

VILLAGE FIELDWORK OVERSEAS VERSUS URBAN RESEARCH AT HOME:
TEXTBOOK DICHOTOMIES IN THE LIGHT OF SECOND FIELDWORK

If it is true that anthropologists are prone to theorise in binary modes what their informants temper with the subtleties of everyday relativism, then second fieldwork can claim the status of a professional cure.¹ None of the oppositions in the title of this essay has escaped unscathed from the gentle gnawings of doubt and common sense that second fieldwork has exposed me to. Ploughing a second field can go far in making one allergic, if not immune, to the glib binarisms paraded in textbooks, agonistic seminars, and much 'methodological' verbiage. What has struck me as new in my second fieldwork is the simultaneity of phases, influences, and roles that in my first were neatly separated. Some of these aspects of simultaneity are due to working in an urban field, some to working 'at home',² and others to both.

My first fieldwork consisted of three periods, of altogether eighteen months, spent in the Nuba Mountains of the Sudan. The

Editors' Note: Dr Baumann carried out his first fieldwork in the Nuba Mountains in the Sudan in the late 1970s. The major product of this research has been a book (Baumann 1987). Presently Dr Baumann is carrying out his 'second fieldwork' in West London.

¹ Very few well-known anthropologists appear to have shunned it, although second ethnographies are rarer than second fieldwork.

² In my case, the phrase applies with qualifications. My second fieldwork concerns predominantly people of South Asian, Caribbean and Irish backgrounds who, unlike myself (born in Germany), are called 'immigrants'.

latter two periods were spent almost entirely in resident fieldwork in a farming village of some 500 people and in its neighbouring settlements. My second fieldwork has, so far, extended over two years spent in a 'town' of some 60,000 people in outer West London, and made use of evenings, weekends, academic vacations, and other time unoccupied by teaching anthropology to undergraduates. After sixteen months of part-time and some ten months of full-time research, the latter helped by two undergraduate student assistants,³ I have embarked on the first year of continuous full-time research, made possible by a research grant from the Leverhulme Trust.⁴ At such a stage, comparisons must be preliminary. To compare the two 'fieldworks' across the binarisms of the title, and across the unweighable variables of being older, in a different decade, and a new place, I shall follow the example of my most articulate informants and start at the beginning, tracing distinctions chronologically.

One's arrival in the field is gradual and staggered in time when doing rural research overseas. Officials and academic colleagues are left behind in the capital as one makes one's way to the provincial centre of the research area; provincial officials and first contacts such as school teachers and traders are again left behind as one leaves, or is taken, 'up-country' to the village, the camp, or other small place where, metaphorically at least, one is to pitch one's tent and begin to live. It is from face to face that local people, powerful, influential, but often competent only locally, decide to tolerate or accept, to help or hinder one's research. It may take patience and must rely on patronage; but all interaction is face to face, direct, and often faster than one's command of local faces and names can keep pace with.⁵ Having steered one's way from an international airport to a far-off village by gradual transitions, even initial disappointments can be borne with confidence and overcome within an entirely local, small arena, the dynamics of which are often easy to grasp. One knows one has arrived, at least.

³ My sincere thanks are due to Mr Bryn Williams and Mr Richard Hundleby who spent the summer of 1987 on their own fieldwork projects. Their original conclusions, as well as questions about fieldwork, have taught me much of ethnographic and reflexive value.

⁴ I gratefully acknowledge the support of the Chairman and Trustees of the Leverhulme Trust in funding my research on 'Cross-Community Peer Orientations in a London Multi-Ethnic Youth Culture'. Without it, even this article could not have been written in time.

⁵ In doing village fieldwork overseas, I found it useful to recognize individuals by their shoes, and necessary even after I had been granted legitimacy as a researcher.

If gradual approach and pointed arrival are the hallmarks of village fieldwork, urban research can indeed appear as its binary opposite. In my own experience, 'arrival' in an urban field is a very slow process indeed. Instead of entering into a small local group through face-to-face interaction, the unknown fieldworker stoops to visiting cards and telephone appointments to break through the constraints of privacy and anonymity, and more often than not depends on the help of officials, functionaries, public figures, or indeed busybodies or eccentrics, to cross thresholds and build networks. Not surprisingly, much 'urban anthropology' has taken the form of 'network' analysis, reflecting the research process itself, or of studies of smaller-scale, well-bounded groups that are highly integrated and afford more face-to-face interaction. The urban researcher who has decided to do fieldwork, not along, but across the boundaries of sub-sets, organized groups or distinctive face-to-face 'communities' may have to wait longer until he or she is sure of having arrived. In such a case, it can take up to a year - and might well be done part-time - until different branches of one's own networks have sprouted or grown together, often seemingly of their own accord.

Having 'arrived', the urban fieldworker is likely to find further differences from his or her rural colleague, which result from the simultaneity of phases which gradual journeys up-country help to separate. While they are more likely to make themselves felt at home, they are by no means impossible also in urban research overseas. A first glimpse of the simultaneity characteristic of much urban research emerges from the continued presence of 'gate-keepers': the officials who control access to key public institutions, such as community centres, youth centres, schools, hospitals, law courts; the volunteers who ward off intruders from clubs and groups, crèches and play-schemes, classes and functions; the bureaucrats weary of the responsibility for helping a researcher who might turn out 'foul' - none of these are left behind in the capital or provincial centre. In urban fieldwork, they remain in one's city, town, borough, ward, to look over one's shoulder, and over their own. It is remarkable how much of the best 'urban anthropology' has been done through religious congregations and networks - unique among urban institutions in the welcome their 'gate-keepers' extend to newcomers and the trust they often place in the researcher's responsibility and admissibility. Most secular 'gate-keepers' insist on more elaborate evidence to unlock the doors they guard, and some are given to jealousy of rival 'gate-keepers' who may have unlocked other doors for the same anthropologist.

A further facet of simultaneity may emerge either because of the urban setting of research, or because research is done at home rather than abroad. It is the simultaneity of fieldwork and 'book-work'. To the researcher going overseas, and especially the one bound 'up country', library research is 'preparation'; it is finished, so far as possible, before one leaves, and certainly well before one arrives in the field. Working in Sudanese villages, I was content for months on end with consulting only excerpts from the sparse ethnographic literature and a cherished copy of *Notes*

and *Queries on Anthropology* (5th edition, BAAS 1929). Urban fieldwork affords the researcher access to libraries and archives that will interfere with, deviate or help field research. Fieldwork at home is certain to expose the ethnographer's attention to relevant or seductive commentary in the media; to helpful or partial comment from academic colleagues; and to an avalanche of usually quantitative data superior to one's fieldnotes and their anecdotal style, yet inferior and perhaps detrimental to them through their lack of experiential content, their loadedness with distant assumptions, and their tempting quotability.

The simultaneity of fieldwork and bookwork takes on starker contours when the fieldworker remains involved, during fieldwork itself, with other academic pursuits and colleagues. This situation, virtually unknown to the single fieldworker in overseas villages, may occur when working in rural parts of one's own country or in urban parts of another. In urban fieldwork at home, such as I have pursued, this simultaneity of being an academic while being a fieldworker can make itself felt in two distinct ways. The first concerns perceptions among and of one's informants, the second, one's colleagues' reactions.

Informants, who in most anthropologists' fieldwork are not themselves academics, can often make little of the researcher who, on the one hand, carries the hallmark or stigma of Higher Education, and on the other is seen to mingle in the sleeves-up, rough-and-tumble pursuits that form as much a part of fieldwork as the well-appointed, professionally tidy interview. Neighbours and 'gate-keepers' in West London asked baffled questions when one day I received respectable academic colleagues, the next day sat in a pub with three well-known local ne'er-do-wells, and on the third had dinner with a local solicitor. The class status of academics is ambiguous enough as it is; and such antics, indispensable as they are in urban fieldwork, can lead to confusion among those who observe the observer. Such instances are by no means a daily occurrence; yet they deserve mention as one of the sharper edges of the issues of class or status in fieldwork. In an overseas village, the Western anthropologist is such an exotic being as to render local comparisons of status and class irrelevant or even incommensurate. In urban fieldwork or in fieldwork at home, by contrast, the researcher's perceived class and status are of immediate influence.

I have heard it said by a number of colleagues working in cities that the urban field does not really allow for the methods of participant observation. Among the reasons given, one can expect to hear generalities about the privacy, anonymity, and variety in city life that render social relations diffuse and often single-stranded. Yet if urban fieldwork is possible at all, these factors cannot, in the end, have proved insurmountable. What, in my experience so far, remains insurmountable is the mere fact that perceptions of class and status will matter, in positive or negative ways, where the fieldworker is not an exotic outsider. In such a situation, he or she cannot hope to be exempted from the distinctions of class or status that informants consider binding; researchers desperate for *declassement* may temper their accent,

manners, dress or other class markers. Such strategies are likely, however, to find neither the respect, nor the credence, of most perceptive informants. They will be recognised as insincere and deceitful, as well as pathetically incomplete; for which fieldworker can change his body language, daily routines, material culture, or contacts outside the field to the point of 'fitting in'; and which informant would waste an hour to explain to the fake 'mate' what any real mate takes for granted? The only strategy I have found in dealing with class expectations and their constraints consists in finding local activities suitable to them, such as teaching evening classes or organizing recreational activities, and generally making available such skills as might make one useful, while at the same time insisting that one may stray from the 'median model' to the eccentricities of both the academic and the local resident with a weak spot for low company and odd pursuits.

The simultaneity of being an academic while being in the field can make itself felt also in the reactions of colleagues, as in one's own reactions to their concerns. This fact is especially pronounced when the fieldworker teaches at a university while in the field, as has been my experience for the past two years. While academic colleagues within anthropology have shown a heartwarming understanding of the tensions sometimes involved and excelled in giving support and advice,⁶ participation in other academic activities can throw into stark relief some of the particularities of anthropological fieldwork.

Social scientists working on contract research are baffled, and at times disgusted, at what appears as the 'leisurely' time frame of anthropological research and the refusal, typical of many of us, to define 'issues' before, and even while, research is proceeding. These fears are not helped by the cult of 'the research project' that British universities and academics have developed in response to 1980s ideologies of 'market place', 'performance indicators', and 'research paying its own way'. Beside the Golden Calf of the neat 'research project', custom-built to standard expectations and streamlined to promise quick results in no time at all, anthropological fieldwork looks decidedly untidy and vaguely ominous. Worse than that, it can be thought entirely illegitimate by committed social researchers from other disciplines who suspect that behind the fieldworker's open brief is a 'liberal' or 'culinary' attitude that steers clear of pre-defined 'issues'. It can be useful in such circumstances to face openly and record clearly reactions to fieldwork itself, and to take seriously the understandable, if sometimes insufficiently reflexive fear of a

⁶ I am most grateful to Professor Adam Kuper for his institutional support at the Department of Human Sciences, Brunel University, and have thrived on his keen personal encouragement. Dr Godfrey Lienhardt has followed my research with the insight, and helped it with the the genuinely reflexive knowledge, that he granted even my first ethnography. Professor John Blacking, who made me an anthropologist, has been as true an inspiration for my second fieldwork as he was for my first.

once colonial discipline thought to harbour 'liberals' bent on 'exoticising' others in order to serve the powers that be. What reprieve from such distrust anthropologists can claim might best be gained by doing creditable ethnography on the precepts cultivated in Trobriand and Azande villages: to live locally, to let oneself in for it, and to publish responsibly.

On the whole, academic influences in the course of second fieldwork can be extremely beneficial, not least when they are of an interdisciplinary range. Unlike the now classic exemplar of first fieldwork, the doctoral candidate supervised by one senior anthropologist and encouraged perhaps by a few postgraduate friends, the practitioner of second fieldwork is more likely to enjoy more freedom in the selection of academic influence. These are more copious when working in cities, and often more accessible when working in one's own country. Two of these in particular may deserve stress. The particularities of anthropological fieldwork and of ethnography have, over the past decade, been discussed and studied both by sociologists and by anthropologists; and it is a direct result of the simultaneity of fieldwork and 'bookwork' in a city of one's own country or language that these debates intermingle with the pursuit of fieldwork *in situ*.

Practitioners of neighbouring disciplines - among them, for instance, those sociologists of science who have taken further Kuhn's notion of 'normal science'⁷ - have begun to validate the 'genre' of ethnography and to subject to their own deliberations the claims and promises of fieldwork. Few of the relevant debates distinguish between living fieldwork and writing an ethnography; yet they have already done much to develop and spread more sophisticated notions of 'reflexivity' as pertain to anthropological research.⁸ As this is not the place for theoretical disquisitions, I shall limit my commentary to the concerns of doing fieldwork. To leave a heated discussion among and with 'informants' for an interdisciplinary round-table on 'methodological issues', there to be questioned by colleagues about the effects of 'observer interference' in anthropological fieldwork, is a challenge of simultaneity that it may take days of further field or book work to overcome. Given the precepts of 'fieldwork diaries' and the age-old striving for reflexivity in one's ethnographic knowledge, the demands of cultivating 'the field' may well conflict with the demands of 'doing fieldwork' in an academically legitimated way. Such experiences have given me all the more sympathy with those who face first fieldwork at a time when 'ethnography' itself is styled into a *problematique* by a few 'postmodernist' students of Geertz. While the discourses of ethnography deserve to be analysed by each practitioner who wishes to serve the 'genre',⁹ even a common-sense

⁷ See Kuhn 1962; cf. the sociological ethnography of Latour and Woolgar 1979.

⁸ See Woolgar (ed.) 1988.

⁹ Useful examples in lucid rather than obfuscatory English can be

understanding of reflexivity would tell one that 'ethnographic authority'¹⁰ is an ass and 'ethnographic responsibility' a Pegasus. Second fieldwork, perhaps irrespective of dichotomies between rural and urban, at home or abroad, is likely to clarify the distinction. At my present stage of research, it appears to me to reflect the differences between taking trouble in fieldwork, and viewing it as a troublesome preliminary to fast publication. The painstaking and reciprocally invigorating pursuit of long-term fieldwork is easily threatened by 'performance indicators' urging us to 'publish or perish', which will prevent second fieldwork being done by colleagues unable to resist the higher speed and, oddly, greater prestige of theoretical pronouncements about fieldwork and ethnography.

The simultaneity of such academic influences and fieldwork practice is, of course, not confined to second fieldwork, but goes with research in the 1980s and is heightened when working in cities where the relevant debates are accessible. The added variable of doing one's second fieldwork may matter in that one is freer to select among intellectual influences and has past personal experience of the links and breaches between living fieldwork and writing ethnography. At the present stage of my second fieldwork, academic analyses of the discourses of ethnography and explorations of the notion of reflexivity have had tangible effects. I now consider documents and interpretations of the research process and the traditional reflexive 'fieldworker's diary' as an integral part of 'the data'. I am astonished, however, at the volume of academic debate that is generated without the authors having experienced second fieldwork (and sometimes even first), without data generated in and out of fieldwork to supplement the data generated through interpretation of printed results, and without any documentation of the effectiveness or otherwise of theoretical disquisitions on one's daily interaction with 'informants'.

The simultaneity of working at a university and living in the field has taught me a more committed respect for fieldwork than my first experience of it could justify. While some factory inspectors misplaced in universities may find it wasteful and dispensable, and post-Geertzians may find it problematic, colleagues from other disciplines have come to view it as promising, necessary, and deserving of reflexive theorization.

The other aspects of simultaneity already mentioned, and perhaps the mere fact of doing fieldwork for a second time, have made me more weary of binarisms within our discipline. This may be a

found in van Maanen 1988, where he distinguishes among 'tales from the field' by their 'realist', 'confessional', 'impressionist', 'critical', 'formal', 'literary' and 'jointly told' conventions.

¹⁰ The phrase is used thematically / theatrically in Clifford 1983.

result, and would be a vindication, of doing second fieldwork generally. For it is likely that commonplaces and formulae will appear glib as one re-enters the cycle from fieldwork through academic conventions to their joint result, ethnography. I no longer believe that villages overseas afford fieldwork, while cities at home offer only research. Much depends, for instance, on the time-span of urbanisation among one's urban informants, as on their perceived class or status, and the urban, suburban or indeed village-like forms of settlement and interaction to be found in all cities. The opposition between fieldwork overseas or at home can be spurious unless one specifies the researcher's relationship to the chosen place. It is hard to say whether an urban-born South Indian anthropologist is more 'at home' in a North Indian village or among South Indians in a British city. Even the categories the question takes for granted beg questions; answers, whatever theoretically validated categories they may be predicated on, will require fieldwork, in this case among anthropologists. Such fieldwork might suggest differentiating between two types of anthropological fieldworker: one who has done fieldwork in a strange place to him or her and makes it familiar, and another who started in a familiar place and makes it strange: both may span the entire range from urban to rural and from home to abroad.

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