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**Editor(s):** Robbins, Derek; Hammou, Karim; Baker, Sarah; Hashem, Rumana; Moreira-Miguel, Julie; Sandino, Linda.

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# YEARBOOK III

PhD research in progress July 2009

Derek Robbins, editor

and

Karim Hammou, Sarah Baker, Rumana Hashem, Julie Moreira-Miguel, Linda Sandino

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**School of Social Sciences, Media and Cultural Studies  
University of East London**

**PhD Research in Progress**

**Yearbook III**

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## Preface

Yearbook I was an attempt to reflect the presentations and discussions of registered PhD students in the School between 2004/5 and 2006/7. During these years I introduced several series of seminars in which I tried to establish an intellectual framework which would make dialogue meaningful between students carrying out research on diverse topics under the supervision of staff with a range of different disciplinary specialisms. My introduction to Yearbook I - "For a socio-genetic understanding of trans-disciplinary research" - reflected the way in which I had tried to frame the discussions. I took the operation of the Centre de Sociologie Européenne, Paris, under the direction of Pierre Bourdieu, as my model. My interpretation of Bourdieu's philosophy of social science and its consequential methodology was that Bourdieu maintained a sceptical position about the explanatory validity of the social sciences. I particularly argued this case in an article of 2003 (Robbins, 2003, reprinted in Robbins, 2006, 149-178) in which I examined Bourdieu's critique of Weber. I claimed that Bourdieu's use of Weberian thinking in his own analyses of Algerian tribes in *Sociologie de l'Algérie* (Bourdieu, 1958) was provisional and that his reading of Husserl led him to become particularly conscious of the limits of rational explanation and to begin to conceive of his adoption of sociological discourse as an instrument for the achievement of *eidetic* reduction, securing access to the pre-predicative basis of our concept formation. (This interpretation of Bourdieu's work has been latent in the consideration of the trans-cultural transfer of social science concepts which has taken place in the Group for the Study of International Social Science since 2002<sup>1</sup>).

Another way to describe Bourdieu's scepticism, however, is by reference to the work of Hume (which Bourdieu once said that he had 'devoured' when on holiday in Scotland as a student). Bourdieu's commitment to the primacy of 'affective' relations, by comparison with which conceptualisations are constitutive of artificial distinctions, could be said to continue the tradition of Humean scepticism which acknowledges the practical, everyday meaning provided by sentiment and habit when reason is impotent (such as the confidence that the sun will rise tomorrow). Similarly, it might also be said that Bourdieu showed sympathy with the Wittgenstein of the *Philosophical Investigations* in his disinclination to speak what cannot be spoken. (See Bourdieu's "Thinking about Limits" (Bourdieu, 1992) for his discussion of the limits of social scientific understanding and see his *Méditations pascaliennes* (Bourdieu, 1997), translated as *Pascalian Meditations* (Bourdieu, 1997) for a full expression of Bourdieu's interest

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<sup>1</sup> For a cumulative report of the discussions of the Group between 2002 and the present, go to [http://www.uel.ac.uk/ssmcs/research/intern\\_soc\\_sci\\_study.htm](http://www.uel.ac.uk/ssmcs/research/intern_soc_sci_study.htm)

in the 'orders' of knowledge and for his use of reference to Pascal to delimit the boundaries of affectivity and cognition).

Yearbook I explored the possibility, therefore, that the foundation of the research community in any institution at any time is to be identified in terms of the affective relations of the cohort of researchers rather than in terms of any impersonal attachment to the logic of disciplinary or interdisciplinary understandings. It so happened that the six contributors (who volunteered that their papers should be included in the Yearbook) found themselves registered as PhD students in SSMAcS in UEL at short term moments in social trajectories which involved trans-national social migration. Two students were of German, two of Turkish, one of Italian, and one of Ugandan origin. In a final 'table ronde' these contributors, therefore, sought to reflect on the consequences of their *social* migrations for their generation of the research enquiries which they were pursuing within a British Higher Education Institution.

Yearbook II continued the debate by acknowledging that the approach pursued in Yearbook I might be excessively reductive, denying too much the autonomy of intellectual disciplines. The attention of the seminars shifted explicitly to consideration of the recent social history of the disciplines covered by the School, with particular reference to the institutional contexts within which these disciplines have developed and been disseminated. The introduction highlighted two historical case-studies. It examined, firstly, the impact of the work of Karl Mannheim in England, in respect of his adaptation of ideas generated in Hungary and Germany between the wars to the post-World War II situation in the UK; in respect of his, related, shift from the philosophical analysis of knowledge and culture to the analysis of social administration and the prescription of welfare policies; and in respect of his, related, move from the London School of Economics to the Institute of Education, London. It examined, secondly, the emergence of Cultural Studies both from the institutional context of adult education (for the early work of Hoggart and Williams) and from the legacy of the discourse of literary criticism towards an analysis of culture which might be independent of prior discipline frameworks, whether literary, sociological, or anthropological, and, instead, generate analyses *sui generis* of a variety of cultural forms, including the media. Contributors were asked specifically to reflect on their work in terms of its relationship to instituted explanatory discourses, and five of the seven PhD student contributors participated in a concluding discussion. One concluded that although she enjoyed "some freedom from 'instituted' disciplines", she was "obviously shaped by my institutional environment. At UEL I have become gradually more engaged with the field of cultural studies."

Although Yearbook II sought to acknowledge that the knowledge construction effected in PhD research is more than a function of the 'individual' interests of researchers and of their social trajectories, it nevertheless continued an agenda inherited from Bourdieu in that it gave disciplines only a limited autonomy in that it examined them as themselves social

constructions or, in other words, as only the 'arbitrary' (historically contingent) embodiments of the interests and social trajectories of earlier generations. To understand the world sociologically, for instance, is to understand it in ways not possible for people before the mid-19<sup>th</sup> Century just as dramatically as it is the case that visually to see the world perspectively is to see it in a way which only became established in the 14<sup>th</sup> Century in Western Europe. (See Panofsky, 1997, and Bourdieu on Panofsky in "Sur le pouvoir symbolique", 1977, translated as "On Symbolic Power" in Bourdieu, 1991, 163-170).

Commencing at the end of August, 2007, I have been in receipt of an ESRC grant: "The work of Jean-Claude Passeron, 1960-present: a case-study analysis of the development of a philosophy of social science" (Ref: RES-000-22-2494). Three consequences of this research work are reflected in this Yearbook. The project has explored the differences in the thinking of Bourdieu and Passeron which were suppressed in their co-authored work of the 1960s (*Les héritiers*, Bourdieu & Passeron, 1964; *Le métier de sociologue*, Bourdieu, Chamboredon & Passeron, 1968; and *La Reproduction*, Bourdieu & Passeron, 1970) but which were clearly articulated subsequently. Primarily, Passeron insisted that there is no precise correlation or causal connection between social and cultural domination. The analysis of the cultures of research practice, therefore, must concentrate, for Passeron, on an analysis of the language used by researchers as an instrument of enquiry rather than, as for Bourdieu, on the social position-taking of researchers (Yearbook I) or the social status of disciplines and the institutions in which they are practised (Yearbook II). The first effect of the project, consequently, is to introduce the possibility that the diversity of research practice in the School should be explored in terms of the language of enquiry and communication adopted by its researchers. Just as Passeron questioned Bourdieu's view that the interests of researchers are functions of their social trajectories, emphasizing, instead, their use of language as an instrument of investigation, so he also questioned Bourdieu's view that the distinctions between disciplines are socially constructed, emphasizing, instead, their distinctive discourses. The second effect of the project, therefore, is that this Yearbook attempts to explore the implications of the logics of the disciplines deployed by researchers without 'reducing' these logics to the sociology of their conditions of production. Although Bourdieu and Passeron explored different ways of resolving what they thought to be the impasse in the philosophy of the social sciences which was the legacy of the 19<sup>th</sup> Century – between positivist and hermeneutic traditions of thought, they were both interested in the trans-national transfer of social science concepts. Before their thinking diverged radically, they co-authored "La comparabilité des systèmes d'enseignement" (Bourdieu & Passeron, in Castel & Passeron, 1967, 21-58). Subsequently, Bourdieu became interested in conceptual transfer as correlative with social mobility (see Bourdieu, 1989) whilst the work of Passeron has influenced the development of 'histoire croisée' which is more concerned with the transfer of ideas. This Yearbook sustains this interest in that it deliberately represents work produced within English

and French institutional contexts. It invites cross-cultural comparison of research undertaken within different intellectual traditions.

This Yearbook is based on two Workshops – held on May 15<sup>th</sup> and June 12<sup>th</sup>, 2008 – which were organised with internal funding provided by the University’s Graduate School. The formal report on the use of the funding is at Annex A. The first workshop focussed on the process of ‘writing’ in the social sciences. There was an introductory discussion of Howard S. Becker’s *Writing for Social Scientists. How to Start and Finish Your Thesis, Book, or Article* (1986, Chicago, University of Chicago Press), followed by an attempt to carry out some of the practices on which his book had been based – encouraging students critically to examine in small groups their use of language in sample pieces which they had brought to the workshop. A link between the two workshops was to hand in that Jean-Claude Passeron wrote an introduction to the French translation of Becker’s book, published in 2004, which he entitled: “Ecrire, réécrire et ‘dire vrai’ en sociologie” [writing, re-writing and ‘telling the truth’ in sociology]. Becker’s book had been written as a practical guide to assist students. It is predicated on certain assumptions about the nature of social scientific explanation, notably those associated with the second ‘Chicago School of Sociology’ of the 1960s. Passeron’s introduction highlights these philosophical assumptions and tries to argue that the use of language is integral to any empirical investigation as much as to the communication of research findings. There is a necessary sensitivity to linguistic meanings both in respect of the logic of scientific discovery and the rhetoric of scientific dissemination. There was an understandable reluctance on the part of students to allow the publication of drafts and re-drafts of their work in progress as indicative of the substantive implications of linguistic modifications. Instead, the Yearbook tries to focus on some linguistic aspects of research work produced in different linguistic cultures, as suggested by the juxtaposition of papers presented in the June workshop by three SSMAcS PhD students and one doctoral student registered in SHADyC (Sociologie, Histoire, Anthropologie des Dynamiques Culturelles) – the research group of the Ecole des Hautes Etudes en Sciences Sociales founded in Marseille by Jean-Claude Passeron in 1992. Part 1 of the Yearbook offers a framework for considering the four contributions published in Part 2. Chapter 1 offers a brief account of the intellectual and institutional background to this research group, as prelude to a response (chapter 2) from a current doctoral student in SHADyC, discussing the extent to which the founding intentions of SHADyC are maintained in current research practice. It then provides, as chapter 3, a translation, prepared by Rachel Gomme, of Passeron’s “Ecrire, réécrire et ‘dire vrai’ en sociologie”, with an introduction (chapter 4) which situates the text in relation to the account given in the opening chapter. Part 2 reproduces the four student papers given at the June Workshop. Part 3 offers some concluding reflections on the differences which arise in research practice from the adoption of terminology which has culture-specific linguistic connotations.

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**Derek Robbins, May 2009**



## Part I



## Chapter 1

### The recent historical antecedents of contemporary interdisciplinary research practice in SHADyC and SSMAcS

#### Derek Robbins

The publication in 1970 of *La Reproduction. Eléments pour une théorie du système d'enseignement* (Bourdieu & Passeron, 1970) was the result of more than half a decade of further reflection on the research which had been undertaken within the Centre de Sociologie Européenne, Paris, in the early 1960s by a team of researchers under the supervision of Bourdieu and Passeron and which had led to the publication in 1964 of *Les Héritiers* (Bourdieu & Passeron, 1964). (The Centre de Sociologie Européenne had been established in 1960 by Raymond Aron who had been appointed to the Chair of Sociology at the Sorbonne in 1955. Aron appointed Passeron to be his research assistant and Bourdieu to be secretary to his research group). Separately and together, Bourdieu and Passeron had written articles which had anticipated, and participated in, the events of May, 1968, in which student unrest at university conditions, expressed in association with more widespread trade union unrest, had been instrumental in removing de Gaulle from power. In association with Jean-Claude Chamboredon, Bourdieu and Passeron published, in 1968, *Le métier de sociologue* (Bourdieu, Chamboredon, & Passeron, 1968), which was intended as the first volume of a handbook to assist young researchers to articulate their projects and to carry them out practically. Only this volume on 'epistemological preliminaries' was published and the planned sequel - an introduction to the use of practical research instruments - never materialised.

In 1976 Richard Nice, who was then a member of staff in the Centre for Contemporary Cultural Studies at the University of Birmingham (which had been directed since the end of the 1960s by Stuart Hall in succession to Richard Hoggart who had founded it in 1962), translated *La Reproduction* into English as : *Reproduction in Education, Society and Culture* (Bourdieu & Passeron, 1977). The translation was published with an introductory Foreword written by Tom Bottomore, who was then (from 1969 to 1985) Professor of Sociology at the 'new' University of Sussex. Bottomore had been Reader in Sociology at the London School of Economics from 1952 to 1964 and, from 1953 to 1959, he was secretary of the International Sociological Association. In this period he had, in collaboration with his wife, published in 1957, as *German Sociology* (Aron, 1957), a translation of Raymond Aron's early *La sociologie allemande contemporaine* (Aron, 1935), and he was already well known as an editor and translator of Marx's work. Bottomore's Foreword, therefore, fostered the view for English-speaking readers that *La Reproduction* was the product of a cohesive, internationally significant research centre,

managed by Bourdieu under the aegis of Aron. Bottomore took the opportunity to celebrate the evidence that he found in the text of a 'continuous interplay between theory and research' (Bourdieu & Passeron, 1977, xiv), suggesting that the usual division between 'thinkers' and 'researchers' can "only be transcended effectively by this kind of long-term involvement in the exploration of a particular broad domain of social life, by a group of researchers who acquire to some extent the qualities of a 'school' of thought" (Bourdieu & Passeron, 1977, xiv-xv) manifest in "the books that Bourdieu and his colleagues have published, but especially in the recently established journal *Actes de la recherche en sciences sociales* which seems to convey even in its title the notion of a continuing process of theoretical-empirical investigation." (Bourdieu & Passeron, 1977, xv).

The true situation was rather different. The Centre de Sociologie Européenne had a precarious institutional existence. It operated until 1968 out of the house – 10, rue Monsieur le Prince – in which Aron had his office, transferring for four years to the rue Turenne before finding a permanent home in 1972 in the newly established Maison des Sciences de l'Homme. Projects attracted loose associations of aspiring young academics/researchers, usually normaliens, rather than any establishment. In 1966, Passeron went to the University of Nantes, where he founded the Sociology department. After the May events of 1968, there was a rift between Aron and Bourdieu, recollected acrimoniously in Aron's *Mémoires* (Aron, 1983). At first there was a secessionary group, focussing on education and culture but, after Aron's appointment to a Chair at the Collège de France in 1970, Bourdieu became Director of the Centre de Sociologie Européenne in toto. By that time, Passeron had returned from Nantes, becoming one of the first appointees to the staff of the experimental 'centre universitaire' established at Vincennes by a governmental, inter-ministerial decree of December 7<sup>th</sup>, 1968, intended as a direct response to the problems of the university system that had been exposed by the events earlier in the year. The 'loi d'orientation' which had been introduced following the May events expected that universities ought to become 'pluridisciplinary and combine as much as possible arts and literary studies with science and technology whilst maintaining a dominant vocational orientation'. The inter-ministerial decree argued that there would be difficulty in re-orienting existing institutions in this way and recommended that an experimental university centre should be established at Vincennes to focus on the relations between arts and literary studies and especially adapted scientific and technical instruction. This centre would complement the already existing Centre Dauphine in Paris which focused on economics, mathematics, linguistics and business studies, and the already existing Centre at Marseille-Luminy which concentrated on the natural sciences. The decree predicted that the Centre would offer a range of pluridisciplinary options and that each option would be 'polyvalent'. It gave two examples of the kind of polyvalence which would be expected:

"Thus the Sociology department, as well as the normal instruction in sociology, will obligatorily include instruction in mathematics, applied economics, and legal science;

the department devoted to the arts will include, beyond traditional instruction, specific openness to contemporary activities.”

The intention of these proposed innovations was that the two-year undergraduate ‘first cycle’ would be oriented towards the world of work and no longer, as was traditionally the case, a bridge between academic secondary education and the preparation of the ‘second cycle’ for teaching or research careers. The inter-ministerial decree recognised that the realisation of these expectations would require original pedagogic methods and it specified four possibilities, recommending a continuous academic year, the suppression of traditional examinations, extensive recourse to professors involved in the outside world, and the organisation of the students into small groups. For the first few years of its existence, the Centre received an annual ‘exceptional’ dispensation to admit students who did not possess the baccalauréat but could demonstrate equivalent learning. The inter-ministerial decree argued that these innovations would be introduced for a short while experimentally at Vincennes and would then be introduced into the mainstream national university system. (For the full text of the decree and for other key documents, see Debeauvais, 1976, 15-18)

It is important to remember that this was the context in which Passeron worked as Head of the Sociology Department from 1968 until 1977. It is also worth remembering that this was the context in which, for varying lengths of time in the same period, Foucault, Deleuze, and Lyotard, amongst others, also worked. As far as Passeron’s work is concerned, the context entailed the challenge to operationalise the findings of *Les Héritiers* and *La Reproduction* so as to offer pluridisciplinary instruction to non-traditional entrants in a way which would prepare them for employment in the modern world. It is not surprising that two of his key interests were to become, firstly, the nature of the relationship between learned and popular culture and, secondly, the nature of disciplinary distinctions in the context of pluridisciplinary action. Whilst Passeron was immersed in the daily struggle at Vincennes, Bourdieu began to articulate a philosophy of the social sciences, consolidating the differentiation from Passeron which had been latent within *Le métier de sociologue*. From his early representation of his understanding of the findings of *La Reproduction*, given as a paper at a conference of the British Sociological Association in Durham in April, 1970 (“Reproduction culturelle et reproduction sociale”, Bourdieu, 1971) through to *La distinction. Critique sociale du jugement*, (Bourdieu, 1979), Bourdieu developed the view that cognitive dispositions and aesthetic tastes are objective correlatives of social position-taking. Differences between cultures are to be understood sociologically as are adherence to different disciplinary language games. Bourdieu’s position came to be known as one of ‘reflexive sociology’; in as much as he was prepared to express skepticism in relation to his own sociological explanations by acknowledging the extent to which they were the products of his own social trajectory.

Passeron wrote relatively little during the 1970s, but his ‘presentation’ of his translation of Hoggart’s *The Uses of Literacy* (Hoggart, 1957) as *La culture du pauvre* (Hoggart, 1970) was

already indicative of his interest in how to analyse popular culture scientifically without patronising it, whilst his 'presentation' of a translation from the German of three early papers by Joseph Schumpeter under the title of *Impérialisme et classes sociales* (Schumpeter, 1972) showed an interest in the boundaries between sociological and economic explanation both in their academic and ideological implications. These newly translated texts, all "centred on the relations between the social history of the dominant classes and their different fractions, the capitalist system of production and the centralised state", as Passeron puts it in his 'presentation', "owe their theoretical importance to the position which they occupy between historical sociology and the history of economics" (Schumpeter, 1972, 10) – to a kind of pluridisciplinarity.

There are some affinities between the experimental 'centre universitaire' at Vincennes (which formally became the University of Paris VIII in January, 1971, and subsequently, in 1980, was physically removed to St. Denis where it is now usually known as the Université de Vincennes à Saint-Denis) and North-East London Polytechnic which was formally established in January, 1970, as one of the 30 new polytechnics established by the Labour government. Whereas the French government claimed to initiate an experiment which might be run out across the whole system, the British government restricted its desire to introduce a comparable responsiveness both to the demands of previously disadvantaged students and to the needs of employers to the encouragement of a system of higher education institutions operating in parallel with traditional universities, in 'binary' opposition to them. North-East London Polytechnic was committed from inception to an 'open admission' policy so as to ensure that it would be a 'people's university' (the sub-title of the book entitled *The New Polytechnics* which had been published by Eric Robinson who was to become the institution's Deputy Director – Robinson, 1967). There was no explicit commitment to 'pluridisciplinarity'. There was a commitment to the transmission of 'applied' knowledge, and departments, such as those of Applied Economics and Applied Philosophy, were founded, but these tended to create 'single subject' degrees with vocational emphases. It was the introduction of a two-year Diploma of Higher Education course in 1974 which was most indicative of an attempt to emphasize 'polyvalence', interpreted as the transmission of generally transferable skills, and to encourage work in groups as well as individual study which might be independent of the parameters established by structured academic disciplines. I wrote an account of this educational experiment, published in 1988 as *The Rise of Independent Study* (Robbins, 1988), and revisited the experience in the first part of my *On Bourdieu, Education and Society* (Robbins, 2006), but, for this introductory chapter, my intention is only to offer some comments on the institutional context in North-East London Polytechnic within which the teaching of Sociology, History, Anthropology, and Cultural Studies developed so as to set this context alongside the one which enabled Passeron to found SHADyC (Sociologie, Histoire, Anthropologie des Dynamiques Culturelles) in Marseille in 1992.



North-East London Polytechnic was a combination of three institutions, located in three boroughs in North-East London, which had existed independently as colleges of technology, offering instruction in the arts and sciences as well as technology. Some of the teaching had been to lead to the award of University of London external degrees. Two of the constituent colleges had possessed departments of Sociology. These were merged and re-located on one site. The college which 'lost' Sociology had a Department of Arts and Modern Languages in a Faculty of Arts. The third college had Departments of Art & Design and Architecture. The Sociology department quickly dispensed with University of London legitimation and secured central approval (from the Council for National Academic Awards) to run a degree in Sociology, which had vocational routes leading, for instance, to Social work employment. Staff in arts and the humanities tried to launch two CNAAs degrees, one in Communication Studies, deploying teachers of Literature, Linguistics, History and Philosophy, and the other in French Area Studies, deploying teachers of French and, again, of History and Philosophy. The sociological orientation of both attempts was a response to the ideological need, imposed by the institution, to reject a scholastic orientation in favour of a disposition to equip students for contemporary social and cultural life. The degree in Communication studies was never approved and the French Area Studies degree was short-lived. As the 1970s advanced, the gap in provision (and employment for teaching staff) was filled by the proposal of a degree in Cultural Studies, which received approval in 1980. It was essentially the creation of the Sociology Department but it provided a framework within which sociologists, historians, and literary critics could collaborate. The development of Cultural Studies at North-East London Polytechnic benefited from the situation at the University of Birmingham. Stuart Hall ceased to be director of the Centre for Contemporary Cultural Studies in 1979, moving to become Professor of Sociology at the Open University, and there was a sense in which precarious staff at Birmingham saw a more promising future for themselves at North-East London Polytechnic. In short, and this needs to be argued and substantiated in much more detail than is possible here, the development of a teaching degree called Cultural Studies absorbed teachers of History and of Literature into a common conceptual orientation which was underpinned by a tacit sociology of culture. It attracted staff and students because it endeavoured to be concerned with working-class culture and with working-class or oral history, in an institution which was still true to its original intention to be a 'popular' university.

'Cultural Studies' at North-East London Polytechnic flourished in the 1980s. The underlying sociological disposition gave the course its coherence without there seeming to be any need to define the distinctive epistemological characteristics either of historical or literary analysis or of the relationship between sociological research and the cultural practices which were the objects of that research. Jean-Claude Passeron left Vincennes in 1977 and, in doing so, moved away from intense involvement in the implementation of the pedagogical innovations prescribed in the original inter-ministerial decree. It is, perhaps, not unfair to suggest that Passeron pursued the thinking that had been sustained practically in relation to undergraduate students

at Vincennes instead, now, in relation to his own research projects and his supervision of research students. From 1977 to 1981, Passeron was on secondment to the Centre National de la Recherche Scientifique (CNRS). He established a research group – Groupe inter-universitaire de documentation et d'enquêtes sociologiques (G.I.D.E.S.) – and, in 1982, became a Director of Studies at the Ecole des Hautes Etudes en Sciences Sociales (EHESS). During this period, he prepared a doctoral thesis at the University of Nantes which was published there in 1980 as *Les Mots de la Sociologie*. It was typical that this thesis constituted a series of essays which arose from research seminars prepared for research students. Passeron saw the text as a completion of the unfinished project which had been only partially represented in the publication of *Le métier de sociologue* (Bourdieu, Chamboredon, & Passeron, 1968). Passeron's intention was to show that sociological research does not operate with pre-existing conceptual systems but, instead, constitutes its own particular discourse as it engages with phenomena in enquiry. He contrasts this emphasis with the approach adopted by Laplanche and Pontalis (sponsored by the supervisor of his own diplôme d'études supérieures, Daniel Lagache) in their *Vocabulaire de la psychanalyse* (Laplanche and Pontalis, 1967), which, in turn, had been modelled on Lalande's *Vocabulaire technique et critique de la philosophie* (Lalande, 1960). As Passeron put it:

"The example of Lalande is enough to show what are the limits and difficulties of an attempt which aims to restore a package of historically determined concepts to their original semantic field" (Passeron, 1980, 162).

He discusses the way in which Martial Guérout had been able to establish the a priori 'ordre des raisons' in Descartes' conceptual system (see Guérout, 1953). Passeron insists that a comparable 'ordre des raisons sociologiques' can only fully define concepts through the organising function which they fulfill in a system of research activities. Such an order "does not lend itself as economically to formulation in discourse as does the order of philosophical reasoning which only ever organises discourses: ..." (Passeron, 1980, 165). In other words, Passeron was arguing that sociological language acquires its meaning in interaction with the practical social situations which it seeks to analyse, whereas philosophical language is self-referential. It was this conviction which was to shape the development of Passeron's thinking, both in respect of the relationship between sociological and everyday language and in respect of the grounds of differentiation between academic disciplines.

Passeron completed *Les mots de la sociologie* shortly after Bourdieu published *La distinction* (Bourdieu, 1979). *La distinction* was a book which angered many of Bourdieu's former collaborators. In particular, Claude Grignon argued that Bourdieu denigrated working-class or popular culture by seeking to analyse it scientifically in terms of values belonging to dominant culture. There was also the sense that Bourdieu had socially betrayed dominated culture by accepting the Chair of sociology at the Collège de France in 1981/2. Passeron's position was rather that Bourdieu had allowed himself to develop an a priori explanatory system of concepts

– ‘habitus’, ‘field’, ‘cultural capital’ – which he then applied to diverse social situations instead of allowing each new situation to generate new conceptual instruments. Bourdieu’s ‘order of reasons’ had become statically philosophical rather than dynamically sociological. Passeron and Grignon debated these issues in an exchange which took place as a research seminar in Marseille in 1983. In that year Passeron had been responsible for the implementation of the government’s decentralisation policy in respect of higher education and research provision. He had been responsible for establishing a base for EHESS and CNRS activities in Marseille. From 1984 until 1992, Passeron was director of a research group entitled the Centre de Recherche sur Culture et Communication (CERCOM) as well as director of the Institut Méditerranéen de Recherche et création (IMEREC). The exchange with Grignon was first published in 1983 as a working paper of G.I.D.E.S, entitled “Sociologie de la culture et sociologie des cultures populaires” (Passeron & Grignon, 1983). It was re-published, as “A propos des cultures populaires” in the first number of a journal – *Enquête* – launched by CERCOM in 1985, and finally published by Seuil/Gallimard, Paris in 1989 as *Le savant et le populaire: misérabilisme et populisme en sociologie et en littérature* (Passeron & Grignon, 1989). Throughout the 1980s, therefore, this text was a point of reference, articulating the implications of Passeron’s view of ‘sociological reasoning’ for research on popular culture and literature. Passeron also reflected specifically on the relationship between historical and sociological research (see, for instance, Passeron & Prost, 1990), and he collected his epistemological work in a volume, first published in 1991, with the full title: *Le raisonnement sociologique: l’espace non-poppérien du raisonnement naturel* (Passeron, 1991). This publication was the prelude to a wider concentration on epistemological considerations in multidisciplinary research in the social sciences, manifest in the establishment, in 1992, of a new research group entitled Sociologie, Histoire, Anthropologie des Dynamiques Culturelles (SHADyC) – the Sociology, History and Anthropology of Cultural Dynamics. As with the earlier research groups, the concern was still with dynamic change in contemporary culture, but, unlike before, the focus is also explicitly on the relationships between the ways in which three disciplinary discourses seek to conceptualise cultural phenomena. Passeron was director of SHADyC until his retirement in 1996 and he has since remained – and remains – closely associated with the group and its publications. The new anthropological dimension was initially provided by the first co-director – Jean-Pierre Olivier de Sardan – who published an article in 1995 in the first number of a re-launched *Enquête*, entitled “La politique du terrain. Sur la production des données en anthropologie” (Olivier de Sardan, 1995). This first number of a new series of the journal, now explicitly called *Enquête, anthropologie, histoire, sociologie*, was devoted to “Les terrains de l’enquête” [enquiry on the ground] which carried articles exploring the epistemological implications of different kinds of practical fieldwork situations. These included Passeron’s “L’espace mental de l’enquête (I): la transformation de l’information sur le monde dans les sciences sociales” (Passeron, 1995) and Jacques Revel’s “Ressources narratives et connaissance historique” (Revel, 1995). The new series ran for 5 numbers until 1997 and was followed by an ongoing series of occasional *Enquête* volumes, but that the nature of the intellectual concern has remained constant is

shown by the sequence of titles: *Les usages politiques du passé* (Hartog & Revel, eds., 2001); *L'argumentation. Preuve et persuasion* (de Fornel & Passeron, eds., 2002) *Pratiques de la description* (Blundo & Olivier de Sardan, eds., 2003); *Penser par cas* (Revel, 2005); *Qu'est-ce qu'une discipline?* (Boutier, Passeron & Revel, eds., 2006); *Naturalisme versus constructivisme* (de Fornel & Lemieux, 2008); and *Ce que le genre fait aux personnes* (Bonnemère & Théry, eds., 2008).

Passeron has struggled to retain an epistemological dimension in social scientific enquiry, seeking, on the one hand, to resist the tendency to detach epistemology from practice and, on the other hand, equally to resist the absorption of social science research into unreflecting instrumentality. The trajectory of interdisciplinarity in the arts and social sciences in the UK and, in particular, at North-East London Polytechnic since 1980 has been rather different. I can only outline an argument as the basis for elaboration, correction, debate and comparison. During the 1990s, Cultural Studies became a separate department within the Faculty of Social Sciences. In 1989 the institution was temporarily re-named the Polytechnic of East London and then when, in 1992, the Conservative government abolished the institutional distinction between universities and polytechnics, the current name of the University of East London was adopted. By the end of the 1990s, the Faculty of Social Sciences comprised departments of Education; Economics; Cultural Studies; Innovation Studies; Social Politics, Languages & Linguistics; Human Relations; and Sociology. These were essentially teaching departments. Cultural Studies accommodated the teaching of History and of Literature. Innovation Studies accommodated Information Technology and Science in Society studies. Social Politics, Languages & Linguistics accommodated the teaching of Politics, four foreign Languages, and Linguistics. Human Relations accommodated Psycho-social studies and a link with Social Psychology at the Tavistock Institute. Sociology accommodated Sociology and, from 1997, an anthropology component which was sufficiently strong to cause the department to be re-named Anthropology and Sociology at the end of the decade. In 2002, the institution abolished Faculties and established, instead, new Schools. In this process, the department of Education became a School of Education; the department of Economics was absorbed into the School of Business; the departments of Cultural Studies and Innovation Studies merged into a School of Cultural and Innovation Studies; the department of Social Politics, Languages and Linguistics was abolished – the teaching of Politics transferring to the department of Sociology, the teaching of Linguistics transferring to the School of Education, and terminating the teaching of foreign languages. The department of Human Relations and Anthropology & Sociology became the School of Social Sciences. In 2004, the institution again decided to rationalise its structure. In this process, the Schools of Cultural and Innovation Studies and Social Sciences were merged to constitute the School of Social Sciences, Media and Cultural Studies. To facilitate this merger, the new School established five 'fields' which administratively grouped undergraduate teaching specialisms. Field 1 covers media, communications, and film studies; Field 2 covers Social Work and Social Policy; Field 3 covers

Sociology and Innovation Studies; Field 4 covers Anthropology, Politics, and Development Studies; Field 5 covers English Literature and History. Since inception in 2004, new 'Fields' have been added.

The 'Field' organisation represents a 'map of learning' for the School, based on the undergraduate teaching provision. From the early 1990s, however, the organisation of learning in UK HEIs has been in part shaped by an orientation to the way in which research has been funded nationally and to the way in which research performance has been assessed. The most relevant funding agencies, both for staff research and for student research scholarships, are the Economic and Social Research Council (ESRC) and the Arts and Humanities Research Board (AHRB) which, by analogy with the other funding agencies, itself became a funding agency in 1997 entitled the Arts and Humanities Research Council (AHRC). Although both funding agencies endeavour to facilitate 'interdisciplinary' funding and training, they operate through committees of traditional subject disciplines with the result that, for instance, Cultural Studies has an ambivalent existence for both agencies. There have been national Research Assessment Exercises in which the research performance of all UK academics has been evaluated in 1992, 1997, 2002, and 2008. The University of East London submitted returns on each occasion in three subject areas: Cultural Studies; Social Work/Policy; and Sociology. In these years, therefore, staff adapted the account of their research to fit the parameters indicated by these denominators of the respective research Assessment Exercises. The third factor in the definition of academic identity in the last decade has been the development of Research Groups or Centres in the School. These tend to be organised around social problems or interests, such as Refugee Studies or London East Regeneration and are independent of epistemological considerations. Doctoral students, therefore, are supervised by specialist staff who may dominantly identify themselves intellectually either with an area of undergraduate teaching, or with a problem-centred Research Group, or with national research organisations which help with the mobilisation of approaches to funding agencies.

I have tried to outline the historical background to the present conduct of research in two institutional contexts. The situation at the University of East London has been representative of a British tendency to be indifferent to epistemological reflexivity. The situation in SHADyC is representative of a particularly philosophical approach to research practice. The intention is that this Yearbook should juxtapose these two positions by reference to student research practice rather than by a priori speculation.

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## Chapter 2



## Pluridisciplinarity in SHADyC seen by an insider<sup>2</sup>

Karim Hammou

« The choice of *pluri* in French stems from a political compromise. Edgar Faure, Minister for Education, seemed to react to one of the May 1968 cries for interdisciplinarity by including the word “pluri” in the law, but by leaving what this new prefix would mean open to interpretation (choose one or several: administrative juxtaposition, continuation of the French model of non-specialized teaching, theoretical interactions, uprising of the concept). »<sup>3</sup>

The birth of a practical pluridisciplinarity in SHADyC's history raises at least two questions. First, the question of its permanence. How was this laboratory, associating three disciplines (sociology, history, anthropology) born and how has it evolved over the last 15 years? Secondly, the question of its transmission. Being a research lab, the Marseille chapter of EHESS is also a doctoral training. What educational tools relay this pluridisciplinary vocation? Without solving these problems, I would like to sketch an answer, through a testimony anchored in my subjective and retrospective point of view, the one of a PhD student at SHADyC since the early 2000s.

### From CERCOM to SHADyC

The Marseille chapter of EHESS did not reflect the interest of Passeron for epistemology and his reflection around the disciplinary divisions from the beginning on. At first, the CERCOM was a research group with a strong sociological identity. It developed in connection with the IMeReC, which rather represented an attempt to strengthen bonds between research and society, via local artistic circles and museums.

Passeron's position on the disciplinary borders became more explicit at the end of the 1980s. Its scientific visibility was at its acme with the publication of *Le raisonnement sociologique*, and Passeron's position found an institutional outcome in the birth of the SHADyC in 1993 – Sociologie, Histoire, Anthropologie des Dynamiques Culturelles. The name of this new research group did not express a “trans-” or “anti-” disciplinary project. Rather, the SHADyC is pluridisciplinary: it found its origin in the conviction that, even though sociology, history and anthropology are not different by their epistemological nature, these three disciplines keep a singular identity through their methods and traditions.

Passeron directed the CERCOM and then the SHADyC until 1996. Throughout this period, his personality constituted one of the intellectual cements of the laboratory. His scientific

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<sup>2</sup> I am especially indebted to Christoph Jankowski and Maruta Herding for their help on this article.

<sup>3</sup> Dubreuil L., « Défauts de savoirs », *Labyrinthe* n°27, online : <http://www.revuelabyrinthe.org/document1892.html>.

orientations and his intellectual curiosity were one of the decisive factors in explaining which researchers joined or frequented the SHADyC. The presence of Philippe Gaboriau and Emmanuel Pedler is connected with Passeron's work on the relationship between the popular and learned cultures. The regular presence at SHADyC of well-known historians such as Paul Veyne - a personal friend of Passeron - and Jacques Revel, then the rapprochement with the anthropologist Jean-Pierre Olivier de Sardan can be partly explained by the intellectual dynamic which will lead to *Le raisonnement sociologique*. Jean-Pierre Grossein's association with the research group echoes Passeron's interest in the work of Max Weber, etc.

Behind the work of Passeron, figurehead of the SHADyC during the 1990s, we thus find a variety of theoretical and epistemological axes which will increase further throughout the 2000s.

### **Pluridisciplinarity and teaching programs**

This variety of researchers doubtless plays a central role in the scientific formation of the students associated to the doctoral training. Seminars, debates, workshops, and more widely sociability between students, create an atmosphere in which the opportunities to meet (or to face) new fields, new theoretical approaches, new disciplinary orientations are multiple.

Beyond this atmosphere, the training of the SHADyC offered a sort of obligation to discover other disciplines for a long time. At the beginning of the 2000s, every student of the EHESS' diploma in Marseille (equivalent of a Master 2<sup>4</sup>), whether he or she was of sociological, anthropological or historical training, acquired a minimal bookish knowledge in each of these three disciplines. This obligation of reading was supplemented by lessons of anthropology, history and sociology, open to all students.

From 2005, the university reforms transformed the SHADyC's doctoral training, which now delivers Master 1 and Master 2 diplomas. The transmission of a pluridisciplinary culture is henceforth transmitted through a collective survey. This survey offers the opportunity to cross methods of different disciplines (for example direct observation and examination of archives), but also to create an explicit epistemological dialogue around the links between anthropology, history and sociology.

### **PhD students of the SHADyC and interdisciplinarity**

The majority of the current generation of young researchers in SHADyC has known one of these two trainings. But the methodology and epistemology of a PhD project are above all chosen by the PhD student and his or her supervisor: we thus observe a huge diversity of orientations in

the PhD of the group research. Even though the mobilization of two disciplines at least is the norm, the balance between both is very variable.

The association of anthropology or sociology with history is the most frequent, but also the least risky: history can be used only as a marginal contextualisation of relatively traditional analysis, on a disciplinary point of view. Several research subjects discuss the link between both disciplines farther, by exploring first-hand documents (Sirna<sup>5</sup>), and / or by developing a questioning at the explicit intersection of history and sociology (Le Pape, Sevin) or anthropology (Saint-Lary).

Anthropology and sociology are also usually associated (Pecqueux, Zotian), and the rapprochement between these two disciplines is an obvious phenomenon in the French social sciences of the past years. Rarer but still present are the research projects using three disciplines (Bouillon, Farnarier), or those who venture on other disciplinary fronts: anthropology and ethology (Savalois), sociology and musicology (Jaffré), anthropology and psychoanalysis (Bonnet), sociology and geography (Fernandez, Moreira), etc.

Rather than a jointly practiced and a collectively framed interdisciplinarity, the research group usually offers a place where the borders between disciplines but also between methods of survey and empirical fields are relatively thin. However, the wider academic institutions bear on the feasibility of interdisciplinarity. Cross-practices are (unevenly) possible, but not always easy.

### **French scholar institutions remain deeply structured in disciplines**

The PhD students of the SHADyC do not escape the institutional constraints of a French university system which remains deeply structured by disciplines. The registration for doctorate demands, in the majority of the cases, a precise disciplinary assignment. French scientific journals, even when they show a pluridisciplinary ambition, publish only few articles tightly crossing two disciplines<sup>6</sup>. The possibilities for recruitment as *maître de conférence*<sup>7</sup>, depend on a qualification delivered by a commission which is, also, disciplinary. The double qualifications exist, but they are unevenly possible depending on the disciplines. Each disciplinary section has its specific criteria and a variable tradition of tolerance for atypical trajectories.

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<sup>4</sup> In France, a Master 2 is the second year of a Masters degree, usually a two-year degree.

<sup>5</sup> Names in brackets refer to post-doc and PhD students of the SHADyC. More informations on their researches are available online, see <http://shadyc.ehess.fr/sommaire.php?id=108> and <http://shadyc.ehess.fr/sommaire.php?id=123>.

<sup>6</sup> Masson P., Suteau M., « Histoire, sociologie, ethnographie. De nouvelles lignes de partage ? » in Arborio A.-M. & al. (dir.), *Observer le travail. Histoire, ethnographie, approches combinées*, Paris, La Découverte, 2008, pp.235-248.

<sup>7</sup> Equivalent to the post of lecturer.

This report, in return, enlightens the role of the SHADyC, which, after all, is an institution, too. As a doctoral training, as a place for teaching, as a support for workshops or publications, the research group offers potential institutional path toward interdisciplinarity.

In a research group which practises pluridisciplinarity rather than interdisciplinarity, we must not underestimate the workload and the time needed for implementing a well-balanced and faithful use of several disciplines<sup>8</sup>. Actually, in the SHADyC as elsewhere, PhD students possess a strong disciplinary identity. The mobilization of another discipline then rarely leans on a large mapping of the researches led in this second disciplinary field. Sociologists, anthropologists and historians have, for the greater part, only a partial vision of the bordering disciplines. The doctoral training of the SHADyC limits this flaw, but does not suppress it. It facilitates on the other hand the discussion with researchers or PhD students belonging to the other disciplines. It is maybe in such collective dynamics, which are hard to institutionalize, that a strong interdisciplinary approach can be elaborated.

Finally, maybe it is worth mentioning that the disciplinary borders are not the only ones that researchers face. Differences in methodologies sometimes follow the disciplinary borders (critical method in the interpretation of archives is the specialty of historians, statistical analyses remain mainly a sociologists' tools). But lines of division also run within a discipline as it was the case with sociology, for a long time split between "quantitative" methods and "qualitative" inquiries.

Criticized in the French university for at least twenty years, this opposition has also been questioned for a long time in the EHESS Marseille. In the middle of the 1980s for instance, Passeron and Pedler suggested to use quantified observation for analyzing cultural reception<sup>9</sup>. Recent works deepen this tradition by combining biographically-orientated interview and multiple correspondence analysis (Djakouane), ethnography and questionnaire (Cornu), or ethnography and network analysis (Hammou).

### **A biographic illustration**

These general considerations inform about the large variety of relationships toward university disciplines which a student can experience in the EHESS Marseille. To exemplify some aspects of SHADyC's intellectual training, I shall conclude with some remarks on my own trajectory. As a student of an *Institut d'études politiques* (Institute for political sciences) before entering the doctoral training of the SHADyC, I already had a strong anchoring in sociology and some superficial knowledge in other disciplines (political sciences, economics, history) before joining

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<sup>8</sup> On practical difficulties of doing interdisciplinary research, cf. Heintz C. et Origgi G., « Rethinking Interdisciplinarity. Emergent Issues », online : <http://www.interdisciplines.org/interdisciplinarity/papers/11/version/original>.

<sup>9</sup> Passeron J.-C., Pedler E., *Le temps donné aux tableaux*, Marseille, ImeReC, 1991.

the EHESS Marseille. Subject to an obligation of pluridisciplinary reading as my fellows, I do not feel that this constraint represented a theoretical turning point in my studies. They nevertheless facilitated more and more frequent incursions into historian works.

The association of sociology and history in my PhD on the emergence of a professional culture of French rappers during the 1990s, comes from several origins. The unease felt with previous scholar books on French rap music became a driving force of my inquiry. Historicizing rap music and rappers' careers gradually became a credo – both a scientific bet and a choice bound to a competition within the academic field.

The membership to SHADyC offered me the material and intellectual means to follow this path. The lessons given by historians introduced me to source criticism, as well as to some current issues within this discipline. My PhD supervisor, Emmanuel Pedler, a sociologist, raised no objection in principle to this disciplinary inflection. I then engaged in the analysis of a corpus of TV programs from 1987 to 1991, as well as in the systematic examination of records released between 1990 and 2004. Informal meetings allowed me to start deeper discussions with historians, to take advantage of their critical reading of my work, particularly in the implementation of a diachronic network analysis.

### Concluding remarks

I would like to conclude with the evocation of the workshop organized by PhD students of the SHADyC in December, 2008. This workshop was designed to discuss, twenty years after its publication, *Le savant et le populaire*<sup>10</sup>. This book analyzes the way sociology, but also literature seize popular cultures. It was co-written by Jean-Claude Passeron who asserted the epistemological sameness of history, anthropology and sociology<sup>11</sup>. Two remarks expressed during the workshop illustrate the ease of the declarations in principle on interdisciplinarity, and the difficulty to operationalize it. First of all, Philippe Gaboriau underlined the weak mobilization of anthropology in the paper preparing the workshop. Jean Boutier, historian and director of the SHADyC, then draw attention to the paradox of the relative subdivision of the debates in sociology on one hand, in history on the other hand – a subdivision which, without his intervention, the workshop would have totally reproduced.

The pluridisciplinarity shown at the EHESS Marseille is not of a radical originality. The *Laboratoire d'économie et de sociologie du travail* (Research group of economics and sociology of work) or the *Maison méditerranéenne des sciences de l'homme* (Mediterranean house of human sciences) in Aix-en-Provence, only to mention institutions geographically close to SHADyC, claim just as much the interest of crossing disciplines. But the virtue of

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<sup>10</sup> Grignon C., Passeron J.-C., *Le savant et le populaire. Misérabilisme et populisme en sociologie et en littérature*, Paris, Seuil, 1989.

plurisciplinary, in the SHADyC as elsewhere, is to institutionalize opportunities for criticism when, for instance, a handful of young sociologists, still too much sociologists in their attention, let the lights of their tradition become a source of blindness.

## Chapter 3

### Writing, rewriting and “truth-telling” in sociology

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<sup>11</sup> Passeron J.-C., *Le raisonnement sociologique*, Paris, Albin Michel, 2006 (1991), pp.84-85.

Foreword to Howard S. Becker, *Ecrire les sciences sociales*, Paris, Editions Economica, 2004

Jean-Claude Passeron

(Translated by Rachel Gomme)

I read *Writing for Social Scientists*<sup>12</sup>, in the French translation published here, with special pleasure because it represented the crystallisation, in book form, of a sociological reflection I had followed through several stages. The book also brought back a vivid memory of my first face-to-face encounter with Howard Becker, in 1985. At that time, over a series of conversations we engaged in during free moments at an international conference on the sociology of art in Marseille<sup>13</sup>, we spoke at length about the difficulty our respective students found in writing or finishing their theses. The conference was organised by the Société française de sociologie in collaboration with EHESS and the CNRS; at the time I was involved in the process of decentralisation of these two research and research training organizations, setting up sections for the study of art and culture in the Vieille-Charité cultural centre in Marseille.

Strolling along the sea front, wandering the beaches and coves of the Marseille shoreline as we took a rest from preparing the papers we were both to deliver at the closing session of the conference – which focused on the question of whether the aesthetic “value” of works of art could be reduced to their social value – we came to talk of the practices and techniques, the problems and tricks of the trade specific to writing up research in the different social sciences. Why was it that not only students and doctoral candidates, but often also established researchers, found it so difficult to begin to write, to show others their drafts, to rewrite, to rethink their scientific data through a series of rewritings and, above all, to complete the refining of the text of their theses, study reports, scientific articles or monographs? On his return to the US, Becker sent me the first draft of an article he had written diagnosing the problems of sociological language which arise at the point when it is “put into text” – a painstaking and painful process which Becker calls “composition”, incorporating the sense of the *dispositio*, *elocutio*, and the *inventio* of Latin rhetoric. Becker had already been offering group therapy for some years, in the form of his seminars at Chicago’s Northwestern University, transforming them into a workshop for analysis and experimentation in the pathways, pitfalls and stumbling blocks involved in scientific writing in the social sciences.

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<sup>12</sup> *Writing for Social Scientists: How to start and finish your thesis, book, or article*, Chicago, University of Chicago Press, 1986.

<sup>13</sup> This conference (13-14 June 1985) was organized by our long-time mutual friend Raymonde Moulin on behalf of the Société Française de Sociologie, of which she was at that time chair, with the support of the Centre National de Recherche Scientifique (CNRS) and the Ecole des Hautes Etudes en Sciences Sociales (EHESS), and in collaboration with Research Committee 37 of the International Sociological Association. Most of the conference papers were collected in a volume edited by Moulin, *Sociologie de l’art [The sociology of art]*, Paris, La Documentation française, 1985 (reprinted by Editions de l’Harmattan, Paris, 1999).

## The nature of the challenge in writing

Was the problem one of psychological inhibition, anxiety or depression, such as may be experienced by any author or artist whose self-esteem is dependent on the judgment of others (peer, boss, reader, editor or commissioning body)? Probably. But are individual "character traits" and the variation in narcissistic equilibrium sufficient to explain the polymorphous anxiety of intellectuals and researchers about the *value* of their performance? Or is it the tasks themselves, and the interactions involved in apprenticeship in the professions, beset as they are by unregulated competition of everyone against everyone else, that feed this insistent, insurmountable and, for some, paralysing doubt? In a society increasingly marked by the fiction of mass education, the publicity given to achievements and winners, and the media's constant rehashing of the same themes, I would argue that we are witnessing, particularly in the intellectual professions, a self-destructive "democratisation" of the childish desire of everyone to acquire, as a birthright, the material and symbolic gratifications of *social rarity* and *cultural legitimacy* that distinguish the "happy few". The personal "glory" which attaches to someone who produces truth or beauty is presented, against the background of the jargon of the moment, as both the just reward for exceptional universal value and a universal goal accessible simply on request or whim. This stimulates the onset and feeds the fever of failure neuroses, punctuated by brief upsurges of hope constantly reawakened by the illusion of having found the miracle recipe, with the suffering and panic which inevitably follows. The ego aspires to the ideal; at all levels of the psyche, the cruelties of the super-ego bind juvenile ambition for the highest realisation of the self as creator of unique values inextricably with the magical fear of failing pitifully, through bad luck or malicious persecution. The two are tied up in knots which are all the more impossible to untangle because the tightest bonds are those between the imaginary and the symbolic.

And why is it that the difficulty, as far as scientific writing is concerned, is restricted to or more acutely felt in the social sciences, and still more specifically in the case of sociology, in the difficult process of learning to put arguments and evidence into writing? Becker's intensive study, along with other "indicators", shows exceptional levels of unease in the discipline among those faced with the "obligation" to write a text, particularly when the writer is in a situation where sooner or later the text will have to be submitted to the criticism of a jury, whether formal or informal. Why should this be? What, for example, is behind the very high rate of abandonment of planned theses, through stress, breakdown or disillusionment? Avoiding the work of writing is primarily an avoidance not so much of the linguistic difficulties of written expression, as of the risk of having to think under the gaze of others. These others, as readers momentarily lifted out of this status of voluntary victim, are then placed in the comfortable position of masters who can examine and test, without risk or obligation, a text offered up to their scalpels, naked and immobile, incapable of defending itself on its own through counter-



attacks or defensive arguments, "a fatherless orphan", as Plato described writing. Improvised speech does not expose the improviser so cruelly to the mockery of his/her listeners, since it can always strike back if it is contested; it is a flow that cannot be immobilised as a target, a thread of fleeting spoken words quickly forgotten in what follows, leaving little trace except perhaps the memory of the prestige, bearing or presence of the speaker.

One of the reasons for the failure of social scientists to express themselves effectively in writing surely lies in the generally diminished levels of willingness and enthusiasm among today's students, combined with a narcissistic rejection of the impersonal labour imposed, in any consistently pursued, in-depth reasoning, by the "formal", burdensome, frustrating demands of scientific communication, always potentially addressed to a "universal audience". But in order to identify the ways in which different writing situations reactivate the childish fear of "submitting" to the mockery of classmates or playground companions, we need to put the rather too general question of the "legibility" or "difficulty" of scientific texts in specifically sociological terms. In what locations, and in the service or exclusion of which social groups, which audiences, which professions, which interactions, which writing strategies, does the semantic fog surrounding sociology's most abstract concepts and schemas form? Why does it accumulate in the vocabularies of causality and meaning of actions and interactions, around criteria of the truth or falsehood of sociological discourse? Where do the pedantic turns of phrase and the nebulous or abstruse words, which in sociologists' writing seem to be prompted by the fear of encountering an informed and critical reader or editor, come from, and why and how do they spread from sociology to other historical disciplines (by which I mean all the sciences which are not strictly formal or experimental)? It is fear that leads the wary sociologist to sense danger in handling phrases which are too clear, unscreened and open to the day, in the uninhibited use of indicative affirmations untrammelled by all their rhetorical shadings and nuances, and to apply the "maximum precaution principle". What are the sources, whether realistic or fantastical, of this fear? The constantly reiterated "It is as if...", the elastic use of various modes of the conditional mood, the superfluous approximations which conceal contradictions, the use of indefinite pronouns and "vague quantifiers" give rise, in all languages, to most of these "empty formulations", padding for a stilted tongue or academic dissertation phrases that French philosopher Eric Weill, with pitiless logic, would annotate in the margin with "not even false". This semantic dilution relieves such formulations of all risk of error, removes their empirical vulnerability; by the same token they are emptied of all assignable *assertory meaning* because, if we analyse the logical significance of these "weak" or cunning systems of assertion<sup>14</sup>, they are no more than expressions devised expressly for the purpose of quietly avoiding the scientific obligation always to formulate assertions which refer clearly and unequivocally to a definite mode of confirmation or objection.

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<sup>14</sup> For an example (of the use and abuse of the "Admittedly... but..." formulation), see Jean-Claude Passeron, "Logique formelle, schématique et rhétorique ["Formal, schematic and rhetorical logic]", in Michel de Fornel and Jean-Claude Passeron (eds) *L'argumentation: preuve et persuasion [Argument: proof and persuasion]*, *Enquête* (new series, II), Paris, Editions de l'EHESS, 2002, p. 159-164.

There is no doubt that recent changes in the educational ethos which shapes the forms and public image of scientific work from the earliest age have had some effect here. As the social and technological conditions of family and school socialisation change, we see in all classes and social groups the spread of, and emphasis on, behaviours characterised by *stop-start commitment* (alternating brief moments of effort with long periods of “cool”, playful or nonchalant attitude); this can be observed in apprentice sociologists just as in other apprentice intellectuals. But we also need to consider other causes for the acute presentation and rapid progress of a professional disease which, in many intellectual professions, dilutes apprenticeship in scientific language and work in a soup of approximations and caveats. For example, it seems – and this hypothesis is epistemologically even more disturbing for sociology – that the original sin, from which the recurrent failure of the social sciences to manage precision in references and assertory monosemy in the empirical description of their “facts” derives, must be *the language itself of scientific theory* in these disciplines. Here grammar, vocabulary and rhetoric of explication and interpretation have not converged to unite in a more or less homogeneous semantics, as they have in the strictly formal or experimental sciences, through reciprocal exchanges between them. On the contrary, the social sciences have diversified and merged in a confusion, over a series of crises in a scientific consciousness which is almost always “unhappy”, dissatisfied with the methodological status of these disciplines, tormented by their perpetual desire for a renewed theoretical foundation.

This is perhaps the nub of the epistemological issue, which explains the lack of rigour and the congenital permeability of sociological language to conceptual approximations, elisions of connotation, authorial jargon, dialects composed through mechanical accumulation of sentences cut and pasted together, which are themselves simply passively mimicking the lazy syncretism of the most academic teachers. To put it more precisely, the popularity of *the language of avoidance* among students and most researchers in sociology is due to the constant concern to minimize risk: the risk of having to make a public statement, about an affirmation too exposed to objections, or of having to respond to a request for clarification about the evidence for or the meaning of this affirmation. The memory of examinations still stings for students who have had to spend a long time skirting the danger of speaking too clearly before an omnipotent judge or a distant sovereign tribunal, whose judgment is experienced as arbitrary because it is always formulated on the basis of implicit expectations and demands, never fully articulated in the traditional pedagogy of “it goes without saying”. In this situation the defensive use of *the academic language of imprecision* seems the only means the student has to maintain some chance of neutralizing the destructive criticism of examiners who specialise in the sarcastic savaging of apprentice texts, particularly in a science like sociology which is almost universally suspected of being epistemologically unstable or “dubious”. The sacrifice of readability results from this tremulous prudence, reflecting an intense fear of “getting wet”. As Howard Becker shows us, we first need to diagnose, without

disciplinary complacency, this disease which attacks some scientific languages more than others. In this way we can formulate and put into practice a pedagogic therapy capable of countering the disease's long incubation in the interactions of university apprenticeship: what is needed is a social re-apprenticeship as interactive as that in which the fear of speaking clearly originated.

### Parallels, divergences, tangential encounters

I had been impressed for some time by the study data and descriptions Becker presented in his earlier work, which posed epistemological questions similar to those I was asking myself in the 1970s. My research was based on logical, linguistic and rhetorical analysis of a body of texts drawn from various texts in the social sciences, both classical and contemporary, deriving from a range of tendencies or schools and engaging different methodologies or styles of argument<sup>15</sup>. In 1985 I discovered Becker's initial studies of the struggles of sociological writing, noting the acuteness of his observations and his interpretations, very close to the epistemological description of the scientific labour demanded by sociological research or reasoning that I myself was sketching out at the time.

I would even go so far as to say, on this point, that Becker's probatory approach seemed to me more convincing than my own, if only by virtue of his focus on an individual "case" – that of writing sociological arguments or proofs in a university learning context. Behind our epistemological agreement, which we noted with mutual satisfaction, I was surprised to discover an unexpected divergence with my "interactionist" colleague. Ultimately Becker was more of a sociologist than I, despite the fact that at that time I believed that I was and presented myself as firmly "Durkheimian", resolved in the name of the *Rules of the Sociological Method* to "explain the social purely in terms of the social". I now see clearly that, in his analysis of the relationship between university language and scientific language Becker, without abandoning his interactionism, was orienting himself with the demystifying vein of the objectivist analyses of *Reproduction*<sup>16</sup> on the "arbitrary" symbolics of university "authorities" and administrations.

Under the Weberian influence of a "sociology of understanding", I had recognized that interpretation is inherent in the construction of historical objectivity in any description of a "state of affairs", and even in the interpretation of the meaning of the simplest statistical table, once we restore it to its context. But by the same token, my recent discovery of the role of

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<sup>15</sup> A first version of this study of "descriptive epistemology" appeared in *Les mots de la sociologie: analogie et argumentation* [*The words of sociology: Analogy and argument*] (State thesis, University of Nantes, 1980); I systematised this epistemology of "theoretical plurality" in *Le raisonnement sociologique: un espace non poppérien de l'argumentation* [*Sociological reasoning: A non-Popperian space of argument*], Paris, Nathan, 1991.

<sup>16</sup> Pierre Bourdieu, Jean-Claude Passeron, *Reproduction in education, society and culture*, London, Sage Publications, 1990.

interpretation in historical explanation, in privileging *clinical observation* of social actions, tended to obscure the *institutional power* of objective constraints – in the case of sociology, the formidable institutional weight of academic idiom, the arbitrariness of which Becker was happily exposing. In short, my conceptual framework for interpreting the intellectual fear of betraying oneself through one's writing had remained Freudian in texture, since, in the analysis of a text or a behaviour as in the treatment of a patient, it only engaged the interaction between two interpreters of a set of symptoms, indicators or signs – the analyst and the analysand, the observer and the text of his observation, left alone together to construct the meaning of their hermeneutic agreement. The semantics of an interpretation based on this psychic grammar can thus only return to the most generic form of the "instincts of the Self" in operation here. The only possible therapeutic outcome is the very relative efficacy of the teacher's exhortations to the anxious writer to dismiss the phantoms and phobias, recommending her/him to undertake rational exercises in rereading and rewriting her/his scientific argument in private. In a sociological research project the transference and counter-transference set in motion by the study, in the form of texts submitted unrefined, cannot be so radically abstracted from the reality and the repercussions, tensions and conflicts of social life; the risk here is that we believe, and lead others to believe, in the universal value and efficacy of interpretations which are exhausted merely in the affective "resonance" of interpersonal relationships formed in a clinical context of "private" exchanges.

There was a simple reason for my over-simplification. As in psychoanalysis – but minus the prior agreement with the student on the way his request for help would be approached – it was only in one-to-one tutorials that I encountered the resistance of doctoral candidates who balked at writing or at stating explicitly the principles of a consistent process of rewriting. They did not hear my offers of help; I did not understand their deafness. Moving to a "doing" therapy was "naturally" impossible: for the subject, undertaking a *procedure* of rereading her/his own work, transformed into a reader capable of standing outside her/his spontaneous actions of sociological writing in order to think more precisely about the meaning of what s/he had written, established a "rule" that the author's narcissism must be put to one side, and thus effectively became a *technique of thinking* as unnatural to this subject as that required by "free association" used to free a patient from the need to reiterate rationalisations for his/her everyday actions and feelings. But in the case of the student, the verbal contract was not embedded in the "analytical situation", where it would hold only for the duration of the session. In a socially "normal" dialogue situation, that is to say one that is open to debate – where each of the partners seeks to occupy the position of both analyst and analysand – the *work of thinking* essential to a fruitful, considered *rewriting* process was always deferred in favour of a voluble reiteration of fantastic, ever more ambitious plans, constantly reformulated through oral improvisation and volatile expression, but abstracted from any rereading. Here I was encountering the same phenomena as Becker described: paralysis, refusal, avoidance, protests and other subterfuges characteristic of the trainee researcher put on the spot by the teacher,

who resists encouragement to write and show a draft text, always putting off the task until later, as if to avoid indefinitely the disillusion, ridicule and humiliation of being "corrected". But I had not succeeded in establishing the rules for a collective catharsis

I admit that I can count on the fingers of one hand the few cases in which I managed, in one-to-one encounters with doctoral students, to "sociologise" the student's thinking through a concerted rehabilitation process based on writing, rewriting and rewriting. Becker, however, had succeeded in introducing a programme of exercises derived from a more appropriate method for analysis of university socialisation: the rules of the game had been formulated and applied within the context of a co-operative process, which still remained an ordeal, but made it possible to restructure the relationship between reasoning and writing, because the evasions were less effective in a group where the members knew one another, as regular participants in the same socio-drama. In this case the sustained observation operates in a context of multiple interactions, themselves embedded in the familiar context of the university institution which vouches for the professional usefulness of such time-consuming and frustrating work, which it justifies by relating it to the memory of previous failures, and the continuity of the participants' projects and already substantial experience. This volume includes lengthy quotations from young researchers who were at the beginning of their career, like Rosanna Hertz and Pamela Richards, who were able to present a retrospective written analysis of their process, its implications and its key points – refusal, block followed by crisis and emergence from crisis (see Becker, 1986, 26-31 and 111-120, translated as Becker, 2004, 33-37 and 117-127). The subjectivity of these testimonies, with their dips and high points, outlines the role of the various *personae* who for each individual symbolically occupy the place of the "good" or "bad" models of writers or readers, teachers or pupils. Hertz's account, for example reveals which models of writing were spontaneously experienced as "classy" (as opposed to "folksy" or "inside dopester"), and which ones gave an indication of the modes of expression that conferred "authority" and the right to say things as they should be said. This demonstrates the role of peers, and of the close or more distant circle of colleagues, present and past, as "assumed readers" – all their real or symbolic weight bearing on the controlled expression of anyone who writes a text for an audience. As we shall see, the majority of Chapter VI is taken up with Richards' retrospective recounting of her anxieties, dreams and daydreams, the "benefits" she gained from analysis of writing exercises undertaken at Becker's recommendation or at his insistent demand for writing: it is effectively written by a collective author derived from an interactive experiment which examines what happens to the dramas and work of writing when they are rewritten during the course of the experiment itself, in order to discover and say what one is experiencing.

Here we come to the heart of the question of why battling against inhibition is less effective as self-therapy when it is conducted as a monologue or in a one-to-one interaction than when it is embedded in a situation of multiple and repeated interactions which bring into play various

intellectual forms of symbolic power, submission and rebellion, as well as the full range of social roles, affects and complexes which have governed the formation of attitudes of romantic exaltation alternating with stubborn resistance to the labour of writing. What has been constructed through and in the context of repeated interaction, through the training and learning, joys and suffering of socialisation, can only be taken up and reconstructed in an interactive context which is itself endowed with revived desire for learning, resupplied with self-representation and thus made accessible to new emotions.

### **Sociological thinking and writing**

During our conversation by the sea, we coincided in our diagnosis of all these issues, particularly since we were able to provide specific examples of the forms of resistance that our attempts at pedagogic intervention in the face of the “fear of writing” or the refusal to “show what one has written” had encountered in our students and colleagues: many, and often amusing, were the instances of evasion and bad faith to be found just as frequently in the digressions of wily experience as in adolescent panic. There were two reasons for our epistemological agreement that *sociological reasoning and sociological writing are indissociable*. On the one hand, we accepted the theory, now widely shared in much writing in the human sciences, that the structures of thinking and of the oral or written expression of that thinking are mutually inherent, whether in everyday or in scientific languages. This thesis runs counter to the impression the individual derives from internal experience of the link between “thinking” and “saying”: analysis of the difficulties or weaknesses of a language communicating information or explanation shows writers who have difficulty with the writing of that language almost always experience and conceive of it as a subordinate tool of, or even an accessory to, thought. In the spontaneous metaphysics of self-consciousness, “thought” – whether communicated clumsily or accurately, expressed or silent, oral or written – can only be conceived as a substantive reality which always remains equal to itself in the interiority of a reflection unaffected by the way in which it is communicated. Furthermore, Becker and I, like all sociologists who rebel against traditional or modern scientisms, subscribed to the premise that we can only describe as social actions those behaviours and intentions for which we can identify the actual or virtual “others” who provide the orientation for these actions (as Weber formulated it).

Sociologists’ relationship with sociological statements is never either immediately, or somewhere in the ether of Logic, a “pure” relationship with the sociological truth of these statements. In describing the forms and evolution of such utterances, we must not forget that this relationship was constructed during the years of study (and already preconstructed in the prehistory of representations of “correct expression” and “truth-telling”). Before it was even conceived, students’ relationship with sociology – like any relationship to the legitimacy of a science or a belief – was represented and perceived as a relationship with a “higher authority”:

a hierarchical establishment on the model of a Pantheon of gods or a family. This authority was fantasised as a supreme court which summoned apprentice sociologists, "caught red-handed" with their incorrect texts, to appear before it, bringing their error-strewn essays as evidence. This court even appeared in dreams and daydreams and, whether cruel or benevolent, was always imagined as entitled, "since time immemorial" and by ancient right, to impose implacably its undisputed power to condemn, without right of appeal, those subject to its jurisdiction – the bad students, who became the ridiculous, incompetent, clumsy, mediocre or uninspired researchers – and occasionally to confer good reports or rewards. This sociology of the origin of intellectual and scientific norms in the minds of students needs at least to be sketched in if we are to understand students' behaviour in accepting or skirting logical and methodological norms, particularly when they mix acceptance and avoidance. Hence the satisfaction that Howie and I shared, I believe, at each meeting a colleague who did not minimise the epistemological importance of this field of sociological enquiry, which was admittedly conveniently close to home, since it was located in our lecture theatres, but which allowed us to get closer to the scientific and pre-scientific meaning of what all the field studies, and their various expressions in text form, signified than we could have done in a less familiar field.

This approach might appear pernicky or pettily sociocentric to most philosophers, who believe that great thinkers are made only by "great subjects" and a remote gaze. But the sociology of scientific practices is, like their history, an indispensable prerequisite for their epistemology. Errors or approximations in articulation are not just errors of vocabulary or language, rhetorical clumsiness or grammatical weakness which only a grammarian or stylistician would find reprehensible. They are in fact logical barriers raised in the path of affirmations of "fact", fences in which the barbed wire abstracts the sense of an argument from the scientific norms of theoretical coherence and empirical verification. Evidence is not subject to the monitoring of "relevant features" required by historical comparison, or to the interpretation of the parameters or results of a calculation of probability, or indeed to the work of argumentation which organises suppositions from various sources as to the social meaning of an action or an interaction into a rational argument: the empty circumlocutions of semi-scientific dialect, more effective than the easily identified naivety of everyday language, dispense with all that.

Writing and rewriting an argument in order to *think what one has written*, while asking oneself *what the reader will read in it*, is the only universal remedy for errors of writing and thought, which are always linked. As Becker emphasises right at the beginning of this book, stratagems or recipes for writing, of which his long study of writing practices has revealed the magical and propitiatory functions, are not simply all-purpose techniques aimed at releasing "writer's block" using a generic treatment, or improving beginners' muddled efforts by means of simple "stylistic exercises". There are plenty of "essays", short and long, which limit their pedagogic

strategy to recommending to everyone the same solo exercises as a way of overcoming difficulties in writing – just as there has always been a multitude of academic handbooks on good writing for use in primary and secondary schools, founded on Boileau’s deceptive aphorism that “whatever is well conceived is clearly said and the words to say it flow with ease”. There are more such handbooks in the United States than in France, and Becker makes frequent reference to them to demonstrate their limitations. Even when they are based on contemporary linguistic analysis, these manuals assume that all forms of writing are equally subject to grammatical and lexicographical analysis of discourses whose texture never varies, and which may be understood and successfully communicated to any readership. Thus they happily confuse the six functions which Jakobson so carefully distinguished – the phatic, referential, emotive, conative, metalingual and poetic<sup>17</sup>. The problem we are concerned with here obviously relates purely to the “referential” (here called the “assertory”) usage of statements made in a natural or artificial language. Thus we clearly need to describe the “logical space”<sup>18</sup> in which the statements of a science like sociology *assert*, before we deny or affirm its methodological “specificity”: is a historical science a science “like all the others”, or not? And if we ignore the complacent “yes” of scientific positivists, does answering “no” mean consigning social sciences to essayist literature, or does it imply a duty to describe a different “regime” of scientific being, a different “style” of handling procedures of evidence?

Becker and I thus discovered that we had in common at least our methodological concern with the “case study” and “collections of cases”. Logical, methodological, juridical or ethical approaches to the individual instance, and particularly to the reasoned “description” of it as a “case”, raise a central epistemological question in all social sciences, once these disciplines admit that they cannot be reduced to the epistemological status of “nomological” sciences (those which can formulate “universal laws”). The case study or the series of cases pose in crucial form – in the sense of an *experimentum crucis* – the question posed in any historical science by the relationship between concepts and individual instances. Becker had already written *What is a case?*<sup>19</sup> in collaboration with Charles Ragin, in which he examined his operative definition of the specificity of descriptions made by the researcher in an individual, localised “terrain”. I myself had begun to explore studies by historians of science looking at the position of Hippocratic collections of “cases” in Greek medicine vis-à-vis mathematics and geometry; I had then followed the succession and reworking of “clinical” approaches as far as Charcot and Janet’s studies of cases of “grand hysteria” and the archetypal “cases” in Freud’s

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<sup>17</sup> Roman Jakobson, “Closing statement: Linguistics and poetics”, in *Style and language*, ed. Thomas A. Sebeok, Cambridge, MA, MIT Press, 1960, pp. 350-357.

<sup>18</sup> Wittgenstein used the term “logical space” (*logische Raum*) to designate the world of logical constraints necessary and sufficient for at least defining the possibility of truth or falsehood attached to the statements of a discourse: *Tractatus logico-philosophicus*, London, Routledge & Kegan Paul, 1922 (first edition 1921), proposition 1.13, p. 3. Hence the rigour, which to Wittgenstein’s contemporaries seemed excessively “logicist”, of the last proposition in this work: “What we cannot speak of we must pass over in silence” (*ibid.*, proposition 7, p. 188). Clearly Wittgenstein abandoned this in his later philosophy of language. In fact this minimum of logical coherence defines the range of what can be truthfully stated in a given “assertory space” very differently depending on the different methodological circumstances of the scientific discourse.

<sup>19</sup> Howard Becker, Charles Ragin (eds), *What is a case? Exploring the foundations of social inquiry*, Cambridge, Cambridge University Press, 1992.



*Five Lectures on Psychoanalysis*, during the emergence of modern clinical psychology. This revealed a relationship between the clinical method and the historical method, which in its turn revealed a relationship between what is required in the description of “cases” in history and in the description of “context” in sociology. What all these approaches have in common, in fact, is that they proceed by descriptions and comparisons of cases, without ever reducing them to the inert status of “examples” which can be interchanged within a given category and are subject to being included in the study as soon as they are shown to conform to the unambiguous criteria for exclusion or inclusion in the category. In effect, the *inductive* approach – a *vertical* “subsumption” – is adequate for identifying such basic elements and formalising them in an abstract structure, and for identifying repeated co-occurrences as scientific facts, in an experiment in the strict sense. Conversely, *thinking by cases* produces intelligibilities, and thus, by traversing and reconfiguring collections of cases *horizontally* – i.e. by treating the “relevant” features of a coherent interpretation of their semantic analogies as an *ideal type* – places the question of what “speaking and writing truth” means in sociology in its true “epistemological place”.

The general problem of the operators and forms of a scientific language – formalisms or contexts, amorphous or structured states, normalities, exceptions or deformities – only acquires its full clinical meaning (both symptomatic and aetiological) when it is posed as we work in depth on “cases” whose sociological relevance is constituted by their social complexity.<sup>20</sup> Reading a number of Becker’s texts<sup>21</sup> had convinced me that with extreme cases, that is to say cases that are extremely localised and very restricted in scope – “marginal” cases in some of the “art worlds” he described, or quasi-pathological ones, like that of the misunderstandings that arise in “inter-comprehension” between sociologists, who whether students or established are all equally lacking any possibility of arriving at a single or dominant theoretical paradigm (and thus are always potential competitors or adversaries in their struggle to defend at all costs the truth of their statements) – with such cases, we enter a realm of epistemological singularity such that it fully exposes the peculiarities of a scientific communication which aims to give an account of such social objects. Communicating a form of knowledge like that of individual historical instances can only operate *without semantic losses* through texts which synthesise multiple dimensions of description. This is certainly the case for sociological writing, in day-to-day fieldwork or literature research, in the simultaneous recourse to several theories of the social, and in the communication of sociological results and the presentation, in legible texts, of both its heuristic progress and the evidence ultimately assembled.

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<sup>20</sup> In anthropology too, at least the anthropology which holds fast to the “casuistic” ambition of the first ethnographers, only in-depth analysis of “cases” can define the scientific function of a “thick description”, by contrast with a “thin description”: see the first part of Clifford Geertz’s book *The interpretation of cultures*, New York, Basic Books, 1973.

<sup>21</sup> Howard Becker, *Outsiders: Studies in the sociology of deviance*, Glencoe, Free Press, 1963; *Art worlds*, Berkeley, University of California Press, 1982.

The vision of sociology is indissociably both synthesising and historical, thus setting the discipline a task which is at once statistical and historical, comparative and clinical. The price of this totalising ambition, limited only within the programme of specialised social sciences, is that today our discipline is condemned to writhe ceaselessly on a Procrustean bed which requires it, simultaneously and at every point in its arguments, to conform both to the prestressed posture of an exact science and to the very different one of an interpretative science of historical "objects". It is a heavy epistemological privilege to have to force oneself, if one is not to refute one's beliefs, to take on the whole package of historical scientificity. Most other social sciences are allowed, by virtue of stylistic habits consolidated over the course of their scientific history, to specialize in a preferred methodology – a position which releases them from the contortions between one extreme and the other of scientific reasoning that are required of sociology. The specialization or autonomy of the "particular" social sciences allows them a wider range of methodological choices to select from. They may use *modelling* of empirical co-occurrences which define a "state of affairs" – an overall historical configuration can always be simplified in a "simulated model", as in economics, demography and linguistics. Conversely, they may use *chronological recounting* of the succession of global social configurations in order to then attempt to reconstruct series or systems of analogies revealed by historical comparison – as do history and historical anthropology, exchanging methods with one another. If we look closely it is only in sociology, whatever the name under which it is practised, that the two criteria for a complete social science have to be satisfied at the same time: it must both operate as an empirical "science", in the demanding sense implied by any scientific handling of quantitative evidence, and must also remain a "historical" science of individual actions and events which does not cut out any actor or any pertinent fact which may have meaning for the explanation. Thus there is a double requirement, for "interpretation" (*Deutung*) of the singularity of actions in the Weberian sense, and "explanation" of social patterns in terms of cause and function, in the Durkheimian sense.

Put to the test of drawing a conclusion, I notice here the difficulty of introducing this book by Howard Becker, since its originality, like that of other ethnographic, interactionist or ethno-methodological texts, does not lend itself easily to an overall summary or a list of the main themes. At once a study of the writing of sociological texts and a theoretical reflection on the texture of practices and paradigms in the social sciences, the richness of Becker's analysis of the social position of sociologists forced into the straight-jacket of precise writing in a "case-based science" suggests a vast range of possible developments, from the most theoretical to the most empirical. It would not be possible to replace Becker's thoughtful overview with a preface summing up the content without losing much of its probatory richness, its convergences of arguments, its paradoxes unravelled and above all the pleasure the reader takes in its agreeable illustrative digressions. As in most of Becker's other books, the reader also has the opportunity to become more fully personally acquainted with Howie and his anti-

academicism, his rigorous observation and his enthusiasms, his humour and his scientific indignation.

*Writing for Social Scientists* is not only a fully realised study, but also a crossroads opening up a multitude of other possible studies, conducted in similar style, using the acute methods of "symbolic interactionism". The text deals with both the words and the wants of sociology, losing nothing of its aim to analyse a "case" as it traverses the vast ocean of disputes among sociologists. It clarifies both the social conditions of truthfulness of sociological assertions and the non-assertory element always contained in the sociologist's utterances, never to be completely eliminated. To a greater or lesser degree, the sociologist is always in a double bind, between the quest for simplicity and the risk of banality, between informed and well-formed sociological reasoning and non-sense or rhetorically overburdened arguments. The epistemology of sociological reasoning proposed by Becker's study explains perfectly why the work of deconstruction, reconstruction and "review" of evidence in a social science is never finished<sup>22</sup> – like a Freudian analysis. It reveals the scientific emptiness of projects which seek to avoid, simply through integral formalism or through the author's rhetoric, the sociological work of continual revision of sociological utterances. But this continuous, detailed work, adjusted to the descriptive patience of "non-monotonal logics" of reasoning, is the only approach which can enable us to overcome the difficulty of being a sociologist without verbal artifice or mathematical conjuring tricks. The words of sociology, congenitally too flexible and incapable of giving "absolutely definite descriptions", do not in themselves explain the assertory ills from which sociological discourse suffers: the cunning or incompetence of some sociologists certainly play a role. But the texts of canny or clumsy sociologists are not the only ones to reveal the impossibility of containing all the words, concepts and assertions of scientific sociology in a single theoretical system: the uncontrolled use of sociological language is just one element in the failure of all attempts to reintegrate the Protean state of research in social sciences within a unified paradigm –albeit one whose rhetorical digressions aggravate this failure. The writing of the best sociologists encounters the same epistemological barrier, the same internal challenge, embedded in the language which must be spoken by a discipline whose central concern is to describe the singularity of contexts in ever more precise detail. The sociological study most devoted to exploring the *empirical* peculiarities of a "field" or a "case" is also a *theoretical* work of continually recasting the language of description of historical worlds, constantly replenished with new "facts" which could not have been observed if a new theoretical framework had not first made them conceivable.

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<sup>22</sup> See, for a recent use of this notion which has become a principle of logical analysis of "natural reasoning" in historical comparatism and ethical and juridical "casuistics", Pierre Livet, "Formaliser l'argumentation en restant sensible au contexte [Formalising argument while remaining sensitive to context]", *Enquête* II, op.cit., p. 49-66.



## Chapter 4

### Notes on Passeron's Foreword to Becker's *Writing for Social Scientists*.

#### Derek Robbins

1.

Writing in March, 2004 a Foreword to the French translation of a book first published by the University of Chicago Press, Chicago in 1986, Passeron begins by establishing the social historical conditions of his first encounter with Howard Becker. His first 'face-to-face' meeting with Becker was in Marseille in 1985 at a conference organised by Raymonde Moulin, the proceedings of which were to be published that year as *Sociologie de l'art*. In the same year, Passeron, Moulin and others collaborated to write *Les artistes: essai de morphologie sociale* [artists: an essay in social morphology], and, in 1982, Becker (with Howard Saul) had published *Art Worlds*. This was the period in the early 1980s – shortly after the publication, in 1979, of Bourdieu's *La distinction* – when, as we have seen, Passeron and his colleagues were anxious to advance a sociological discourse about art which did not 'reduce' (in their terms) art appreciation to strategic social position-taking. The emphasis, therefore, was on the development of a sociology of aesthetic discourse. Passeron had already completed his thesis on the 'words of sociology' in 1980. Whilst an undergraduate and later a graduate student at the University of Chicago in the 1950s, Becker had also worked as a professional jazz pianist. His first book was *Outsiders: studies in the sociology of deviance* (Becker, 1963). His doctoral research was a sociological study of Chicago teachers. This background led to an interest in research methodology as a process of *improvisation* and in teaching and learning as a process of *communication*. In 1970, he published a collection of articles about his research methodology entitled *Sociological Work. Method and Substance* (Becker, 1970) in which he argued in his Preface:

"Over the years I have engaged in many large research projects, some reported here, some published elsewhere in works on education and deviance. In the course of trying to gather and make sense of a body of data, I often found myself improvising techniques and using ideas I later realized I did not fully understand. Having done things a certain way, I tried to understand what I had done and make its logic clear; having used a certain concept in an uninspected way, I tried to work out what it might mean in some extended theoretical context. I call attention, without going into the matter in detail, to how this differs from approaches that develop 'correct' methods *a priori* and choose concepts by seeing what will 'fit' with already established theory." (Becker, 1970, vii)

He was conscious, in other words, that he deployed in research enquiries a conceptual discourse instrumentally and only clarified the 'logic' of his meaning retrospectively. The emphasis of *Writing for Social Scientists* - which Becker was probably writing or planning when

he and Passeron met in Marseille - was to focus attention on the need to clarify linguistic meaning when communicating research findings. Typically, Becker's book represents writing as a process of improvisation - that meaning is constituted and refined in the process of producing drafts, revised drafts, and final drafts. *Writing for Social Scientists* concentrates on the retrospective clarification of language use when communicating findings. He was to write a further book which argued that linguistic reflexivity should be an integral part of the research process itself, but, even here, he makes this case retrospectively. In his Preface to *Tricks of the Trade* (Becker, 1998), Becker comments:

"Having to explain what you do to students pushes you to find simple ways of saying things, examples that give concrete form to abstract ideas, and exercises that give students practice in new ways of thinking and manipulating what they learn in their research." (Becker, 1998, ix).

He found that he developed 'tricks' from his own research experience which he could communicate to his students, with the result that:

"... having written a book on the problems of academic writing (Becker 1986b), I decided I could follow that up with a book on 'thinking' if I started with the materials in the file of 'tricks' I had started." (Becker, 1998, ix).

In other words, Becker extrapolated 'tricks' derived from his own research experience to generate a battery of tricks to be deployed by students when undertaking their research. Passeron makes no reference to this later Becker text, but there is a clear affinity between Passeron and Becker in their common interest in the integral function of concept formation in relation to research practice and in relation to the communication of research findings. Arguably, however, Becker's 'tricks' are almost unconsciously prescriptive or directive whereas Passeron seeks an analysis of "writing, rewriting and 'truth-telling' in sociology" which is itself sociologically sensitive to the relative positions of staff and students.

2.

In introducing the French translation of *Writing for Social Scientists*, Passeron noted that Becker had already in 1985 been offering seminars on writing as 'group therapy'. The first question to be considered, therefore, was whether the problems experienced by students and researchers in writing in the social sciences was one of 'psychological inhibition' as might be experienced by 'any author'. Posing the question in this way, Passeron was really asking two separate questions: firstly, are psychological inhibitions discretely independent of the social conditions in which they become manifest, and, secondly, are there factors particular to the writing of social science which induce inhibition? Passeron suggested that individual character traits do not adequately explain inhibition nor do they explain the anxiety of intellectuals about the 'value' of their work. Although Passeron proceeds to use the language of psychology ("The ego aspires to the ideal; ..."), he insists that the psychological condition is induced within "a society increasingly marked by the fiction of mass education". He blames a social situation in which

celebrity or, in this instance, intellectual distinction, are thought to be effortlessly accessible to all as of right, and in which, correlatedly, failure is thought to merit stigma.

3.

Passeron then turns to the second part of his question. Why is inhibition most acutely felt in the social sciences, as evidenced by “the very high rate of abandonment of planned theses, through stress, breakdown or disillusionment”? He suggests that the problem does not lie in the difficulty of linguistic expression but in the disinclination to subject written texts to judgement precisely because, unlike spoken conversation, written communication entails a commitment to unmodifiable meaning. But this does not yet explain the prevalence of inhibition in writing in the social sciences. He goes on to claim that the particular problem in the social sciences arises from their assumed universal applicability and, hence, to the reluctance to debate proximate findings in immediate and local contexts. Passeron takes many of the characteristics of social science writing exposed by Becker in examples from the work of students and researchers and tries to account for what he calls the “semantic fog surrounding sociology’s most abstract concepts ...”. What he calls the “semantic dilution” of many sociological formulations is paradoxically explicable in terms of a crisis in scientificity. As he puts it: “... they are no more than expressions devised expressly for the purpose of quietly avoiding the scientific obligation always to formulate assertions which refer clearly and unequivocally to a definite mode of confirmation or objection”. They are expressions which are deliberately resistant to verification or falsification, whether of the testability of a Bachelardian kind outlined by Bourdieu, Chamboredon, and Passeron in *Le métier de sociologue* (1968, 1991) or of a Popperian kind.

4.

Sociology of education can explain how contemporary society has diminished this capacity to emphasize intellectual apprenticeship, but, more fundamentally, Passeron believes that the social sciences have “not converged to unite in a more or less homogeneous semantics”. In *Le métier de sociologue*, Bourdieu, Chamboredon, & Passeron argued that the social sciences needed to develop a distinctive epistemology which was neither dependent on positivism borrowed from the natural sciences nor on hermeneutics operating on idealist assumptions. Passeron’s work has been dedicated to the tasks of establishing such a distinctive epistemology of the social sciences and of differentiating between disciplines within the social sciences (between history, anthropology, and sociology in particular). Passeron concludes that the inhibition experienced in social science writing arises from a fear of risking explicit articulation and the consequent confrontation with disagreement. The cultivation of “the academic language of imprecision” is the consequence of the social conditions of pedagogic encounter and what is needed is “a social re-apprenticeship as interactive as that in which the fear of speaking clearly originated.”

5.

After this general consideration of the problem, Passeron moves towards a closer comparison between his own work and that of Becker. He suggests that Becker's 'interactionism' was strengthened by his acquaintance with the exposure of systems of objective reproduction in *La Reproduction* (Bourdieu & Passeron, 1970, 1977), whilst he claims that Becker's tendency to locate writing and communication in a classroom context was a corrective to the influence on himself of Weber's 'sociology of understanding' and also of his persistent inclination to understand communication in terms of inter-personal, Freudian encounter, without sufficient reference to systemic social context. Passeron attributes the supposed weakness of his perception of the problem of social science writing to the fact that it was only in "one-to-one tutorials that I encountered the resistance of doctoral candidates who balked at writing ...". This denied him the possibility of sociologising the pedagogical encounter which was the supervisory situation. He admitted that he had mostly failed "to 'sociologise'" the thinking of his students, whereas, by contrast, Becker's book was the product of group sessions in which writing of social science itself became socially collective.

6.

After this expression of his admiration for the success of Becker's writing seminars, Passeron proceeds to articulate the two reasons for the epistemological agreement between himself and Becker – "that *sociological reasoning and sociological writing are indissociable*". Both shared the view that 'thought' does not have autonomous existence, preceding expression. It follows that problems of 'expression' are not simply problems of writing but, inseparably, problems of thinking and expressing. Both, equally, were opposed to 'traditional or modern scientisms' in believing that 'social action' cannot be understood abstractly but only by reference to the concrete actions of individuals acting in particular times and places.

7.

Passeron proceeds to emphasize slightly differently his conviction that sociological explanation cannot be abstract. Sociological statements are never, he insists, abstractly 'true' and this enables him to give more philosophical force to his earlier psychological argument that the difficulty experienced by researchers in writing social science arises from their fear of judgement. That fear exists because researchers are initiated early into the belief that their findings will be assessed by established authorities against universal criteria of truth and falsehood rather than dialogically by other researchers who are also fellow practitioners. This means that, for Passeron, the kind of 'epistemology' of the social sciences which is possible is distinctive not by virtue of its particular logical status in abstract but by virtue of the particular dialectical relationship in the social sciences between fact and explanation. As he puts it; "...the sociology of scientific practices is, like their history, an indispensable prerequisite for their epistemology", and this applies to every researcher. Passeron, therefore, asks for *linguistic* reflexivity in the conduct of research on the understanding that this does not involve



a detached scrutiny of language use in terms of logic or grammatical correctness but, rather, a constant scrutiny of the constitutive function of language in identifying and solving problems. It is for this reason that Passeron admits to a skepticism, which he shares with Becker, about the standardised manuals which purport to teach how to write. He argues that these manuals confuse the six functions of expression articulated by Roman Jakobson. Social scientists are involved in the 'referential' use of language – which Passeron calls 'assertory' – rather than, for instance, in poetic or emotive uses. In order to define the epistemologies of specific social sciences, Passeron then contends that we need to define the 'logical space' within which distinctive dialectical language/phenomenon usages operate. Passeron's footnote here to Wittgenstein's *Tractatus logico-philosophicus* relates his understanding of the identification of different 'logical spaces' for different disciplines or discourses to Wittgenstein's essentially socio-linguistic criterion for determining truth or falsehood. Unlike Becker, perhaps, Passeron adds his philosophical concern with truth and verification to Becker's more pragmatic orientation.

8.

Passeron elaborates next the second conviction which he shares with Becker – that sociological explanation is dialectically involved with social actions. The corollary of this shared conviction was that both emphasized "concern with the 'case-study' and 'collections of cases'" (p.9). Passeron points to Becker's co-editorship, with Charles Ragin, of a book devoted to the question of what constitutes a case-study (Becker & Ragin, 1992) and to his own work in exploring the use of case-studies in the histories of Greek medicine and modern psychology. The important point, for Passeron, is that case-studies are vital elements in the research process – that is to say that they do not exemplify pre-formed theories but participate in the theory-making process. The difference here is between the inductive approach used in the natural sciences in which particular cases are subsumed vertically in a hierarchy of universality of explanatory laws and an approach involving '*thinking by cases*' which operates 'horizontally'. The former procedure in the social sciences assumes falsely the validity of a research model derived from the natural sciences whereas the second approach focuses on the particular characteristics of social scientific analysis. Whereas induction is effective in generating explanations of the universally common behaviour of atoms and molecules, thinking by cases grasps the continuous contingency of human behaviour. It "places the question of what 'speaking and writing truth' means in sociology in its true 'epistemological place'" – recognizing, that is, that social scientific explanation is pragmatic rather than referential, and, therefore, involves a distinctive criterion of 'truth', one which is always provisional and never absolute.

9.

This articulation of an epistemology of the social sciences which is neither exclusively positivist nor hermeneutic enables Passeron to return to his opening sense of the crisis of writing in the

social sciences. The anxiety of researchers and students about communicating their findings arises from the misguided pursuit of a wrong kind of truth. Passeron refers to Becker's classroom research to reiterate that sociologists, "whether students or established are all equally lacking any possibility of arriving at a single or dominant theoretical paradigm (and thus are always potential competitors or adversaries in their struggle to defend at all costs the truth of their statements) ...". By his choice of language, he here introduces another way of talking about the problem. The error is to pursue a *single* theoretical paradigm, or, to put this in terms more associated with Raymond Aron's critiques of Durkheim and Bourdieu, the error is to seek any *totalising* social explanation. To relinquish this totalising orientation is to liberate discourse and diminish anxiety. However, the rejection of the quest for one total social scientific explanation does not imply a retreat to working within delimited discourses such as those of economics, anthropology or history. On the contrary, Passeron argues that we should embrace pluridisciplinarity. We need to adopt plural methods to establish pragmatic truths rather than singular procedures to define a singular truth. As Passeron puts it: "Communicating a form of knowledge like that of individual historical instances can only operate *without semantic losses* through texts which synthesise multiple dimensions of description."

10.

Passeron accused Bourdieu of allowing his battery of conceptual instruments – 'habitus', 'cultural capital', 'field' – to become referentially true accounts of social reality, and of developing 'reflexive' sociology to enable sociology to become a meta-science which could explain all other science. Nevertheless, Passeron's commitment to theoretical and methodological 'plurality' has its own totalising orientation. As he moves to a conclusion, he discusses the 'vision of sociology' which, as a discipline, has its own distinctive problem of being "indissociably both synthesising and historical" by contrast with "most other social sciences" which have developed autonomous procedures. Economics, demography and linguistics are cited as operating with simulated models whilst history and historical anthropology are cited as involving "chronological recounting", and these contrasts are related to the distinctive traditions which have been the legacy of the work, respectively, of Durkheim and Weber. Passeron's vision of sociology does appear to be of a meta-science which eclectically and pragmatically deploys the specific approaches of 'other' social sciences to generate transient truths which transcend their circumscribed perspectives. It is Passeron's belief in transience or continuing provisionality that saves him from totalisation. If sociology is indissociably both synthetic and historical, Passeron tries to subsume the synthetic under the historical whereas he would claim that it was Bourdieu's mistake to try to do the reverse.

11.

Passeron returns to a summary of the strengths of Becker's book before articulating his own concluding remarks. In these final comments he repeats his opposition to the pursuit of a "unified paradigm" and emphasizes that he advocates theoretical plurality in order to ensure that research in the social sciences remains an ongoing process, constituting meaning dialectically through the continuously sensitive use of language. He concludes: "The sociological study most devoted to exploring the *empirical* peculiarities of a 'field' or a 'case' is also a *theoretical* work of continually recasting the language of description of historical worlds, constantly replenished with new 'facts' which could not have been observed if a new theoretical framework had not first made them conceivable."

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## Part II



## **'... now that's cultural studies!': reconsidering a conference paper on retro style**

**Sarah Baker**

Many academics recycle their work and due to increased workloads and pressure to publish research this is not surprising. This paper, which was given at the PhD workshop on 16<sup>th</sup> June 2008, is not so different in this regard. It reuses a paper I gave at a workshop called 'Putting Pierre Bourdieu To Work' on May 15<sup>th</sup> of the same year. However, the paper is also an attempt to reflect on the academic context in which the original presentation was given and to consider the response that I received. While this may seem like an exercise in 'navel gazing', by completing this exercise I hope to consider the relationship between sociology and cultural studies and the role that institutions have in shaping the position of PhD students and their work.

I suspect that this 'paper within a paper' is more suited to spoken presentation, than the written word. However I believe it adequately demonstrates the process of redrafting and revision involved in academic research, which Derek Robbins highlights as one of the focuses of this yearbook, and thus is, I hope, a useful exercise. The paper begins by describing the context in which the original presentation was given. I then insert the original paper that I gave and the questions I was asked. I reflect on these questions and consider how my research has progressed since this experience. I conclude by discussing my own position within academic disciplines.

### **'Putting Pierre Bourdieu To Work'**

'Putting Pierre Bourdieu To Work' was a workshop hosted by *The Centre for Research on Socio-Cultural Change* (CRESC), a research centre funded by the *Economic and Social Research Council* (ESRC) in collaboration with the *University of Manchester* and *The Open University*. The centre has received £3.7 million of funding until 2009. Whilst interdisciplinary in focus, the majority of academics involved in the centre are from sociology departments. Their research interests are divided into four key themes. These are 'Cultural Economy', 'Transformations in Media, Culture and Economy', 'Culture, Governance and Citizenship' and 'Cultural Values and Politics'. In researching these areas many of the academics involved in the centre draw on the work of Pierre Bourdieu. Thus Mike Savage, the director of CRESC, and Loïc Wacquan, the originator of the 'Putting Pierre Bourdieu to Work' series of conferences, decided to hold the workshop at CRESC. Previous workshops had been held in New York, Berkeley and at Ann Arbor in Michigan.

The call for papers defined the workshop as an 'interdisciplinary vehicle for doctoral students and post-doctoral researchers conducting social research deploying Pierre Bourdieu's concepts to present and discuss their work and to learn from each other's approaches, problems, and solutions.' However, this open and inclusive intention, was made difficult by a number of factors. According to the organisers they had an unexpectedly high number of abstracts in response to the call for papers. This meant that the workshop was extended from one to two days and the time given for individual papers was shortened from twenty to twelve minutes. In addition when the conference schedule was published it was surprising to find that many of the speakers were established academics. This may have been due to a resurgence of interest in Pierre Bourdieu's work after his death, particularly in a British context where economic inequality seems to be growing (Savage, 2000). It may also have been evidence of academic rivalry and position taking in regard to the legacy of his extensive body of work.

That aside, this meant that there were only four postgraduate students participating in the whole two-day workshop, which significantly changed the premise of the event. For example participants were encouraged to email papers or chapters prior to the workshop. The papers that I received were polished and some had already been published. As one of the four postgraduate students, who didn't have a paper to circulate and was hoping to use the conference to discuss ideas for a chapter, this was particularly disconcerting. This is characteristic of the academic anxieties regarding performance that Jean-Claude Passeron (2004) documents in his discussion of the pressures of academic research.

The discrepancy between the call for papers and the workshop itself may also have partly been a result of a model of workshops usually used in the American academic context being transposed into a British one. Indeed Loïc Wacquant seemed disappointed with the lack of dialogue between the delegates and tried to encourage more open discussion. However, this was also inhibited by his own symbolic capital within the field, as it seemed as though anyone who used Bourdieu's concepts 'incorrectly', in his eyes, would be subject to a weighty critique. This was intensified by a comment that he made, which also seemed to contradict the interdisciplinary nature of the conference outlined in the call for papers. In part of his own lecture when discussing social class, cultural change and identity, Wacquant said:

'People don't have identities that change quickly, you know, twenty identities at once, switching them at will ...now that's cultural studies!'

Whilst this was an 'off the cuff' comment, it reflects an attitude towards cultural studies that Bourdieu and Wacquant previously voiced in 'On the Cunning of Imperialist Reason' written in 1999. In this article they (1999:47) argue that cultural studies is a mongrel domain that does not exist in the French university, and that it owes its international reputation to a successful publishing policy. The impromptu comment of Wacquant at the workshop seems just as



uninformed and outdated as the attitude towards cultural studies evident in 'On the Cunning of Imperial Reason'. It is based on a particular strand of 1980s cultural studies, of which most later cultural studies research, mine included, is critical of. Since its conception, cultural studies has been engaged in investigating and critiquing the perpetuation of social inequalities determined by class, gender, race, age and sexuality, which I would argue makes it largely concerned with similar issues to those of Bourdieu, even though it may take a different methodological perspective. This is a discussion I will come back to further on in this paper.

My feelings of irritation at this statement are, of course, a product of my own position and institutional background. Having studied for an MA and PhD in a department that combines the disciplines of sociology, media and cultural studies, and in which my lecturers and supervisors have encouraged me to read widely across disciplines, I did not feel that I had a position within academic disciplines until recently. Although my work uses the theories of Bourdieu and sociological research more generally, I have, however, increasingly felt engaged with the discipline of cultural studies. At the conference the influence of cultural studies on and in my work was continually emphasised. For example in the panel on 'Class, Culture and Consumption', in which my paper was situated, I was the only person who had conducted ethnographic research and had not used multiple correspondence analysis. Whilst methods are not, and should not, be thought of as inherent to specific disciplines, they are illustrative of their wider characteristics.

Multiple Correspondence analysis was used by Bourdieu in *Distinction* (2005[1979]) and maps cultural consumption in space according to two or three variables or axis. It makes analysis of large numbers of data, common in sociology, easier to analyse. However, I find the use of correspondence analysis to analyse cultural consumption largely problematic, as it does not allow for changes in the meaning of cultural forms. Correspondence analysis can often imply that taste and class are largely unrelated and thus that class is irrelevant in the contemporary context. For example data would show that opera has become consumed by a wider strata of the population. This method does not account for changes in taste hierarchies within opera itself, or that new cultural forms that may have taken its place in creating and reflecting cultural capital. In addition, this more objective approach also seems to discourage reflexivity, as opposed to Bourdieu's general recommendations. For example the other panel members were largely unreflexive about their role in the research process, neither keen to recognise, or include, their position in their own analysis. Although prior to the conference I had envisaged these differences and hoped that this would make for an interesting group of papers, my paper seemed to stand out; as the 'mongrel' paper within a 'classic sociology' panel. This affected the questions, or lack of questions, I was asked. These will be discussed after the original paper, which is inserted below.

## **Practice and field; a discussion of the internal and external distinctions of retro enthusiasts**

Decorating an interior, whether simply putting up a poster or completely gutting a room and redesigning it, is a practice that most people will have been involved in. Significantly fewer people, however, commit large amounts of time and money to their interior. Even fewer define their interior as retro and dedicate their leisure time to making it perfect. This paper draws on 13 video interviews conducted with people from this group. The interviews, mostly with couples, took place in homes across the South East of England in 2006/07. In these interviews I found that an interest in retro is not only a stylistic preference, but it also structures interviewees' understandings, procedures and displays of decorating and their cultural practices more generally. Drawing on both Pierre Bourdieu's (2005[1979]; 1990[1980]) concepts of habitus, capital and field and Alan Warde's (2005) theories of practice, this paper conceptualises retro as a disposition within the wider practice of decorating. It considers the relationship between the internal and external distinctions generated by, and involved in, the making of a retro interior.

In comparison to other decorating dispositions, I would argue that retro maintains a high position both in the cultural and consumer cultural fields, and in social space more generally. Making a retro interior both demands and generates high levels of cultural capital. For example knowing about, finding and displaying an original 1970s sideboard demonstrates knowledge about the history of design and contemporary fashions. Enthusiasts also emphasise that their interest in retro style is motivated by a desire for original and authentic objects that both reinforce and contribute to their unique and authentic identities. These values are similar to the ones that Sarah Thornton (1995) associates with high subcultural capital, and are also those that James Clifford (1988) identifies as positive in the hierarchy of value and as structuring the art-culture system.

It is not only knowledge of past styles and fashions that is needed to make and define an interior as retro. In order that objects and styles are not misconstrued as old fashioned, owners must have the appropriate identity and context for the object. For example the meaning and value of a sideboard changes whether owned by a working class elderly woman and displayed in her front room, or by a young middle class couple and placed in their designer apartment. Whilst age and gender clearly influence the categorisation of an object as retro, the ability to appropriate and transform the value of old-fashioned objects into retro ones is also the product of a particular class position. The capacity to appropriate past objects and styles and make them legitimate is an example of symbolic capital that theorists, such as Pierre Bourdieu and Loïc Wacqaunt (1992:148), and Bev Skeggs (2004), have associated with the middle classes.

Indeed, in my research it does seem that the consumers involved in making retro homes are largely middle class in terms of their own occupations and lifestyles. However, their backgrounds, capital composition and levels and manifestations of commitment to the making of a retro interior vary. The interviewees can largely be split into two groups; those with an eclectic retro taste, and those fully committed to one particular era.

### **Eclectic tastes**

Andy and Louise are examples of the former group. They are passionate about retro, however they appropriate styles from different periods. They both come from middle class backgrounds, and their love of retro was encouraged by their parent's interest in design and antiques. They are enthusiastic about 50s, 60s and 70s styles and enjoy mixing contemporary design with their retro pieces, for example they have a contemporary kitchen.

The eclecticism of Andy and Louise demonstrates their knowledge of a variety of periods and their 'good taste' across a range of objects and styles. Eclecticism or omnivorousness, as Mike Savage (2000) has suggested, is often viewed as a less hierarchical way of acting and can be an attempt to disassociate with the fixed tastes and snobbery associated with high culture. Andy and Louise were not only keen to disassociate themselves with high culture, but also with the fashionable interiors shown by retailers or in the media, preferring to frame themselves as choosing objects and making them fashionable themselves.

However, Andy and Louise's tastes for retro objects or mid-century modern mixed with the contemporary are not unlike those displayed on the pages of high-end lifestyle magazines, such as *Elle Decoration*. In addition to being eclectic, like the styles in these magazines Andy and Louise's taste is also dynamic. They often change their interior and buy new retro furniture from boutiques and from ebay. They were also keen to tell me that their tastes had become more sophisticated over the years. Thus I would suggest that the values and tastes of Andy and Louise maintain and reflect the dominant values of the cultural field and of advanced consumer culture, in which individual and decommodified material objects are bestowed with high value.

Interestingly Andy and Louise work in the media industries themselves; Andy as a publisher and Louise as a graphic designer. Bourdieu (quoted in Swartz, 1997:130) argued that there is often a structural homology between cultural producers and cultural consumers, and in this case they are the same people. Thus habitus and field mutually reinforce each other. Although not directly, Andy and Louise's roles in the media industries and their middle class backgrounds enable them to legitimate their own tastes and enhance their exchange value; Louise can appropriate her Grandad's 1970s wineglasses and define them as cool.

## Specialist tastes

In contrast, Peter and Mary are highly committed to 1940s style. They purposely bought a 1940s house and are keen to fill it with original 40s objects. Even in the interview they wanted to give me an authentic 1940s experience; they played 1940s music throughout and Peter lit the fire rather than putting the heating on. Their attention to detail and in-depth knowledge was also replicated in their interior. For example they had sourced an original 1940s pink bathroom suite that they were about to install.

Their high levels of commitment to retro and the 1940s means that within the practice of retro they gain admiration and respect. This is both from the other 1940s enthusiasts they encounter at the swing dance groups and vintage car rallies they go to, and from other less committed retro enthusiasts. The former have similar styles and interests, and the latter, whilst critical of 'slaves to period style', appreciate their commitment to acquiring original, authentic objects. As Alan Warde (2005:148) notes 'judgements of performance are made internally with respect to the goals and aspirations of the practice itself, and proficiency and commitment deliver satisfaction and self-esteem.' In the case of Peter and Mary their proficiency and commitment make their practices highly specialist. They have a very strong idea of the objects they want in their home, which is guided by an adherence to the styles of the period. This affects their shopping practices, as these quotes demonstrate;

Peter: 'If we decide we need a bed we can't just go out and buy a bed like normal people would do, just go to MFI or Ikea and think 'oh yer that one will do'. Ok it will be in the back of our minds that we need a new bed, but it might be three, four, five years before we see the one that we want and we might happen upon it in the most strangest of places and think we've got to have that bed. Then we work out the logistics of getting it home from Newcastle or wherever it is you happen to be when you see it...you know it is a different philosophy.'

Mary: 'Sometimes, you think, oh gosh you know it would be really nice to like the stuff that is normal taste, and it would be easy, but it's just really really satisfying when you are looking for the one, not even when you are looking for the one thing, when you see the one thing, and you just think that is absolutely, absolutely right.'

Peter and Mary's style and their shopping practices, 'waiting for the perfect objects for their home', do not seem wholly compatible with the dominant discourses of consumer culture, which encourage the adoption of new objects and styles and a play of difference. Although their knowledge of the 1940s is exhaustive, they do not have such a general interest in fashion and design and thus have less cultural capital than the other group of interviewees. Whilst Peter and Mary's retro style is socially recognisable and valued because of their commitment to authenticity, it is so specialist that it does not transfer easily into other fields. For example knowing the manufacturing process of an early 1950s English Rose kitchen in detail gives little external reward. This is unless, however, lifestyles such as Peter and Mary's are appropriated and legitimated by the media, which is the increasingly the case.

Peter and Mary, like the other interviewees committed to one particular era that I interviewed, had less institutionalised forms of cultural capital. They had not been to art school like the other interviewees. Although Peter now works as an IT consultant and Mary as a district nurse, they also came from less privileged backgrounds. Indeed it could be these factors that create the propensity towards a more specialised interest, for fear of being interpreted as old-fashioned rather than retro. This also makes their cultural capital more difficult to convert into economic capital without institutionalisation through the media industries or the education system.

## **Implications**

So what does this very brief expose of two groups of retro enthusiasts offer to discussions of the work of Bourdieu? In the abstract I stated that I would discuss the adequacy of Bourdieu's theories for understanding the internal relations and practices of enthusiasts, and this is the point I want to go back to. Theorists such as John Fiske (1989) and Sarah Thornton (1995) have created concepts like popular and subcultural capital in an attempt to recognise that popular culture has resources of its own and to describe the relations of fans or subcultures. Whilst I agree that the conceptualisation of working class or popular culture as lack that Bourdieu (2005[1979]) makes in *Distinction*, is problematic, I also consider Fiske's and Thornton's theorisations to be equally unsatisfactory. Aside from the fact that theories of subculture assume there is a homogeneous culture to be 'sub off', in attempting to democratise the theories of Bourdieu, Fiske and Thornton argue that popular or subcultural capital is less bound by class. In my analysis of retro style and the internal and external distinctions of retro enthusiasts I have certainly not found this to be the case. Whilst the practice of decorating itself is inevitably varied in class terms, the disposition to create a retro interior and the levels and manifestations of commitment to that interior do not seem so varied.

Thus the model I have found most useful is one that combines the concepts of practice and field. This acknowledges that valuable cultural knowledge and resources can exist and circulate within practices without being considered valuable in the fields of cultural production or consumer culture. By separating cultural knowledge and resources from fields we can focus on the process of legitimation; what and whose values and practices are recognisable as capital and/or made symbolic. In terms of the retro enthusiasts in this study who are in a relative high social position this seems to be the majority of their values and practices. Yet, for other enthusiasts in less stable social positions this may not be the case.

## **Questioning, revising and restating**

After giving this paper at the ‘Putting Pierre Bourdieu To Work’ workshop I was asked three questions. The first was addressed to all the speakers on the panel and was related to issues of taxonomy, for example the categorisation of individual enthusiasts and the definition of their class positions. The second question asked whether the participants I had interviewed with specialist tastes continued their interests outside of the home. The third question asked whether I was arguing that subcultures create value in their own practices because their classed identities are not valued in the wider fields of power. Having previously considered these questions in other chapters of my thesis, apart perhaps from the last, I found them relatively straightforward to answer.

I addressed the first question by discussing my methodology more generally. I said that in most cases interviewees had identified themselves as enthusiasts and as having a passion for retro, vintage or mid-century modern. But of course because I argue that retro is a social, cultural and aesthetic judgement, I have had to question the extent to which this project is simply an exercise of my own taste. To try to address this I have adopted a reflexive practice throughout my PhD, which attempts to make my own journey obvious by accessing the knowledge required to recognise retro style.

I gave a more descriptive answer to the second question stating that Peter and Mary, like the other interviewees who had specialist interests, were keen to continue displaying their tastes outside of the domestic environment. However this was made more difficult by conventions in the workplace. In relation to the third question, whether subcultures create value in their own practices because their classed identities are not valued in the fields of power, I said that yes it was the case that extreme levels of commitment to a practice, which I prefer to the term subculture, may be a reaction to less external rewards generated in the wider fields of power, however I only had evidence of this in regard to retro style.

Being asked and answering these questions made me aware of themes that needed to be emphasised in my thesis, for example discussions regarding the differences between private and public space. However, I was surprised and disappointed by the lack of questions regarding my use of Bourdieu’s theories of capitals, field and practice because I had used these terms differently to their typical usage. Firstly I had expected a question on why I had interviewed couples together, rather than separately, as in most analysis of cultural consumption cultural capital is viewed as the property of individuals. I would have answered that this was both a product of interviewees assuming that they would be interviewed as couples, but also indicative of the collective production of taste, which, in my opinion, studies such as *Distinction* (Bourdieu, 2005[1979]) do not address adequately.

I had also envisaged a question regarding my suggestion that cultural resources should be viewed as separate from fields and was ready to answer and to argue this point. I would have

made it clear that I was not suggesting that practices are unrelated to contexts, just that some practices are not valued by fields and are not exchangeable. Thus I would agree with Skeggs (2004:24) when she says that 'it is possible to re-work cultural capital not just as high culture if we think more generally about culture as a resource or use-value which can be separated from fields and means by which it is exchanged.' In addition I expected a question regarding my critique of the concept of subcultural capital. I would have replied by recapping on Sarah Thornton's (1995) theorisations of subcultural capital in which she argues that it does not correlate in a one-to-one way with class. She (1995) suggests that class is wilfully avoided by subcultural distinctions. This contradicts the findings in my study and I would argue that this is because the values integral to high levels of subcultural capital have become those integral to high levels of cultural capital. This means that Thornton misrecognises cultural capital and middle class values.

Usually I can roughly predict the questions that I will be asked. However in this instance there was a mismatch between the questions I expected and those I was asked. The lack of questions regarding subcultural capital may have been a product of the audience being unfamiliar with the term and evidence of a difference in emphasis in studies of cultural consumption within the discipline of sociology. But the question regarding the way I had used the concepts of capital, field and practice; had I got it so wrong that no one wanted to embarrass me? Or was it a product of trying to minimise risk and making my argument so subtle that no one understood it? The latter, as Passeron (2004) argues, is common among students and researchers and due to the anxieties I felt due to the change in the premise of the workshop would be unsurprising. On reflection I feel that I was nervous of critiquing the theories of a highly acclaimed academic on which a whole workshop was based and in the presence of some of his most avid followers. With the advice of Becker and Passeron (2004) in mind I will attempt to rewrite the argument of my paper clearly and directly below.

In my research, theories of capitals and fields seem highly useful when describing the practices of retailers, the media and the power relationships of the middle classes. However when it comes to the practices of retro enthusiasts in their domestic spaces, especially those in less dominant positions, I need an additional theory or concept, otherwise all actions are reduced to the accumulation of capital. Thus I require a theoretical framework that focuses on use as well as exchange value, a theory that combines Bourdieu's theorisations of capital and fields with one that looks beyond practice as motivated by the accumulation of capitals. This mix and match approach is, of course, typical of a 'mongrel discipline' like cultural studies.

**Conclusion: Cultural Studies the 'mongrel' discipline**

In light of my experience at the 'Putting Bourdieu to Work' workshop, and the process of returning to the comment made by Wacquant, has only made my alignment with the discipline of cultural studies stronger. Whilst I feel that the research emanating from cultural studies should be more concerned with epistemology, a lesson that could be learned from sociology, a mongrel discipline has many advantages. A mongrel is unsure of what it is and thus is relatively open to different approaches. It is also continually questioning, and trying to find, its identity and thus is never complacent. At UEL, at least, I feel this makes for a research environment that advocates interdisciplinarity and the use of theory and method as tools rather than inherent to one particular approach or discipline.

As for the critique of cultural studies research as owing 'its success to publishing policy' and arguing that 'identities are can be switched at will'; whilst some theorisations influenced by cultural studies, particularly those concerning style, consumption and postmodernism in the 1980s/90s over emphasised freedom from categorisations of identity (e.g. Polhemus, 1994), they are matched, and in some cases, exceeded, by theories that denied the relevance of class to contemporary culture and echoed neo-liberal discourse originating from the discipline of sociology (e.g. Giddens, 1991). Theoretical perspectives from both disciplines, it seems, have advertently or inadvertently contributed to the discourses of neo-liberal capitalism. However, at this point in time, in Britain at least, it seems to be theories emanating from cultural studies that are being most critical of these developments, and this is certainly a project with which I would like to align myself.

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## Chapter 6

### Framing Nation-State and Gender\*

Rumana Hashem

In her recent book, "The Body of War", Zarkov (2007) explored the ways in which women were subjected to the ethno-nationalist violence in former Yugoslavia and the ways media representation of female and male bodies were gendered. Mukherjee (2008), in her article, *gendered embodiments* of the nation in Bangladesh showed that violence in Bangladesh Liberation War in 1971 was inscribed upon the bodies of women. While men soldiers were fighting on the frontier for the sake of the motherhood; their mothers, daughters and sisters left at military camps and/or at so called 'home' were subjected to massive reproductive and sexual violence by the Pak-Military soliders. Furthermore, feminist scholarship such as Cockburn (2004, 2001), Enloe (1993), Giles and Hyndman (2004), Hyndman (2008), Korac (2006, 2003, 1998) and Moser and Clark *et al.* (2001) have articulated that there is a notion to see political violence and armed conflict as "male domains, executed by men, whether as armed forces, guerrilla groups, paramilitaries or peace-keeping forces" (Enloe 1993 cited in Moser and Clark, 2001) and that there is a tendency to see women as simply victims in the armed conflict and gendered violence. However, there has been much academic work that demonstrates the complex links between gender, nation, state, and armed and ethnic conflict. The doctoral thesis, from which this paper is extracted, has been informed by these literatures. The thesis explores the gender dimension of violence in the armed conflict in the South-Eastern part in Bangladesh which is a hilly area largely populated by indigenous people<sup>23</sup>. It investigates into the causes of gendered violence in the Chittagong Hill Tracts (CHT) addressing the militarization and extreme human rights violation in the CHT that explores the ways in which the indigenous land became *Bengali Settlers* land.

#### Nation, State and Gender

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\* The paper was presented in the PhD workshop in June 2008 to demonstrate research progress and with an intention to share my ideas and data for the research. In the workshop, I have briefly discussed the research design, theoretical and methodological stance of the thesis as well as attempted to outline some background issues that generated the armed conflict with a view to provide an overall presentation of the thesis. However, this paper is an edited version of the presentation which does not cover all of the above issues, rather it provides with an understanding of the theoretical stand point of the research clarifying and defining the several concepts central to my analysis. The paper has been largely derived from the theoretical chapter of the thesis. It argues that gender, nation and state are intermeshed and shaped by each other which then produces nationalist conflict that produces armed conflict and violence.

<sup>23</sup> The indigenous people in the Chittagong Hill Tracts constitute the most significant number in terms of the other minority groups in Bangladesh. There are thirteen different ethnic minorities in the Hill who belong to the same community see themselves as *Jumma*.

This chapter is set aside to define some of the important concepts will be employed in the thesis, as well as to elaborate its conceptual framework. Firstly, I will attempt to describe the notion of the nation and nationalism in accordance with a growing body of historiographical work. Second, I elaborate a working definition of the state. Given my claim as to the significance of the state to the politics of gender and ethnicity, I undertake to defend-albeit with qualifications-the treatment of the state as a meaningful social actor. While, I am concerned, in particular, to define a necessary conception of the nation and state that sets out the relationship between gender and the modern nation-state, the chapter provides with a discussion that encompasses the totality of state practices. The actual intention is to enable the readers to see the ways in which various social axes intersect each other and how various state apparatus together constitute nationalist ideologies that produces gender specific violence in the nationalist and armed conflict.<sup>24</sup>

Thirdly, I will attempt to analyse the concept of gender and the important historical relationship between gender and the nation-state. For an understanding of gender dimensions of nationalist conflicts and violence, it is crucial to understand the relationship between gender and the nation-state. To meaningfully understand why nationalist movement and armed conflict generate violence with gender implication, one needs to look at the different ways in which the discourse on gender and that on nation-state tend to intersect and construct each other (Yuval-Davis, 1997). It is also necessary to look at each discourse separately as this is the only way to see how they are informed by each other. However, in accordance with a growing body of literatures that include work in sociology, history, political science, post-colonial studies, anthropology and psycho-social studies (as interprets the constructions of identities), I will demonstrate the centrality of gender to the material and ideological creation, justification, and maintenance of the nation-state. An understanding of the gender politics of the nation-state depends not only on an awareness of the content of this history, but moreover on the explanatory mechanisms it set in place, still dominant processes that strongly bind the idea of gender to conceptions of national identity and belonging. I go onto consider how the relationship between gender and the state has been colluded in social and political theory. By insisting on the continuing gender politics of the nation-state, finally, I set out the general structure within which my argument will take place in the thesis.

To begin with, I would like to examine what precisely is signified by the concept of 'nation' and 'state' as is important to analyse the two objects of the concept 'nation-state' separately. The first term 'nation' can simply be described as a population who live in a specific boundary. This boundary is either a geographical territory or an imagined border. The second term 'state' indicates a political apparatus that derives its meaning from the legitimacy of its sovereignty over a socio-geographical population, and, that of the 'nation'. Yet, this is only a self-generated definition and thus over simplistic which could not provide us with any information

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<sup>24</sup> My argument is certainly informed by the deconstructive feminist approach which analyses the gendered construction of the nation and state on the one hand, the intersectionality of other social axes such as race, ethnicity and class, on the other. This is to claim that these axes together construct state and nation which latter forms in gendered violence in the nationalist conflict.

about its historical genesis that could reveal the structures of power underpinning the nation-state or any other state apparatus. For a clear understanding of the process of how 'nation' and 'state' is constructed as one entity that works like a 'syntactic hyphen in yoking together' (to use Pitcher, 2007:18), it is essential to look at the different theorizations of nation, nationalism and state as they have been debated as separate social phenomena.

### **The notion of the nation and nationalism**

Nation and nationalism are conceptualized variously by different schools. As Smith noted, "definitions of the nation range from those that stress 'objective' factors, such as language, religion and customs, territory and institutions, to those that emphasize purely 'subjective' factors, such as attitudes, perceptions and sentiments" (2001:11). Notwithstanding, nations are mainly reproduced by women, both biologically and culturally (Chatterjee, 1994; Mukherjee, 2008; Yuval-Davis, 1997; Yuval-Davis and Anthias, 1989; Zarkov, 2007), most of the hegemonic theorizations of nation and nationalism have ignored gender relations as relevant to this discussion. The primordialist definition of the nation is based on a naturalized image which claims that nations are not only 'eternal and universal but also constitute a natural extension of the family' as well as kinship relations. In these constructions the family and kinship units are actually 'based on natural sexual divisions of labour in which the men protect the women and children' (see Yuval Davis, 1997:15, also see Enole 1989). Definitions that stress 'objective' factors, for example, Joseph Stalin's assertive concept, "a nation is a historically constituted, stable community of people, formed on the basis of a common language, territory, economic life, and psychological make-up manifested in a common culture" (1973:61 cited in Smith 2001:11), certainly isolate important features of the nation. This idea is partially supported by scholars, such as, Bhabha (1995:2), when he notes that the concept of the nation as a symbolic force and 'an idea whose cultural compulsion lies in the impossible unity' and the 'epithet' most commonly used to express the 'authenticity of cultural location'. Such definitions reveal by turn that culture alongside other aspects of nations (ethnic origins, religion, tradition and economy) plays an important role in construction of nation.

However, what seems to be most important for the construction of the nation, as is explored in much of the literatures on nation and nationalism, nations are mainly produced and reproduced by the idea of 'other'<sup>25</sup>. This others can be ethnic, cultural, or religious or racial depending upon the context, as Hutchinson and Smith noted, 'it is generally in the context of other [...] groups who are opposed by the first one' (1996:4). Hutchinson and Smith also pointed out that 'ethnicity', the meaning of the term which is 'equally uncertain as it can mean "the essence of an ethnic group" or "the quality of belonging to an ethnic community or group", or "what it is to be an ethnic group"', is one of the most important tool for the construction of a nation. Taking account of Eriksen's (1993) assertion, the two scholars argued that ethnicity

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<sup>25</sup> The term is widely used by scholars in the field of humanity and social sciences that identifies various ways of the construction of the 'other'. One of the common constructions of the 'other' in nation-state is ethnicity (to use Smith *et al*, 1996), while the other powerful constructions include race and gender (see Anderson 1983 and Hart, 2006, Giles & Hyndman *et al*, 2004). However, this thesis looks at the tools and process that constructs the other, that are ignored by the hegemonic notion of the nation, such as, gender, militarization and internal colonialism (as part of the so called globalization) and the ways in which the state produces 'other' and shapes ideologies of nationalism.

is the common tool for the 'classification of peoples and the relations between groups, in a context of "self-other" distinctions' (Eriksen, 1993:4, Hutchinson and Smith 1996:4). For example, Greeks who are differentiated as 'barbarian other' (racially different from English) tend to characterize the English and the 'non-Greeks-peripheral' as *ethnea*- 'foreign barabairan'. For Greeks, 'others' are those who are ethnically different, the people who's culture, religion, language and traditions are uncommon to Greek ethnic origins. Hence, they can not have membership in the same group and can not belong to the same nation.

Hutchinson and Smith noted,

What these usages have in common is the idea of a number of people [...] who share some cultural or biological characteristics and who live and act in concert. But these usages refer to 'other' peoples who, [...] belong to some group unlike one's own. (Hutchinson and Smith, 1996:4)

However, the dichotomy between a 'non-ethnic "us" and ethnic "others" has continued to dog the concepts in the fields of ethnicity and nationalism' (Chapman et al., 1989:15).

To an account for another core feature of the process through which 'others' are constructed and nations are constituted, it is important to look at the colonial histories. As explored in post-colonial scholarship, apparently, colonial powers redesign the nations and rules as 'mandates or protectorates' (Said, 1995). The colonial mission of the West in the name of the development of the modern institutions and technologies of power created conditions for new nations in the countries of Asia and Africa through. On top of the colonial praxis of extreme economic exploitation to capture land and resources of its colonies, the colonial rulers often create assumptions around class and race to such an extent that produces conditions for the colonial populations to understand themselves "as inferior, subhuman or otherwise" not like the colonial superiors. The marginalized colonial populations long for national sovereignty and follow self-determination as the only way to liberate themselves from the racial and class boundaries, as such (Chatterjee, 1986; Said, 1978; Pitcher, 2007:22) .

The modernist historians of nationalism, such as, Benedict Anderson (1991), insist that the construction of 'the nation' is an 'imagined community' and that nations are not eternal and universal phenomenon. Anderson and his fellow historians (for example, see Gellner, 1983; Hobsbawm, 1990) argue with varying emphases that the phenomenon of nationalism, 'as a description of the convergence between the political state and the population over whom it has jurisdiction' can be traced back no further than the social and Industrial revolutions of the modern period. Contrary to this notion, other hegemonic definition (for example Smith, 1998) emphasised on the ethnic genesis of nations and by doing so Smith locates nation's origins in a common identity antedating the state. For Smith, "ethnic identities and communities constitute a large part of the historical and social background of nations and nationalism" yet, it is not an ethnic community (2001:1 &12). Rejecting the earlier notion of nations, he argues that both "subjective" and "objective" definitions are somewhat vague and problematic, as such. Drawing from Max Weber's (1948) investigation to the construction of nation Smith argues that the objective criteria of the nation, for example, language, religion, and territory often 'fail to include some nations' and such conceptualizations are "stipulative", while subjective definitions of nation and national belonging 'makes it difficult to separate out nations from other kinds of

collectivity such as regions, tribes, city-states and empires which attract similar subjective attachments' (2001:11). However, this does not tell us anything about gender dimension of the nation but enables us to see the seemingly ignored features of nations, which sometimes are 'quite intentionally' ignored (to use Smith, 2001).

None of the above hegemonic theorizations about nations and nationalism has recognised gender relations as relevant to the construction of nation and nation-state. Contrary to such gender blind and hegemonic theorizations, the deconstructive approaches to nation (for example, Chatterjee, 1986, 1994; Mukherjee, 2008; Yuval-Davis, 1997; Zarkov, 2007) articulate that the notion of 'the nation' has to be analysed in relation to gender and to nationalist ideologies that includes not only ethnicity but also race, class and gender. Furthermore, the institutions of the state that enable us to see the dichotomous public/private locations of men and women as well as the construction of 'other' need to be related to the notion of the 'nation'. Such an approach also suggests that nations are situated in specific historical moments and are 'constructed by shifting nationalist discourses promoted by different groupings competing for hegemony' (Yuval-Davis, 1997:4, also see Giles and Hyndman, 2004). However, from the hegemonic notion of the nation, it becomes possible to identify a process by which gendered locations of women and men in the nation-state has been effectively rationalized. While women are the main reproducers of the nation, they have been hidden from the history, and the notion of the nation, except a small (but growing) group of deconstructionist feminists, does not see it is necessary to include a gender dimension of the nation in the discussion.

Indeed, the idea of the 'nation-state' that is derived from nationalist ideologies should be seen as different from that of nationalist projects. Furthermore, it is important to take into account that the membership of 'nations' can be seen as 'sub-super-and cross-states' (Yuval-Davis 1997:3-11) depending upon the nature and structure of the collectivity. History demonstrates that there are people who live in a particular society and within a state but do not necessarily see themselves to be considered as members of the hegemonic nation or the hegemonic nation does not consider them as obvious members (see, for example, Said 1978, 1998,2001;Bhabha 1996;Tibi 1996 ). At the same time, members of national collectivities are living in other states and nations with no state or are divided across several states are widespread. This suggests, as argued Yuval-Davis (1997), that the concept of 'the nation-state' which is often assumed to have 'a complete correspondence between the boundaries of the nation and the boundaries of those who live in a specific state' is a fiction. Most of the boundaries of nations do not virtually correspond with the 'nation-states.' However, such deconstructive approaches to nationalism argue that assuming nations to be coincided with those of the 'so-called nation-state' is certainly a fiction and that this fiction has been at the centre of nationalist ideologies.

Highlighting the crucial importance of social and economic power relations and the 'cross-cutting social divisions in which any concrete historical social categorization is enmeshed', Yuval-Davis rejects the 'extreme post-modernist construction of contemporary citizens as disembodied 'free floating signifiers' (1997:3; also see Wexler, 1990). She argues that these

social divisions have organizational, experiential and representational forms that can have implications for the ways they are linked to other social relations and actions. However, these are “not reducible to each other but have different ontological bases” argued Anthias and Yuval-Davis (1992).

### **The discourse of Nationalism**

Bhabha notes indicating the orthodox and ‘pure type’<sup>26</sup> of nationalism that the nationalist discourses persistently producing an idea of the nation as a ‘continuous narrative of national progress, the narcissism of self-generation, the primeval present of the *volk*’ (Bhabha, 1995: 2). Smith insisted agreeing with others<sup>27</sup> that a nation is not a state and “the concept of the state relates to institutional activity, while that of the nation denotes a type of community”(2001:12). Apparently, the concept which has brought into the term ‘nation’ and ‘state’ together as an essential link and an entity is ‘nationalism’. Post-modernist idea of nationalism has accordingly been based on the ‘presumption of a common linguistic heritage, a religious tradition, the idea of monarchy, or some combination of all three’. The invention of antecedents thus created legitimacy and stability for the radical new institutions and social structures of rapidly industrializing modern nation-state. This is not to suggest that nationalism should be regarded as a ‘mere-fig leaf for some teleological explanation of modernist progress’, but, as rightly pointed out Pitcher, that its development as a social, cultural and political phenomenon was an intrinsic component in the forging of the embryonic nation-state (Pitcher, 2007:19).

However, the definition of nationalism that allows us to see the political construction of nation-state and the fiction of nationalism in the modern state is, in essence, the modernist concept of nationalism. The modernist theorizations indicate that it is “a theory of political legitimacy” that requires that ethnic boundaries are not supposed to cut across political ones and that “ethnic boundaries within a given state should not separate the power holders from the rest” and thus “state and culture must be linked” (Gellner 1983: 1-36 cited in Yuval-Davis, 1997;11). While analysing this definition Yuval-Davis (1997) argued that the “fiction of nationalism is to naturalize the hegemony of one collectivity”and its access to the “ideological apparatuses of both state and civil society”. Yuval-Davis insisted that such naturalization is at the roots of the inherent connection that exists between nationalism and racism,

It constructs minorities into assumed deviants from the ‘normal’, and excludes them from important power resources. It can also lead the way to an eventual ‘ethnic cleansing’. Deconstructing this is crucial to tackling racism on the one hand and to understanding the state itself on the other (1997:11) .

This suggests that the development of nationalism in a sense develops racism and ethnicity which allows state to practice otherness and exclusionary politics to ‘other nationalists’. If nationalism was first and foremost a construct means of securing the viability of a

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<sup>26</sup> See Partha Chatterjee’s (1986) *Nationalist Thought and the Colonial World* for a detail debate on the problem of conceptualization of nationalism. Chatterjee’s work also enable us to see that much of the post-colonial literatures are highly influenced by the Western conceptualization of nationalism.

<sup>27</sup> Despite their differences in conceptualization of nation, scholars concerned and devoted to study the subject of nation, nationalism and nation-states, based in various schools and disciplines, agree that nation and state are two different concept. For example, see Max Weber 1948, Stalin 1973 and Anderson, 1991 for detail.



homogenous national culture, the form I have described it limited to the extent that it remains the idealist creation of national elites, “the bourgeois of bourgeois revolutions” (to use Pitcher, 2007:20). For the concept of nationalism to become thoroughly naturalized as hegemonic description of national unity, it needed to be embodied in an organic form that would rely not on the authority of its propagandists, nor on abstract bonds of allegiance to the capitalist state, but, as suggests Pitcher, “on a supplementary concept that would link together a disparate population in a fictive bond of kinship to which all could lay claim by virtue of their living within the territory of the nation –state”. The supreme organic ideology of nationalism finds form in the concept of race. However, this is not to suggest that the spectre of race arises (of its own kind) from the nationalist project. Rather, as Benedict Anderson argues, its intimate connection to ideas of ‘blood and breeding’ could be best understood to have originated, in the ideologies of class rather than nation, as the means by which “European aristocracies defined and maintained their elect status in the rule of subject populations” (1991:149-50, also see Anderson, 1998). Nevertheless, appointed to nationalist projects it becomes possible to identify a process by which race has effectively been democratized<sup>28</sup>. The same process also allows democratization of gender inequalities in the nation and the nationalist ideologies. Yet, again, what worth investigation here, is, who identifies the differences between groups and who constitutes line between nations and ethnic groups that provoke people’s desire of self-determination and enable people to follow the problematic ideologies of nationalism? Who develops the concept of race, ethnicity, class and gender that shapes nationalist ideologies? As pointed out Hutchinson and Smith, in a plural society “the ethnic and cultural segments are institutionally separated and incorporated differentially and unequally into the national state” (1996:237). Apparently, it is the ruling regimes of the state and the nationalist leadership that produces ‘other’ using various differences between cultures, traditions and religions. The ruling regime of the nation-state identifies ethnic origins of people and creates conditions for membership in certain groups. People’s failure to demonstrate sameness to certain groups and legitimacy to certain groups are identified as other and they become marginalized in the nation-state, as stated earlier (see Enloe, 1996, Smith 1996, Korac, 1996). Furthermore, the dichotomy between ‘us’ and ‘others’ has reproduced in the ways in which the Latin *natio* was applied to distant, barbarian peoples, whereas the Roman term for themselves was *populus*.’ Hutchinson and Smith argues that it is also in ‘the English and American (White Anglo-Saxon Protestant) tendency to reserve the term ‘nation’ for themselves and “ethnic” for immigrants peoples, as in the frequently used term “ethnic minorities”’ (Hutchinson and Smith, 1996:4-5). Such exclusionary politics create conditions for resistance and the marginalized groups of people in the nation-state often find self-determination is the only way to respond to such oppression. Nationalism has its roots here. Apparently, the discourse of nationalism, is so kaleidoscopic and seemingly paradoxical a set of phenomena.

### **Theorization of the state and its institutions**

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<sup>28</sup> Furthermore, the concept of race does have much to prevent the disaggregation of individual groups by its inscription at the common level of the nation-state. See Anthias and Yuval-davis 1992 and Ben Pitcher 2007 for a

Now, if we look at the theorization of the 'state' as a separate sphere from that of 'the nation' and 'the civil society', which, indeed is essential for an adequate analysis of the relationships between gender relations and national projects, we will see that state often plays an important roles in both gender relations and nationalist projects. Likewise nation, the concept 'state' has been analysed from different theoretical perspectives and in very different ways (for example, Lenin,1977, Stuart 1984; Yuval-Davis and Anthias 1989) and positions of different theorists vary from each other to a large extent. Some of these are extremely idealistic and, thus, problematic. Stuart Hall's definition of the 'modern state', as one of those problematic, claims that state should be seen as an institution which must have a 'shared power', 'constitutional and legal definition of rights to participate in government', 'wide representation' as well as 'power' that is 'fully secular'. He also argued that state must have 'boundaries of national sovereignty' that are clearly defined (1984:9-10 cited in Yuval-Davis, 1997:12). This concept of Hall is challenged by the deconstructionist feminists who articulated such definition does not allow us to see the ways in which contemporary nationalism interrelate with racism. Further to this, the term 'boundary' and 'national sovereignty' requires contextual justification. History demonstrates that state produces boundaries that exclude 'other nations' and construct ideologies around race, ethnicity and gender to rationalize exclusionary politics within the same state (see Said 2000; Enloe, 1980; Loomba, 1998; Zarkov, 2007). Studies in state, conflict and violence also argue that in the present international state system geography of absolute national spaces has been mapped which is then seemingly reconfigured through organized human violence (see Cowen and Gilbert, 2008). ). This violence is either produced by internal colonialism (see Stone, 1996) or enforced by global imperialism (Hyndman,2008; Said 2004).Thus, national sovereignty is persistently under threat and borders of the states are produced and reproduced by colonial and imperial powers.

Another important attribute of the state is militarization through which modern states maintain exclusionary politics. Work on the political economy of armed conflict and violence explicitly emphasise on the intimate and perennial relationship between modern state and militarization (for example, see Cowen and Gilbert et al, 2008; Giles and Hyndman, 2004; Blentine and Sherman, 2003; Said, 2004,2000,1995; also see Huggan, 2001). Apparently, military is the core element of the post-colonial states through which they function and violate citizen's rights in the globalized humanitarian era and constructs 'race-nation', 'ethnic nation' and 'gendered nation'. Through its military, state shares power with ruling class and silences civil society, whether secular or religious, as Said (2004) articulated in his eloquent writing *Enemy of the State*. In the modern state people who have the 'temerity to speak out' against or criticizes state's exclusionary policies in public and 'displeases the powers that be', they are usually 'severely cut down' and 'systematically punished' through state's emergency decree (i.e. military rule) and imperialism. Thus, exclusionary praxis and construction of 'other' could not be defended at any level in the state. Whereas nations and communities were abandoned to colonial powers in former states, Said warns

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detail discussion.

[...] there is now a situation of such profound antagonism, whereby the individual citizen can be threatened with near extinction by government and ruler, that the entire balance between various interests in the state has lost all meaning. (2004:82).

Despite the claim for civil society and multiculturalism within the society, the terrible abuse of power, in various levels in the society, is one of the main features of the modern nation-states. Said suggested that 'neither a constitution nor an election process has any real meaning' in modern states as "suspension of law and justice" takes place frequently "with the relative acquiescence of an entire people" (2004:80). Most striking is that the concept of the modern democratic state was found in the context of distortion of anarchism in the society. The concept of the state has been defined by Smith as "a set of autonomous institutions, differentiated from other institutions, possessing a legitimate monopoly of coercion and extraction in a given territory" (2001:12) while simple Marxist (1954) theory of state indicates that it is an independent institution that serves the interests of and built for the ruling class only. Such theorization explores the nature of many modern nation-states as they maintain the trend of 'shared power' as part of the politics of neo-liberalism and globalized humanitarianism. However, the territories of the state are produced/reproduced using various power tools of the state such as race, ethnicity, class, and are enforced by colonial and imperial power. Now, to account for another important function of the state, we need to turn our attention away from the hegemonic theorizations and move towards the often ignored dimension of the nation-state. It is important to discuss the theories that enable us to see the gender politics of state (colonial and postcolonial, ancient and modern, imperial and democratic) and its institutions, how these are seemingly producing 'gendered -nation'. The deconstructive post-modernist analyses reveal that state is seen as a single and individual identity in terms of international relations, while dealing with relations between state and society it becomes heterogeneous in character<sup>29</sup>. In her investigation into the widely influential classical theory of 'the social contract', Carole Pateman (1988) found that the sphere of civil society has been divided into two 'binary opposite'-the public and private domains. Furthermore, men's location is the public domain while women are located in the private domain and the domain which is considered as politically irrelevant. However, state should be understood as an institution that is 'not unitary in its practices, its projects or its effects' (Yuval-Daivs, 1997:14). Anthias and Yuval-Davis have long-drawn-out this concept and defined state as

a body of institutions which are centrally organised around the intentionality of control with a given apparatus of enforcement (jurisdical and repressive) at it's command and basis...Different forms of the state will involve different relationships between the control/coercion twin which is the residing characteristic of the state. (Anthias and Yuval-Davis, 1989: 5)

This suggests that in the modern democratic state, various social institutions, for example, schooling and the media, can be used for ideological construction. Hence, argued Anthias and Yuval-Davis, these cannot be seen as 'inherently part of the state' and they are 'not even owned' by the state. There is a need to differentiate between state institutions and civil society institutions and the domain of family and kinship relations, in order to understand the state

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<sup>29</sup> See Yuval-Davis, 1997, p.12-15 for detail of such theorization of the state.

and society. Indeed, civil society includes only those institutions, collectivities, groupings and social agencies which lie outside the formal custom of state parameter outlined but which both inform and are informed by these. The domain of the family includes social, economic and political networks and households which are organized around 'kinship or in friendship relations' (Yuval-Davis, 1997:14).

However, all three of the above domains<sup>30</sup> construct their own ideological contents and in different states they would have differential access to economic as well as political resources. As none of these spheres is ever homogenous, different parts of the state can act in contradictory ways to the other. Moreover, their effects on different ethnic, class, gender and other groupings in the society could vary as well. The states nature can also differ in the extent to which their powers of control are concentrated in the central state government or in local state governments. They also differ in their tolerance towards different political projects which are in conflict with those that hegemonic within central government. These questions of the correspondence, in political projects, of the different components and levels of the state also involve the questions of what are the mechanisms by which these projects are being reproduced or changed; of how state control can be delegated from one level to the another, and most importantly, of how sections and groupings from the domains of civil society and the family gain access to the state's coercive and controlling powers. It is within this context that the relationship between 'nations' and 'states' as well as between other forms of ethnic groupings and the state has to be analysed as a precondition to understanding the ways women and men affect and are affected by these processes. Only then we will be able to understand that the ideological and political construct 'nation' is separate from that of the 'nation-state' (Anthias and Yuval-Davis, 1989; Randall and Waylon, 1998; Yuval-Davis, 1997).

### **Gender and the nation-state**

To account for the notion of the nation and the construction of nation-state, it is necessary to look at the discourse on gender –how the concept is informed and how it tends to intersect the discourse on the nation and the state. An account for the gender construction of the nation-state is also crucial because gender, ethnicity and class- albeit 'with different ontological bases and separate discourses' - have been intermeshed in each other. All of them are articulated by each other in concrete social relations. They cannot be seen as preservative and 'none of them can be prioritized abstractly' (Kennedy, 2005; Yuval-Davis, 1997; Anthias and Yuval-Davis, 1992). However, the discussion, here, on 'gender' is based on the deconstructive feminist approach that sees the 'theoretical debates around the category of 'woman'<sup>31</sup> and 'the relationship between the notions of 'sex' and 'gender' as essential. This is a means by which one can understand the debates on the different power relations between men and women on one hand, analyse the ways the relations between women and men affect and are affected by various nationalist projects and processes. The ways notions of femininity and masculinity are

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<sup>30</sup> The state, civil society and the domain of the family.

<sup>31</sup> The category of woman is important for our discussion in terms of class, race and ethnicity. As these are the ones which articulate different power relations and they construct nationalist ideologist which lead in the formation of the race-nation and ethnic nationalism, the category of woman discourse needs to be understood well.

constructed within nationalist discourses could also be seen through such a framework (Yuval-Davis, 1997:4).

The concept of gender, within sociology and feminist literatures that explore the intersections of different power apparatus of the society (see, for example, Anthias, 2001; Anthias and Yuval-Davis, 1992; Enloe, 1980; Kennedy, 2005; Knapp, 2005; Lentin et al. 1997; Stanley, 1979; Stassiulis and Yuval-Davis, 1995; Yuval-Davis and Werbner, 1999;) has usually been understood as a power relation between men and women, while some gender-blind literatures attempt to define gender with 'sex', ignoring the social, cultural and political construction of the term. However, gender differences are not embedded in sexual characteristics of women and men, it is the meaning people give to this biological reality. Women are seen as the reproducer of the nation and 'the ideology of woman as mother is a dominant symbolic imagery through which the position of the woman becomes visible in national projects' (Mukherjee, 2008:36, Walby, 1992; also see, Zarkov, 2007). The predominant culture in every society does identify women as the caretaker of 'home' and responsible for (re)producing children for the nation through which women become increasingly precious property for the nation (as well as the subjects of the enemy of the nation) which in turn provide men of the nation with the right to protect and control women (Korac, 2006, Walby, 1992). Such rights enable men to see women as powerless on one hand, the essential role of reproduction and the identity of 'precious property' of the nation/and its men construct women's subjectivity to any potential violence and danger, on the other. Whatever their actual ethnic background is, women's role and location is always identical. Further to this Giles and Hyndman argue that gender relations and identities are often "reproduced by governments, militaries, militia's schools, sports and media" and thus, gender is an important analytical tool for understanding the "internal dynamics of pre war, war and post-war situations" in the nation-state (Giles and Hyndman, 2004:4).

Gender is always embedded within other social forms (Holmwood, 2000 cited in Walby, 2005:331) and the differences between men and women could easily be found in their social positioning and the dichotomous constructions of social spheres such as the 'public/ private domains' (Walby, 2005). While much of the feminist analysis of women's oppression has been recognized it is related to their location in a different social sphere than that of men, others recognized the divide of nature/civilization is similarly oppressive to women. Such divides enable us to see the power differences between men and women on one hand; construct gendered roles for women and men in the nation building process, on the other. As women's naturalized location is the private domain, which is not considered as politically relevant, it becomes possible to reject the central role of women in the nation building process. Yuval-Davis rightly pointed out "as nationalism and nations have usually been discussed as part of the public political sphere, the exclusion of women from that arena has also affected their exclusion from that discourse (1997:2). Although women have been the chief reproducers of the nation, women's inclusion 'explicitly in the analytical discourse around nations and nationalisms is only a very recent and partial endeavour' (1997:3). Ignoring women's significant

role in the national production and reproduction the notion of the nation often relates the state bureaucrats and intellectuals to national growth. Furthermore, materialist analyses of nationalism have given primary importance to state bureaucracy and other state apparatuses in establishing and reproducing national and ethnic ideologies as well as boundaries. National and ethnic divisions also operate within the civil society though, "it is the differential access of different collectivities to the state" that dictates the nature of the dominant national ethos in the society (Yuval-Davis, 1997:2).

The validity of these divides has been challenged by a growing number of feminists (for example, Pateman, 1988; Yuval-Davis and Anthias, 1989; Grant, 1991, Yuval-Davis, 1997); nevertheless, both of these dichotomous lines are actively contributing to the formation of different role for men and women in the nationalist movement. Feminist analyses of such divisions between women and men have pointed out that public realm could not be understood entirely in the absence of the private sphere and 'meaning of the original contract is misinterpreted without both, mutually dependent halves of the story' ( Pateman, 1988: 4 cited in Yuval-Davis, 1997). It is argued that these divisions are 'fictional' to a large extent and they are both gender and ethnic specific, and that often 'this division has been used to exclude women from freedom and rights' (Phillips, 1993:63 cited in Yuval-Davis, 1997:5, also see Rowbotham, 1973), yet nation-state continue to praxis such exclusionary process of women. It is also suggested that the line between the public and the private is a 'completely inadequate tool for analysing constructions of civil societies is post-colonial nations and that a 'non-westocentric analysis of gender relations cannot assume the boundary between the public and the private as given' (Chatterjee, 1990, cited in Yuval-Davis, 1997: 6). Yuval-Davis,

the identification of women with the nature has been seen not only as the cause for their exclusion from the civilized public political domain, but also as the explanation of the fact that in all cultures women are less valued socially than men (1997:6).

Pnina Werbner (1999: 221) argues that the overt space occupied by 'political motherhood' in the public domain enables a breaking down of public/private dichotomies in which women are stereotypically situated. It creates conditions for the 'feminisation' rather than essentialist definitions of male and female qualities of citizenship which show that women are not victims but active agents of their destiny with a strong sense of concern for family and community' (1999: 231 cited in Mukherjee, 2008:36). Contrary to this Mukherjee (2008) argues that the notion of viewing the "motherhood as a space of public agency for women" is somewhat problematic. Investigation into the factors what for instance are the implications of using motherhood as 'the dominant image through which women are symbolized in the nationalist project for the imagery and subjectivities of women who are not part of that maternal mould", is essential. This requires justification whether the nationalist project makes its public, symbolic space available only to women with certain specific subjectivities. However, analysis of the processes of gendering that are drawn upon to secure 'political motherhood', could bring answers to such questions. Further to this, Mukherjee insists that 'focusing just not on manhood and womanhood but on the processes through which gender roles are embodied

enables us to understand the contradictions inherent in the "fraught relationship between gender and nation" (Mukherjee, 2008:1).

As a means by which the nation-state reveals its problematic notion of women's subordination in the society is patriarchy. The notion of patriarchy claims if patriarchal male identity depends for its existence on unconscious dependence on women as the symbolic bearers of loss and lack, women's refusal to accept this position threatens some men with psychological extinction (Minsky, 1996: 170). The theorization of patriarchy suggests that gender relations should be reduced to and isolated into necessary effects of biological sexual difference, which is certainly problematic. However, deconstructionist feminist approaches argue that any generalisation of women's subordination can 'detract attention from historically specific ways in which gender relations are constructed and in different societies and the ways they are reproduced'. This is to suggest that not all women are equally oppressed or subjugated in the same way and to the same extent. Studies on gender and nation articulate that even within the same society and at any specific moment women have been subjugated variously and to different extent.

Anthias Floya and Yuval-Davis (1992:106-9) have rejected the notion of patriarchy as a distinct social system which is autonomous of other types of social systems such as capitalism and racism. Rather, they argued that women's oppression is endemic and integral to social relations with regard to the distribution of power and material resources in the society. Yuval-Davis argued:

Gender, ethnicity and class, although with different ontological bases and separate discourses, are intermeshed in each other and articulated by each other in concrete social relations. They cannot be seen as additive and no one of them can be prioritized abstractly. (1997:9)

This is to suggest that gender should be understood not as real social difference between men and women, but as a mode of discourse which relates to groups of subjects whose social roles are defined by their sexual/biological difference as opposed to their economic positions or their membership in ethnic and racial collectivities. Sexual differences should be understood as a mode of discourse, one in which groups of social subjects are defined as having different sexual/biological constitutions. However, both 'gender' and 'sex' can be analysed as modes of discourse, but with different agendas. Unless there is separation between the discourse of 'sex' and that of 'gender', as they suggested, biology would be constructed as destiny in the moral and political discourse of that society (Anthias, 2001, Kennedy, 2005, Walby, 2005).

It is arguable that the category 'woman' can be perceived as a unified category only if all these other differences are suppressed, as was the situation in feminist white middle class 'consciousness-raising groups' in the late 1970's which aimed at their participants 'discovering' that the condition of all women is essentially the same (Yuval-Davis, 1997:10, see also, Yuval-Davis 1984). One of the most important differences among women is their membership in ethnic and national collectivities which needs to be taken into serious account before going to any gender analysis. Likewise other differences among women, their membership in the different collectivities should be understood within structures of domination and as articulated by other social relations. These can affect not only the status and power of

some women versus others, but also, as argue Yuval-Davis, 'the extent to which their membership in the collectivity constitutes a 'forced identity 'or can become little more than post-modernist' concept of identity (1997:11). However, this analysis suggests that relations between nations and states in specific historical circumstances play a central role in all these constructions.

Furthermore, the intersections between gender and nation which has been examined by Yuval-Davis in her *Gender and Nation* reflects that 'from the naturalized roles of women as biological reproducers of the nation, through their roles in the cultural constructions of nations, to the ways civil constructions of nation-hood , via rights and duties of citizenship' (to use her own word) , are all 'gendered' (1997:4). Nationalist projects also become gendered through the nature/civilization divide. In this section I have set out the theoretical focus in which the thesis as a whole is located. I am arguing for an analysis in which one can see the gender relations and the dichotomous positioning of men and women are deliberate construction of the nation-state. Such an approach also requires an analysis that addresses the question of gender and ethnic structuration within the context of political, economic and ideological relations (Anthias, 2001; Anthias and Yuval-Davis, 1992, Enloe, 1989; Kennedy, 2005;Zarkov, 2008;). This position also suggest that the phenomena of gendered violence in armed conflict should also be understood in relation to the ideologies, whereby gender is seen as a doctrine that specify women's subordination. Indeed, the construction of gender and gender relations should be looked at with regard to practices and discourses within a range of social forces that includes social classes, race, ethnicity, the nation and the state. For an account on nationalist conflict and gendered violence, there is a need to understand the process of contestation and negotiation amongst these different social forces.

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**Julie Moreira-Miguel**

## **Power struggle for public space between a street art festival and its neighbourhood in Marseille**

Last year I began writing my thesis at the SHADyC under the supervision of Emmanuel Pedler, co-director of the research unit, former student and now colleague of Jean-Claude Passeron. I study performing arts in public space, its role in public policy and its impact on local population. My project focuses on analysing the conflicts that happen during these types of event and what they reveal about social ties and how the groups involved perceive each other. To introduce my talk, I will briefly present links between performing arts in public space (which are called *arts de la rue* in France, *street arts*). In a second part, I will present my fieldwork. It consists in a cultural project named "Small is beautiful festival". I will explain why I can qualify it as a "militant neighbourhood project" and which type of conflicts can emerge during this kind of event because of the way different groups figure each other.

Sorry in advance for the homemade translation of concepts.

### **Street arts and public policy in France**

Street arts as they exist today, emerged in the 60s and 70s as a reaction to conventional forms of theatre and the elitism they are connected to. Street arts were to attract new audiences to art through proximity, accessibility and because they are free but especially by the artists' skill of involving passers-by in the performance and turning them into members of the audience. Performers in public spaces claim to be closer to **all** audiences without compromising their artistic quality or the subversive and dissenting potential of their performance. This potential however, obviously varies depending on the theatre companies and the performances.

You probably know that in France, culture is considered a state matter, and that institutions' positions influence the artistic landscape on the production as well as on the reception side, either through their strength or their absence.

Public institutions use culture as a political tool for educational, entertainment or civilising purposes. In the last 20 years, cultural and social issues have become mixed up with each other in the institutional discourse which prioritises social cohesion, the importance of « citizenship » and of « participation » to treat social ills.

Street arts are imbued with the notion that social ties can be born out of a shared aesthetic and/or festive experience in public space. This is why national, but especially public institutions would consider street arts as a « mobile, temporary and ephemeral cultural tool ».

Also, street arts that forty or so years ago would have considered themselves as counter-culture, have progressively entered the lines of either subsidised art forms or entertainment performances that are programmed by local councils to give life to urban public spaces. It is therefore difficult to class street arts according to Bourdieu's legitimate culture or non-legitimate culture. Audience research has shown that a significant part of street arts festival-goers consider this art form as legitimate as traditionally highbrow forms of art.

### **The « Small is Beautiful » festival : a militant neighbourhood initiative**

My work, which I consider as halfway between sociology of culture and urban sociology, concerns a festival which is organised by an arts organisation called Lieux publics in Saint-André, a popular urban district of Marseille.

Lieux publics is an organisation whose main mission is to produce performances for public space. To a lesser extent, they also programme externally produced performances. They have the status of *Centre national de création* which means that that it receives state subsidies. Lieux publics also benefits from the support of local institutions at all levels such as the region, city... and is hereby has a mission of public service.

For fifteen years, Lieux publics has been based in a popular neighbourhood, almost a village in its own right, which is home to a relatively mixed population : lower middle-class families, elderly people who have always been there but also many families of north-African origin, Turkish and Kurdish families and a high number of settled gipsy families. These populations are stigmatised because of their social conditions and ethnicity.

The staff of Lieux publics believes that the local population are not used to seeing many artistic performances, either out of lack of interest or accessibility. They also believe that they don't use public spaces correctly: not used enough as meeting space, for social exchange, between age groups for instance. Part of Lieux publics' work is understood as wanting to change the under-use of public space. The projects that they run are also meant to be small solution to the ills of urban life.

Lieux publics neither set out to fulfil the audiences' expectations or tastes, nor does it want to transform completely its tastes in cultural consumption. Above all, Lieux publics' objective is to develop the audience interest in performing arts. The programmes' content and its quality is entirely up to Lieux publics. According to Albert Hirschman, the audience has no other choice

than *loyalty*, which is generally the case of the usual audience which travels from the city center, to « *exit* » or leave, or to *voice* its criticisms.

The local inhabitants are in no case at the project's initiative and when they do participate, it will be on the strict terms of Lieux publics.

Lieux publics action is based on a militant approach which favours a particular vision of culture and values which make up part of their identity. To defend this attitude and values, they make use of violence which they consider as « legitimate » when faced with an « illegitimate » violence based on alienating mass culture. In a way, Lieux publics runs a neighbourhood missionary undertaking of which they define the means, the places and the forms.

I don't consider Lieux publics as an instrument of state ideology which would impose an institutional vision of culture. Indeed, the case of street arts contains a lot of contradictions in its relationship with institutions, law and order, and also in their conceiving of highbrow and lowbrow forms of art.

Even though through my education and my own cultural tastes, I share Lieux publics *ethos*, faced with their pessimistic (Passeron would say *misérabiliste*) view of the local inhabitants I am tempted, as an apprentice sociologist, to take the opposite stand.

### **How is Lieux publics perceived in the neighbourhood ?**

Lieux publics being present in the neighbourhood for fifteen years, Small is Beautiful was the first programme run here, in September 2007. The inhabitants that do know of Lieux publics' existence don't really know its work. According to my source at Lieux publics, they were even known as the « weirdoes ».

It is these *weirdoes* that opened the village to foreigners, artists and performing arts professionals from all over Europe that accompanied them, and especially the regular audience having made the trip from the city centre. Lieux publics' communication strategies being reliant mostly on Marseille's cultural networks, this regular audience can be easily identified. In short, allowing myself a moment of spontaneous sociology, as Passeron might well do himself, people of a certain cultural capital (arts and cultural workers, students, academics...), people sharing the same *ethos*, having the same « normative interpretation of the world and its order »<sup>32</sup>.

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<sup>32</sup> Au sens où l'entendent Bourdieu et Passeron dans *La reproduction - Éléments pour une théorie du système d'enseignement* (éthos de classe, éthos pédagogique, 1970).

I believe that this cultural project constitutes a *mixed contact* situation in Goffman's sense. Goffman considers *mixed contacts* as risky interactions between « normal » people and stigmatised people. According to him, an individual is considered stigmatised when he displays an attribute that disqualifies him in his interactions with others.

### **Examples of power struggles and temporary marginalisation**

As I don't have time to precisely and exhaustively describe and all these tensions I have observed, I shall give you several examples to illustrate my point.

What I call power struggles during a performance is for example when young people in the speak loudly over a silent audience or when teenagers throw gravel at a clown, whose performance is nevertheless seasoned and successful;

When a speeding car-driver doesn't brake for a large crowd of people moving from one venue to another, simply shouting at scared and angry pedestrians: "*Oi, this is my place!*".

Another example is also striking: a gipsy family noisily leaving a performance, complaining that both actors were taking off almost all their clothes with the eldest girl telling a spectator: « Showing a man in underpants to children is a normal thing for you, but not for us! » This incident would seem trivial, but as a result of it, some angry teenagers acted out their frustrations on the village square.

I will conclude this series of examples with a case of unintentional marginalisation of a part of the inhabitants by Lieux publics. Lieux publics were approached by a group of retired residents to organise a feast to close the Small is Beautiful festival, as a neighbourhood dinner. However, most of the people taking part of the feast were almost the usual audience or people from the cultural field. Also, one the hand, none of the people who asked for the feast came, and on the other hand, a large part of the village inhabitants couldn't take part in the meal because it was during Ramadan. To explain the absence of people who weren't fasting, my source in Lieux publics simply assumed that "maybe they didn't want to have dinner in front of us".

This may sound anecdotic, but I think that these examples illustrate well what can happen during *mixed contacts* between people who have different norms, different uses, different rationalities. Here people are defining each other in terms of « them » and « us ». One of my issues will be to elucidate what is hidden behind these different « them » and all these different « us » and how these representations can affect all the way people receive an artistic performance and their tolerance to a cultural project they didn't initiated.



For the next Small is Beautiful festival, Lieux publics will be reconsidering its programming and its communication strategy in the light of this experience and will, in a way, change its ways. I hope to follow these developments in the project's implementation, as well as its effects and representations, through ethnographic observation of longer-term interactions and through qualitative interviews and questionnaires.

### To conclude

According to the sociologist Fabrice Raffin, cultural projects linked to territory always contain a dimension of identity defined by the culture they convey. This dimension limits the symbolic space of each project. I would add that this project's identity, rather than converting or integrating, first plays the role of *indicator* and gives ground to sensitivities that already exist.

Isaac Joseph's analysis [*La Ville sans qualité*, 1998], seems to fit my particular case very well:

In situations where *normal people* and *stigmatised people* are in physical proximity of each other, no-one knows what the other group "really" thinks of one, every one has the feeling of "performing" ["d'être en représentation"]. He who is subject to gaze of the "normal" can either "make himself really small" or on the contrary "put on a brave face", being sometimes small, sometimes large, an instable size."

Only, on my own fieldwork, one doesn't really know yet who the normal and who the stigmatised people are.



## Chapter 8

### From Oral History to Narrative Research

Linda Sandino

#### Oral History: the problem

Despite the increasing tendencies to proclaim the inter/disciplinarity of fields of research, the lack of boundaries can create conceptual difficulties because of the impact on what kind of questions one asks of one's material, and to whom the answers might be of interest. On the one hand, institutions create communities of shared interests but Phd research can pivot between inclusion and exclusion to accepted research terrains, or the 'home' we left behind in our search for new ways of thinking about problems. What follows below is a consideration of reconciling Oral History with Narrative Research, in which the former is characterized as a method for gathering information about the past for History, while the latter is more concerned with the construction of narrative forms within, but not exclusively, social science research (Andrews et al. 2008; Hyvärinen 2006 p. 79). In Britain Oral History is particularly associated with 'history from below' as espoused by Raphael Samuel, and Paul Thompson whose *The Voice of the Past* (1988) has been a standard text for historians interested in using oral sources. In common with Narrative Research, British oral history work is used to 'treat narratives as modes of resistance to existing structures of power' (Andrews et al, 2008 p. 4) but the truth claims of narratives remains a problematic issue. Whereas historians may use other sources for verification, the emphasis in Narrative Research is more about personal, cultural, or social meanings and the diverse 'truths' that can emerge in individual story telling. Although life histories contain 'facts', they sit more comfortably as 'narratives of experience' (Squire 2008).

In my attempt to reflect on the experience and meaning of recording artists' life histories for a public oral history archive held at The British Library, the question of whether I was being told the 'truth' was less an issue for me personally than it was for my sense of responsibility towards the archive. However, what does 'truth' mean in life history work, especially with artists for whom imaginative and creative constructions permeate their thinking and working lives? Conscious of taking part in an 'oral history project', the participants were respectful of certain 'facts' of their careers but, like the chronicle, the C.V. does not constitute a personal narrative of experience, or 'thematic biography'. The archive's concern with factual accuracy, was I felt, missing the point: life histories as occasions for making sense of the past from the vantage point of the present, narratives of experience with which listeners can engage and expand their sense of the world and its meanings. However, as Joan W. Scott has pointed out, therein lies another danger: the privileging of experience as 'incontestable evidence and as an

originary point of explanation - as a foundation on which the analysis is based' (Scott 1991 p. 777).

## Reconstruction and Construction

The privileged status of oral history interviews comes from their position as testimony, as zero-degree of separation from events in the past. The fallibility of memory has been used to challenge, or problematize reliance on such testimony as historical evidence (Thompson 1988; Tuchman 1996) reinforcing History's opposition to fiction. Although historians have explored histories-as-text in order to demonstrate its textual constructed-ness (Thompson 1988; White 1973; White 1978; White 1987), the positive intersection between history and fiction and how this occurs has remained largely unexplored except for the work of Alessandro Portelli. In *The Death of Luigi Trastulli* (Portelli 1991) explores the significance of a misremembered date in order to show how memory functions as meaning-making in the present about the past.

Portelli's interviewees provide a coherent emplotment of the events that led to the death of Trastulli but one that signified what the shooting meant to them and the community of workers in the past seen through the prism of the present. Concluding his investigation, Portelli notes:

...if oral sources had given us "accurate," "reliable," factual *reconstructions* of the death of Luigi Trastulli, we would know much less about it. Beyond the event as such, the real and significant historical fact which these narratives highlight is the memory itself. (p. 26) (emphasis added)

Portelli's provides a compelling example of how testimony as narrative is subject to Ricoeur's conception of configuration from the 'prefigured' elements demonstrating the narrators competence as story tellers of politically important events in Italian history. The term 'reconstruction' indicates the positivist character ('factual') of conventional historical accounts; historians are involved in reconstructing past events into meaningful accounts. Testimony, however, even as a narrative, functions as a trace of the past because it is never complete, is always partial, awaiting the 'historian' to use the trace in order to create (another) plot. (Portelli also comments on 'the division of labor between *informant* and *scholar*', hinging on 'the ambiguous utopia of objectivity, which connects the supposed factual objectivity of the source with supposed intellectual objectivity of the scholar' between which lies the 'uncertain borderland of ...of subjectivity'. (Portelli 1997 p.79)

Ricoeur locates the reality principle of historical texts as a form of debt, which historians pay to the dead. Reconstruction, therefore, has at its root an ethical responsibility to assemble the traces of 'what was one day 'real' because historians 'know themselves to be bound by a debt to people from earlier times, to the dead' (1988, p. 100). It is this ethical imperative that binds historians to the documents, or traces:

As soon as the idea of a debt to the dead, to people of flesh and blood to whom something really happened in the past, stops giving documentary research its highest end, history loses its meaning. In its epistemological naiveté, positivism at least preserved the significance of the document, namely that it functions as a trace left by the past...(1988, p.119).

Ricoeur notes the institutional character of documents as a 'proof' or 'warrant' that 'nourish [history's] claim to be based on facts', commenting also on the expansion of documents to include 'anything that can inform a scholar' (1988, p.117). Oral testimony functions as such a document but my focus here is to untangle the testimony itself as a narrative construction, or in Ricoeur's terminology, a *configuration* (emplotment) that makes sense of the various elements and is an operation common to both History and fiction; both open 'the kingdom of *as if*' (1984, p. 64). Although at this stage Ricoeur sets aside the distinction between the "imaginary" of fiction, and the "real" of historical narratives, it is to situate emplotment as the paradigmatic function of *configuration* 'without regard for the differences that concern truth claims of the two classes of narrative' i.e. historical and fictional. Even false testimony still arises from a real situation in which something happened, and 'To the extent that historians perform the historiographical operation well they give a substitute representation of the past. A well made substitute is faithful to the available evidence and so deserves to be called true even though it is always amendable or reformable' (Dauenhauer 2005).

In order to understand, and untangle the various threads that unite oral history, narrative, and fiction, Paul Ricoeur's *Time and Narrative* (1984, 1985, 1988) has been particularly important for me because through its engagement with the representation of temporality in narratives, it considers history and fiction as the two principal narrative modes in Western culture, distinguished by their different registers of what is 'real' - rather than focusing on the instrumentality of 'truth'. By complicating the concept of mimesis, Ricoeur is able to do justice to the complex referentiality of texts, their truth-claims, and their reception.<sup>33</sup> Mimesis, for Ricoeur, draws on the Aristotelian tradition in which mimesis is 'a fundamental expression of human experience in the world' (Kelly 1998 vol. 3, p. 233) rather than the suspicion of mimesis within the Platonic tradition which could be seen to drive positivist interpretations of narratives (Josselson 2004). Ricoeur's tripartite conception of mimesis (mimesis 1, 2, and 3) provides a framework, or 'hermeneutic circle', within which one can grasp the mimetic correspondences of history and fiction: *prefiguration* as the competence needed to understand the world and actions; *configuration* as the work of emplotment; while the work of *refiguration* marks the 'intersection of the world of the text and the world of the hearer or reader' (Ricoeur 1984 p.71). Under mimesis3, narratives create 'worlds' and it is as such that oral history narratives of experience might be better understood.

## Recapitulation and Reconstruction

Ricoeur has posed the fundamental question for historians and narrative researchers working with oral history documents: 'How are we to interpret history's claim when it constructs a

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<sup>33</sup> This paper will concentrate on *Time and Narrative v.3* although the earlier two volumes also address time, narrative and mimesis. The successive, developmental aspect of the 3 volumes (in fact all of Ricoeur's work) provides a parallel

narrative, to reconstruct something from the past? What authorizes us to think of this construction as a reconstruction?' Rather than focusing on the principle of truth, Ricoeur proposes an exploration of 'reality' and 'unreality in narration' (1988, p. 5). This reframing of the problem collapses the opposition of truth versus untruth, and allows one rather to understand oral histories as both *reconstructions* of historical events and narrative *constructions*.

The historian in Ricoeur's formulation is a specific kind of scholar, a professional, academic researcher rather than the oral history witness whose testament becomes a trace ('data') for use by the academic historian. Clearly, though, witnesses too feel a debt that their accounts seek to repay. But in what ways is an oral history interview a trace of the past? Traces are by their nature incomplete whereas oral histories are attempts to provide a more complete account in the form of a story. Through the use of descriptive passages, narrators *construct* stories in order to *reconstruct* events. They too become historians, not just in terms of their recollection of past events in order to feed the historian, but as creators of meaning about those events (Portelli 1991). Oral history offers the opportunity, and narrative provides the means whereby individual interviewees become historians.

The term 'oral history' seems to cover both the history that is told, as well as the History that is then written from the oral sources; a double interpretative operation in which the narrator recapitulates 'the told in the telling' while the historian reconstructs the told in his/her telling. Narrative is therefore the fundamental core of oral history and in order to understand the meaning of its accounts, attention needs to be paid to its configuration, which, as is the case with Portelli's respondents, will draw on a variety of already-told versions; for Ricoeur the interplay between sedimentation (tradition) and innovation (1984 p. 68-9). In oral history, therefore, a construction is always, a *reconstruction*, forever subject to retellings, or more accurately, *reconfiguration* since reconstructions provide the occasion of thinking again, or thinking differently about past events. As Ricoeur so aptly states, the individual in interpreting his/her life 'appears as both a reader and the writer of [his/her] own life', and 'the story of a life continues to be refigured by all the truthful and fictive stories a subject tells about himself or herself. This refiguration makes this life itself a cloth woven of stories told' (1988, p.246). So, rather than assuming the interview/recording stands with the final authority of a historical document, the accounts should rather be understood as situated within a particular context, achieved in and for a specific moment or function which changes each time the story is told.

This point is reiterated by the psychologist Jerome Bruner (Bruner 1987) in his suggestion that 'stories are made and not found' but he extends it to account for ways in which:

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'emplotment' of the themes that preoccupy his thinking, aspects also explored in *Memory, History and Forgetting* (2004) but not a text I use in this paper.

the ways of telling and the ways of conceptualizing that go with them become so habitual that they finally become recipes for structuring experience itself, for laying down routes into memory, for not only guiding the life narrative up to the present but directing it into the future. I have argued that a life as led is inseparable from a life as told—or more bluntly, a life is not "how it was" but how it is interpreted and reinterpreted, told and retold.' (1987, p. 708)

This instability is an anxiety in oral history but a productive problematic in narrative research as Bruner suggests. Telling the same story twice, not only alerts researchers to their significance but provides resources for understanding how stories are reconfigured but allows one to explore the possible conditions and the forms of their reconstruction, as in the thrice told story by the master weaver Peter Collingwood of the events surrounding a joint exhibition with the ceramic artist Hans Coper (1920-1981) held in 1968.

Three versions of the story exist: an illustrated, written lecture (April 2003), an audio recording (May 2003), and a video film (October 2007), the latter two both conducted by me. Further, it is likely that Peter would have recounted this story elsewhere because Coper, an influential figure was famously reticent and shy, but someone whom Peter knew and greatly respected.

On opening night we saw the catalogue for first time. It was well-designed but there were NO PRICES or any HINT that everything was for sale!! A year's worth of work, and then no prospect of a return. "Right", said Hans, immediately and furiously, "I'll take all my stuff and sell it on the street outside." Eventually we calmed down and a notice appeared saying INQUIRIES AT DESK. But I found subsequently that I only got orders by hanging around the exhibition almost every single day!![Lecture, April 2003]. (Collingwood 2003)

<p>Audio May 2003</p> <p>And we went there on the day of the opening and we were shown this very handsome catalogue they'd produced. And immediately we saw there wasn't a mention of a price or that anything was for sale. So Hans blew up and said "Right, I'll take everything out and sell it on the pavement outside". Because the V&amp;A's view was that, "It's not our position to put a value on your work. If we say this hanging is worth 50 quid, it would be like us saying, us putting a valuation on it and we're not in the business of doing that".</p>	<p>Video October 2007</p> <p><i>Were you able to sell work from that exhibition?</i></p> <p>Well, the V&amp;A didn't think we should [<i>laughs</i>] because when we got there on the opening night we saw this rather nice catalogue, we said "Well where is the slip that gives the prices?" They said, "Well you know the V&amp;A can't give prices on things like this". So Hans Coper was furious. He said, "I'll take everything out of here onto the pavement outside and sell it to the public out here". But he didn't.</p>
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Certain features are present in all three versions above: the catalogue, the lack of sale prices, Coper's fury and threat. The lecture provides a succinct recapitulation of the complete event,

which resulted in the notice ('INQUIRIES AT DESK') and the need for Peter to take care of sales himself ('!!'). Emphasis is provided by key phrases in upper case capitals and exclamation marks (which the audio and video transcripts here do not provide<sup>34</sup>). The subsequent tellings are lengthier mainly because they offered more time (the audio recording amounts to 13 hours, the video to 4 1/2 hours) but each of the narratives responds to the specificity of its context. The lecture must be to the point but as a public performance contain some drama as well as providing a particular memory of Coper, possibly unique to Collingwood. The audio recording (the 'oral history') provides a 'thick description' of which this is only a short section, but it includes the Museum's reasons for not providing a price-list. In the video, undertaken for an archive of craftspeople, I found I had to insert a question so that Collingwood could tell this part of the story since we both knew that it was important: as a Coper story, as an exhibition story, as Museum story, as well as a story of collaboration and solidarity between two master artist-craftsmen (the shift from 'Hans' to 'we'). For the video, I am an interviewer rather than an oral historian. However, the meaning of the narratives, a leitmotiv throughout all three accounts, is the financial pressure: 'A year's worth of work, and then no prospect of a return'; 'NO PRICES or any HINT that everything was for sale!!' The audio version has: 'immediately we saw there wasn't a mention of a price or that anything was for sale'; in the video 'we said, "Well where is the slip that gives the prices?"' The audio version is the least dramatic in that it forms part of much lengthier description of the gallery space, the design, the architect, the V&A personnel. The lecture and the filmed version, however, provide a more 'entertaining' version with more reported speech, destined for a larger audience as opposed to one oral history researcher.

## Conclusion

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<sup>34</sup> A paradox in oral history research is the reliance on transcripts. It is possible that eventually multimedia presentations may change this restriction.



What do the above differences mean to an oral historian? Is the lengthier audio version 'better' than the others? In a straightforward sense, it is because it explains more but what it explains is Collingwood, even though it also provides a description of the exhibition. However, the meaning of the story is not the exhibition, but a story about both Coper and Collingwood's resourcefulness in dealing with financial difficulties. From the above excerpts, it would seem that oral history has no claim to privilege here since the story is told elsewhere. However, the fact that the audio recording was a life history, is important in understanding that it answers the question 'Who?' and 'the identity of this "who"... must be a narrative identity' (Ricoeur 1988, p. 246). It is not sufficient, therefore, to treat oral history as a transparent account of the past but to examine its function as a particular form of narrative construction and reconstruction in which a subject (the self) begins to emerge. Rather than using individuals as objects of research, Narrative Research provides the means of seeing them as subjects. OH 'offers less a grid of standard experiences than a horizon of shared possibilities, real or imagined. The fact that these possibilities are hardly ever organized in tight, coherent patterns indicates that each person entertains, in each moment, multiple possible destinies, perceives different possibilities, and makes different choices from the others in the same situation. These myriad individual differences, however, only serve to remind us that, beyond the necessary abstraction of the social science grid, the actual world is more like a mosaic or patchwork of countless different shapes, touching, and overlapping, and sharing, but also cherishing their irreducible individuality. As sciences of the individual, oral history and literature deal with portions of the mosaic that cannot be subsumed under the grid. They give us unwieldy representations, often harder to handle and work with, but perhaps more consistent not only with the presence of subjectivity but also with the objective reality of things. In conclusion, as Squire suggests:

By focusing on narrative, we are able to investigate not just how stories are structured and the ways in which they work, but also who produces them and by what means; [and] the mechanisms by which they are consumed...  
(Squire, 2008)

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## Part III

## Chapter 9

## Concluding Discussion: Sarah Baker, Rumana Hashem, Julie Moreira-Miguel, Linda Sandino

### Introduction Derek Robbins

Yearbook III has attempted to continue the discussion introduced in Yearbooks I and II. The nature of that continuity is articulated in the Preface to Yearbook III. There has been a movement from an attempt to frame PhD discussions by reference to the place of research in the social trajectories of individual researchers (Yearbook I), through an attempt to frame them by reference to the socio-historical construction of discipline discourses impinging on individual projects (Yearbook II), to an attempt to consider Passeron's position in relation to these two essentially Bourdieusian orientations and, in consequence, to test Passeron's emphasis on the constitutive function of language in research enquiry and the communication of research findings (Yearbook III). Whilst keeping an eye on this kind of continuity between Yearbooks, Yearbook III has also been trying to use elements of two PhD research workshops of 2008 – May 15<sup>th</sup> and June 12<sup>th</sup>. On May 15<sup>th</sup>, the workshop focused specifically and practically on Becker's *Writing for Social Scientists* and tried to replicate some of the exercises which he carried out with his students. No-one wanted to be 'guinea-pigs' in relation to writing and editing for use in Yearbook III (by analogy with Becker's use of long passages from two of his students) and so, instead, Yearbook III coheres by using Passeron's Preface to the French translation of Becker, considered at the workshop on June 12<sup>th</sup>, as a bridge between the focus on language and the additional focus on a comparison between the institutionalisation of research for PhD students at UEL and Marseille.

Yearbooks I and II each concluded with a chapter in which contributors were invited to comment on the relationship between their pieces and the overall theme of the Yearbook. For the concluding discussion which follows, the contributors were asked to offer some reflections *either* on just their use of language in their work *or* on the extent to which their research practice is framed by the national context within which it is undertaken, *or*, possibly, on these two as essentially the same in that, perhaps, national differences of conceptual framework are embodied in linguistic differences (e.g., perhaps, different meanings of 'class'). The following editorial 'prompts' were given and are reproduced here since they help to explain the emphasis of the contributions which, to some extent, are responses to the prompts:

- Sarah gives the paper (Chapter 5) which she gave at a conference and surrounds that with a critical reflection on the context and the criticism given. In itself, this is a perfect exemplification of the gap between what dialogue could be and what it has been institutionalised to become, however you might examine how your use of words

- Rumana's paper (Chapter 6) is explicitly an initial review of concepts. To that extent it is already doing as a piece what is suggested for the contribution to the conclusion. However, you might focus on the trans-national application of concepts. You are defining 'nation, state, and gender' with respect to an enquiry about Bangladesh, but these words are defined by reference to their historical meaning in Western discourse. There is, perhaps, a supposition that these categories are universally meaningful in the same way as instruments of analysis, but, what, for instance, might be thought to be the indigenous meaning of these terms in Bangladesh? Given the relative newness of Bangladesh as a nation-state, have Western ideas of nationhood become embodied following Western colonisation or are there languages of cultural identity (perhaps related to caste systems) which provide alternative truths about the situation you analyse? The same points apply to the use of 'intersectionality'.
- Julie (chapter 7) could discuss the use of 'public space and, also, discuss the transferability of the use of Goffman's notion of 'mixed contacts'. For the UK reader, it would also be great if you could say something more about the work of Fabrice Raffin and Isaac Joseph – how would you locate them in discipline terms and how far have their definitions of the problem affected yours?
- Linda's piece (Chapter 8) is already a discussion of truth claims of narratives. It is difficult to know how you might respond. You write about the process of interpreting information provided in different media. Perhaps you can pull out from this some comment on the relationship between the use of language in the different media and how this relates to your use of language in writing about it? You might also say more about the way in which you use concepts derived from Ricoeur, although you have done that already really, perhaps.

## Sarah Baker

After considering the role of language in influencing my enquiry and in the dissemination of my findings, there are a number of words or concepts that I think warrant further discussion in terms of their meaningful transfer across disciplines and national boundaries. Although part of the following meta-commentary is only based on my rather-limited experiences<sup>35</sup> of discussing my work with academics from other national contexts, I hope that my suppositions raise some interesting points for discussion. In addition whilst Derek Robbins did not raise the issue, I think that transfer of words and concepts across time, as well as space, is worthy of analysis and is a factor I consider below.

First of all I would like to go back to the issue of identity, particular social class, that I raise in my paper. In my exposition of the disciplinary context in which I presented my work I consider and critique the way that particular understandings of class have come to be associated with, and are generalised as inherent to, specific disciplines: cultural studies with a conception of identity which is fluid and changes quickly, and sociology with one that is more entrenched. Although I am highly critical of this caricature, it does resonate with some of the comments I have received about the use of the concept of 'class' in my work. Using the term seems particularly tricky because it is, and has been, so integral to Marxism. Over time the labour theory of value has been challenged, and the direct relation between class background and behaviour has been questioned. Thus although I attempt to make clear that I view class as simultaneously inscribed by different symbolic systems such as age, race and gender (Skeggs, 2004:3) and consider tastes and lifestyle choices to be one of the ways that class inequalities are played out rather than the direct cause of action, I have been accused of class determinism from some individuals within cultural studies. In order to address this throughout my analysis and in my writing style I try to continually emphasise the contingent and constructed nature of class classifications. The problems that I have experienced have made it clear that terms, like class, may be understood very differently from one academic context to another.

Trans-national discussion of class may also be difficult. As Mike Savage (2000:4) notes, whilst it is possible to find discussions of social class in other national contexts, class seems to be a particularly British concept that is wrapped up with the historical identity of the British social sciences. Thus I wonder the extent to which my findings regarding the production and consumption of retro style would be reproduced in other national contexts. This would, of course, need to be the product of further research.

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<sup>35</sup> One of the advantages of the workshop on the 12<sup>th</sup> June was the opportunity to discuss work with those from different institutional contexts.



The term subculture, which I refer to in my paper, is also a product of a particular moment in British social and academic history. Whilst I would argue for the continued relevance of class to describe particular social relations, I believe that the concept of sub-culture is problematic in the contemporary British context, hence my critique of sub-cultural capital. I would suggest, therefore, that rather than all research contexts and situations requiring new conceptual instruments and terms, this is only necessary when the old ones seem inappropriate. However, from my own experience it does seem that the use of 'old' concepts, such as class, can create problems in terms of their meaningful transfer across space and time. Interestingly, these problems seem less evident in relation to 'retro'. As one of my respondents commented whilst the meaning and value of retro is hard to pinpoint and complex for a number of other reasons, most people in the West seem to have an idea of what it indicates. This, I would suggest, says something about the symbolic power of the media in communicating concepts across national boundaries, in comparison to the ways in which academic discourses are generated and disseminated.

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## Rumana Hashem

Indeed, it is fairly interesting for me to talk about use of language in my work. Before going into detailed discussion on the use of language in my paper, let me admit a couple of points I feel important.

Firstly, it may sound interesting as Derek already presumed that 'no one wanted to be a guinea-pig', I, however, re-wrote the whole chapter for the second time before extracting the paper for this Yearbook. To my understanding, I have already turned out a guinea-pig, no matter whether I wanted to be or not. Apparently, all PhD candidates in Europe have to turn into guinea-pigs in terms of language and institutional rules, at some points, more or less, as this is the only way to get the work accepted in European academia. Secondly, I have a slight problem with Becker (1986) and Passeron's (2004) idea of 'decent'<sup>36</sup> language. Indeed, I am very unsure about the need for particular rules for language in academic writings. For me, the term 'decent' may vary from person to person and I see it is fairly problematic to become preoccupied with the idea of 'descent' terms before starting to write an article. What I understand is essential for researchers and academics to have a clear perspective rather than leaving the paper with an indefinite stance. The notion of 'decency' in academic writing only makes the paper hard to access for the common readers and thus, excludes a wide range of readers outside the academia and restricts the paper only for a certain group of people within academia. To my knowledge, much of the European literatures are only accessible to some academics who can sit and talk for hours in seminar rooms or in the conference panel with no resolution on how to plan ahead for implications of the research output while recent human era has been marked by the need for a strong integration of in-depth research and operational work. While I entirely agreeing with Passeron (2002:159 ), where he says that academic writings are often not well-articulated as it often becomes uncertain and ambiguous with a hesitant use of 'admittedly...but'<sup>37</sup>, I do not believe in imposing rules on the use of language for academic writings. Thirdly, and more importantly, I am unsure about the fact that all researchers and academics are interested in 'truth telling'. Much of the literatures I have come across throughout my educational attendance for about twenty years have talked very little about the 'truth', but avoided any potential 'truth' carefully. Thus, the need for 're-writing and re-writing' for 'truth telling' is not well-understood to me. From my experience, people with a clear politics can tell the 'truth' at the first instance where as many researchers end up with identifying 'no truth' after a long way of writing and re-writing.

However, to respond to the queries whether the terminologies of the paper and my research practice is framed within the national context in which it is undertaken or are they framed by

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<sup>36</sup> See Passeron, J-C . 2004. The text is available in the Part 1 in this Yearbook III.

<sup>37</sup> Cited in Passeron, J-C. 2004. Foreword: Writing, re-writing and 'truth-telling' in sociology, Part 1, Chapter 3, Yearbook III.

any particular institutional practice, I would like to elaborate on the constant 'shifting' (to use the term often used by my supervisor Nira Yuval-Davis) in my educational as well as socio-political life, so will be easy to understand the 'truth' about my writing. Let me first clarify my actual position as an 'activist-researcher' being actively engaged in political and human rights activities that involve serious operational work alongside scientific research. Apparently, the project I am undertaking for my PhD is a political project involving issues of national and international power politics. I do it deliberately as part of my political commitment. Also applying epistemological knowledge and scientific research tools in political project makes the work more significant and accessible to a wide range of readers. When I write I use to draw on both sources of literatures- academic and other<sup>38</sup> literatures and choose terms that wouldn't confuse any of my reader- academic and non-academic. Thus, words like class, race, gender, nation, state and intersectionalities are derived from the literatures and sources that are deconstructive in approach, no matter to what extent they are based in local or international literature. I keep using the terms as long as they explain my political philosophy. Let me also note that my ideas are certainly anti-class and anti-power hierarchic, not necessarily always Marxist though. To response to Derek's question on the use of 'intersectionality', I could have used other terms, for example, 'interrelation' or 'intermesh' which I actually did several times keeping the originality and assertion on intersectionality as it has been emphasised in the literatures by which my ideas are informed. However, I have carefully avoided the term 'correlation', unlike some writers who mixes up the term 'correlation' with 'intersection', to avoid any risk of misinterpretation of the term by liberal readers. My tendency to use and reuse the term 'intersectionality' and 'interrelation' clearly refers to the deconstructive approaches and literatures I have reviewed while writing the theoretical framework of the research. Using alternative words and replacing the original terms of the literatures have informed and shaped my ideas, could have weaken my theoretical stance.

The origin of the terminologies of my paper are also influenced by the 'routining and shifting' (to use Nira again) of the disciplines in which the PhD research has been developed. Indeed, the current research project is built upon the original dissertation I started at the University of Munich in October 2001. Having held a PhD fellowship in the Post-Colonial Studies Programme, I have worked on the dissertation entitled *Conflict induced displacement of women: the case of the Chittagong Hill Tracts*. In the University of Munich I have worked within the discipline of Social and Cultural Anthropology and being based in the school of post-colonial studies programme I had to review post-colonial writings. However, I started the current PhD project in 2003 in the School of Geography, Politics and Sociology located in the University of Newcastle-upon-Tyne. One important reason to move in Newcastle was to work with Prof. Liz Stanley, one who calls herself a 'purely a feminist sociologist'<sup>39</sup>. Hence, originally the project

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<sup>38</sup>Columns in newspaper, reports published in magazines, newspaper and political bulletin as well as creative writings and novel.

<sup>39</sup> See Stanley, Liz (2003:1). A Methodological Toolkit for Feminist Research: Analytical reflexivity, accountable knowledge, moral epistemology and being 'a child of our time'. The chapter was

was expected to be firmly based in 'feminist sociology' which, latter, did not happen by incident. Followed by some personal circumstances, I have had to move in the School of Social Sciences, Media and Cultural Studies in the University of East London that has brought the chance to work in an interdisciplinary framework. The work is now based on gender studies while the framework of the thesis is informed by a wide range of literatures in social sciences spanning from work in sociology, gender studies, development studies, migration and refugee studies, cultural and social anthropology to post-colonial studies. However, the language of my paper in this book has been derived from a number of disciplines of social sciences<sup>40</sup> and the terminologies often tend to cross the disciplinary boundaries given the interdisciplinary nature of the work.

For another important account of my language, I must admit that I was originally trained as a journalist and have worked as a feature reporter in various national and international newspapers for four years which affected my written language to a great extent, I suppose. My supervisors use to say that my writings often turn out a report of newspaper than an academic article. During my undergraduate studies I knew that my career would have been a journalist of a leading international news agency<sup>41</sup>, eventually that 'newswoman' became a scientific researcher to know the 'truth of the truth' (to use the term of an ex-faculty of the school, Judith Burnett, 2008) from a more scientific perspective and to protest the power politics in which media plays an important role. Turning out a European researcher from female journalist in South Asia must have strongly influenced my research framework as well. However, the often simple sentences and straightforward style of my writing are the positive influences of the 'journalistic' as well as 'political activism' experience. This also suggests that the linguistic difference in my work is originated by my activism and multi-disciplinary institutional practices rather than nationality.

To respond to Derek's comment whether I am arguing for a trans-national meaning of the concepts, I would say this is correct and the 'supposition' (to use Derek) that the concepts have been derived from European discourse is also correct. However, given the history of 200

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published in Heather Piper & Ian Stronach (eds 2003) Educational Research: Diversity and Difference Aldershot, Hampshire: Ashgate. I read an electronic version of the chapter received from the author.

<sup>40</sup>I have got my undergraduate degree in Public Administration and did my first MS thesis in Women and Development Studies. Then moved on to Gender and Migration Studies for my post-graduate diploma and switched to Social and Cultural Anthropology for Mphil. Now doing my PhD in Sociology and Gender Studies in the school of Social Sciences, Media and Cultural Studies.

<sup>41</sup> This view has certainly changed and I discovered myself being the luckiest one and not caught within the media machine any longer.

years of British colonization in Bengal, it is difficult for me to find out the aboriginal (if any) meaning of the concepts of nation, state and class. Although 'caste' system has been an issue equally important to class in Indian society, class and caste should not be interchangeably used as the concept of caste is closely related to both-'race' and class. Last but not least, it may be useful here to admit that my terminologies are mainly derived from the European discourses though the concepts are not dependent on any regional discourse. This is correct that I am a 'product' of European academia given the fact that my research life has begun in Europe in particular. But my ideas are shaped largely by the Indian and Bengali deconstructive approaches.

However, to conclude my reflection on the linguistic aspect of this paper let me also admit the fact that I am writing the thesis in a foreign language which makes the task often complicated to phrase arguments. I came to Europe with a first degree from a Bangladeshi University, an academic institution in a relatively new nation and predominantly influenced by Western discourses, and the medium of instruction in the University is Bengali. Hence, when I write in English I have to think and search for the words several times until I find the direct meaning of the word in Bengali dictionary. As one of my ex-colleague says her tongue 'slows down' when she talks in English<sup>42</sup>, in fact, my pen slows down being 'hesitant' and unsure about the most suitable word for the exact expression of what I want to say, and I often end up being confused and dissatisfied with my writing as if nothing has been said clearly and there is still a lot to say to explain what I mean.

## **Julie Moreira-Miguel**

Language is a particularly tricky point when you try to study a field of public action, which art and culture can be in France. Indeed, you have to determine exactly which terms belong to the

vocabulary of the social actors (users or workers of the cultural field), from those which belong to the institutional vocabulary, and from the conceptual tools of sociology. And, in the few cases we try to determine the veritable origin of the words we use, the porosity between these three lexicons makes the task difficult.

Even only within the sociological field, we know how much a researcher's words can reveal, for instance, a *populist* or a *miserabilist* tendency of one's point of view. That is exactly what Jean-Claude Passeron and Claude Grignon pointed out in *Le savant et le populaire* in 1989. In short, they denounce in this book what they name *populism in literature and sociology*, which consists in considering popular cultures as if they were autonomous, that is to say as if they were not subjected to relations of domination existing in general in the society. Conversely, the *miserabilist* tendency considers popular cultures only in terms of lack (of education, of money, of access to highbrow culture).

Raffin is a young French sociologist who doesn't work in the academic field, but has produced several interesting papers about culture and cultural policy. Some of his work concerns the use of culture to reconquer urban territories that have been disused and deserted (as former factories, for instance). I was particularly inspired by the way he discusses the notion of *public space*, noticing that a space might momentarily lose its public character when its control is seized by a cultural event, which is exactly the case I am studying. This theory made me pay attention to the fact that a cultural event can be considered as a violent act because it literally monopolizes a place whose uses had already been step-by-step negotiated between inhabitants. One of my aims is to describe how people work out a new deal for the spatial and temporal sharing of this space.

Reading Isaac Joseph warned me about the danger of borrowing too quickly the categories of those who organize cultural events and in a way represent the institutional culture. The risk is all the greater as I myself tend to consider highbrow culture as the legitimate one because of my own background. I think that the interactionist sociology allowed me to escape from these dilemmas and develop a more neutral view on actors and interactions. Isaac Joseph is a French sociologist, whose works have been deeply influenced by the Chicago School of Sociology and its heirs. Inspired also by Habermas, he discusses the concept of *public space* and is particularly sceptical about authorities' discourses on it. He is also interested in the specific skills which city-dwellers develop and mobilize in the urban context. He translated several of Erving Goffman's books into French. He studied interactions between users of cities and behaviours in public spaces and it is precisely this aspect of his work that I am interested in.

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<sup>42</sup> While reflecting on the influence of institutional practices in her research, Ulrike Vieten (2007) wrote that her tongue 'slows down'... it simply happens'. Ulrike is now teaching in the Netherlands. See *Concluding Discussion* (page 3) in *Yearbook I*.

People in my fieldwork have heterogeneous social and cultural characteristics. Especially during the cultural event that I am studying, the audience is composed on the one hand of people living in the neighbourhood who generally belong to the working class and aren't used to seeing these forms of art, and on the other hand of people from outside, who are already connoisseurs and came especially for the festival. Among the audience, some elements enable us to identify those who don't have a lot of experience as a spectator of a live performance. For instance, some people (often the youngest ones) – ignoring the implicit rules of good manners during a performance – can disrupt the show, even if they seem to find it pleasant. Some can stay away from the audience group – others obviously misunderstand what is happening. Others might consider the event as a disturbance and complain and loudly criticize. Differences are made visible not only by the behaviours but also by the clothes: lifestyle (leisure, cultural practices) is partly visible in the way of dressing as well as the *body hexis* reveals social origins. People are aware of these differences. Those who visibly don't belong to the cultivated fringe are identified and stigmatised by the others, and vice versa (as I explained in my paper). Erving Goffman defines stigma not as a person but as a point of view. Even if his book, *Stigma*, is focused on the handicap, Goffman explains that ethnical minorities and members of inferior classes are stigmatized as much as handicapped people can be. Having conducted several interviews among the organizers of the event, I noticed that they seem to have a *miserabilist* vision of the inhabitants of the area and describe them as lacking opportunities of opening one's mind through performing arts. That is why I chose to borrow Goffman's notion of mixed contacts in each case when the so-called less cultivated inhabitants are confronted to professionals or to the experienced audience.

**Linda Sandino**

Positioned at the intersection of science and art, History has not created for itself any distinctive disciplinary jargon or language. Analysis of terms such as 'the past', or 'memory' is rather evidence of historiographical concerns about the meaning and practice of the discipline, which, finally, can only be based on what historians have written (or said). Engaged at one level in an act of translation of the past and of its traces, histories are always produced for a reason and for an audience. It shares with fiction, therefore, a desire to communicate, to address a community or individual but notoriously treating 'language as a transparent vehicle of representation that brings no cognitive baggage of its own into the discourse' (White 1978 p. 127). However, as White demonstrates, this conception is itself a legacy of History's empirical, or 'certaintist' delusions. Oral history itself supports this assumption through its preoccupation with getting testimony directly from participants 'in their own words' (thereby also privileging a superior authenticity to speech). Furthermore, the question and answer method is also used to encourage a 'natural' setting rather than the rigours of a research context. Nothing should dispel the illusion of an 'everyday' situation in which two people meet and talk. Moreover, the very 'naturalness' of oral histories is part of their democratic function and appeal; it is the history of everyone for everyone. Listening to my interviews, I can easily be seduced by the sheer delight of listening to the sound of stories. It is only by means of transcription that the material is transformed and stabilized into a research document.

The moment oral history testimony becomes research, it ceases to function purely as story telling as the narrative is dismantled and analysed either for its content (for historians), or for how it functions as a particular type of narrative (for historiographers/ narrative researchers). As a researcher, I create another narrative using an appropriate set of conceptual tools (case Ricoeur's concept of mimesis 1,2, and 3) to understand how accounts work in different contexts, whether their meaning remains the same, and what the purpose of stories is. Prefiguration, configuration, and refiguration are also stages at work in my own reconstruction. Figuration is metaphorical work; it always stands for something and is 'made' just as history/stories are made. Ricoeur's terms are functional as well as metaphorical: functional because they are able to maintain the gap between language and reality while nevertheless creating the possibility of talking about the space between words and things. Reconfiguring oral histories as narrative research makes explicit what happens when documents become research although it could be objected that narrative research destroys, or at least takes no account of the historical embedded in the account; form is sacrificed to content.

However, if personal testimony is accepted as a legitimate historical trace, it is surely important to explore how testimonies function as particular reconstructions and recapitulations about the



past. As White neatly puts it: The principal difference between history and philosophy of history is that the latter brings the conceptual apparatus by which the facts are ordered in the discourse to the surface of the text, while history proper (as it is called) buries it in the interior of the narrative, where it serves as a hidden or implicit shaping device' (White 1978, p. 127).

White, H. (1978). Fictions of Factual Representation, *Tropics of Discourse: Essays in Cultural Criticism*. Baltimore MA: Johns Hopkins University Press.



