK KAIKAI WANTOK:
IRONIJA OPAZOVANJA Z UDELEŽBO ALI OSEBNA OPAZOVANJA

Ta članek je zasnovan na različnih oblikah terenskega dela v avtorjevem domačem massimskem področju Papue Nove Gvineje. Izkušnja s terena v zadnjih dveh desetletjih mu da misliti, da je Malinowski v svojih razglabljanjih o naravi terenskega dela pozabil na zelo pomemben faktor. Pri Malinowskem je posebna pozornost namenjena tistemu obdobju terenskega dela, ki naj bi zadostovalo za to, da bi se raziskovalec lahko popolnoma poglobil v védenje, razpoloženja in duševnost subjektov, ki jih proučuje. Tu misli predvsem na **faktor časa.** ***Potem ko etnografa domačini končno vpeljejo v osnove vsakdanjega življe-***

nja, pa mu običajno vseeno ostane premalo časa, da bi se lahko poglobil v duševnost in razmišljanja ljudi pri katerih opravlja terensko delo. Avtor meni, da je za to potrebno precej daljše obdobje, nekje od pet do deset let.

Pri krajših obdobjih, naprimer po osemnajstmesečni terenski raziskavi, lahko kaj kmalu odkrijemo, da je bilo zelo malo časa namenjenega resničnemu učenju, da bi stvari, odnose in dogodke res razumeli, še manj pa je bilo pravega vključevanja v različne družbene debate, ki so krojile usodo okolja v katerem je etnograf delal. Tako postane očitno, da se precej več časa porabi za to, da se majhna količina podatkov, ki jih je etnograf zbral, prilagodi nekemu domišljijskemu modelu.

Sodobna antropologija bi morala več pozornosti nameniti izobraževanju ljudi iz tistih obrobnih kultur, ki so v preteklem stoletju bile glavni laboratoriji za etnografske raziskave. Sodelovanje s treniranimi lokalnimi raziskovalci bi nedvomno obogatilo in poglobilo zbirko podatkov ter skrajšalo trajanje projekta.

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FIELDWORK IN THE AGE OF GLOBALIZATION

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ABSTRACT:

This paper assesses the role and possibilities of fieldwork as an anthropological research method in light of anthropology's coming to terms with the reality of globalization. Globalization in this context refers to a realization of the profound changes to the relationships between cultures which were previously presumed to exist separate from one another. How has this affected the practice of anthropology, a discipline defined by its focus on cultural difference? To answer this question the paper discusses critiques of fieldwork which have problematized the equation of anthropology with fieldwork as a research method and the implications that this may have on developments in anthropological research and on the effectiveness of fieldwork as an analytical tool in a globalized world. Finally, the paper concludes with a brief introduction of the author's own field research as an anthropological project within the interdisciplinary field of science studies in order to discuss present challenges to conducting fieldwork and the possible contributions fieldwork may have to offer anthropology as well as other disciplines.

Anthropology as a discipline has been historically defined by its focus on cultural diversity.[2] Although anthropology's interest in cultural diversity may have been influenced by different driving questions during the course of its history, anthropologists have always been dedicated to the study of cultures across the globe. How has this fundamental precept been understood within the discipline? How has this understanding affected the methods employed to answer its questions and what sort of presumptions have these methods embodied?

These questions have become more relevant in recent years as anthropology as a discipline has been coming to terms with many issues, one of them being that of globalization. By globalization I am referring to the realization of profound changes in the relationship between nations and cultures which were presumed — at least theoretically speaking — to exist separate from one another. Technological advances in communication and trans-

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2 Here I would like to stress that my formation has been almost exclusively in the Anglo-American traditions of social or cultural anthropology, and thus that many of my comments will be limited to these traditions. Other anthropological traditions, including that of ethnology and anthropology in Central and East Europe, may have different perspectives concerning the issue of fieldwork. Incorporating these traditions would be a productive means of opening up the discussion of fieldwork as it has developed in Anglo-American traditions but which I will not be addressing here.

portation are understood to have brought the world closer together in what David Harvey has termed a "time-space compression" (Harvey 1989) heralding the dawn of a new age. How has this affected anthropologists' understanding of the world, and the way that anthropology is conducted? What sort of repercussions does this have for fieldwork as the distinctive anthropological methodology?

In this piece I will outline the way that anthropology is processing these understandings and the way that this redefines the identity of the discipline and the effectiveness of fieldwork as a methodology. In a presentation of the manner in which these issues affected the way I approached my own work I will discuss the challenges to conducting fieldwork and the possible contributions that fieldwork still has to offer anthropology as well as other disciplines.

Many have pointed out that the present understanding of fieldwork as one of anthropology's identifying features has precluded questioning its effectiveness. Instead, fieldwork understood as "intense, long-term research conducted among a community of people" (Barfield 1997:188) has attained the status of a rite of passage that identifies "real anthropologists" and has become the criterion by which to evaluate "real anthropology". Anthropology, according to many of its internal critics, is identified more by its methods than by its central questions:

> In other words, our difference from other specialists in academic institutions is constructed not just on the premise that we are specialists in difference, but on a specific methodology for uncovering or understanding that difference. Fieldwork thus helps define anthropology as a discipline in both senses of the word, constructing a space of possibilities while at the same time drawing the lines that confine that space (Gupta and Ferguson 1997a:2).

While I would question the extent of the straightforward causality implied in fieldwork's defining the discipline, I would agree with the point made by Gupta and Ferguson in that fieldwork's often unquestioned position in anthropology precludes a discussion of the implications of the practice of fieldwork in the development of anthropology as a discipline. I think that they, as well as many others who problematize fieldwork in anthropology today, are referring to the role of fieldwork in what George Marcus terms the research imaginary:

> What is lacking in discussions of anthropology's signature research practices is... a sense of the changing presuppositions or sensibilities — what I have called a research imaginary — that informs the way research ideas are formulated and actual fieldwork projects are conceived...This is a key area of discussion and development over a decade after the critique of ethnographic writing opened the current reassessment and readaptation of anthropology to its changing circumstances. This level of consideration, crystallized as new strategies brought to the early conception of research, anticipates many of the issues that might arise later as to what the actual implications of such research would be for the conduct of fieldwork and what the resulting published ethnographies from such fieldwork would look like (Marcus 1999:10).

Marcus locates the crucial issues of anthropology in a space before their implementation, before fieldwork, focusing on the way that projects are conceived. I would argue that the concept of research imaginary illustrates a gap between current theoretical discussions of "reassessment" within anthropology and their implementation in practice; presuming field-work to be an unquestioned, emblematic anthropological research paradigm maintains this gap. Expanding debates of reassessment should include a demystification of field research which would investigate the dialogic relationship between fieldwork and certain key values, question and concepts in anthropology. In this way anthropologists will be able to deal with the theoretical and practical challenges that the reality of globalization implies. Furthermore, given the relationship between fieldwork and theory within anthropology, anthropologists' processing of these issues in fieldwork may also provide them with insights that may contribute to discussions concerning globalization that transcend the boundaries of the discipline.

I believe that it is worthwhile to remember the historical development of fieldwork as a methodology not in order to essentialize fieldwork's origins but to counteract the naturalization of fieldwork as well as to understand the research imaginary to which it once belonged. Fieldwork did not always occupy this sort of position within anthropology. This naturalized emphasis on fieldwork came about as late as the middle of this century, with histories of alternative modes of research being relegated to the margins.

Henrika Kuklick adds her perspective on fieldwork to those of many other historians of anthropology and situates the origins of fieldwork in anthropology by analyzing the larger scientific community of which anthropologists were a part. As she writes, the position of fieldwork within anthropology was not at all similar to the current situation described by Gupta and Ferguson. Instead, Kuklick describes a strict division between theorizing and field research that was embodied in a division of labor between gentlemen armchair theorists and unskilled fieldworkers, who were even sometimes slaves.

> The intellectual elite arrogated to themselves the labor of articulating theories to account for the diversity of nature...The act of analyzing data collected by others was believed to be so straightforward that knowledge of the provenance of scientific materials was considered virtually irrelevant to their interpretation....A strict division of labor between theorists and fieldworkers was often advertised as conducive to superior science (Kuklick 1997:53-4).

Kuklick situates this division of labor between theory and practice within the context of a community of scientists before its professionalization (during a time when only gentlemen could be scientists) molded by the natural sciences whose practitioners — naturalists — were dedicated to observing and recording all natural phenomena.

According to Kuklick, debates within the natural sciences professing the relevance of experience to the production of knowledge, the professionalization of anthropology, and the restructuring of universities gave rise to a generation of professional anthropologists who quickly inverted the hierarchy of labor imposed by gentleman armchair theorists, arguing that scientific training and direct experience in the field were indispensable to the development of theory. In so doing professional anthropologists not only asserted their disciplinary authority but also combined the previously distinct roles of fieldworker and theorist. Thus fieldwork slowly attained the professional prestige accorded to Malinowski's work with the Trobriand Islanders, and anthropologists soon became known instead as those who traveled to the far (exotic) corners of the world to observe other cultures.

Before going further, I wish to emphasize that anthropology's interest in the cultures of the world inherently implies that anthropology has always been global; in this sense anthropology as a discipline has played a positive role portraying the richness of cultural diversity. Thus posing questions concerning globalization does not entail expanding anthropology's practices on a global scale,[3] it already has, but to examine the then reigning research imaginary that defined the way that this globalization was articulated and enacted within the discipline and to pose analogous questions concerning the existing research imaginary in relation to the present challenges posed by globalization. Going back to the pre-professional period described by Kuklick, the research imaginary was defined by gentlemen theorists whose thinking (in their armchairs) was molded by the larger community of naturalists and their intellectual aims. The elaboration of a single scale of human evolution as a disciplinary goal implied a belief in certain universal laws which in turn presumed particular relations among the peoples of the world and, finally, a proper way to conduct research in these terms.

> Anthropologists engaged in theoretical generalizations were able to make sense of the material they acquired from diverse sources by postulating that human development everywhere followed an invariant sequence of progressive stages, and that lacunae in their knowledge of any given people could be filled with information about any other population judged to be in an equivalent stage (Kuklick 1997:55).

The fusing of the roles of theorist and fieldworker entailed re-evaluating the role of field research, specifically emphasizing the need to observe a culture directly in order to be considered knowledgeable of a particular culture. Thus the institutionalization of fieldwork as the sole indicator of proper research in turn developed along with the construction of a particular global research imaginary based on a culture of space and difference, a construction which has now become the subject of critique.

We see here that the concept of fieldwork was not exlusive to anthropology; instead, anthropologists "borrowed" the method of fieldwork as well as their professional aims from the dominant naturalist research imaginary. One must also add here that fieldwork in anthropology later defined a particular trajectory within the boundaries of the discipline. At the same time, Kucklick's piece also makes an important point: that one must also look beyond the boundaries of the discipline, to the broader intellectual context of which it is a part, to understand the role of certain values, concepts and methods of a discipline. Attempting to resolve the issue of fieldwork solely within the confines of anthropology can be analytically misleading.[4]

As I mentioned earlier, the canonization of work such as that of Malinowski aided in constructing the image of anthropologist researcher-theorists and world travelers. However the distances that anthropologists traveled did not constitute empty space; instead

3 Actually the implications of "globalizing" anthropology by subverting the unmarked privileged position of the Western anthropologist/observer and by expanding it to include any number of possible anthropologist/observer positions across the globe would be far-reaching for the discipline.

4 See Trouillot 1991 for an excellent albeit analogous discussion of the construction of anthropology and the Other.

that distance was socially construed, representing social relations of difference defined in large part by anthropologists themselves.

James Clifford incorporates anthropological fieldwork into a larger history of travel, distinguishing anthropologists among other travelers (i.e., missionaries, traders, colonial officers) by their particular disciplinary practice of travel and dwelling, implying the mobility of the anthropologist traveling "to the field", dwelling among the culture under scrutiny, and departing "from the field" to write (Clifford 1992). The previously distinct practices of research and writing were now combined in the role of the mobile anthropologist; however, the acts of writing and theorizing were still done from the unmarked location of home (the armchair).

Here it is important to keep in mind that "home" is both a physical and intellectual unmarked position. Physically speaking it is from this presumably central position that anthropologists map out a geography of culture areas as categories of difference which become naturalized as part of the disciplinary canon. As Joanne Passaro explains, a certain culture area is not only a geographical delimitation but also an intellectual frame into which a field research project must fit if it is to be considered worthy of being carried out. In Passaro's case, she was confronted with "situating" her project within the culture area of the Mediterranean:

> Two central assertions — the existence of a unified Mediterranean area and the existence of codes of honor and shame that delimited it — provided the ontological and epistemological foundations for a whole field of inquiry that then could set itself the task of "discovering" and "documenting" its very conditions of possibility (Passaro 1997: 149).[5]

Arjun Appadurai argues that the mapping of a mobile or traveling anthropologist requires as well the production of "the incarcerated native" bounded in time, space and identity, situated in this manner by the concepts of culture area and the presumption of static societies which the practice of fieldwork seems to imply (Appadurai 1988).[6] According to Appadurai, the challenge for anthropologists lies in recognizing the fact of mobility of those identified as Others and in assimilating this into their theories and their fieldwork.

> As groups migrate, regroup in new locations, reconstruct their histories, and reconfigure their ethnic "projects", the *ethno* in ethnography takes on a slippery, nonlocalized quality, to which the descriptive practices of anthropology will have to respond. The landscapes of group identity — the ethnoscapes — around the world are no longer familiar anthropological objects, insofar as groups are no longer tightly territorialized, spatially bounded, historically self-conscious, or culturally homogeneous (Appadurai 1991:191).

The mobility to which Appadurai alludes is not simply physical but intellectual, in that the peoples who are often subjects of anthropological research are not a priori determined by

5 See also Appadurai 1988, Herzfeld 1987 and Gupta and Ferguson 1992 for discussions of the concept of culture areas.
6 See also Des Chene 1997, Fabian 1983, and Stocking 1983 for discussions on constructions of time and the practice of fieldwork.

(anthropological) constructions of location and history but that they transcend this categorization and in effect have a certain amount of control in how they articulate their identity. And here we turn to the unmarked intellectual position (counterpoint to the unmarked physical position) taken by anthropologists that I mentioned above. My use of this distinction is inspired by the way Richard Fox articulates the relationship between anthropologists' physical and intellectual location:

> It [ethnography] too often specifies a physical location — an inhabited jungle clearing, a village community, an urban barrio — in place of an intellectual position... Ethnography then has to claim authority on the basis of "having been there" and the special empathy that it creates. Otherwise how could it justify its construction of "fieldwork" as meaning physical, rather than scholarly placement? (Fox 1991:96).

The emphasis on physical location in the field and the presumed forging of a particular sort of relationship with a culture in the field based on empathy provides the basis for professional anthropological authority. Endowed with this authority, an anthropologist's physical (as well as intellectual) location — i.e., his/her experience in the field — is privileged, as is the ethnography based on this location. It is assumed that the anthropologist has a unique insight into the culture observed in the field, and his/her ethnography represents this insight. Yet this privileging at the same time implies a distance, both physical and intellectual: the physical distance between home and the field which is the distance from which an anthropologist writes about his/her experience in the field; the intellectual distance is that of the dispassionate, distant gaze of a privileged, well-trained observer, an intellectual distance retroactively imposed from the locus of theorizing: the unmarked intellectual position of the center belying the moment of empathy in the field.

The work of critics such as Appadurai have highlighted the constructed, paradoxical, and authoritative location of the privileged anthropologist (positioned ultimately in the center) and the fixed identity of those observed in the field. The non-localized quality of the ethnoscapes Appadurai describes signals a subversion of this relationship as well as a decentering of the anthropologist as the privileged observer. In fact, the challenge now lies in recognizing the constructedness of this global geography and addressing the implications that this has for anthropologists' positions as fieldworkers and for the production of anthropological knowledge. However, as Gupta and Ferguson point out, the fact that fieldwork as a method reigns as one of the defining distinctions of the anthropological discipline has allowed for the emergence of a paradoxical position:

> On the one hand, anthropology appears determined to give up its old ideas of territorially fixed communities and stable, localized cultures, and to apprehend an interconnected world in which people, objects, and ideas are rapidly shifting and refuse to stay in place. At the same time, though, in a defensive response to challenges to its "turf" from other disciplines, anthropology has come to lean more heavily than ever on a methodological commitment to spend long periods in one localized setting. What are we to do with a discipline that loudly rejects received ideas of "the local", even while ever more firmly insisting on a method that takes it for granted? (Gupta and Ferguson 1997a: 4).

The point that Gupta and Ferguson raise is an important one in that globalization, referring to new understanding of the relationships between people, places, object and ideas, requires not only a recognition of previously implicit understandings but also a reworking of existing concepts and research methods. However, the problem lies in ascertaining field-work's position in this reworking; does it necessarily embody all the received ideas of the local, as Gupta and Ferguson affirm? If so, how may this be remedied?

In order to discuss the issues that I raised above in relation to fieldwork I will discuss my own work with economists as anthropological research situated within scholarly discussions that define the inter-disciplinary field of cultural studies of scientific knowledge. At first it might seem strange that I will be citing discussions outside anthropology as a means of responding to particularly anthropological concerns about fieldwork and globalization. However, as was pointed out earlier, anthropology forms part of a broader intellectual community whose values, concepts and methods also inform trends in anthropology. As I proceed I will highlight the parallels between discussions within cultural studies of scientific knowledge and anthropology, pointing out how they may shed light on the issue of field-work. Cultural studies of scientific knowledge is one of the terms used to refer to a large body of research conducted by historians, philosophers, sociologists, scientists, anthropologists and others that is focused on studies about science.[7] These reseachers by definition deal with one of the subject matters often equated with globalization: the rise of transnational cultural formations in light of the diminished distance between cultures and nations, formations that are considered to transcend cultural boundaries or exist independently of cultures.

Cultural studies of scientific knowledge focused on the study of science as one of the formative culture formations of the modern world by analyzing it independently of the images that science had of itself. These studies are based in part on the work of Thomas Kuhn who demystified the generally held belief of science as an objective progression of knowledge towards an ultimate goal of truth and scientific method as the only valid means of attaining objective truth. In analyzing the history of scientific practice, Kuhn, as later do others, points out that the image of scientific progress does not reflect the sequence of research activities involved in the production of scientific knowledge. Instead Kuhn provided an alternative theory of paradigm shifts based on an investigation of the workings of the scientific community itself and the way that it incorporated scientific discoveries. Imagining the history of science as an objective progression of knowledge was replaced by a series of paradigm shifts (Kuhn 1970).

The work of Thomas Kuhn and others such as Paul Feyerabend, whose work focused on scientific method (Feyerabend 1978), began analyzing science not according to the standards science itself produced, but instead as a social phenomenon. Thus the focus of the history and philosophy of science shifted onto the producers of scientific knowledge, the scientists, as socially and historically contextualized actors; this inspired the first ethnographic laboratory studies aimed at exploring the role of social conditions in the process of

7 I would like to point out here that cultural studies of scientific knowledge is one of many scholarly lines of thinking that comprises science and technology studies. The brief description that I will sketch out below mentions a small number of the main scholars within this field of study.

scientific production (Knorr-Cetina 1981, Latour and Woolgar 1979). Turning the tables on their colleagues, these researchers observed interactions of scientists in the controlled setting where science was literally produced — among scientists in a laboratory — in an effort to understand the role of scientists on the way science develops.

The works of anthropologists rendered the use of ethnographic method more precise. While the scholars cited above employed such methods to gauge the effect of "social factors" on the production of science — and going so far as to affirm scientific facts are socially constructed — anthropologists focused on the concept of "social factors." For example, anthropologist Sharon Traweek engaged in a study of physicists which presumed them to be a community of knowledge, a culture like any other that has come under the scrutiny of an anthropologist:

> I wanted to find out how the physicists generate the shared ground that all members of the community stand upon; how they define the established terrain within which debate can occur, the recognized strategies for making data and equipment and reputations, and the ground rules for contesting data, machines, and reputations... I believe that to understand how scientific and technological knowledge is produced we must understand what is uncontested as well as what is contested, how the ground state is constructed as well as how the signals called data are produced. When I speak of the shared ground I do not mean some a priori norms or values but the daily production and reproduction of what is to be shared (Traweek 1988: 8-9).

Traweek carried out her research in a high-energy physics laboratory in accordance with the traditional norms of anthropological fieldwork, thus producing a fine-grained ethnography of the culture of US physicists within the confines of their community of knowledge. Her work not only expanded the horizons of anthropology and its concepts of culture, but brought to bear anthropology's understandings of culture to scholarly discussions whose main dilemma was how to conduct research of groups who by definition were considered as having no culture. For these researchers the notion of a culture of scientific expertise provided a means of thinking about scientists that was not informed by the way that scientists viewed themselves: as pursuing a form of knowledge that operated according to its own laws, independently of any social or cultural "factors." Traweek's ethnography of scientists was to transcend the distinction scientists make between science and culture, which in effect privileges scientific method and practice as a way of knowing about the world.

The anthropological premise of cultural holism — the interconnection of social spheres — enables researchers to transcend the prevailing distinction between science and culture. Here I am not referring to a sort of holism that presumes culture to form a coherent seamless whole: instead I employ the notion of cultural holism to counteract the social belief in the discreteness of certain social spheres, including, in my case, the economic sphere.[8] However, the key to conducting research in these terms is being explicit about the concept of culture upon which field research is based, and the processing of these issues. For example, conducting fieldwork based on a concept of culture as self-contained and fixed in space and time by the anthropologist risks not questioning, and even strengthening, sci-

8 The difference in cultural holisms I employ here is based on a distinction elaborated by George Marcus. See Marcus 1999.

entists' own views of science as autonomous and self-contained; this would naturalize the very boundary between science and culture that researchers wish to question.

In this light the work of feminist science studies has been useful, as their work has highlighted links between the practice of science and the gendered identity of its practitioners. The gendered nature of scientific epistemology practiced by scientists is not considered to be the result of the local context of scientific production as a self-contained sphere, but instead as part of a broader inquiry that transcends the boundaries of science (Harding 1986). Thus the consistent focus on gender on the part of feminist studies of science has shed light on the links between science and culture, subverting the discourses of discreteness and autonomy of science and culture.

This belief in what Joseph Rouse has termed the porous nature of science's cultural boundaries (Rouse 1992) enabled me to elaborate my own research project, which was an ethnographic investigation of the cultural authority of economics as a form of knowledge in Slovenia during the transition process. My interest in economics lay in trying to understand the way economic discourse as a form of explanation operated socially, without accepting the explanation offered by economic discourse itself which is in turn based on economics' image as a universally applicable form of knowledge operated according to objective laws. The transitions from communism in Central and Eastern Europe seemed to only confirm the seemingly self-evident superiority of market economics as a economic system.

In an effort to ground this abstract image, I planned to shift the focus from economics as a discourse to the producers of this discourse in Slovenia: Slovene economists. In this fashion I traveled to Slovenia in 1994, prepared to conduct a study of the cultural community of economists in Ljubljana. However, I soon realized that a study of the internal workings of the community and its developments in the last decade before Slovenia's independence was not helping me understand the way that economics as a form of knowledge operated socially. An ethnography of the culture of economists was not going to explain their cultural authority which they exercised outside the confines of their community.

I soon realized that I had assumed that knowledge about economics, as in the case of science, flowed from inside the community of expertise to the world beyond and that an ethnography of the economic community would have been the logical means to investigate the issues raised above. Emily Martin, whose latest work is an ethnography mapping out the emergence of a shift in American ways of thinking about health and the body, writes about this assumption which is in effect the very image that scientists have of the flow of knowledge between the scientific community and the outside world. When she began to trace the emergence of the concept of the immune system in everyday American discourse about health and the body, Martin presumed that she would begin with studying molecular biologists, when she realized that molecular biologists' notions of the immune system were in turn informed by the outside world. In designing her research, she attempted to trace the emergence of this idea in a number of different sites, conducting field research in places as different as an immunology laboratory, various AIDS activist volunteer organizations, a corporation, and an urban neighborhood:

> Ethnographic inquiry into the "ramified surface extensions" of processes or phenomena would be as likely to trace connections between propensities or disinclinations in the "public" and what is thought a desirable project in science, as to trace connections in the other direction (Martin 1997:138).

In the case of Emily Martin, whose research focused on American ideas about health and the body, their circulation and social power, this research format seemed to have been the most useful. This kind of research is in effect massive in scope and raises many questions concerning the implementation of such multi-sited projects. How far can a single field worker expand his or her number of fieldsites and be capable of conducting in-depth research? Furthermore, how could this affect the classic image of the lone anthropologist/fieldworker?

In my case the issue lay more in a possibly reductive approach to the cultural identity of economists. The emphasis on producers of science on the part of cultural studies of scientific knowledge has brought about important theoretical changes in science studies that were difficult to enact in research. Anthropological fieldwork among scientists expanded understandings of scientists' identity from embodiments of "social factors" informing the production of scientific knowledge to members of a community of knowledge. However, at the same time anthropological fieldwork among scientists runs the risk of over-localizing a fieldsite; I experienced this in my own research. In observing economists' practices I soon realized that most economists played very active roles outside the boundaries of their specialized community, either as consultants, media figures, businessmen, policy advisors, politicians or writers. In trying to track the role of economic knowledge and even of economists I could not limit myself to what occurred within the boundaries of, for example, the economics academy. In adding another connotation to Appadurai's ethnoscapes, my aim in my fieldwork became instead to follow the cultural geography mapped out by economists' practices as they traversed different cultural spheres and appeared in different media. Attempting to ascertain the different aspects of economists' cultural identity (while not presuming a seamless whole) became the new framework for my field research.

This in turn brings me to a final point concerning the intellectual position of anthropologists and a possible future scenario for anthropology. I have tried to highlight here the effects of over-localizing one's fieldsite in ethnographies of science, which, while it is a characteristic move of disciplinary authority, ultimately reinforces the very distinction between science and culture that one is trying to question. Philosopher Joseph Rouse links this attempt at analytical control with a particular project concerning scientific knowledge whose aim is not so much to understand the way science operates in the world but to "explain" it, to replace the scientist's own privileged scientific methods of explanation with social ones (Rouse 1992). This line of intellectual inquiry would, instead of reconfiguring the opposition between subject and object, observer and observed, simply reverse the oppositions with another form of knowledge occupying the unmarked position of objective authority. An alternative set of relations is one which economic historian Donald McCloskey outlines in terms of rhetoric as a dialogue between different self-conscious forms of knowledge:

> Rhetoric, then, might be a way to look at economic talk, and a way to make it better. Better, not less rigorous, difficult, serious, weighty...Were economists to give up their quaint modernism and open themselves officially to a wider range of discourse, they would not need to abandon data or mathematics or precision. They would merely agree to examine their language in action, and converse more politely with others in the conversations of mankind (McCloskey 1985:35).

In these last pages I have been compiling what could become the rhetorics of cultural studies of scientific knowledge, in which anthropology could play an important role among situated (albeit still differently valued) knowledges. I believe that this could also be the future research imaginary for a decentered anthropology as well as for a reconfigured and revitalized tradition of fieldwork.

POVZETEK

TERENSKO DELO V DOBI GLOBALIZACIJE

Članek obravnava vlogo in možnosti terenskega dela kot antropološke raziskovalne metode v času, ko se antropologija sooča z realnostjo globalizacije. Globalizacija v tem kontekstu pomeni zavedati se temeljnih sprememb pri odnosih med kulturami, za katere je bilo mišljeno, da obstajajo ločene ene od drugih. Kako je ta realnost vplivala na prakso antropologije kot vede, ki preučuje kulturne razlike? Da bi odgovorila na to vprašanje, avtorica v članku obravnava kritike terenskega dela, ki problematizirajo (pogosto neraziskano) enačenje antropologije s terenskim delom kot raziskovalno metodo, kakor tudi implikacije tega enačenja na razvoj antropologije in na učinkovitost terenskega dela kot analitičnega orodja v globaliziranem svetu. Da bi obravnavala izzive pri terenskemu delu ter možnosti, ki jih terensko delo lahko ponudi antropologiji ter drugim vedam, se članek konča s kratko predstavitvijo avtoričine terenske raziskave kot dela antropološkega projekta v kontekstu interdisciplinarnega študija znanstvenih ved.

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"ON THE OTHER SIDE": DOING FIELDWORK AMONG NON-WESTERN PEOPLE IN A WESTERN COUNTRY

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The distinction between "the field" and "home" rests on their spatial separation. (Gupta & Ferguson 1997:12)

GETTING THERE

On a dark and rainy day in November 1986, I boarded a plane to Singapore which should be the first stop of our travel through Indonesia. While the first few weeks were well planned, in terms of time as well as place, the rest of the journey should be guided rather by flexibility and improvisation than by strict organisation. As a result, I stayed longer than expected and went to more distant locations than I had imagined a few months earlier. Like many other travellers who had been to that area I finally ended up in northern Australia, at the end of a chain of incidental events and unexpected encounters.

After nine months of "adventure", I returned to university and a life in Vienna which, at the time, seemed fairly dull and uneventful to me. Although I still had several more courses to pass, I was advised to start thinking about a topic to write my dissertation on. Since I had never doubted that this would include an extended period of fieldwork — carried out in a far away country — I ventured the possibilities of Australia as a future field location.

From the only marginal and superficial contacts I had with Aboriginal people while living in and travelling through various regions in the north and centre of the continent for more than three months, I was quite aware that "black"-"white" relationships in those areas were far from easy-going or tension-free. I am still not sure why and how I was so certain, at the time — despite more or less subtle "warnings" from Australian scholars —, that such difficulties would not stop me from my project and that, somehow, I would overcome them. Had I realised that those anthropologists did not generally object to my plans but only tried to raise my awareness to some problems that, most likely, I would face, I might have given up in the first place. But since that was not the case, I proceeded with the academic and logistical preparations of finding an appropriate field site and eventually getting there.

WHERE IS THE FIELD?

Despite repeated criticism, and perhaps due to a lack of better alternatives, "participant observation" continues to be a main component of anthropological fieldwork from which the discipline largely draws its identity and uses it as a distinction marker towards others. Although most anthropologists no longer hope for an idealised field situation pictured in a

Malinowskian way — that of "total immersion" which would lead us to our final goal, namely, to "grasp the native's point of view" — the basic principles have not changed: the idea behind still is that living among the people we study and participating in their everyday life would enable us to observe their behaviour and reactions in a wide range of situations, under different conditions, and towards a variety of other persons. Furthermore, such long-term involvement with a fairly large number of people and building up rapport with several of them, at least, would place ourselves in a favourable position as inquisitive researchers.

Having experienced a fairly traditional university training in social anthropology, it seemed to me, too, that participant observation was the most adequate method, or rather "research strategy" (Davies 1999:67), to choose for my own fieldwork among Australian Aborigines. Today, several years after having completed the work and the doctoral thesis which followed from it, I am still convinced that it was the right thing to do although, at the time of my decision, I had only a theoretically shaped and, thus, vague idea about the general implications of such an undertaking and the concrete conditions in my future fieldwork location.

In the last ten years, I have often asked myself about my motivations for working in a society whose "traditional culture" had already been well studied (some would even say "over-studied") by other anthropologists and whose members live on the margins of a dominant Europeanised society. There certainly is more than one answer to this question; one is that I was fascinated by the idea of "crossing borders". It was the same situation which had attracted me in the beginning that later proved to be one of the greatest obstacles to research.

Although my sojourns at Aboriginal communities on Groote Eylandt, near the north-western coast of Arnhem Land, and in the mainland Northern Territory, lasted only for a couple of days each time during my first visit to Australia in 1987, it was obvious to me (as well as to everybody else who went there) that communication between Aborigines, who formed the majority of residents, and people of European descent, the minority, was not always on easy terms. Both groups seemed to prefer keeping to themselves and living almost separate lives. Since "white" Australians' motivation, and also justification, for living in an Aboriginal community is generally related to their jobs as teachers, administrators, nurses etc., much of the social interaction between Aboriginal and non-Aboriginal people is related and reduced to that official function. I did not get the impression that changing from one side to the other, was an easy thing to do but it was a challenge I wanted to take on. I was interested in learning about Aboriginal people's more traditional ways of life and worldviews as well as about their forms of obviously necessary adaptation to the specific colonial and postcolonial conditions they have experienced.

It is common knowledge in anthropology that doing fieldwork is also a form of crossing boundaries by stepping out of your own society and culture and entering into a new "field" which, most likely, is unfamiliar in many ways to the researcher. It also is not an unusual situation that the researcher is not immediately accepted and welcomed by everybody in his/her host society and that, in this process of being accommodated, one has to learn and adapt to new patterns of communication which apply in that society. Without denying the particularity of each field location and its conditions and relations, I would argue that there is a fundamental difference in the relationship researcher — "informants", depending on whether the latter belong to the "dominant" society in that particular country or region or to a discriminated minority, as it is the case with indigenous peoples all over the world. In this case, as in Aboriginal Australia, the fieldworker moves not only "out of home" and "into the field", but within this field, moves between different "sub-fields" which are characterised by the ethnic/cultural affiliations of their members which are in hierarchical positions to each other. Gupta and Ferguson (1997:12) talk about a "*hierarchy of purity* of field sites" which is related to the proximity or distance (not only in spatial terms) of the field to the researcher's home:

> After all, if "the field" is most appropriately a place that is "not home", then some places will necessarily be more "not home" than others, and hence more appropriate, *more* "fieldlike". All ethnographic research is thus done "in the field", but some "fields" are more equal than others — specifically, those that are understood to be distant, exotic, and strange (Gupta & Ferguson 1997:13).

The authors are concerned here with field sites in different geographic areas, such as Africa versus Europe, which are still hierarchically ranked within the discipline itself — despite postcolonial, postmodernist claims to withdraw from an "orientalising" (Said 1979) or "exoticising" attitude towards the "other" whom we are supposed to study. In Australia, as in few other places of the world, such different fields, which I have called "sub-fields", are located side by side or, rather, they are quite often overlapping. Choosing to work among the Aboriginal population does not mean going to a more "fieldlike" place because Aboriginal cultures and lifestyles would qualify for that category. It actually means moving between more and less fieldlike "spaces", between some that could almost be called "home" and others that definitely belong to the "foreign" field. The hierarchy among them does not only exist in the anthropologist's mind but is a reality for the people living in and between those fields which are mainly defined by "racial" or ethnic identity and social class. Belonging to a particular social group, classifies a person immediately as "outsider" by others. It is not considered "normal" to cross boundaries and move into another field for no obvious reason; such actions, therefore, are watched with suspicion by people from both sides.

Being of European descent, an anthropologist working in Australia is, with or without his/her consent, identified by others as belonging to the dominant, "mainstream" and "white" population. Anthropologists working in outback Australia have noticed and commented on the implications of such inscribed categories, the ways in which they had affected their working situations, and the difficulties encountered in overcoming them (e.g. Bell 1983; Glowczewski 1989).

Compared to the more densely populated and urbanised regions in the south and east of the continent, where Aboriginal people have been exposed to European influence with greater intensity and over a longer period of time, Central Australia was "discovered" by "white" explorers and settlers only little more than a hundred years ago. Although the impact of "white" settlement on the indigenous population has been enormous, it has never reached the scale of similar development in — for the colonists — more accessible states. Aboriginal people in Central Australia not only had greater chances of physical survival but could also maintain more of their precolonial cultural traditions than in most other parts of the country (cf Elkin 1986; Reynolds 1987). While this is of special attraction to anthropologists, who still favour the "pristine", such greater differences between colonisers and colonised make group affiliation more divisive and border crossings perhaps more alarming.

Drawing on my own experiences in the field(s), I will show that, even in this environment with seemingly clear-cut divisions, a person's "identity" and "belonging" are not fixed categories but vary according to the context and the person defining them.

My decision to choose Central Australia as geographic field was largely influenced by the offer to work in a volunteer position for an Aboriginal organisation in Alice Springs. At first sight, this looked like an ideal condition to meet Aboriginal people and perhaps even find some valuable long-term informants in this circle. Without thinking twice, I traded my dream of spending a year under tropical palmtrees by the blue sea — which I had opted for in the first place — for the less attractive vision, as it seemed to me at the time, of a harsh semi-desert environment with extreme climatic conditions and a less easy-going way of life. My general research interest was influenced by theoretical and ethnographic feminist writing which had taught me not only that it was very important to look at women's "business" and hence contribute to a more balanced view on Aboriginal society, but also that it seemed to be easier and more appropriate to study issues related to one's own gender[1]. From this point of view, as well, the job prospect at the organisation seemed to fit perfectly since most of the people working for and in the place were female. I was assigned a position at the Arts Development Office which had been installed only a couple of years before and was still struggling with the lack of all kinds of resources, including workers. Knowing this well in advance, I accommodated my research project to the circumstances, which seemed most sensible to do. From a female and feminist perspective, I would look at the rapidly increasing market of Aboriginal art and artefact production in Central Australia for which Alice Springs was, and still is, the commercial centre. This new direction of my research focus, combined with the "promised" access to female artists, looked fairly reasonable and feasible to do, even to Australian anthropologists "of high degree" whom I met in Canberra before going into the "proper field".

At the end of January 1991, I finally arrived in Alice Springs. It turned out that the accommodation which a friend of mine had arranged for me was only about a hundred metres down the road from the organisation. Soon after my arrival, I walked over to my new work place and introduced myself to the people I had corresponded with. They had already expected me and were pleased to see that everything was alright and that I could start working almost immediately — *almost,* because the woman who was in charge of the arts section was away and would only come back in a couple of days.

Before arriving in Australia, I had received some brochures about the Institute which gave me an idea of its structure and aims. It was primarily a teaching institution funded by the Uniting Church in 1971, placed under Aboriginal control since 1978 and today linked to several other Aboriginal organisations in Central Australia. The courses offered were designed to give teenagers and adults a general educational background or teach certain skills which would help them in everyday situations and enable those on a higher level to find a job in an Aboriginal organisation or even in the "outside" world[2]. I had also noticed that many of the teachers and other employees in responsible and higher positions were "white" people. However, the tendency to "Aboriginalise" as much as possible had already started, which meant that the organisation aimed at replacing those positions by Aboriginal people. Such an ideal of "racial self-sufficiency" is a common policy among Aboriginal

1 In Aboriginal English, the term "business" is a generic term which includes all kinds of matters of everyday life, of social relationships, but also political, religious and ritual affairs.

2 Due to lacking skills but also discriminatory attitudes of non-Aboriginal employers, it is very difficult for Aboriginal people to find a reasonably paid job in the administrative sector of private companies.

organisations and becomes understandable when looking back at a history of 200 years of violence, discrimination and paternalism towards Aboriginal people.

What I did not know in those days, however, was that not all in the organisation had a positive image of anthropologists in their minds. It did not take me long to realise that anthropology as an academic discipline was regarded with suspicion, if not contempt, by many people (Aboriginal as well as non-Aboriginal) in that particular social environment. It was convenient to blame anthropologists for past injustices, wrong directions in Aboriginal affairs and misunderstandings in intercultural communication. It is certainly true that, in the past as well as in the present, anthropologists have occasionally collaborated with the government, which was not always in the interest of Aboriginal people and for their benefit.[3] It is further an undeniable fact that the concentration of "past" and "present" anthropologists in Central Australia, whose job it is to do some kind of fieldwork "out there", is probably higher than in most other parts of the world. Aboriginal organisations, like land councils, women's councils, and so forth, usually have their "own" anthropologists who provide them with the information needed to answer a certain question or solve a specific problem and which is oriented towards a clear political agenda. Against the background of the fairly dense distribution of applied anthropologists, there does not seem to be much room left for the freelance workers, the academics who obviously only work for their own profit and without returning anything to the people they study. I do not intend here to discuss whether such accusations are justified or not, but would like to remark that it is important to bear such conditions and attitudes in mind when we talk about the anthropologist's position and identity in the Central Australian context.

Apart from the obvious criterion of being "useful" to Aboriginal people or not, there is another important factor which distinguishes applied anthropologists from those doing "mere" research: the first are thought to be "controllable" whereas the latters' actions and knowledge seem to be "out of control" and, thus, potentially dangerous. This brings us back to my very first day in Alice Springs and at the Institute where I was told that I had to make my research transparent to the organisation and keep them informed on my progress. Such claims were certainly legitimate, considering the fact that I had free access to most of the Institute's facilities, could join some of their classes, and that through my working position (which had nothing to do with anthropology) I got an insight into Aboriginal organisations and their politics and met the people who, in the end, became most important for me, on a professional as well as personal level. Despite this general concern, however, the Institute's interest in my project seemed to be very limited.

As anthropologist, I was a kind of "lonely wanderer" in the organisation. In a way, I belonged to the minority of middle-class "white" people who did not fully accept me as one of them, since I had no real employment, no specific job description, no fixed working hours, and besides, because the real reason for my presence was my research which some of them objected to. From their point of view, a rather distanced attitude made sense because I was not only a "lonely" but also a "free" wanderer. I seemed to be interested in everybody and everything, appeared and disappeared at public events, meetings or social gatherings

3 Adolphus Elkin and Theodor Strehlow are two well-known anthropologists who, in the first half of the twentieth century cooperated in government policies to exercise greater control over the Aboriginal population and to facilitate their assimilation into the dominant society.

and, with time, became particularly attached to two different groups of female Aboriginal students of different age and ethnic/linguistic backgrounds.

When my colleague from the arts department unexpectedly left one day and the whole office was at risk of being closed down, my job finished, too, a few months earlier than planned . However, the Institute's staff was generous enough to allow me access to their premises whenever I wanted to, and I could still join some of the bush trips organised by teachers, as well as other events.

These first months at the Instiute gave me the opportunity to meet Aboriginal people of different backgrounds and with different objectives and interests. I could observe different lifestyles, listen to different opinions, and learn about Aboriginal politics, religion, and social relationships. When it was time for me to go, I was already "independent" enough to find my own way and, by that time, I had made enough contacts outside the organisation which helped me collect the information I needed.

In contrast to many "classical" field studies, where the anthropologist lives a "settled life" in a small defined, and ideally fairly homogeneous community, my "field" was characterised rather by heterogeneity and, most of the time, I was "on the move". My main informants came from different geographic regions, covering all Central Australia, and spoke several different languages. Some of them had lived in Alice Springs for most of their lives, others were newcomers; some went back to their "home countries" on a regular basis to visit their families, others hardly ever left town. However, mobility is highly valued in Aboriginal society and most people I knew lived according to that. My own mobile lifestyle corresponded in some ways to that of my Aboriginal friends who, however, managed to remain even more flexible and independent from daily chores. Whenever I could, I took the chance to travel to Aboriginal communities within a radius of up to 500 kilometres from Alice Springs. This way, I could compare the difference in living and working conditions of town residents with those of rural dwellers. Since my main research focus was on art production, I also visited the art centres in those communities. There I met male and female artists (although all my long-term informants were female) engaged in doing paintings, textile crafts, wooden artefacts etc., as well as the, mostly non-Aboriginal, coördinators who were responsible for the organisation and marketing of the local production. These so-called "art advisers" were generally less reserved and more open-minded towards me and my project than many of their colleagues in established organisations. Identifying themselves as "lonely fighters", who were characterised by being overworked, underpaid and frequently misunderstood by bureaucrats, businesspeople, and the artists, they did not seem to feel threatened by me and often enjoyed talking about their views, experiences, problems as well as their success in the promotion of Aboriginal art. Invitations by art advisers and other "white" staff in different communities, introduced me to their very particular way of life, their opinions on it and their motivations for having chosen such circumstances. It also gave me an insight into their attitudes towards Aboriginal people generally and their relationships with certain individuals.

Looking at such a situation from the Aboriginal perspective, I was identified as another person working in the field of "Aboriginal affairs", and my close contact to the "white" personnel was considered most appropriate and in accordance with my social standing. Although those were informative, entertaining and fairly easy-going events in some ways, the relative closeness to the non-Aboriginal workers made it more difficult for me to get to know Aboriginal artists and their kin in an informal and relaxed atmosphere. This rather was the case when I visited places and people with my Aboriginal "informants", who then "looked after me" in that, for me, new environment. They introduced me to their family, showed and explained me sites of personal and/or mythological significance and encouraged me to join in at singing, dancing or painting sessions, or to try my luck in "hunting" small animals, like reptiles or insects, which we all ate with great appetite afterwards (fortunately we did not rely on **my** — insufficient — hunting skills).

These so-called "bush trips" were the most relaxed moments I spent with my Aboriginal friends, where "proximity" seemed to outweigh "distance", and where nonverbal communication ridiculed any attempts at structured interviews. It would be wrong to say, or even think, that Aboriginal people in such situations were not aware of my "difference" and thought of me as "belonging" to their own society. My incapacity of accomplishing the simple tasks of their everyday lives, like finding animal tracks in the sand and the right edible ants under a bush, as well as my ignorance in matters of religion and kinship, would have constantly reminded them of the discrepancy between my little traditional knowledge and, compared to that, my fairly advanced biological age.

When being "at home", in Alice Springs, I lived in a shared household with two "white" Australians, a man and a woman. It took a while until the first Aboriginal women dared to come, first into the garden, and then into our house. A main reason for their uneasiness was the presence of a man whose relationship to me and, accordingly, to them was uncertain because we obviously were not "family". The situation became more relaxed with time, and most women got used to Will and the occasional other male visitors. In fact, the women increasingly enjoyed sitting in the garden, playing cards and drinking tea; not least, because it was very accessible — very close to the Institute and the Todd River, a popular camping and meeting area for Aboriginal people — and, at the same time, a quiet spot where they could escape the pressures and tensions of camp life. This situation was somewhat exceptional in town as it was not common for a "white" household to have Aboriginal visitors on a regular basis, and vice versa. My frequent visits to Aboriginal town camps and houses were, therefore, equally "strange".

However, the house not only served as a place to meet and relax for Aboriginal people but it was a refuge for me, too, when I got home exhausted, tired, and just wanted to be left in peace. And it was a meeting place for our non-Aboriginal friends in town and for visitors from interstate and overseas. The house seemed to be "home" for me although it was located on the "other side" of the globe, and the language I spoke, the food I ate, the people I socialised with, and the rhythm of life I practised therein, were different from "home" in Vienna. But it was also "a field" because I learned much about my host country and "white" Australian culture, social behaviour and values and, furthermore, because I received visitors — Aboriginal and non-Aboriginal — who were, directly or indirectly, related to my work.

CHANGING SIDES

In the previous section, I have tried to show that my experience of anthropological fieldwork in Central Australia, which altogether lasted about fifteen months in the years of 1991-92, was characterised by inconsistencies in terms of places, people, social and work life. As I generally enjoy a changing life pattern, I did not miss too much the kind of "daily routine" I had experienced before although, occasionally, I thought it might have been helpful in organising my work schedule. Besides, most of our European understanding of "work" is associated with a more or less fixed time table and a sort of routine which enables the worker to accomplish his/her tasks. Lacking this support made it more difficult to convince myself and others that I was not only "having a good time" but actually did "serious work".

Despite the overall unsteadiness and unpredictability in my life, some changes had a greater impact upon myself and my project than others did. These "changes of consequence" were often related to my moves between different "fields" (or, should I say "subfields"?). This implied changes of my own position in relation to others which, again, provoked changes in people's perceptions of my respective positions and relations.

This difficulty of placing me into one of the existing social categories was not only a source of personal insecurity for myself but probably for others too. This may, in a way, explain the reactions of mistrust, aggression, or reserve which I experienced from some people. Socialising with Aboriginal and non-Aboriginal people on more or less equal terms, without having an obvious need to do so (and my research was not considered as such by many observers), was not a common thing to do, as I have mentioned before. Interestingly enough, more signs of objection or suspicion came from non-Aboriginal people. Whether it was at the Institute, at another organisation, or in a private surrounding, the majority of Aboriginal people reacted rather positively towards my attempts to learn more about their culture and way of life, and my willingness to share my time and resources with some of them.

However, identification with a certain position was not always easy, neither for myself nor for others. When I went on a camping trip with a group of women and their teachers from the Institute, whose "role" should I play, I asked myself then? Should I place myself on the teacher's side (either "white" or Aboriginal), who normally took care of the whole preparation and organisation of the trip, or on that of the Aboriginal participants who were primarily consumers on such occasions? I mostly ventured for a position somewhere "in-between". Isn't that the way we have been taught how a "real anthropologist" should behave — not taking sides? In my situation, however, this was far from ideal and accompanied by uneasy feelings from all sides. Sometimes I could not keep the "balance" and had to "take sides" more explicitly. It could then easily happen that, within a short while, my position changed from being "one of us" to "one of them" or to someone who does "not belong" to any group, depending on the oberserver's position and interpretation.

With the following short story I would like to show that in this particular fieldwork setting, and perhaps in many others as well, the anthropologist's identity and belonging is less defined by acceptance than by rejection or opposition, because people may not tell you who you are but they tell you who you are **not**.

A SHORT TRIP TO PAPUNYA

In September 1992, I arrived in Alice Springs to continue my fieldwork from where I had left it about nine months ago. A few days later, I decided to go with one of the most famous female painters, Paula Napangati, and her non-Aboriginal partner Derek to an Aboriginal community approximately 250 kilometres west of Alice Springs. This time I had a "proper bush car" of my own, a Toyota which belonged to the Australian funding agency and I could use as part of my grant. The prospect of driving in a comfortable and fast car was probably the main incentive for Paula to ask me to join. For her, it was a "business trip" because she had to attend a meeting between traditional owners of a nearby area (over which Paula, too, held rights) and members of the Central Land Council. The possibilities of mining with its potential risk's and rewards should be discussed and decided upon. Paula, too, had traditional rights over that place and it was important for her to be involved.

After several hours on the dirt road, we arrived at Papunya fairly late in the evening and stopped at one of the camps before getting into the proper township. One of Paula's sisters and her family lived there and Derek also knew them quite well. For that reason, he immediately joined the group at their humpy which was a small shelter made of corrugated iron and other scrap materials, formed in the shape of a windbreak. Paula and I made our little fire a short distance away and started to cook the meat I had brought along. When dinner was ready, Derek and our male host, Dean Tjampitjinpa (Paula's brother-in-law) joined us and we ate together. Paula then explained to Dean the purpose of my visit to Central Australia, that I was interested in paintings and their stories.[4] Dean obviously had some experience with anthropologists and, most certainly, with art advisers and traders, and he quickly offered me a deal, "I tell you the stories and when you go back to Alice Springs, you get me a big brown canvas — not the cheap white one — and paint in the four colours, one of it is real red like the bottle over there. Okay?"

I agreed, and then Dean and Paula turned to more important matters related to the meeting on the following day and the mythological sites and tracks which had been left by the ancestral heroes in the landscape. These "Dreaming" stories are the basis of land ownership and therefore of greatest significance in decisions which, for instance, concern mining in a certain location. Since both of them spoke in Luritja to each-other most of the time, I understood only very little of the conversation.

After a rainy night, which Paula and I spent half outside and half in the car, we had breakfast together with Dean and his wife (Paula's sister) the next morning. Everybody was in a good mood, despite the sleepless night, and we finally drove right into the township where the meeting should take place. Fortunately, I had thought of getting myself a permit from the Land Council before starting the trip because several people, including the community administrator, who was an Aboriginal woman, asked me for it immediately after our arrival. [5] Derek had not done so and, although nobody seemed to worry in his case in the beginning, because he was not a stranger to the people and the place, it finally caused him trouble. During the meeting, Paula got involved into a serious argument with several other people whose interests obviously were different to hers. Derek tried to protect his partner and, finally, got into trouble too. This was the time when anti-"white" sentiments and the issue of Derek's missing permit came up. Those of us, who had been "just hanging around" and doing nothing, chose to move into the car and waited there for the meeting to end. It did not last much longer because everybody seemed to be interested in getting it over and done with. Although initially, Paula and Derek had planned to stay for at least another night at Papunya, this incident made us return to Alice Springs immediately after the meeting.

My own function during that trip, as on many other occasions, was that of a driver, supplier of provisions and cook — a "typical whitefella" role, some people would say. When introducing me to her family, however, Paula emphasised my interest in Aboriginal

4 Almost all "stories" contained in acrylic paintings produced by Aboriginal people in Central Australia tell about the lives and deeds of ancestral beings of the mythological past, the "Dreaming".

5 Every non-Aboriginal person needs a special permit issued by the respective land council when s/he wants to enter Aboriginal land in the Northern Territory.

culture. What would be a more appropriate description for an anthropologist? Dean got a different message: my interest in "stories" was associated with that of art dealers who needed such stories to sell the paintings as "authentic" objects of tradition. As a prospective business partner, I could be placed into a familiar category and my presence made sense to him. People at the local council were more worried about strangers, and people like myself were not really welcome at the meeting. I certainly was no traditional owner, not even Aboriginal, and had no position at the Central Land Council which was the legal representative of the land owners. I obviously did not belong to any acceptable category. For many of the bystanders I would have been just another "white" person whose main function it was to provide transport and food for Aboriginal people, and they may have associated me with one of the Aboriginal organisations. The Land Council people themselves looked at me with, probably, mixed feelings although some of them were very friendly: I was not one of "their" anthropologists but a PhD researcher which made it necessary to treat me with caution. On the other hand, I had arrived with Paula who was a well known and respected Aboriginal woman who was able to defend her rights and interests. Nobody from the "white" staff, therefore, dared to openly criticise my presence. Although Paula's decision to "take me along" the trip was, most likely, motivated by self-interest, she did not neglect her responsibility but "looked after me" when we stayed at Dean's camp and later at the community because she knew of my vulnerable position without being assigned any official function.

CONCLUSION

Anthropologists almost always are "outsiders" when doing fieldwork, even if they do it in their own country. In that case, it would still be very unlikely that they study people who are not at all "different" from themselves.

In the Central Australian context, I was confronted with a wide range of social groups which distinguished themselves from others through "race", class, language, gender, profession and ideology, as main categories. Depending on the situation and the people therein, I was assigned either to one of the, in their minds, already existing categories or classified as completely "foreign" altogether. Due to the heterogenic composition of participants in many situations I experienced, such as meetings, bush trips or rock concerts, I was always "on the wrong side" for some people, and crossing the — sometimes fairly straight — borderlines, was not always watched with benevolence.

Aboriginal people seemed to worry less about such moves, about changing fields and positions, and about the blurring between "the field" and "home". Eileen, an old Pitjantjatjara woman, reminded me of that before I left Alice Springs,

You have to go back because your mother is in your country and she is too old to move. Young people can go somewhere else — not like us, we are too old. But we will sing that you'll come back because you have got two countries now.

POVZETEK

"NA DRUGI STRANI":
TERENSKO DELO MED NEZAHODNJAKI V ZAHODNI DEŽELI

V tem članku nam avtorica poda svojo osebno zgodbo o začetku terenskega dela in o vzrokih, ki so botrovali k temu, da si je za svoje raziskave izbrala avstralske Aborigine. Avtorica, na katero so naredile močan vtis ideje o "prehajanju meja", se je, potem ko je dobila ponudbo, da lahko dela kot prostovoljka v aboriginski organizaciji v Alice Springsu, odločila, da bo živela v Osrednji Avstraliji. Njen raziskovalni namen je bil, da bo s feministične perspektive analizirala hitro naraščajočo proizvodnjo in trgovino z aboriginskimi umetninami in drugimi izdelki. Že v začetku terenskega dela se je morala soočiti z negativno "slavo", ki so jo imeli antropologi tako med Aborigini kot med Neaborigini. Zato ni čudno, da so na nekoga, ki je šele vstopal v svet antropologije, gledali sumničavo. To je pomenilo, da se je morala avtorica še posebej potruditi, da si je ustvarila položaj resne terenske delavke. Tako se je neredko soočila z nezaupanjem, večkrat pa se je znašla pred dilemo za katero stran (npr. učitelja ali učenca) naj bi se odločila in je, ne da bi to sama hotela, večkrat menjala strani. To je seveda povzročilo pri nekaterih ljudeh negodovanje, prepirljivost in celo napadalnost.

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QUALITATIVE METHODS IN SOCIOLOGICAL RESEARCH: HISTORY AND PERSPECTIVES IN SLOVENIA

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This article will summarize the main phases in the development and use of qualitative methods of data collection and processing by sociologists as well as social psychologists, social workers and criminologists in Slovenia. In the last twenty-five years there have been a number of successful uses of the qualitative method worthy of mention. Even so these methods have always remained marginal and have not been accorded their deserved place in the education process. The problem lies partly in the unsystematic and sporadic use of these methods, in a one-sided thematic and *methodical* focus and in too little epistemological (self)reflection.

EARLY USES OF QUALITITATIVE METHODS IN SOCIOLOGICAL AND SOCIAL-PSYCHOLOGICAL RESEARCH IN SLOVENIA

As we will show, the majority of early Slovene research based on qualitative methods was conducted in marginal or interdisciplinary arenas as well as by research groups composed of researchers from different disciplinary backgrounds. In discussing the expanding use of qualitative methods in Slovene research we cannot, therefore, limit ourselves by focusing on "pure" sociology, but must also take into account the remaining, thematically related, research.

In the 1970s, researchers of the then Institute of Sociology and Philosophy at the University of Ljubljana were the first to use qualitative methods in sociological research. Matija Golob included the method of participant observation in investigations of social stratification for which at that time quantitative methods were predominantly employed. Interested mainly in the influential (clandestine) groups and the dynamics of tradition and innovation in village life, Golob spent periods of several months across many years in villages, trying to become part of village life and participating in farm tasks. He recorded the results of his observations in systematically organized diaries, which he then combined with quantitative data and analyzed then according to the statistical method of so-called *square symmetrical matrices* (Golob 1972).

The first large, exclusively qualitative method research project was entitled Preventive Volunteer Social Therapy Work with Children. It was carried out by an interdisciplinary research group of sociologists, social psychologists, psychiatrists, and social workers in conjunction with groups of student volunteers (Stritih et al. 1977). This was the first, relatively comprehensive, twice repeated (summers of 1975 and 1976) action research which dealt with the introduction and evaluation of alternative therapies in work with behaviorally disturbed children and adolescents. In addition to already recognized qualitative methods such as participant observation, the taking of field notes, and keeping personal diaries, researchers also employed methods and techniques which were employed for both research and therapeutic purposes: unstructured interviews, simulation games, psychodramas and role-playing. The sociological portion of the project also included institutional analyses, particularly in relation to the problem of resistance to innovation and to new approaches to work organization as well as to therapeutic-pedagogic doctrine.

Research employing qualitative methods, which was conducted at the Institute of Sociology and Philosophy and was considered to be a continuation of the above-mentioned project, was limited mostly to semi-structured or flexibly structured interviews. At the same time the substantive focus of this sort of research expanded considerably to include the problematization of the socialization of children and families in socially less privileged local communities (Adam and Podmenik 1976, Stritih 1979). The research groups who conducted the first projects continued with their interdisciplinary practice on account of their research subject and the fact that these groups were composed mostly of sociologists and (social) psychologists.

At the end of the 1970s, Silve Mežnarič's research on workers from other republics of the former Yugoslavia living in Slovenia was being conducted at the then Faculty of Sociology, Political Science and Journalism. The research group, which included Slovene students mostly with a social science background, theoretically based their work on Anthony Gidden's concept of structuration and employed the methods of semi-structured interviews to ascertain the problems of identity and adaptation of guest workers from Bosnia and Hercegovina living in Slovenia (Mežnarič 1986). The project was carried out over a number of years both in Slovenia and in northwest Bosnia.

In the 1980s, researchers from a number of institutions employed qualitative methods in their analyses of social issues and various forms of deviance. A number of projects were conducted using qualitative methods at the Institute of Criminology. Bojan Dekleva (1982) employed the method of action research to study of juvenile delinquents; later he employed qualitative methods to focus on the issue of drugs and addiction (Dekleva 1998).

At the Slovene Research Institute in Trieste, E. Susič and D. Sedmak in the fields of sociology and psychology (1993) employed in-depth directed interviews to study processes of assimiliation among the Slovene minority in Italy. During that time a number of projects employing instruments of qualitative methodology were conducted at what is now the University College for Social Work (for example, Mesec 1998).

Researchers at the Institute of Sociology (formerly the Institute of Sociology and Philosophy) continued with the use of qualitative methods in sociological studies. In the field of sociology two researchers employed a combination of unstructured, semi-structured and structured interviews to analyze the use of institutions aiding families (Boh and Černigoj-Sadar 1985). Veljko Rus and Frane Adam (1986, 1989) employed a combination of qualitative methods — participant-observation, semi-structured interviews and qualitative text analysis — to collect data for a project in the field of industrial sociology in which they studied multi-level processes of gaining, maintaining and *inducing* power on the part of individual groups in work organizations. This was one of the best-known — for the most part because it deal with a typical sociological issue — and systematic demonstrations of the use of qualitative methods in Slovenia or the former Yugoslavia at that time (the book was also translated into Croatian).

At the end of the 1980s the use of qualitative methods also expanded to those areas which foreign sociologists have been studying with qualitative methods for a good twenty years: marginal groups. Researchers in interdisciplinary projects which also included a sociological approach employed a combination of techniques in their studies of adolescents and so-called civil movements — women, peace activists, ecologists, lesbians — including case studies and semi-structured interviews (Ule 1989).

RECENT QUALITATIVE RESEARCH AND THE EXPANDING USE OF QUALITATIVE METHODS

Within the range of existing qualitative methods, the use of unstructured and semi-structured interviews expanded considerably in the 1990s; in many cases these techniques were combined with quantitative methods — structured interviews or survey questions. At this time other qualitative methods, which until then had not been implemented in Slovenia, were introduced in sociological (and related) research. The subject matter and sociological subfields in which qualitative methods were implemented also remained varied throughout the 1990s.

Along with an ever more systematic inclusion of semi-structured interviews among accepted methodological research techniques, researchers also began to focus more on qualitative modes of text analysis, partly as a way to complement interviews and partly as an independent research method employed primarily in the context of an interpretative sociological approach. Darka Podmenik employed qualitative analysis of the texts of political party programs in her research on the voting decisions of the Slovenian electorate (Podmenik 1993). In her investigations concerning the public image of interactions among trade unions, employers and the government Podmenik used a combination of quantitative and qualitative text analyses as well as narrative analysis of media newspaper-journalistic texts (Podmenik 1994).

Rajko Šušteršič, a researcher at the Radio and Television Research Center has been conducting a quite specific action research project and case study of the *Slovenian Radio and Television Station* (Šušteršič 1995).

A research group composed of researchers and collaborators of the Center for Theoretical Sociology at the Faculty of Social Sciences as well as researchers from the Institute for Ethnic Issues have been employing a trio of techniques (Adam et al. 1996): comprehensive semi-structured interviews, elements of biographical method and the comparison of data resulting from the use of qualitative methods with results of surveys conducted concerning Slovenian public opinion. One of the main purposes of this study was to analyze interpretations of Slovenian micro and macro social reality as well as Slovenia's position in an international framework, with a particular focus on the formation of multiple identities. The researchers and colleagues of the Center for Theoretical Sociology conducted a number of smaller research projects and surveys in which they employed mostly semi-structured interviews; in most cases this method was used in combination with other sources of data.

From 1998 onwards, the same center has been conducting a smaller field research project, the results of which will contribute to the formation of expert foundations for the establishment of a regional park near the Dragonja River in Slovenian Istria. The purpose of the project was also to contribute to the revitalization of Istrian villages and to illuminate questions concerning the relationship between the local community and the future Dragonja Regional Park (Adam et al. 1999).

Research employing qualitative methods has largely focused on studies of marginal groups and adolescents (Sadar 1998, Ule 1999). Mostly the methods of interviews and focus groups were used, as well as case studies and semi-structured interviews. In another, more comprehensive research investigation (Stankovič, Tomc, Velikonja 1999) researchers employed the methods of participant-observation, semi-structured interviews and text analysis of primary and secondary sources to study youth subcultutes (punks, bikers, gays and lesbians, metalheads, rockabillies, ravers, skaters and skinheads).

Qualitative methods also attracted undergraduate students who attempted in the 1990s to conduct qualitative research (for example, Brešar 1993, Hrastelj 1996, Zlodej 1997; Nadarevž 1998). The undergraduate thesis entitled "Resocialization of refugees from Bosnia and Hercegovina in the Republic of Slovenia: The Case of the Črnomelj Refugee Center" (Nadarevž 1998) is an example of a successful and ambitiously designed undergraduate qualitative research project. The author of this study employed the following qualitative methods: direct participant observation, informal interviews, in-depth semi-structured interviews, life histories, and discussion. The research investigation is also interesting in that it illuminates the issue of refugees, adaptation to life in a refugee center and the integration of refugees into Slovenian socio-cultural space from the point of view of the refugees.

DISCUSSION CONCERNING GENERAL-METHODOLOGICAL, EPISTEMOLOGICAL AND STRATEGIC-METHODICAL PERSPECTIVES ON THE QUALITATIVE APPROACH

There are very few theoretical or epistemological works concerning qualitative methods or the qualitative paradigm published in Slovenia. Frane Adam (1978, 1980, 1982, 1985) has written the most on these topics. His main ideas are gathered in his doctoral dissertation which he defended in 1981 and published in a revised version in 1982. In "A Critical Guide to Sociological Research" he discusses epistemological and general methodological issues of the qualitative approach as an alternative option to quantitative sociological research. In his opinion this relation is not exclusive and he is clearly in favour of triangulation. Special attention is given to the concept of action research (Adam 1982:195-239). While he focuses on the issues of research method (for example, the role of the researcher, methods of collection of empirical data, etc.) and analyzes the organization of the research process in detail (especially from the point of view of the sequential approach), he devotes less attention to issues concerning the processing and analysis of qualitative data (*remark by* Mesec 1998:6). One must also keep in mind that he deals with these problems and provides useful solutions in the book written with his colleague, an expert in industrial sociology (Rus and Adam 1986).

Blaž Mesec deals with this operative-technical aspect in his newest work (Mesec 1998) which is the only work dedicated in full to the issue of qualitative methodology. In this work he discusses all three aspects of social science research: epistemology, organization of the research process and concrete models for gathering and processing data. Mesec is the author who has written the most on qualitative methodology and action research in relation to social work (1977, 1988, 1993, 1994, 1999). One must also mention two other authors in this field who link action research and social-therapeutic work with cybernetics and systems theory (Stritih and Možina 1992).

In recent years Tanja Rener (1993, 1995, 1996) has written a series of articles concerning the use of the biographical method in women's studies. The biographies of women as well as women's life histories represent a "subjective" reconstruction of a historical and social period as well as of the position of women in society. For this reason the use of biography as a method has a completely different role in women's studies than in social science research (Rener 1996:759-763).

One must also mention here the significance of Tina Kogovšek's master's thesis (being conducted in the field of statistics and computer science) which also focuses exclusively on the issue of qualitative methodology. Her thesis deals with the questions of measurement, validity and reliability while at the same time discussing the scientific status of the qualitative approach in the context of postmodernity and post-positivism (Kogovšek 1998).

QUALITATIVE METHODS IN THE PEDAGOGIC PROCESS AND IN TEACHING MATERIALS

The methodology courses for students in the social sciences in both Slovene universities — in Ljubljana and Maribor — are still to a great extent "positivistically" focused: students primarily learn to use quantitative methods and statistical analyses in the research process. They are acquainted with qualitative methodology only in a superficial manner. The exception is a methodology class ("Methods of sociological-culturological research" taught by *Silva Meznarič and Žiga Knap*) offered at the Faculty of Philosophy at the University in Ljubljana in the field of sociology of culture; in this class students are acquainted in equal measure with both quantitative and qualitative research methods. Yet the only subject that focuses exclusively on qualitative research methods is taught at the Faculty of Social Sciences at the University in Ljubljana in the field of cultural studies ("Methods of Qualitative Analysis" is taught by *Frane Adam*). In this class, third-year students of cultural studies learn about the qualitative paradigm as well as the epistemological aspects of, the strategic solutions offered by, and the concrete modes of implementation of qualitative methods for the collection and processing of data. Great emphasis is placed upon including students in the research process, as well as in seminars and interactive-group work. Students are required to write a *seminar report* on the basis of fieldwork. The literature used in teaching the above-mentioned course is mostly foreign (this also holds in the case of the course taught at the Faculty of Philosophy); however at the same time students are to also become acquainted with qualitative research conducted by local researchers. The above-mentioned course also discusses action and evaluation research as well as quasi-experiments. Lately students in this course have also been informed of techniques such as deliberative polls, scenarios, and SWOT analyses.

Slovene textbooks and teaching material for methodology courses in the social sciences primarily discuss issues concerning quantitative empirical research. An example of this is Niko Toš's work entitled "Methods of Social Science Research" (first edition in 1975, the last one in 1998). While the author also deals with themes which are relevant for qualitative research approaches (for example, participant observation, in-depth interviews, group discussion, etc.) it is clear that the quantitative paradigm predominates in the text as a whole. The fact that the author dedicated one chapter of the book to qualitative research (ibid: 199—202) for the first time in 1988 — thirteen years after the book's first edition —

is evidence of this. (The above-cited chapter was also published in later editions of the book in 1997 and 1998). There he equates qualitative research with action research and in an elemental fashion presents it as an alternative to the quantitative approach.

There exists only one other work in book form that has been recently published and that may be used as a qualitative methodology textbook. This would be the previously mentioned work by Blaž Mesec "Introduction to Qualitatitve Research in Social Work" (1998). In this work students may find strategies and phases of qualitative research as well as concrete guidelines as to how to carry out a research project, how to prepare and process data. Yet Mesec's book does not succeed in illuminating all issues pertaining to qualitative methodology (for example, epistemological issues, history and typology of qualitative methods, etc.). The problem lies as well in the sorts of research cases cited by the author, for they deal with subjects and issues particular to the field of social work. Students of cultural studies and sociology would also need more examples from organizational and institutional life as well as a greater emphasis on linking micro and macro social realities. Thus a new and up-to-date methodological text is needed which would provide students of the social sciences and the Slovene public intellectual sphere a complete introduction to the issues of qualitative methodology which — thanks to a recent "post-positivist turn", but also thanks to new computer-based programs for processing data resulting from qualitative research — is becoming more and more influential in the international social science context. Previously published texts, such as Mesec's work, will be a sound basis for advanced studies later on.

RECAPITULATION: THEMATIC AND METHODOLOGICAL ONE-SIDEDNESS AND *INCONSISTENCY*

If we list all themes (or objects of analysis) for which qualitative methods were employed, we come up with the following:

- youth, subculture
- women's studies and family
- work organizations
- local communities
- social-therapeutic work and social problems
- migrations and guest workers from Bosnia and Hercegovina
- collective identities, ethnic minorities
- refugees from Bosnia and Hercegovina
- trade unions and social partner institutions, judiciary, new political parties and voting behaviour.

It is interesting — and at the same time problematic — that the majority of research (roughly three-quarters) in which qualitative research techniques were employed focused on adolescents/youth and subcultures. This is definitely the case for research conducted in the 1990s. One would expect that the thematic range would be greater and that research of this sort would also focus on phenomena such as: local elites, processes of decision-making and organization in local groups (municipalities, towns), political parties, new companies, conflict situations...non-governmental organizations and civil initiatives. Yet this has not occurred, despite the fact that a democratic system would generate more incentives in order to encourage research in these directions. Sociologists also make little use of case studies, to say nothing of more in-depth phenomenological studies.

As far as the applied methods are concerned, we can observe a predominant use of unstructured and semi-structured interviews while the use of biographical methods is only beginning. The application of long-term fieldwork and participant observation is still a rarity. The use of computer-based programs to analyze qualitative data is also only in the early stages.

A survey of the use of qualitative methods shows that in the last twenty-five years some of the qualitative methods recognized abroad have been used in research conducted in the field of sociology as well as in related fields; at the same time sociological qualitative research is unsystematic and lacks continuity. One can explain this in large part in light of the instability of material resources for this line of research and the related problem of not being able to form stable research teams to implement qualitative research.

PERSPECTIVES AND TRENDS

Qualitative methods are gaining more and more recognition in social science research, are being used in the context of evaluative research and are also being combined with computer programs. They are important not only as research instruments in purely research frameworks but they also encourage action and participatory research in fields such as: informal and social teaching ("a learning society"), civil initiatives, the development of democratic political culture, the revitalization of local communities, solving unemployment, non-government organization, etc. The qualitative researcher — who must go through training similar to that of a psychoanalyst - who has an understanding of group dynamics, negotiation skills, and a trained attention to detail while being capable of complex thought, is becoming an important actor in inter-cultural communication, solving conflicts, moderating workshops, and leading project groups and research teams. Even Slovenia, which has neither a great nor systematic tradition or consistency in this light, boasts some very successful individual models. We will have to act in a more cooperative and synergetic manner. Only in this fashion — as well as with international cooperation — will we be able to compensate for a small critical mass and limited resources.

POVZETEK

KVALITATIVNE METODE V SOCIOLOŠKEM RAZISKOVANJU:
HISTORIAT IN PERSPEKTIVE V SLOVENIJI

V tekstu so na sintetičen način prikazane glavne faze v razvoju in uporabi kvalitativnih metod in postopkov zbiranja in obdelave podatkov v raziskavah slovenskih sociologov, delno tudi socialnih psihologov in socialnih delavcev ter kriminologov. V zadnjih 25 letih se je nabralo kar nekaj omembe vrednih in uspešnih poizkusov uporabe kvalitativnih metod. Kljub temu so te metode še vedno na obrobju in v izobraževalnem področju še vedno niso dobile mesta, ki ga zaslužijo. Problem je tudi v nesistematični in sporadični uporabi teh metod, v enostranski tematski in metodični osredotočenosti ter v premajhni epistemološki (samo)refleksiji.

Pregled važnejših raziskav pokaže, da so raziskovalci uporabljali kvalitativne metode na naslednjih socioloških (pod)področjih: mladina, subkulture, ženske študije in družina, delovne organizacije, lokalne skupnosti, socialno-terapevtsko delo in socialni problemi, migracije oz. gostujoči delavci iz BiH, kolektivne identitete, etnične manjšine, begunci iz BiH, sindikati in institut socialnega partnerstva, sodstvo.

Zanimivo in problematično obenem je, da je večina raziskav usmerjena na mladino in subkulture (po grobih izračunih več kot tri četrt vseh kvalitativnih raziskav). Zlasti to velja za 90. leta. Pričakovali bi namreč, da bo tematski spekter širši, da se bo raziskovalna pozornost usmerila na pojave kot so: lokalne elite, procesi odločanja v organizacijah in lokalnih skupnostih (občinah, mestih), politične stranke, nova podjetja, konfliktne situacije... nevladne organizacije in civilne iniciative. Vendar se to ni zgodilo, kljub temu, da bi demokratičen sistem moral vzpodbujati k takim in podobnim raziskovalnim temam. Sociologi se tudi malo poslužujejo t.i. študijev primera in sploh bolj poglobljenih fenomenoloških študij.

Kar zadeva uporabljene metode, lahko ugotovimo prevlado nestrukturiranih in polstrukturiranih intervjujev, uporaba biografske metode je še na začetku. Le izjemoma se prakticira dalj časa trajajoče terensko raziskovanje oziroma opazovanje z (delno) udeležbo. Prav tako je še v povojih uporaba računalniško podprtih programov za analizo kvalitativnih podatkov.

Sklenemo lahko, da je bil v zadnjih petindvajsetih letih v slovenskih socioloških in sociologiji bližnjih raziskavah uporabljen vsaj del v svetu uveljavljenih kvalitativnih metod, da pa sociološkemu kvalitativnemu raziskovanju primanjkuje sistematičnosti in kontinuiranosti. V precejšnji meri lahko to pripišemo nestabilnosti materialnih virov za tovrstne raziskave in s tem povezano nezmožnostjo oblikovanja stalnih teamov, ki bi se ukvarjali s kvalitativnim raziskovanjem.

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FIELDWORK, RESEARCH METHODS AND ETHNOGRAPHY: A SELECTED BIBLIOGRAPHY

Selected by BORUT TELBAN

The literature about fieldwork and ethnography is extensive and I have no wish to claim that this selection is the best or the most complete possible. It is just a small contribution towards this kind of bibliography, fit for this thematical issue. Moreover, it covers primarily the field of social and cultural anthropology (only the books and articles published in English), and neglects fieldwork in other areas such as linguistic anthropology, physical anthropology, and archaeology. It could be regarded as a kind of a guidance for all those who are just beginning to set foot in social and cultural anthropology, assistance for all those who thought of writing about fieldwork but did not yet go deep enough into its theory and practice, and for all those who would like some new information about the traditional and most recent sources in the field of research methods and ethnographic practices in sociocultural anthropology. This bibliography has been coming together over many years: first, because of my own need and interest, and second, because I was asked on several occasions to talk about my own fieldwork experiences in both the Highlands and the Sepik region of Papua New Guinea. I have talked about fieldwork on various formal and less formal occasions and presented this kind of seminars for undergraduate and postgraduate students at different anthropology, ethnology, and sociology departments at The Australian National University, University of Manchester, University of Ljubljana, and University of Heidelberg. Therefore, I do hope that all those interested in research methods, in questions to be asked in the field, in finding the best informant, in the manners and difficulties of writing notes, in ethics and postmodern and feminist dilemmas in fieldwork, or in examples of how the actual fieldwork appeared to some renowned anthropologists, will find something beneficial in this compilation. In the British and US sociocultural antropological tradition the term ethnography is applied to both research in the field (participant observation) and to the final ethnographic written product (articles, but most of all monographs). Based on the first-hand study of society and culture — both small-scale communities as well as large ethnic groups and nation-states — these kind of studies combine descriptive, historical and analytical elements, and comparatively address other societies and cultures contextualized within different streams of theoretical thinking.

Before proceeding to bibliography I would like to mention that a new interdisciplinary journal called *Field Methods* was launched in September 1999, published by Altamira Press. It is a successor to CAM, the *Cultural Anthropology Methods* journal, and it intends to cover methods used for the collection, management and analysis of data about human thought and behaviour. I would also like to mention — regardless that many are already familiar with this fact — that Sage Publications have over the years specialised in qualitative and quantitative research methods and have published many essential textbooks. One has to be aware, however, that the methods discussed in the Sage Publications mentioned in this selected bibliography do not refer specifically to anthropology but rather to a broader fields of cultural studies and social sciences (sociology, social policy and social work, health care, nursing, education, marketing, business, law). Also, because of this issue's emphasis on qualitative methods, those books which focus primarily on statistics and quantitative research in the social sciences are excluded from the list. Sage will in the near future become the publisher of a new interdisciplinary journal addressing ethnographic study of social and cultural change, i.e. the identification and formulation of the different possibilities of 'social becoming' in an era of intense change. The aim of this new journal called *Ethnography* will be to promote embedded research that fuses close-up observation, rigorous theory and social critique. The first issue of *Ethnography* is due in July 2000.

The selected bibliography is organized into several thematical fields — in a similar manner to that often used by course syllabuses in field methods — both to show the variety of highlights within the broad field of anthropological research and to ease the exploration of the literature. A similar classification was used by Malcolm Crick in his compilation of "fieldwork literature" in *Anthropological Fieldwork and Field Methodology: A Selected Bibliography*, a book he edited together with Bill Geddes in 1993. The majority of publications compiled by Crick, along with many others, are also included in this bibliography. Some references, of course, could be placed into several thematical fields at once, but to avoid repetition, I had to pick out a single one. I divided the bibliography into the following thematical fields:

1. Introduction to fieldwork
2. Research methods (language learning, informants, interviews, questionnaires, surveys, photography, film) in anthropology and beyond
3. Ethics and fieldwork
4. Sex, gender and fieldwork
5. Writing ethnography
6. History and theory of anthropological research
7. Personal accounts of anthropological fieldwork

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