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SOSTRIS AT THE LEVEL OF THE COMPARATIVE INTERPRETATION OF CASES[1]

Background

I have spent a long time teaching and researching qualitative methodology, but only recently have become aware of the biographic-interpretive method ('BIM') which seems to me powerfully productive both as a method of interview design and as a method of interpreting the interview material. It has been very exciting to be involved in the thematising of cases in London over the last year and learn the practice of the constant comparative method of grounded theorising which enables the local Sostris teams to derive their eventual 'account of the reconstructed case structure' of each particular case .

There were and will be difficulties in moving on from this point, in moving on beyond the level of the accumulation and juxtaposition of 'case reconstructions'. These notes are a contribution to clarifying some of these difficulties for those involved in this or similar projects.

I argue that some of these difficulties might be attributable to:
- epistemological traditions within the 'grounded theory movement' itself which have been the subject of discussion in recent times,
- certain 'semi-conscious ideologies' within sociology more generally,
- and (most speculatively) to 'working assumptions' within national schools of sociology.

These rough notes deal with each in turn.

Biographic-interpretive method is necessary but not sufficient for cross-case/cross-national comparison and theorising

Wolcott's distinction of 'description-analysis-interpretation'

Harry Wolcott's *Transforming Qualitative Data: description, analysis and interpretation* (1994 Sage) makes a good case for using different terms to mean different things. He is talking about 'descriptive data', but we can think in terms of 'interview material'.

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[1] This paper is an expanded version of the notes "Bim-Bam-Goth" written in June 1997 as part of discussions in London following the Sostris meeting in Gothenberg (May 1997) and preceding the Sostris meeting in Hydra (September 1997).

"I propose that there are ,,, three major ways to 'do something' with descriptive data.

- One way of doing something with data in rendering an account is to stay close to the data as originally recorded. The final account may draw long excerpts from one's field-notes, or repeat informants' words so that informants themselves seem to tell their stories. The strategy of this approach is to treat descriptive data as fact. The underlying assumption, or hope, is that the data "speak for themselves".

- A second way of organising and reporting data, one that typically builds on the first, is to expand and extend beyond a purely descriptive account with an analysis that proceeds in some careful, systematic way to identify key factors and relationships among them.

- A third way, calling for interpretation, may follow from the second, or spring directly from the first. It does not claim to be as convincingly or as compulsively 'scientific' as the second, being neither as loyal to nor as restricted by observational data only[2]. The goal is to make sense of what goes on, to reach out for understanding or explanation beyond the limits of what can be explained with the degree of certainty usually associated with analysis....[3]

By no means do I suggest that the three categories -- description, analysis and interpretation -- are mutually exclusive. Nor are there lines clearly drawn where description ends and analysis begins, or where analysis becomes interpretation[4].... I do suggest that identifying and distinguishing among the three may serve a useful purpose [5] (Wolcott, 1994: 10-11)" [6]

How can this argument by Wolcott be useful for understanding changing pattern of the tasks facing SOSTRIS ?

The SOSTRIS-interview: from 'the relevancies of informant' to 'the 'relevancies of the researcher'

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[2] For our purposes, this could mean the systematic treatment by BIM's constant-comparative method of analysing interview transcript material

[3] For our purposes, this means the level at which we start to use the 'case definitions/analyses' generated by BIM for the illumination of SOSTRIS-commissioned issues.

[4] The interpretive resources of the interpreting team are present to a certain extent in the 'analysis phase' but are less evident. If the English team had perfect translations of the Greek transcripts (e.g.), they would have to develop a lot of local-cultural knowledge and historical sensitivity to develop good hypotheses and case-reconstructions and so understand the significations of the material.
The interpretive resources are much more and ostentatiously evidently significant in the phase of post-case-description 'interpretation and theorisation'

[5] at different stages of the SOSTRIS work, and in different disagreements about precisely what should be done in what way at any given stage in a given arena

[6] Bearing the last paragraph in mind, Wolcott's formulation may be more systematically treated in terms of 'fuzzy thinking' as developed by Kosko (1993) in which all the elements which can be differentiated along a 'continuum' , or 'spectrum' are all present at all points in proportions ranging from 0% to 100%.

It may be helpful to use the analogy of the phases of the biographic-interpretive method's interview, as described in Breckner (1998). In the interview there are three phases:

1. The main narration
2. Narrative questioning
3. External questioning

The first two of these interview phases are governed by the 'relevancies' of the informant; the third is governed by the 'relevancies' of the researcher. The method prescribes maximally clear lines between each phase, and especially between the end of phase two and the start of phase three.

The practice of biographic-interpretive analysis ('case reconstruction') has a similar tripartite or dialectical structure. On the basis of distinguishing between the 'lived story' and the told story' and analysing them separately, these two abstracted 'elements' are then reconciled in the end-product of the biographic-interpretive method, namely 'the [reconstructed] structure of the case'. As far as possible, the impact of the 'system of relevancies' of the interviewer/researcher are minimised throughout the practice of case reconstruction.

Sequential & biographic data analysis and the psycho-sociological imagination of the research team

It should be noted that the psycho-sociological imaginations of the analysing team are called upon as early as the 'sequential analysis' of the biographical data and then of the thematic fields. The team's stock of knowledge -- though not their system of relevancies -- comes into play at this point

Both in respect of biographical data analysis and in respect of thematic field analysis, the team are called upon to generate hypotheses about the possibilities contained in a given unit of empirical data, hypotheses as to the shape of possible further items (future biographic events/data; following themes and text-sorts)and, in a third step, contrasting these with the actual outcome....

> "The aim.. to reconstruct the structure of the case...leads to questions such as whether the[interviewee], in opting for a particular course of action, systematically eliminates other possible interpretations or actions, that is, whether there are certain underlying rules to be discovered
> (Gabriela Rosenthal, 1993)"

It is the researching team's knowledge and imagination of psychological and sociological *universals* ("any young woman of that age might be expected.."); their *cultural-historical local knowledge* ("in Naples, at that time, of course, it is quite likely that..") , and their *slowly-emergent knowledge of the individual under consideration* ("well, given what she has said so far, I would expect her not to ....") that leads to the generation of relatively finite and relatively appropriate hypotheses of alternative possible choices, courses of action, and eventual interpretations.

Despite what I have said in the above paragraph about the importance of the stock of knowledge of the analysing team, it is clear that in the *early stages* of BIM where concern for the 'relevancies' of the informant are paramount, variation in the systems of relevancies of local-national teams should be very minor in determining the product of the interviewing and not too major in determining the outcome of the analysis. The strength of the method lies in the relative insensitivity of case-analysis to variations in the system of relevancies of the interviewing team and (to a lesser extent) of the team involved in developing the case reconstruction[7].

However, according to Wolcott's model, in all qualitative research, there is a later reaching out "for understanding or explanation *beyond* what can be *explained with the degree of certainty usually associated with* analysis [my italics]" and this is where the method most appropriate for some phases of the research cycle starts to lose its relative dominance and requires reconsideration and supplementation.

The research team at the second level of interpretation: the phase of cross-case comparison and theorisation

My view is that the biographic-interpretive method ('BIM') ceases its considerable 'systematic sufficiency' at the point at which systematic analysis of each case (case reconstruction) starts to give way to 'interpretation' in Wolcott's terms . This is the point at which the 'stock of interpretive knowledge' and the 'repertoire of interpretive issues' peculiar to each interpreting analyst (or national-local team thereof) comes more strongly, if not for the first time, into play[8].

In the SOSTRIS case this seems to be at the point of the systematic comparison of 'described cases'. BIM produces a very definite (first) level of 'interpretation' in the reconstructed 'case structures', but beyond that point there is then what we might call a 'second level of interpretation' which has to be understood in its own terms as a distinct phase of the overall research project.

This may appear in terms of a question "what issues are raised by the cases?" . I would argue that a better question would be " which of our repertoire of SOSTRIS-issues and national-local team concerns can be creatively illuminated by the case-descriptions to hand, supported by whatever knowledge-resources we have to hand or can acquire?".

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[7] Though this tends to be assumed rather than tested, and may depend on 'national-local contemporary knowledge' more than we like to think.

[8] In formal terms, for Wolcott, BIM is focused upon what he calls "the analysis of key factors and the relations between them". In his terms, the 'biographic-*interpretive*-method' (BIM) would be called the 'biographic-*analytic*- method' (BAM), leaving a level of what he calls 'interpretation' to come afterwards. This emergent level may be alternatively be identified as that of 'theorisation'. ..... We do not have to adopt his usage, but I value his distinctions.

In any qualitative research project, research foci — and consequently interpretive parameters — will vary between people, between teams, and over time.[9] Acts of judgement are being made all the time.

> A thematic sequentialisation sufficiently fine-grained for its original purpose may need to be refined further for a new one. Alternatively, an over-refined sequentialisation may need to be revised into a broader-brush treatment to ensure the wood can still be seen, despite the hyper-development threatened by the trees.

> Even at the level of hypothesizing the possible biographical data consequences of particular life-events, one researcher may be particularly interested in the psychology of family dynamics -- and develop hypotheses in relation to these -- while another is particularly interested in the evolving societal context and state interventions, and develop hypotheses around these areas.

Clearly, as long as *teams* are reasonably balanced between legitimate foci of interest, such natural specializations of knowledge and ignorance are unlikely to cause harm.

On the other hand, the *third* part of the BIM *interview* -- external questioning -- involves questions posed relating to the relevancies of the interviewer. If these are not team-constrained but idiosyncratic to the interviewer and to the situation, then unbalanced single-member interviewing might conceivably lead to certain types of information not being gathered. This was frequently discussed early on in the Sostris project.

However, in Sostris, the fact that each national research team is composed of at least a core of two or three people; that typically each team creates larger panels for discussing cases, and that frequent cross-national workshops are held in which the products, arguments and assumptions of national teams become apparent and debatable is I think a major factor in maintaining the relative objectivity of national teams and their BIM-procedures and conclusions, as well as on the higher level of 'increased objectivity' of the cross-national Sostris team as a whole.

To sum up, therefore: the fertility of BIM and 'methodological authority' of BIM depends upon virtually no (or no apparent) concern for the relevancies of the researcher in the first 2 parts of the BIM-interview and in the earlier parts of case-analysis leading up to the case-reconstruction.

However, I would argue that, in the later phase of each case-reconstruction and then especially when we get to cross-case cross-national comparison/ interpretation (what might be called the 'second level' of interpretation, or theorisation) , SOSTRIS and

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[9] Sostris case material might be used as an archive for quite different research questions. Obviously, the more at a tangent such a new research purpose was from the original research purpose for which the BDA, the TFA, and the description of the case structure were developed, the larger the amount of time and energy that may need to be devoted to the original interview / report materials to enable them to yield the desired data for the new research purpose.

any other team doing similar work must rely to a qualitatively increased extent *particularly on the knowledge-bases*   and *also on the issues-repertoire* of the 'national-local research team' as contextualised by the general concerns, (the "system of relevancies") of the SOSTRIS briefing.

At Wolcott's 'higher level', work must be BOTH *more oriented* towards the commissioning-agency's concerns AND *more influenced* by the cross-mational team resources of personal, local, and academic discipline/professional culture and expertise.

 This needs to be made explicit, since my sense at the time of writing the original version of this paper (June 1997: Bim-Bam-Goth) was that it went against some of the spontaneous ideologies of a fair number of team members. Appeals to 'stick to the method' were mistaken as a way towards a solution, because the method in question was not a method-for-that-level-of-analysis.

SOSTRIS cross-case and cross-national theorisations and policy-suggestions

Formally speaking, in terms of pure research, there is much to be learnt from a relatively unrestricted   pluralism of interpretive-issues and repertoires that each national-local team finds itself addressing or might imagine for itself.

However, applied research (eg that of Sostris) is an exercise in applied research to a specific contract. This contract does not just suggest the types of cases who are to be interviewed (the social excluded); it also requires the final reports to address certain issues and to interpret the large number of cases expensively interviewed and analysed for a final report which uses these materials for a certain complex purpose, itself partly negotiated and defined by the researching agency (in this case, Sostris).

In the period up to and including Gothenberg, these issues were addressed by a variety of means including the following:

- two or three formal discussions on the concept of 'social exclusion' vis-a-vis that of 'risk'
- work in ' free associative' small groups on the term *social exclusion* to explore both members' personal investments in the issues and also their different understandings both of local national commonsense and of professional knowledge and approach

 In addition, the initial comparison of cases was always undertaken not at the national level but in the cross-national Sostris workshops. By the time of Gothenberg, strong overall themes had already developed. After the period in which these notes were written, a clear shareable framework of analysis was identified and developed more formally at the next meeting, at Hydra.

This process was naturally uneven and conflicts did develop. This would be inevitable in any similar cross-national research endeavour, and it is the purpose of the

remainder of these notes to explore, for the sake of future research teams, why it can be difficult to move from the analyses of particular cases to the comparison and theorisation of cases already analysed, difficult to accept that different procedures obtain and that the two levels should be distinguished in the way that Wolcott distinguishes them.

Obstructions to moving to the comparison and theorisation of cases

Obstruction: ideology of inductivism
One reason is Glaser and Strauss's early inductivism. Their *Discovery of Grounded Theory* (1967) promoted a belief in the possibilities of pure induction and a hostility to received knowledge. 'Inductivism' tends to imply that 'all reasonable people' will find the same things/patterns/truths "in the data" regardless of their cultural and societal differences. Strauss and Corbin (1994) make a significant admission when they write:

> "Too rigid a conception of induction can lead to sterile or boring studies. Alas, grounded theory has often been used as a justification for such studies...the initial presentation of grounded theory in *Discovery* has led to a persistent and unfortunate understanding about what was being advocated..... Glaser and Strauss overplayed the inductive aspects. Correspondingly, they greatly underplayed both the potential role of extant (grounded) theories and the unquestioning fact (and advantage) that trained researchers are theoretically sensitised. *Researchers carry into their research the sensitising possibilities of their training, reading, and research experience, as well as explicit theories that might be useful...*(1994: 277 italics added)[10]"

Though they have accepted that 'pure induction' by the open non-knowledgeable mind has its limits, but the original 'ideological drive' remains quite strong (Glaser, 1992)[11].

Layder (1993: 51-70) is helpful here in suggesting the ways in which issues of power and other 'macro-structural and macro-institutional' features can be obscured or denied by inductivist research refusing to go beyond the phenomenological discourse and immediate behaviour of the face-to-face interviewee.

> "What seems to be missing from the interactionist's analytical scenario is a parallel concern with the wider, structural or macro aspects of social life, as they are implicated in the...phenomena which are their characteristic focus.... Situated activity possesses its own partly independent properties, but it is a mistake to imagine that the micro-world is self-contanied and self-sufficient.

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[10] They shift from first insisting that only extant *grounded theories* should have potential, and that researchers need only be *trained* -- a stress most compatible with the original ideology -- to a larger concession that *reading and research experience and explicit theories-that-might-be-useful* are to be supported and celebrated. Interestingly, non-research experience and local knowledge are not emphasised here.

Everyday behaviour takes place against the backdrop of wider social, economic and political circumstances which impress themselves upon this behaviour just as much as these circumstances may be seen as the eventual product or outcome of this type of behaviour....

The very fixity of this concentration [of grounded theory on micro phenomena] is a factor which prevents grounded theory from attending to historical matters of macro-structure as a means of enriching contemporary or, as I shall call them, present-centred forms on research on micro-phenomena. It should be possible to augment the processual and dynamic analyses of interactional phenomena by a parallel focus on the historically antecedent forms that provide their institutional backdrop (Layder, 1993: 67-68)"

Interaction between interviewer and narrator in the narrative interview is a very precise micro-phenomenon. It is strongly formed and influenced in the final 'external questioning' part of the 3-part interview by the system of relevancies but also by the stock of theoretical and historical knowledge of the interviewer and their "understanding of the wider social, economic and political circumstances" in which the situated activity and life-world of the interviewee has taken place. The ideology of inductivism can function either as a *block against understanding the relevance of the research-team's system of relevancies and of knowledge,* or as a block against *bringing these fully into play* in the micro-processes of interviewing, case-reconstruction, and moving on to comparison and theorisation.

Obstruction: Ideology of paradigm incompatibility
A second reason from the other end of the spectrum is the tendency within theory-conscious sociology and social-science -- particularly if influenced by a skimpy (mis)-reading of T. Kuhn's *Structure of Scientific Revolutions* or by Left politics or both -- to 'absolutise' and 'ideologise' differences of approach and phase into 'incompatible paradigms'. If we are all totally controlled by our paradigms, then 'individual interpretations 'only appear to be 'individual': they are merely repetitions of the foundational truths. Such epistemological warfare is now tending to give way to an acceptance of principled designed combinations of selected approaches that can be shown, not assumed, to be compatible for that design (Jennifer Mason, 1996).

Obstruction: Early social science's exclusively 'generalising' concern
A third reason has been social science's formerly dominant concern for pan-cultural and pan-structural 'universals' in the form of nomothetic generalities or phenomenological essences of a 'universal human condition / predicament'. This is now giving way to a more 'historical-comparative' concern where local space-time variations (across Europe and across SOSTRIS-categories) are both respected as a focus of inquiry but also themselves a material for careful local-regional theorising. Ethnography and the concern for 'unique configurations' of culture, structure and personality at the micro level has been more developed in European imperial-colonial anthropology but this sub-field was relatively segregated until relatively recently. . Another reason may have been the division between the academic disciplinary formation of sociology (generalising) and history (particularising) though this too was never complete and is being overcome from both sides.

Obstruction: Going beyond the safety of 'method as sufficient' is uncomfortable
A final point may be that ideologists of 'grounded theorising' and 'technicians of the BIM paradigm' may feel uncomfortable about going beyond the level of the 'case reconstruction' since that is the limit of the BIM method, and a very good job has been achieved by that point. Alternatively, and in perfect good faith, ideologists of 'grounded theorising' and 'technicians of the BIM paradigm' may wish to insist that the level of 'interpretation of the described cases' can be done in exactly the same way as, and using no further resources than, the BIM that has proved so productive so far. Their expectation would be that there must exist a way of comparing case structures, one dimension at a time, which would be just as systematic and systematic in the same way as the case-reconstruction procedures in the earlier phase. I think they are mistaken.

A reasonably strong claim can be made for saying that the BDA and TFA and case-structure depiction are relatively-insensitive to whichever team produces them and are reasonably useful for a variety of related research purposes.[12].

My argument is that the Sostris-governed relevancies of the research team have some pertinence in the external questioning of the interviewee; that their stock of knowledge becomes very relevant in the case-reconstruction by the analysis team; that both their stock of knowledge and their system of relevancies become significantly more important at the level of the comparison of cases (tacitly) and and perhaps even pivotal at the level of overall theorisation (more explicitly). I take this discussion further in a later paper (Wengraf,1998).

Typologising questions in social science

The generation of typologies is one that automatically emerges as central and controversial as one moves to the development of theorising on the basis of comparison. Since the use of typologies is discussed in Mike Rustin's accompanying paper (Rustin, 1998) I shall not discussed this crucial question here, beyond making the following three brief points:

1. Max Weber's pioneering discussion of the difficult relationship between general ideal types and particular idea types should be drawn upon. General concepts are logically entailed in any account of particulars; any general account has (definite, though incomplete) implicature for any account of particulars to which it leads.

2. What we are attempting to 'type' may not just be individual people as 'cases' of the type but other analytical concepts such as 'situational adaptions' or 'institutional maladaptions' (exclusions, discriminations); types of 'support adequacy for excludees', types of milieu; types of collective strategy, etc.

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[12]However one can never exclude a research purpose for which the raw interview material is highly pertinent but for which the BIM-processing may be fairly irrelevant. Analysing the conceptual complexity of the utterances (Tetlock and Suedfield, 1988) or the logical adequacy of their argumentation (Kuhn, 1991) might be two such purposes.

3. Howard Becker's question "what is this case a case *of*?", (Ragin and Becker,1992), is always a crucial question, given that it could be seen as a case of an indefinitely large number of theoretical categories. It became clear pretty fast that the categories in which Sostris sought informants and the *typologies of case* that they are turning out to be cases of are not the same.

National-cultural obstructions

Obstructions which may relate to differences of national culture are much more difficult to evoke, let alone agree upon. What follows are provocative hypotheses about 'causes of national-team difference' involving phantasies based on relative ignorance : rebuttals and alternative hypotheses would be very welcome!

USA

1) 'American' grounded theory/phenomenology was founded within the pragmatic concern for finding out the universal features within nursing environments in US hospitals. The professional culture was supposed to be homogenised or homogenising and the identification of 'normative work-tasks' and 'best practice' within a (nursing) bureaucracy did not lend itself to the exploration of racial or national or 'local-cultural' difference.

2) 'America' is so big that only self-proclaimed anthropologists have studied the differences of the internally-colonised peoples, and this has not really impacted on the 'ideology of grounded theoreticians'. Their belief in the inductive method as sufficient, in the putting aside of prior theories and prior knowledge to avoid premature contamination and closure, has not sharpened their sensitivity to the dangers of 'excessive unconscious generalisation', and to social relations which an all-American team of researchers and researched take-for-granted.

3) A certain brand of phenomenology (for example Max von Manen, 1990: 40,42) can also have an assertive lack of interest in what are seen to be contingent differences. An abstract humanist concern for "what is the essence of parenting?", "what is the essence of X?", leads to an ignoring of difference.   The universalist researcher looks perhaps not for universal "laws" (the original nomothetic impulse) but looking for universal 'essences" instead. The practice of discounting and ignoring cultural-historical differences is identical, whatever the legitimisation.

4) American grounded-theory sociologists are not employed in the internal or external US 'colonies', nor are they sent out to look at 'national differences' in order to explore the 'degree of vulnerability to capitalist modernisation' of various societies (though maybe in Eastern Europe/Russia?). Consequently, their interest in such differences -- except as indicators of 'deficit' or 'danger' -- is likely to be low.

5) The general concepts used by US social researchers are likely to be functional theories of the 'adaptiveness' of the individual actor, to be theories of 'the stages of economic growth' and 'Western modernisation', of 'adaption to risk society', of a variety of euphemisms for 'late capitalist world economy' but not understood as such. These are virtual 'pseudo-universals'.

What is often crucially absent, and crucial, is the concept of *historical-local mediation*. for such 'universals'. The 'mediation' of (e.g. 'German structures and cultural struggles in the 1990s') is not usually postulated., let alone made a focal point of inquiry, explanation and eventually even policy-change. The 'national-local specificity' of the Sostris-concerns is a strong corrective to such bad universalisms, but lazy sociological universalisms do not wish to be too bothered with awkward local-historical facts!

6) In general, in anglophone sociology, the desire for a 'Western market' for research products may lead to an ignoring of 'mediating differences' in 'cross-Atlantic sociology', just as in 'cross-Atlantic popular music' everybody sings in a 'Western present' dis-located in space and time so as not to reduce 'market appeal' by too much (local-cultural) idiosyncrasy or particularity.

Whereas the anthropologists focused on the 'anthropological present' of a 'distant time reconstructed by selecting out traces of modernity', the universalist focuses on the 'anthropological present' of an 'imaginary (risk) Western post-modern community' pre-constructed by selecting out traces of non-modernity.

GERMANY

1) Anti-Communism contributed to a banishing or abstraction of the Marxist concern of a Barrington-Moore (1966) variety of 'national comparative studies' of 'periodic national settlements' of class-struggle and the balance of class-forces and class-regimes. Instead, there is the 'normative communication-ology' of Habermas.

2) The 'universalism of the Enlightenment' carried by Napoleonic forces across Europe after the French Revolution. This led to the Romantic tradition of exploring 'national-cultural difference' developed by the German resistance at the start of the 19th century . Did the very justified fear of reviving Nazism nationalism lead after 1945 to a 'denial' of 'national-cultural' difference?

3) The West German desire to appear to be 'Western' might suggest a non-exploring of the structures in and around Germany except in a 'constitutional Westernism' direction in which East Germany and the Communist countries could be seen merely as deficient in 'free-market organisation' and 'entrepreneurial mentality'. Post-war West Germany had no serious colonies in which they might have developed the concern for minute cultural-local and historical differences as developed in Britain and other imperialist countries.

4) A German philosophical tradition looking for "the essence of man" also plays into a neat fusion with the American assumption that the assumptions of the late 20th USA are what the essence of man is all about.

5) If histories of difference are a long way away from the sociology of universalising Westernisation and capitalist modernity; this might help to account for the 'down-playing of (recent) history of national-cultural differences' by German sociologists.

6) On the other hand, concern with 'emotional life' of individuals led to the development of the biographic-interpretive method of case-study analysis from which we are all benefiting and which is the one common ground and approach so far of the SOSTRIS group and which has been so beneficial for all concerned.

BRITAIN

1) British 19th century tradition of empirical statistics of reforming Fabians did not encourage the development of systematic qualitative work in the social sciences. 'Close attention to the transcript-text' was to be dealt with by literary critics[13] gifted by 'natural' or 'class' endowment to make intuitive appreciations. 'Mechanical systematic analysis' was more like a despised 'German' or 'American' practice. Hence the backwardness and difficulties of BIM in Britain.

2) Compared to anti-Communist States like the USA and West Germany, Marxism was much less savagely repressed in the off-shore island in the 60s and 70s. There was a minority tradition of comparative anthropology which was developed first by 'nostalgic' anthropologists in the colonies and then applied to Britain after the war: it led to a concern for 'detailed description of the specific' which at the London School of Economics and Political Science leaked slightly into Sociology training.

3) *New Left Review* after 1962 and other influences led to an interest in comparative-national-difference studies in leftist sociology in the early 1960s onwards. Its large and primary defect was a 'determinist theoreticism'. This was all to do with the supposed determination of individuals by the specific 'structures' in which they found themselves. The study of 'collective psychology' was not much approached, and certainly not by way of individual biography.

4) The 1920s-1950s US school of 'culture and personality' not having many British prolongations, culturalism developed in a non-comparative fashion. The later development of 'psychoanalytic interest' was on a purely individualist basis and the juncture with the Marxist concern for social structures was more like an awkward juxtaposition than a successful integration of perspectives. It has not yet led to an integration of the relevant methodologies

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[13] Going back to the Wolcott excerpt at the start of this paper, there was a jump from the raw data to 'complacent interpretation' without the mediation of systematic and careful team analysis, one of the hall-marks and very strong points of BIM.

Conclusion

Prue Chamberlayne's early Sostris paper "Social exclusion: sociological traditions and national contexts" (published later as Chamberlayne, 1998) argued presciently as follows:

> " The preparation of this report has also drawn attention to the different sociological, political, and social policy perspectives which the national teams are likely to bring to the research. Our original application over-simplistically presumed a common 'European' framework. Appreciation of the sociological diversity amongst us is critical for the processes of theoretical and policy interpretation which are involved in qualitative methodology, and can be helpful in negotiating communicative difficulties in the project. The national reports highlight the multi-dimensionality of perspectives which come into play in our interpretations of interview material, and cross-national comparisions cause us to reflect back on the particularities, even the narrowness, of our own particular traditions. Discussion of broad national differences in sociology and social policy also provides a springboard to discussion of the more personal and individual meanings which we bring to the research. For we are individuated, just as much as our interviewees, while we are also representatives and products of particular cultures. An attempt at self-conscious understanding of both the personal and the social levels is necessary to a critical approach to our research both as a process and as a product....
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> ... In the process of commenting on each others' interpretations and even more at the stage of making cross-national comparisions between the findings , we will also be engaged in a process of dialogue between different sociological perspectives. To a degree such processes are involved in any comparative project. But the research design of the Sostris project involves particularly close and ongoing collaboration, in a process of emergent and inductive theorising which spans deep-rooted sociological cleavages in Europe (1998: 1; 11)"

Excited and unified by the evident fertility of the biographic-interpretive method at the level of analysis, the Sostris teams had some difficulty in managing the movement from individual case-depictions to cross-case and cross-national comparision and theorising, as the guidance and constraint of BIM became less. The explicit discussion of differences at the cross-national Sostris workshops was and will continue to be crucial in overcome mostly relatively unconscious obstacles to scientific creativity. This paper argues that a key obstacle was an epistemological failure to distinguish sharply between levels of transforming qualitative data: the very power and success of the biographic-interpretive method for case-reconstruction made it difficult for Sostris to realize that the next stage of interpretation was a very different stage. To speed up such inevitable transitions in the future can be helped by understanding key questions of methodological investment and transition.

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