REAL MEN DON'T COLLECT SOFT DATA

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Preface

Real men don't collect soft data is a booklet which presents some reflections emerged during the research project «Qualitative methods for organizational analysis». This project has been carried out jointly by both the Dipartimento di Politica Sociale of the University of Trento and the Organizational Research Unit of the Department of Sociology, University of Exeter, U.K.

A Seminar on qualitative methods has been held at the University of Trento in september 1986, where Barry Turner presented «The cognitive processes associated with the generation of grounded theory». Silvia Ghe-

ardi received the 'honorary research fellowship' from the University of Exeter for the academic years 1985-86 and 1986-87 to develop studies and applications of grounded theory in organizational studies.

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Altough a fully collaborative effort, in this version the principal contribution in part I has been made by Barry Turner and in part II by Silvia Gherardi: the booklet reflects work in progress and the authors would work on comments and issues raised.

Parte One

Introduction

Assumptions ultimately mean choice, and the exploration of assumptions involves the exploration of choice.
(Morgan, 1983: 382)

A common usage in discussion of social science links quantitative styles of inquiry and data collection with a 'hard' view of the world, and qualitative approaches with a 'soft' view. As with many unexamined language patterns, these distinctions serve to convey tacit attitudes about the topic under discussion: the connotations of these terms are such as to suggest that 'hard' social science is masculine and to be respected, whilst 'soft' social science is feminine and of a lower order of activity. The message conveyed in these tacit usages is that quantitative work is courageous, hard biting, hard work.

Collecting hard data means making hard decision, taking no nonsense, hardening one's heart to weaklings, building on a hard core of material, using hard words to press on to hard won results which often carry with them promises of hard cash for future research and career prospects. By contrast, soft data is weak, unstable, impressible, squashy and sensual. The softies, weaklings, or ninnys who carry it out have too much of a soft-spot for counter-argument for them to be taken seriously, they reveal the soft underbelly of the social science enterprise, are likely to soft-soap those who listen to them. They are too soft

hearted, pitying and maybe even foolish to be taken seriously so that it is only right that they should be employed on soft money.

These contrasts are sufficiently firmly established for journal editors to refuse to accept the phrase 'hard qualitative data' on the grounds that it would be «confusing to readers», and they deserve discussion because the current usage serves to bias assesments of current new directions in social science research. They are used as code words for a cluster of issues relating not only to the *style of inquiry*, to the *style of questions asked* and the *style of answers* sought, but also to the association of quantitative investigations with major institutionalised patterns of research, and the consequent access to machinery, to research aides and to control over concentrations of resources. The 'hard', macho image is also likely to be associated with the distancing of senior researchers from 'subjects' on 'objects' of inquiry; with the reduction of threats to the self by the use of anxiety-reducing research rituals of execution and research presentation; and with a reduced willingness to tolerate ambiguity in procedures and findings. (Silverman, 1985)

The recent growth of interest in

qualitative research makes it important to challenge these clusters of assumptions which get smuggled into discussion of research — and also of research funding — and to question the extent to which such views can automatically be held to be correct. A parallel argument has recently been advanced in the field of management theory, where Basoux (1987) has noted that management was originally formulated as a rational-deductive task to be tackled by men:

> «The good manager is aggressive, competitive, firm, just. He is not feminine; he is not soft or yielding or dependent or intuitive in the womanly sense. The very expression of emotion is viewed as a feminine weakness that would interfere with effective business processes.»
>
> (McGregor, 1967; 23)

But with the recognition of the central importance of cultural issues in management (Peters and Watermann, 1982) and the need to cope with incursions into the West from Japanese firms, management virtues are now seen to include consensus, involvement, patience, compromise and moderation. The new view promotes a modified role model of the manager as «intuitive, nurturing and accessible — a job description which women are well-placed to fulfil». (Basoux, 1987)

In the complex world of contemporary social science, similar shifts are taking place, and the issues to be confronted are too subtle and too important to be handled by means of a crude and over-simplified dichotomy, es-pecially when this presumed opposition is accompanied by properties derived from sexist stereotypes. In the remainder of this paper, we wish to explore some of the complexities of the current changes which are being handled within social science research, and to relate them especially to the developing trends of investigations within the field of organisational sociology.

The process of research

The bureaucratisation of scientific research proceeds on the assumption that the task of research is one which is amenable to the same kind of hierarchical division of labour as are tasks in manufacturing industry or in official administration. This view of research is reinforced by much of the abstract and elaborate edifice of 're-search methodology' which the social sciences have generated, (Willer & Willer, 1974; Zetterberg, 1954; Hage, 1972) and which has generated its own momentum and its own autonomy as an area of abstract learning. But there is an obscurity about how the detailed and rule-bound practices advocated for the definition of concepts and for the costruction of theory relate to the actual process of ongoing research. Such writings, while pursuing theoretical and logical rigour, produce systems of abstraction with normative undertones — this is how research should be done — whilst retaining a problematic relationship to the processes which they claim to explicate. In a similar manner, as we shall see, attempts to absorb the new emphasis upon qualitative research into existing orthodoxies of research methodology produce an illusory clarity, for they do not look closely enough at the research process.

As always, we find that social reality confounds our simple armchair theorising: it is more messy, more convoluted and more surprising than we thought it would be. Fortunately this realisation, prompted by the growth in the sociology of natural science, has now given us some accounts of how research is pursued in fact, rather than in research methodology. Hammond's pioneering collection *Sociologists at Work* (1964) has been supplemented by other accounts (Bell & Newby, 1984; Bryman, 1988).

There is, then, another opposition, between the structures advocated by the methodologists and the 'theorologists' on the one hand, and the accounts of research as it is done, and theory generation as it occurs, which have been produced by Hammond and his successors. To resolve this opposition, we need to look for an emerging middle ground which presents guides to research procedure and theory generation which accord more with the nature of research practice. And, when we look for this middle ground, we find also that we must tackle issues which blur the simple qualitative/quantitative, hard/soft contrasts with which we began.

We are helped in our task by the way in which investigations into the sociology and the philosophy of natural science have shown it not to be the gleaming aseptic edifice promoted in developments after World War II, but to be a human enterprise, fraught with all of the personal, emotional and political difficulties displayed by any human undertaking. Natural science is, as Ravetz (1971) has demonstrated, a craft process: judgement, craft skills and intuitive knowledge are deployed by natural scientists in the assessment of the satisfactory operation of equipment; in descriptive and other scientific reporting skills; in the intuitive adoption of appropriate preliminary theories about a given context; in judgements about the appropriate use and the appropriate fit of mathematical models; in evaluations of the reliability of data collected, making the transition from collected data to usable information; in the elaboration of argument; (Feyerabend, 1975) in the appropriate use of pre-existing information gathered by other researchers; in the development of tools and techniques and in the acquisition of skills in using them; in the avoidance of pitfalls characteristic of the field of inquiry; and, throughout, in the style of research pursued:

> «The investigation of a scientific problem is creative work, in which personal choices as well as personal judgements are involved at every stage up to the last ... Even though (the scientist) is concerned with properties of the external world the work he (*sic*) produces will be characterised by a certain style unique to himself ...

> There is no conflict between a highly individual style in the investigation of problems and the production of results which meet the socially imposed criteria of adequacy for the field.»
>
> (Ravetz, Ch. 2, 1971)

It is clear that all of the craft elements which Ravetz identifies with the procedures of natural science, and which account for the distinctive development of 'schools' of natural science associated with 'master-pupil' pedigrees stretching over centuries (H.T. Pledge, *Science since 1500*, HMSO, 1939) will be evident within even the most quantitative of social sciences. Judgements about equipment, procedures, abstracting and typifying activities, theorising, modelling and so on pervade any quantitative social science investigation. Baldamus (1976) has drawn attention to the way in which fundamental approaches to theorising and abstraction are embedded in such commonplace social science activities as cross-classification and the construction of two-by-two tables, and has pointed out the manner in which the practical procedures of survey research are saturated with theoretical and value-judgements. Survey research is almost entirely conceived of as a rule-bound methodology, but its operations depend upon the tacit knowledge, developed by researchers in the process of use, which underpins the process of cross-classification, of elaboration analysis, the handling of variables in a statistical manner and so on (Rosenberg, 1968). Without such implicit theoretical techniques, the rules of survey research would enable us to build, as Ravetz phrases it, a plane which would not fly.

And, again, the strictures of the research handbooks have to be reconciled with accounts of how research is actually carried out. Lowe's (1971) analysis of the accounts in Hammond demonstrate that, with one exception the projects embarked upon by these distinguished researchers all reached a point of disruption, where the original plan, the original project, the original rationale for the research suffered a breakdown, precipitating a crisis and requiring activities of what Lowe calls theoretical 'patchworking' or theoretical 'bricolage' in order to repair the breakdown and to present an appearance of coherence in the work. Of course, if research is recognised to be a journey into the unknown rather than a task which can be fully specified and planned in advance, then such breakdowns look less surprising, and we can look (Lowe suggests) at the patchworking as the injection of a creative element into the process.

All of the above is intended merely to make the point that the process of research, even natural science research, is one which involves the use of judgement, craft skills and what Polanyi (1959) calls tacit knowledge, that it frequently does not follow a preordained path and that the intelligence needed to pursue research is not wholly rational-deductive. Witkin (1971) has argued that all creative developments, in the field of both arts and science, involve the use not only of cognitive intelligence but also an affective intelligence, an 'intelligence of feeling': the objective which a scientist pursues is likely to have sensuous properties, and to be affectively or emotionally charged. The personal manner in which this objective is symbolised will direct the process of interaction between the investigator and the particular medium or context which is being investigated. Such a process is not a completely random one, or one without rules, but the rules will relate to the particular combination of [objective] and [investigatory context]. The rules will not be a universally applicable array, but will be context specific, and generated in the process of interaction between investigator and investigatory field, as part of the process of the exercise of the cognitive *and* the affective intelligence. As the American physicist Bridgeman commented, 'science is doing your damndest to understand with no holds barred'.

The rules of scientific investigation to which we have been referring are not those common to research methodology, nor are they the rules for the socially approved presentation of research findings to the scholarly community. They are the rules generated in interaction to guide further investigation in a manner likely to be scientifically fruitful. As the rules of a painting by numbers kit relate to artistic creation, so do sets of research procedures relate to successful scientific investigation.

Let us turn our attention, then, to the related question of number and counting. We would not wish to take up the obscurantist position sometimes encountered among qualitative researchers, that qualitative researchers are not permitted to count. If we are to understand the natural or the social world 'with no holds barred' then we need to deploy whatever appropriate means come to hand. As we suggested at the start, such simple oppositions as 'numers *versus* no numbers' are inadequate for discussing and understanding the full complexity of the research process. We need, instead, to look a little more closely at what is meant by number and how number might be appropriately deployed in different research contexts.

But number is a metaphor. In counting and quantification, number is employed to draw similarities. Mathematics provides us with accounts of systems of logical operations and interconnections. In the use of mathematics we place sections of the world which we are interested in alongside portions of mathematical reasoning and assert that the two bear some resemblance to each other. This process is as true for the juxtaposition of a row of sheep and the numbers 'one, two, three' as it is for the explorations of parallels between the behaviour of a national economy and the properties of a complex econometric model. We should, therefore, be clear about which of the metaphorical properties of number we wish to use to assist our understanding of the social and

organisational world. Is the case to be made that there are no relevant properties? That mathematical counting and related operations cannot possibly help us because we are interested in processes and properties which are inherently uncountable. Or, in a related position, one suggested by William Blake in his famous picture of a god-like figure leaning down from the heavens with a pair of calipers, do we wish to make the case that the process of measurement, of quantification in itself is damaging to the stature, to the quality and to the dignity of humankind? Or are the relevant distinctions not between «counting» and «not counting», but between «counting to one» (unity) and «counting to more than one», or between «counting to one, two and three» and «counting many» (as in several language forms).

Again, to resolve such questions in a research context, we need to think clearly about what we are intending to do when we are suggesting that properties of the systems and activities we are interested in are usefully close to the properties of abstract number systems. When the parallels are close ones, properly constituted, we are able to assist our enquiries because transformations which can be carried out powerfully and with great facility through manipulations of numbers tell us about properties of the social world, but in order to use these we need to ensure that the operations which we apply to our numbers are appropriate ones. The weakest

kind of numbering is simply using numbers to identify and label phenomena (as in Mark I, Mark II, etc. James I, James II). These numbers identify unambiguously, but we can do nothing useful if we try operations such as 'James I + James II =' or still less if we ask questions about the square root of James II. At the opposite extreme, we have numbers which reflect all of the properties of natural numbers, so that we can add, subtract, multiply, divide and so on with impunity, confident that the operations carried out on the numbers also mean something for our data. Coleman's *Introduction to Mathematical Sociology,* particularly in the earlier chapters, provides an excellent discussion of these issues. It is important to spend time on these topics because of the peculiar standing of numbers in our civilisation. On the one hand we are mesmerised by numbers, even when they are pseudo-numbers, those who deal with them frequently no less than those who are thrown into a panic by them. On the other hand, the general standard of teaching about mathematical issues is so poor that few people understand fully the nature of the properties of the numbers and number systems which they are advocating or excoriating.

A new and useful notation has recently been developed to draw attention to properties of numbers which are frequently ignored. Ravetz and Funtowicz (1986), alarmed by the misuse of numbers in debates about nuclear safety levels, constructed the

NUSAP notation which, though intended for application to natural science and engineering in the first instance, can also helpfully clarify thought in the social science area. The essence of their system is simple: that a single number standing alone is misleading. To evaluate its meaning, we need an additional four pieces of information. Ravetz and Funtowicz express this as N:U:S:A:P - Number: Units: Spread: Assessment: Pedigree. When presenting others with a number to try to elucidate a portion of our research, we will mislead them if we allow this *number* to stand alone. Our audience will need to know the *units* of our measurement, and some measure of the *spread* or distribution around the point specified. And then perhaps even more importantly they need to know where the number has come from, its *pedigree* – is it based upon an exhaustive and detailed measurement process covering every possible variable which might influence the outcome, or upon a snap judgement from someone over the telephone? Associated with the pedigree, the audience would be assisted by an *assessment* of the standing of this number in the eyes of those well placed to make a judgement. Do those in the field think that this is a sensible guess, or a shoddy estimate, is it the best possible attempt at measurement, or a figure over which commentators disagree?

In qualitative analyses, there are no reasons why numbers should not be appropriately deployed: Suttle's use

of small tables showing how many gang fights took place in a given period of time in a Chicago slum, or how many members of one ethnic group visited a shop owned by a member of another ethnic group in an afternoon (Suttles, 1978) help to inform us about his territory and add an element to a study otherwise based upon qualitative data gathered through participant observation. When Suttles specifies five fights in a month, we know that he is counting in units of one, that the number could rise above or fall below that in the preceding or following months, that he got the figure by talking to people around the neighbourhood, and that the trust that we can place in the figure is about the same as the trust which we can place upon Suttles general account, as a competent but human individual observer spending some time in the vicinity. This kind of background information is needed, these kinds of judgements are made, tacitly or explicitly whenever *any* number is presented in a research context.

When is it difficult or inappropriate to count at all? Here it seems to be useful to introduce a distinction between standardised and non-standardised data. To count, the items or the features to be counted need to be available in a standardised form. And, since the world is rarely standardised in itself, we need to have strong rules to declare certain variations in the data as 'error' which can be safely disregarded; when we are happy to do this, we generate data which can be useful-

ly tallied up, in conditions where we judge that anything that is not standard can be safely ignored. By contrast when we are reluctant to specify units, or when we are reluctant to declare the variety in quality between units to be unimportant, or to disregard it, we find ourselves dealing with non-standardised data. We have weak rules for classifying portions of the data as 'error' and we have to find ways to cope with the resulting variations in our analysis which do not make use of the analogous properties of number systems.

To these distinctions we need to add others concerned with broader and deeper philosophical and epistemological issues: with whether our organisational research is to be concerned with prediction, with the generation of theory, with the acquisition of interpretative meanings, or with the informing of political action. Reason and Rowe (1981) offer a distinction, in discussing new approaches to organisational research, between those who are seeking theory as prediction and those who want theory as pattern. Broadly this distinction coincides with that between positivistic inquiry and what Lincoln and Guba (1985) somewhat controversially call 'naturalistic' inquiry. The distinction here is not merely between the uses of theory but also between the different canons which may appropriately be applied to the judgement of the research. Lincoln and Guba (1985) suggest that, whereas positivistic inquiries are judged according to their

rigour, naturalistic inquiries should be assessed on the basis of trustworthiness and authenticity. Rigour conventionally looks at the truth value of propositions, at their validity or generalisability, at their reliability and at their objectivity. Research which fails to meet these criteria is confounded, atypical, unstable or biassed. By contrast, they suggest that appropriate criteria for assessing naturalistic research, appropriate criteria of trustworthiness would be that the research should be credible, its findings should be transferable, dependable and confirmable, and they suggest techniques for the improvement of all of these qualities of 'naturalistic' research ([1]).

A further distinction in research which can usefully be added to our considerations is that made by Glaser and Strauss (1967) between research which is concerned with *verification* and research which is concerned with *discovery*. In the former type, theory serves as a framework to guide verification. In the latter, theory is the 'jottings in the margins of ongoing research', a kind of research in which order is not very immediately attained, a messy, puzzling and intriguing kind of research in which the conclusions are not known before the investigations are carried out. This

([1]) Note that this usage is intended to refer to studies of social phenomena in their 'natural' settings, but it should be distinguished clearly from the long established philosophical use of the term 'naturalism', a position close to positivism and far removed from naturalistic inquiry.

does not mean that the researcher is unprepared for investigation - «fortune favours the prepared mind». A domain of inquiry needs to be identified, and much preparation can be carried out by becoming familiar with the empirical and theoretical literature concerned with the given domain, several different literatures possibly being relevant to different facets of the research domain.

Against this background, empirical research can commence, with a sharpened perception and an array of questions, uncertainties and doubts. In 'discovery oriented' research, the extent to which the researcher acts as the research instrument is likely to be maximised. An openness of mind requires a faithful attention to the sensations offered in the field situation, but at the same time the essentially active part played by the investigator may be symbolised by the use of the term 'capta' rather than data (Miles and Huberman, 1984) to stress the extent to which information is captured from rather than given by the social setting. Non-standardised information, 'capta' acquired as a result of close attention to a portion of the social world relevant to the research domain will provide the fidelity, trustworthiness and authenticity which Lincoln and Guba have advocated, giving such data an authority which is difficult to overturn in relation to the context in which it was gathered, but which then poses immediately the question of the extent to which the findings are transferable to other contexts.

Theory Generation from qualitative data

We are labouring these basic points about painstaking fieldwork because they seem to us to be essential preparation for the work of theory generation in qualitative research.

Good fieldwork can be helped by guides currently available to researchers which explore the problems of data gathering, access and so on (Burgess, 1982; Burgess, 1984) and which alert us to the importance of being appropriately prepared and equipped, of being suitably attentive and sensitive, of assessing the quality of our observations (Turner, 1988) and of being able to negotiate entry to the domain of inquiry. A more difficult process however, and one which is little discussed, is that which follows on the acquisition of a full set of field notes or interview transcripts - the process of typification and categorisation of the data in the initial steps of analysis.

We have learned enough over the past century about cognition and understanding and about the nature of social reality to realise that good research is not simply a matter of reportage, of listing events and encounters to show where we have been. The account of 'reality' which is being sought in the research process is a little different from that which might be offered in a chatty essay or in a short article for a colour supplement. What is wanted is not a social 'shopping list' which records what has been noticed,

but an account of a series of interactions with the social world in a form which plausibly alerts us to the possibility of a new order not previously seen - a theoretical account.

This theoretical account of reality has to be one in which we, again, are active contributors. We are active in attending to various facets of the encounters which we experience, we are active in the early stages of analysis when we divide up our experiences into fragments, dimension, characteristics and features which we make noteworthy and we are active in the new syntheses which we start to make as we structure our own past experiences and future expectations. As a young writer, V.S. Naipaul spent much time searching for real 'writer's material' and worrying because he could not find it. He did not, as he later realised, see the richness of the material which surrounded him all the time when he had arrived in his boarding house in post-war London, and only now, thirty years on, is he finding himself able actively to re-evaluate and reinterpret his experiences at that time, seeing or creating their meaning. (Naipaul, 1987) The ordinary encounters which a new researcher is involved with in the field may not look very exciting. He or she may well need reassuring that they are real 'sociologist's material' and further, may need to be convinced that they are accessible in some way to theoretical interpretation.

Of course, as we are structuring our own expectations, we are also trying to structure the expectations of others. To perceive, we typify: there is no alternative. Without structure, perception is chaotic and any account of the world must typify. One of the most difficult tasks in qualitative social science research is deciding just what kind or level of typification is useful in the appraising of field notes and interview transcripts in order to allow the material to release its sociological messages (Martin & Turner, 1987; Strauss, 1987; Turner, 1988).

In the approach known as 'grounded theory' (Glaser & Strauss, 1967; Turner 1981) a crucial but little discussed stage involves precisely this matter of the appropriate level of typification which will serve to articulate a theoretical understanding of a given set of non-standardised data. Both Glaser (1978) and Strauss (1987) recognise this difficulty and suggest that this step can best be learned by example, in research meetings or in collaborative research workshops. This is good advice, and it is even possible to make use of such sessions to train engineering researchers concerned with analysing non-standardised data collected about engineering practice. (Turner, 1987a)

But what is happening in such encounters? What tacit research skills are being passed on? One such skill is the ability to judge what level of generality it is helpful to work with (Strauss, 1987) and another is the reassurance that the researcher has to *contribute* some elements to the data in order to generate a meaningful or an insightful pattern of typification. Only by coming to feel comfortable about 'putting meaning in' can the researcher structure the situation, initially for him or herself and subsequently for the readers of the research account.

A parallel can perhaps be drawn with the painter's contribution to the process of Chinese *hseih-i* watercolour painting. Although in this style, the paintings of plants, animals and landscapes may be based upon many detailed studies and sketches from nature, these form no more than a preparatory stage in the work. The final painting is made from memory, in the studio:

> «We would search in vain for the concrete scenery of Huang's pictures. Instead we would recognise how he unravelled the core of that immense confusion of natural miracles and caprices to create a dignified yet simple landscape core which does not confuse our vision. Huang created a picture of the Yellow Mountains in their sensual and spiritual character out of thousands of views and thousands of experiences.»
>
> (Hejzlar, 1987; p. 52)

Or, again, in relation to Wang Ch'ing-Fang's studies of fish:

> «His pictures of the ink carps and small fishes, or golden acquarium fishes in bright red were made with an understanding of the creatures' rhythm of movement and the resistance of the invisible element, water. Boldness and elegance of expression are the result of Wang's immense patience as an observer.» [2]
>
> (Hejzlar, 1987; p. 57)

The ideal type, as Weber recognised, bears a similar relationship to a particular set of empirical data as these paintings do to the nature sketches which preceded them. In making sense of our experience we need to produce from a set of qualitative data a theoretical account which summarises our understanding of possible regularities associated with the set. These regularities will have the potential of unifying not only the empirical data which we have already dissected, but also other material which we have not yet seen. Without this potential neither science nor human life would be possible.

The theory will not list our experiences, but will offer an arrangement of elements of those experiences which we find useful and which might be useful to others. We are engaged in the generation of theory not primarily as a predictor of variables, but as a pattern which we will recognise when it recurs (Reason & Rowe, 1981). If the theoretical pattern is sufficiently recognisable, useful and sensitively constructed; and if our segment of the world is not too unrepresentative of aspects of that pattern, it may turn out to be recognisable, appealing and useful to others as well.

[2] We are indebted to Nedira Yakir for the information that Gauguin, too, commented that the only things worth painting are those which are remembered.

Research communication over theory

One of the benefits from trying to make more explicit the processes of qualitative research is to make formerly hidden procedures and practices which have had to be discovered and rediscovered with varying degrees of success by each generation of researchers more accessible and open to discussion and improvement. We are concerned with universal processes of data transformation and the explicit discussion of them is likely to allow for collective improvements to be effected in the manner in which they are tackled.

In consequence of the explicitness, it is possible for communication about the intermediate stages of qualitative analysis to take place, between research principal and research assistants, between research collaborators, between research students and supervisors. Communication may occur by several means: through making explicit the 'low-level theoretical labels' which are generated in the early stages of data analysis as initial coding categories are invented; through discussion of the definitions of the most important and central of these concepts; through theoretical memoranda generated alongside the processes of detailed data analysis; through the use of sections of a research journal if one is being kept by the researchers in question; through communication of those occasional moments of high energy creative synthesis which is one of the rare delights of research and which Glaser has called the 'drugless trip' (1978). It is assumed in each of these possible strands of communication about research that writing is a research skill. We wish to urge a clear separation between writing as a means of organising and presenting final results to an audience and writing as a research skill. The goal of writing every day not only helps to avoid writing blocks, but gives regular practice to the qualitative researcher in externalising thoughts about the issues and evidence of the research in hand. Regular writing for oneself avoids the anxiety associated with having to write a paper for a seminar or a journal. Also, it demonstrates practically that writing is a skill which can be acquired and improved by practice.

Writing externalises thought and in doing so makes it less personal, more durable and more specific. Even without external intervention, writers are likely to learn from feedback from their own externalised thoughts, and this process can be augmented by comments from colleagues, critics and helpers who are able to read the written output (Barzun & Graaf, part III, 1977; Strunk & White, 1979; Mullins, 1977; Elbow, 1981). I am sure that we all know people with rooms full of research material which they have never been able to publish because they have never been able to write about it. The researcher who makes a habit of writing regularly will find it easier to prepare drafts, discussion papers and outlines to be analysed in discussion session, and will find fewer problems in making the transition from the use of writing as a research skill to the use of writing for the presentation of accounts of research to a professional audience.

Links with existing theory

In the nature of the process outlined above the final theoretical stage will involve the building of bridges between the analysis of the field observations and theoretical aspects of existing studies. One would expect that some of these links will reflect the themes explored in initial literature searches, as the analysis picks up, amplifies, questions or modifies previous theoretical views. However, linked with the element of discovery which we have discussed above, we would also expect to be making use of other, more unexpected sets of theoretical writing.

We can trace several sequences in the preceding accounts of the pursuit of soft data. Given that one of the defining characteristics of such research is its stress upon interpretation and upon negotiation. Agar (1986) suggests, following Gadamer, a sequence of interpretations as follow: Encounter; leading to surprise and breakdown; then to resolution and finally to the production of a coherent account.

As Lowe's inquiries have suggested, following breakdown the qualitative investigator has also, however, to strive to achieve some degree of coherence, or to move towards some mode of operation which does not in practice cause too much personal distress, between his/her relationships with the observed, and his/her relationships with the scientific community. Ways of seeking coherence may include the following:

* searching for observed actions to locate typical motives, typical ends, typical means in typical situations so that they can be placed in a frame or plan for communication *back* to those observed or *onward* to the scientific community. (Goffman, 1975; Burke, 1969)

* searching for inferences derived from the observations for onward transmission to the scientific community, and in doing so, recognising that there may be a need to use varied forms of logic - logic which is 'fuzzy', or 'plausible', or statements which are only acceptable if they are hedged: «not exactly...», or «sort of...».

* searching for ways of accomodating to the difficulties provoked by competing accounts which are offered - Castenada's works pose these difficulties nicely. (Castenada, 1973)

Transformation may be seen as an aspect of context, the elements which comprise a context and the relationship between the elements (information, objects, actions, symbols, identities). A context is a framework of memory, a set of related elements which gives sense to elements brought into relationship with it.

Transformation then, is a process, although not all processes are transformations. We may change elements or relationships within a context, or we may shift the same element to another context without transformation. (Bateson, 1972)

However merging of contexts frequently generates inconsistency and thus transformation. Transformation requires some shift in the interdependency between context and content.

Transformation may occur when ambiguity or uncertainty appears - there is a strain to consistency in our handling of data, and in resolving inconsistencies at certain levels we may produce transformation. Position and dominance are important in these processes, as are aesthetics and play, and some symbols have transformation inherent within them.

In transforming data we are likely to cluster them or to link them by chronological sequence, or by spatial contiguity, or more generally, by the characteristics of the data gathering operations. In re-sorting and writing up, we rearrange according to criteria of interest, fitness for argument, relevance to certain issues or topics or propositions. In a sense, too, the transforming operation is a problem solving operation, where the starting point poses questions or problems and the task is to answer those questions or to solve the problems. This parallel makes all of the problem-solving literature potentially relevant to these processes, it also throws into importance the difference between information in

a channel of communication and information in a channel of observation. Related to this is the distinction between research in a context of verification and research in a context of discovery. Inquiries designed to solve a given problem, to *verify* can be regarded as treating information as if in a channel of communication. The data located can be assessed according to how far they fill in the gaps in a known puzzle.

Each additional piece automatically has the property of reducing uncertainty, and learning, knowledge acquisition is synonymous with information acquisition. (Turner, 1977)

By contrast, however, inquiries designed to solve an unstated or an ambiguously stated problem, to *discover,* have to treat information as if in a channel of observation. The data located have to be assessed for relevance according to criteria of relevance generated by the observer. While a provisional puzzle may be delineated, and progress made towards solving it, the observer will also monitor this progress and may use judgement to jettison this goal and substitute another, particularly after *surprising* information has been acquired. This mode of progress is consistent with all of those accounts of puzzle-solving which require a reframing or a respecifying of the problem-assumption built into the original problem, transforming or understanding of it.

It is very helpful in this context the observations by the natural scientist Hans Selye about the manner in

which he saw natural science ideas and theories coming into existence:

> «The human brain is so constructed that it refuses to handle thoughts unless they can be wrapped up more or less neatly in individual IDEA-PACKAGES. It is astonishing how much confusion has been caused by the failure to understand the following three simple cafts:
>
> (a) Thoughts, like fluids, can be adequately handled (isolated, measured, mixed, sold) only when put up in individual containers;
>
> (b) The thought packages contain previous experiences; only the selection within the wrapping can be new. We have no thoughts of things whose likeness we have never perceived before.
>
> (c) The thought-packages, the idea units, are very loosely bound together and their contents are not homogeneous». (Selye, 1964, p. 268)

As Selye says, we put into packages those things which we have seen before, but rearranged, and metaphor may be thought of as a way of rearranging them. We see that this part of the world is like another entirely different part of the world, and we use this vision as a guide to our reordering. How can we formalise this? Choose a metaphor. Then rearrange incoming data to resemble the metaphor. That of course is absurdly over-simple, but how must we complicate it to make it more realistic? We would want to bear in mind the complex, pervasive, connotative symbolic qualities of metaphor rather than looking upon them solely as an information processing device. Miles and Huberman refer to metaphor, but they seem to have a very impoverished idea of what metaphor is or does, and not to realise that virtually all language and thought is metaphoric.

Part Two

Soft data in organisation studies

There is a renewed interest in qualitative methods within the field of organizational studies. In 1979 the *Administrative Science Quarterly* dedicated an entire issue to this subject and since then a number of books have been published which aim to defend the scientific credentials of qualitative methods, and sometimes to stress their divergence from quantitative methods as far as criteria of evaluation are concerned. It is therefore worth asking why this new interest has arisen and whether more lies behind it than merely a passing whim or an emotional counter-reaction to the dryness of calculation and the boredom of numbers. Is it, perhaps, nothing more than a reaction to a disappointing liaison with the computer: when the promised access to the hidden laws governing organizations fails to develop, is it only natural to search for a more humanistic understanding, one which involves the heart rather than the head?

Of course the new technology offered new opportunities which, in turn, influenced the types of questions that researchers could ask themselves and this conditioned the field of research. The resulting enquiries were not sterile. The problem was not that no results were produced, but that the results which did emerge were not those which the generation of researchers concerned had hoped to obtain. The frustration which this gave rise to is, perhaps, understandable, even if it is not wholly justified. But it has made us aware that we have now reached a stage where our understanding of research into organisations needs to be reevaluated. We can now affirm not only that there is no «one best way», not only that there is a high degree of uncertainty even when we limit ourselves to looking for explanations which offer only a moderate 'fit', but that there exists nothing which could be called the 'laws of organisation'.

It becomes inevitable, therefore, that researchers review and question the ontological and epistemological assumptions which form the basis of their knowledge. Is an organisation to be considered as an instrument? If so, is it a rational instrument? And how does the rationality of the organisation relate to the rationality of its individual members?

To the extent that organisational studies can be said to have possessed a core set of assumptions and approaches, this core paradigm has recently been subjected to criticism by several writers, and even denounced as being 'in crisis'. The criticisms formulated by Mary Zey-Ferrell in 1981 and the debate which they provoked can be taken as particularly significant. The dominant formulation within organisational studies was reproached for:

1) holding overly rational image of the functioning of organizations;
2) constructing theory which reifies organizational goals;
3) generating ideologically conservative assumptions and methods of analysis;
4) viewing organizational systems as integrated through the value consensus and common interests of its employees/members;
5) conducting a historical analysis of organizations;
6) emphasizing only the static aspects of organizations;
7) de-emphasizing power in organizational analysis;
8) holding images of organizations which are overtly constrained;
9) holding images of humans as non volitional;
10) viewing organizations as the exclusive unit of analysis.
(Zey-Ferrel, 1981)

Criticisms such as these have not, in themselves, changed the course of thought and research on organisations. Rather they have made evident and crystallised a discontent and a discomfort which was already being felt in the work of many authors by the end of the nineteen-seventies or the start of the nineteen-eighties. Structuralism and positivism were already under attack and this became linked with unease about the manner in which quantitative methods had been developed in order to try to test empirically models derived from a Weberian image of bureaucracy and to carry out comparative research using the 'dimensions' of organisations which had been derived from such models.

If, therefore, there was dismay at having to admit that it was impossible to formulate universal laws, it had further to be acknowledged that the measurement of bureaucracy did not unveil the working of sets of harmonious relationships between the constituent parts of organisations, and, indeed, that such criticisms could not be met by more empirical research of the quantitative type. In such circumstances, it is not surprising that researchers should have rediscovered qualitative methods, or that they should be questioning themselves about the irrational, unexpected consequences of social action, about the subjective viewpoints of organisational actors, about the conflict of interest and about the differing interpretations of reality to be found within organisations. In opposition to the various 'determinisms' these rediscoveries have stressed the volitional and decisional aspects of action within organisations.

However, as we have implied above, it would be historically wrong to link all criticisms of the structural-functional approach with quantitative methods. Qualitative research has always been carried out within organisations, and, indeed, is an inseparable part of

almost all organisational studies. The organisational researcher finds it difficult to avoid such qualitative inquiries. For one thing, it is quite common for research to be linked with consulting, an arrangement which offers easy access to many areas of organisations. As a consequence, the researcher carried out qualitative fieldwork, whether this was intended or not. A research consultant goes around an organisation with eyes and ears open, alert to the appropriate language which will permit effective contact with the various groups within the organisation. When this is done on purpose as a research task, it is called 'participant observation'. The researcher has to place a degree of trust in his or her own professional ability and experience, in interpreting what is encountered. Every competent researcher could be questioned about the extent to which a given analysis has been based upon those aspects of an organisation which have been measured, as against the extent to which personal knowledge, judgement and intuition have been involved. Putting this question to friends and colleagues, and without making any major claims for these unsystematically sought answers, we ascertained that all off them seemed to apply, more or less consciously, some qualitative methods. And, even where there is no reference to it in the 'methodology of the research' we would find it reasonable to assume that all case studies in which the author was also the researcher, will have used qualitative

methods, even if the data thus gathered has not been subjected to the kind of systematic analysis now common in qualitative research.

In line, therefore, with our earlier criticisms of the stereotypes of 'hard' and 'soft' research, we would wish to assert that qualitative research is an integral part of the study of organisations. The training of organisational researchers and the development of research skills to be used in organisations contain many elements of tacit socialisation into the use of qualitative methods. There is thus nothing substantially new in qualitative analysis in this field. But the crisis of the dominant paradigm (understood as a conventional event which we can use to establish a 'before' and an 'after') has produced a division as far as *reflection* about such methods is concerned. And the trends which can now be discovered within organisational studies are associated both with an intensification of the use of such research methods and an intensification of reflection about their use.

To explore some features of this increased awareness of the significance and the prevalence of qualitative methods, we have tried to summarise the field in two tables which we shall use to guide the remainder of our discussion. In Table A we have identified the main research methods in organisational studies as they might have been seen before the conventional event of the 'crisis of the dominant paradigm' which we have already mentioned. In Table B we have identi-

fied some methods which have been emerging subsequent to this crisis. We would stress that tables constitute little more than simple lists which do not claim either to be guides to the methods themselves or to offer extensive illustrations. Rather they provide a quick reminder ([3]) based on classification according to the following categories:

a) *The cognitive strategy of the researcher.* This refers to the manner in which the researcher establishes contact with the organisations and to the methodology which is used to guide this contact. An appreciation of this cognitive strategy should provide some indication of the questions being pursued by the researcher and also of the interests in the resulting knowledge.

b) *Organisational Phenomena.* That is to say, those aspects of the organisation which the researcher will be concerned to observe, given a commitment to a strategy and a methodology.

c) *Cognitive problem.* What is the core problem which concerns the researcher? What issues will the knowledge collected be used to resolve?

([3]) Unfortunately there is little reflective literature on methods of organisational analysis. To allow the reader to follow our own reading strategy, we have decided to rely mainly on two works, Ciborra (1978) and Morgan (1983), supplemented as far as the semiotic approach is concerned by Broms and Gahmberg (1987). We are very grateful to these authors, although the type of interpretation which we have applied to their work is solely our own responsability.

d) *Cognitive analogy.* Knowledge develops by establishing similarities between what is known and what is unknown. In organisational research, the organisation or some of its features form the unknown terms in any analogical equations which are proposed. We want to know therefore, what analogy a given theory of organisation has developed. Here we have drawn extensively on the work of Morgan (1986) on images of the organisation, although we have sometimes moved away from his work to suggest new metaphors or analogies. Our intention is to stimulate the reader by means of a key-word which evokes a mental picture and thus recalls to memory further elements of knowledge.

Before the paradigm crisis

Table A uses these four categories to examine six established methods of organisational analysis, methods or approaches which could be regarded as being firmly consolidated before criticisms about the dominant paradigm arose. These are: structural-functional analysis; systems analysis; socio-technical analysis; action research; social analysis; and institutional analysis.

Structural-Functional Analysis. The aim of such analysis is to discover the elements of uniformity in the structures of empirical processes. Formal organisation is the structural expression of organisational rationality and

TABLE «A» - ORGANIZATIONAL METHODOLOGIES BEFORE THE PARADIGM CRISIS

Methods	Knowledge Strategy	Organizational Phenomena	Knowledge Problem	Organizational Analogy
Structural-Functional Analysis	towards a knowledge of organizational process in its formal and informal aspects, the relation between them and changes of the formal system in order to meet environmental demands	structures, functions; (latent and manifest) formal organization, informal organization	efficiency	machine living organism
System Analysis	the simulation of the functioning of the organizational system, given certain input, and constructing an abstract model	the boundaries of the system and component sub-systems, objective functions, flow of material, people, information, control circuits; analysis of task	integration of the sub-systems for the conjunct optimization	cybernetic system (thermostat, black box)
Socio-Technical Analysis	the singling out of the technical requisits of the process in order to rationalize the confrontation between social system and critical variation of the technical system	the intrensic requisits of the process; the critical variations; the intervention of the social system upon the variants	the matching between control requisits and those of the social control	methaphor ergonomics
Action Research	some important information is not obtainable unless something is given in exchange to the the examined object. The commitment of the researcher is therefore essential within the perspective of a social change	the participation in a process of organizational change and the implementation of the change, especially as regards the task, the satisfaction and microdemocracy	it is the social question which orientates and legitimatizes the definition and conceptualization of the problem	therapeutical model of reliatioship: client-researcher
Social Analysis	to consent members of the organization to explore in a conscious way the latent reality in order to remove the social defences established against anxiety and to clarify the desired aspects of the organization	the organization as a defence system which has a protective function, a global community and as a cultural mechanism	the adjustment of the formal structure to include the informal structure which is a symptom of «discomfort» and attempt to overcome it	group therapy
Institutional Analysis	in the process of analysing an istitution the dynamics between the istitutionalising structures and the istitutionalized ones are recreated	the institution as a «fact», that's to say a globality of norms and the institution as a «process of institutionalization» which creates a starting point and a meaning to the behaviour of its members	the irrecuperability of the institutions is witness to their crisis	the political unconsciusness of the global society; the psychical prison

therefore the task of the researcher is to ask «What could be the differential consequences for the system of two or more alternative outcomes of a dynamic process? Such consequences will be found to fit into the terms of maintenance of stability or production of change, of integration or disruption of the system in some sense» (Parsons, 1959, pp. 27-29). The organisational phenomena examined through such an analysis are: the structures, the manifest and latent functions, the relation between formal and informal organisation.

The main problem which underpins the analysis relates to the efficiency of a given organisation. These theories are based on the development of an analogy between organisation and machine in the first instance, and between organisation and living organism in the second.

Systems Analysis. The aim is the formal description of the behaviour of the system so as to construct an abstract model which represents the functioning of the organisational system. Such a model may then be used for a computer simulation of the organisation-al system, viewed as a pattern of 'inputs' and 'outputs'. The sub-systems which comprise the system are analysed according to their tasks of input, output, transformation or coordination, and to the level of differentiation from or integration with other sub-systems (Lawrence and Lorsch, 1967). The organisational phenomena which are under examination are: the boundaries of the system and its sub-systems; the respective objectives; the functions; the exchange of materials, people and information with the environment; the circuits of control and regulation; communication networks and the analysis of the tasks and sites of decision. The theoretical approach is based upon an analogy between organisation and cybernetic systems (thermostats, feedback loops, etc.) which links knowledge in the theory of organisations to that from cybernetics, thermodynamics and information theory.

Socio-technical Analysis. The analysis of the organisation concerns mainly the nature of the integration between social system (roles, procedures, etc.) and technical system (machinery, tools, buildings). These interrelated systems are seen to be in dynamic equilibrium and to be subject to a 'control law' which regulates the overall system. The group provides a systemic control of the productive system, mediating between the single individual and the wider organisational system. The phenomena observed are mainly the essential requisites of the control process, the crucial variations in the development of the process and the means by which the social system may intervene in the control of the process. The organisational problem for which such analysis seeks a solution is that of matching the requisites of technical control with those of social control (Herbst, 1974).

The metaphorical themes which support this approach to organisations refer back on the one hand to the

kind of interactions between man and machine explored in ergonomic principles, and, on the other, to the extension of psycho-dynamic models from the individual to the work-group.

Action Research is a methodology which takes such psycho-dynamic elements further and explicitly seeks direct and active involvement of the researcher with the client organisation in a relationship similar to that developed in psycho-analytic therapy. Assuming that much important knowledge can be made accessible only if something is offered in exchange, the investigator enters into intense social interaction with the initiators of the research and with the subjects involved (Fester, 1972). The researcher accomodates to the client's demands that their definition and conceptualisation of the problem be recognised and legitimated. At the same time the researcher participates in the process of change and in the joint elaboration and implementation of new collaborative models. The implementation is likely to stress both work satisfaction and the promotion of small-scale democracy in the work-place.

Social Analysis is a methodology which takes us further along the psychoanalytic path, for it is based on the assumption that an organisation offers both a conscious, manifest reality and an unconscious, latent one. The former is concerned with cooperation aimed at the acheivement of specified organisational objectives, while the latter is clustered around the fantasies, desires and anxieties of mem-

bers, the tensions which they generate, and the emergence of aspects of informal organisation which might overcome these tensions. If members of the organisation are willing to allow the latent reality to be explored consciously, they are thought to be able to alert themselves to the sources of their resistance to threatening organisational changes. By this means they can free themselves from hindrances to rational cooperation. The organisational phenomena examined by such methods are therefore: the cultural mechanisms which support the social structure; the systems which defend it from anxiety; and the degrees of flexibility available to allow it to adjust to the external environment (Jaques, 1951). In this method, organisational thought not only extends the parallel between individual and corporate therapeutic relationships, but it takes over the therapeutic methodology.

Institutional Analysis approaches the study of organisational relationships by locating them within the dynamic processes which link newly emerging to established social structures, rather than reducing them to group relationships, or to relationships between individuals. One area where these dynamics are likely to be visible is the contact between the researcher and institution, the hypothesis being that the institution will reproduce itself in this analytic relationship. As an institution the organisation can be seen both as a *social structure* with an associated system of norms structuring the activities of its members, and

as a *process* of institutionalisation making sense of the actions of the individuals who contribute to it so that they are lived as a 'natural' process (Lapassade, 1974). The organisational phenomena considered initially by such an analysis are the so-called 'analyseurs', those situations, events and deviant behaviours which provoke the institution into revealing its full characteristics. As an act of criticism from outside, institutional analysis denounces the crisis of the institution and casts doubt upon the institution's capability to deal with it. For this reason, the parallel between individual and individual unconsciousness, and institution and political unconsciousness is clearly important, as are the analogies developed between organisations and prisons.

After the paradigm crisis

Having reviewed briefly these more or less established modes of organisational analysis, we may turn now to the approaches set out in Table B, approaches which have been emerging during and since the 'crisis of the dominant paradigm' to which we have already referred. We use the same categories here to review a further seven methods or approaches used for organisational analysis: organisational learning; interpretative interactionism; longitudinal analysis and life histories; organisational symbolism and corporate culture; cognitive mapping; semiotics; and the dramaturgical ap-

proach [1]. These methods are of interest either because they have recently been updated or recently transferred to the organisational field from other disciplines, or because they are wholly new. We now briefly consider each of them in turn.

Organisational Learning is a methodology which singles out that heritage of organisational knowledge developed by a cooperative system in order to carry out those tasks which lie within its competence ('theories-in-use'). A cooperative system follows regular patterns of performance based upon the mental image of required processes carried in the heads of individuals, upon the organisational norms objectified in the structure, and upon the mutual adjustent of actions within organisational groups (Argyris & Schön, 1978). These patterns are thus derived from a collective heritage of assumptions, strategies and theories in use.

They evolve according to the group's ability to recognise and to correct mistakes using unfolding events to initiate a learning process. The phenomena inherent in the continuous processes of the organisation are thus of interest to such an approach.

Theories of how to produce organisational action are developed, re-

[1] References about the resource dependence model, the population ecology, market-hierarchy approach, interorganizational networks are not included because these approaches fall within the tradition of structural-functionalist analysis.

TABLE «B» - ORGANIZATIONAL METHODOLOGIES AFTER THE PARADIGM CRISIS

Methods	Knowledge Strategy	Organizational Phenomena	Knowledge Problem	Organizational Analogy
Organizational Learning	to pin-point the process through which a cooperative system develops its own theories and actions, modifying them with time, learning to correct the action, thus producing new theories in use	organizational theory in use at time (T.1). Organizational enquiry, Organizational theory in use at time (T.2)	producing knowledge through action	with the cybernetic system; single-loop; double-loop
Interpretative Interactionism	the description of the practices and meanings which support the social structures through interaction between the participants of the organization	power; knowledge; control in everyday life	the interpretative task which supports everyday life	with social contract
Life History, Longitudinal Analysis	to reconstruct a collective memory of the ideas, associations, events, actions and prejudices which are unique for every organization	the trails which significant events leave in the memory of the members of the org., in the organizational grammar and decisional premises	the coherence between past and future events	with language as a historical process which changes continuously
Cultural Approaches, Organizational Symbolism	to realize how people interpret their reality either as single individuals or in relation with others through conscious action	language, rituals, ideologies, myths and symbolical meanings	through a symbolic interaction of the group activities which enables them to become an organized entity	with culture
Cognitive mapping	to understand how people in organizations think, pattern their experience into knowledge and utilize this knowledge to organize themselves and others	the cognitive mapping used to represent oneself, the others, the situations and org. events	how action in organization is based on thinking, meaning, knowing	with human brain
Semiotics	to read the signs which transmit organizational discourse	narrative structures, cultural artefacts	the construction of org. image through comunication and autocomunication	with the literary text
Dramatism	to reconstruct and make the subjects relive all the elements which compose social action in order to unveil the mystifications	organizational life as a theatrical performance	to favour self- reflection through de-mystification	with the theatre

quired changes are tackled as a research process, and new theories in use are constructed in order to take account of the collectively produced learning which results. The final aim of this method is to reproduce knowledge through action and to make people conscious of the processes through which they have learned. This approach is supported by the cybernetic analogy: in single loop learning, results are verified and then corrected, but to this is added double loop learning where parts of the system also learn to learn.

Interpretative Interactionism is a method of analysis with a long tradition in qualitative sociology, aiming primarily at the interpretation of phenomena rather than at their causal explanation. It is applied to daily life, or to the 'taken-for-granted-world'. Everyday interaction becomes problematic and complexities are created by the power of self-reflection (Denzin, 1983). At a given time and over time, the meanings, intentions, motives and emotions of subjects give shape to social structures. By these processes, relations are created between the members of the organisation, relations of power, knowledge and control, thus constituting the organisational reality.

Longitudinal Analysis and Life Histories are part and parcel of a methodology which privileges the historical dimension, both for individual experience and for organisational development. Individual life stories and organisational life cycles offer the researcher knowledge about the pro-

cesses of growth of organisations, about socialisation of members and about their career development. The critical events within the life of an organisation and within the life experiences of major actors signal moments of change in the constitutive rules of the organisation: that is to say, those changes which cannot be contained within existing structures and which thus lead to a new organisational grammar. The focus of the analysis is the temporal dimension which establishes a coherent connection between past and future, between individual organisation and environment (Gherardi & Strati, 1988).

Such changes manifest themselves, for some, through changes in the language (Jones, 1983), or, for others, in overcoming critical problems (Zan, 1986). The organisational phenomena examined are not, therefore, single, isolated factors but rather the trail left by organisational decisions, both in language and, finally, in the history of the organisation itself. The analogy which supports this methodology is one of language as a historical product which changes continuously over time as the community of speakers also shifts in composition.

The approach of *Corporate Culture and Organisational Symbolism* uses a methodology of analysis which sees organisations as a flow of relations between symbolic form and organisational evolution (Ouchi & Wilkins, 1985) - that is to say organisations are explored as a network of shared meanings woven and unfolded by various

groups within them. Every organisational culture has its own ethics and its distinctive characteristics which are elaborated and supported by symbolic forms: language, rituals, ideologies and myths. The superficial layers of organisational phenomena have, in fact, much deeper, symbolic meanings. Language, organisational rituals and myths are analysed to illuminate the methods by which people in organisations actively construct their reality and endow it with enduring significance (Gherardi, Strati, Turner, 1988). The analogy which supports this methodology transfers the image of culture from anthropology to the organisation.

Cognitive Mapping is a method which analyses how organisational members represent their mental reality on a cognitive level, how they organise their thoughts in recurrent mental models and scripts, how they store knowledge and how they use this knowledge to organise themselves and others (Eden, Jones, Sims, 1983). Shared cognitive maps support and favour concerted action, while dissonant cognitive schemes hinder a common comprehension of the same phenomena. Cooperation and conflict are thus considered to be deeply rooted, even at the level of mental representation. The organisational phenomena which such a methodology analyses are the ways of thinking and knowing in organisations, as these are made explicit in verbal or documentary accounts. The methodology, initially descriptive, has subsequently been developed as a system to assist decision-taking. In this cognitive model, the organisation is understood by analogy with the human brain.

The Semiotic Approach is a traditional linguistic methodology which is applied to organisational discourse in order to highlight its narrative structure and in a more general way to read visible signs, from ways of dressing or presenting oneself, through organisational style and architecture to those artifacts which speak of the identity of the organisation (Broms & Gahmberg, 1987). The hidden messages which communicate the values of the organisation to outsiders and to those within are to be revealed. The organisational phenomena analysed here are the contents of all forms of communicative codes which make up the 'thought' of the organisation, which shape its identity and legitimate its practices. The aim of the approach is to bring to the attention of the organisational actors these communicative practices which are used to formulate the organisational image, to consciously construct the mission of the organisation and to make its members accept shared values disseminated via an organisational narrative. The analogy which supports such a methodology is that between the organisation and a literary text, both offering many different possible interpretations.

The Dramaturgical Approach is a method of analysis which considers organisational action as a theatrical performance, to be interpreted according to the five elements of the pentad: act, scene, agent, motive, agency (Mangham & Overington, 1987). If one of these elements is missing or is hidden by prevailing explanations of one social action, then we have a mystification of reality. The analysis is therefore directed towards the process of demystification. It aims to locate mystifications in organisational discourse and to substitute for them convincing explanations of what is taking place. Theatrical criticism offers the tools and the parts which may be taken up alternately by the client and the researcher as they enact the events of the organisation. The main instrument for understanding the performance of roles within an organisation is to be found in critical self-reflection upon these performances. The analogy which supports both the thought and the practice is clearly that of the theatre.

Which trends in organizational analysis?

The preceding pages give some indication of the many different forms of qualitative analysis which are now being actively pursued by organisational researchers. We are all too aware of the possible distortions which we may have produced in trying to give an oversight of such a variety of complex and subtle material in such a short span. To interpret, we have, in any case to select, to abstract and to make our own intentional distortions (Schwartz and Jacobs, 1979) but the process of condensation may have further biased our accounts. Such dangers are worth courting, however, if we can gain an enhanced understanding of the nature of the development of qualitative methodologies in organisational studies today. One aspect of this understanding can be gained by reading vertically the columns of the two summary tables, for this offers the possibility of constructing a diachronic (but not evolutionarist!) interpretation of developments which have taken place before and after the paradigm crisis.

Looking first at the columns which review the *cognitive strategies* of the various methods, we can discern a shift in the position of the researcher in relation to the people and organisations which form the focus of the research: a movement *from a position as detached observer to one demanding more personal involvement*. A detached observer who studies an unknown system searches for the laws governing that system independently of the people who are caught up within it. It will be seen as desirable to take care that observations do not disturb the phenomena under observation, or interfere with attempts to produce objective knowledge which is independent of the researcher and of general and universal value. Such a researcher is one who does not take account of the so-called Heisenberg effect (Ciborra, 1978; Schwartz and Jacobs, 1979) which asserts that in social systems, as in sub-atomic physics, the

act of observing is likely to change the phenomena under observation; or one who chooses to study those kinds of organisational phenomena which appear to make it possible to keep the resultant modification to a minimum.

At the other extreme, we find the researcher who deliberately gives up the role of detached observer. Recognising that the patterns observed will be changed anyway, the involved researcher accepts the need to play a part within a form of research which is also inescapably a process of social interaction. Such an investigator must be reconciled to being at the same time the subject and the object of study, not least because the ability to read the reality 'out there' depends on 'assumptions' about that reality, upon the researcher's own cognitive maps. Although this post-Heisenberg researcher will examine organisations from within in order to get to know them as empirical reality, in the end they will remain a kind of theoretical reality. This research activity is thus explicitly aimed at generating a kind of knowledge which will modify the knowing subject. At the limit, the change itself constitutes the research strategy.

If we now look at the second column which illustrates the organisational phenomena which constitute the central foci of the various research processes, then we notice a slow shifting of the research topic: *from facts to meanings*. The processes through which, and the manner in which actors attribute meanings to events and situations take precedence over structures. Closely connected to this shift in concern is the knowledge problem with which the researcher engages. A movement can here be identified *from nomothetic to idiographic types of data*. In the first case the knowledge problem revolves around organisational themes which in principle interest all organisations, around the search for generalised solutions which might be applicable to many situations. In the second case the problem is more context-specific, and more likely to be tied to one organisation in so far as the particular cluster of problem elements under review is regarded as unique. Explaining activity then gives way to understanding it, and, at the same time the organisation becomes less a 'thing', a product exterior to individual human beings and more an activity of organising, a trace left by the relations between people and their social and material environment.

Finally, if we consider the last columns in our tables, those which catalogue the analogies used, we may see a movement from analogies which are predominantly mechanistic, which see an organisation as a machine, to predominantly social analogies, considering organisations as some kind of social product. With this shift we become accustomed to the possibility of regarding organisations from a variety of points of view, and realising that the complex phenomenon which is an organisation can only be grasped by recognising its many levels of operation. It could thus be argued that the concept of organisation has moved *from a single level of reality to one which is many-sided and multiplex.*

If we try to summarise aspects of the methodological trends which we have been reviewing as we have considered both well-established and newly emerging methods of organisational analysis, we may suggest, at a rather abstract level, the identification of three tendencies, an identification which has some similarities to diagnosis by Morgan (1983) and Silverman (1985) to which we have already referred. These are:

a. *A tendency towards the acceptance of diversity.*
   The various levels at which reality may be read, the plurality of realities within the same phenomenon, and the emergence of conflicting interpretations all encourage us to use a diversity of methodological approaches in a positive and a complementary manner.
   (Morgan, 1983)

b. *A tendency towards the acceptance of uncertainty.*
   The problematic nature of social phenomena and the limitations on our capacity to know mean that uncertainty must become one of our ontological presuppositions. As a result, our ways of knowing must inevitably become provisional rather than foundational.
   (Gherardi, 1985).

c. *A tendency towards accepting incompleteness.*
   Since we can regard organisations as machines, as organisms, as prisons, as texts, we are forced to recognise that these possibilities depend upon the particular nature of the interaction between ourselves as researchers and our object of knowledge. One of the contributions of the researcher can then be seen to make real some of the possibilities for knowledge. But, by the same token, some other possibilities must remain unrealised.
   (Hofstadter, 1979, Morgan, 1983)

Such tendencies do not, of course, go unchallenged. Organisations which sponsor, promote and direct research have strong reasons for assuming and asserting that their own paradigm is the dominant one. Since they are typically organised in a bureaucratic fashion, they tend to want to translate research into their own bureaucratic terms, and to consider research activities as assemblages of decision premises, office procedures, daily routines and official structures. Any widely disseminated change in the understanding of the basic procedures of research could endanger the legitimacy of such research-directing institutions.

It should also be recognised that, discredited though positivism is in many intellectual circles, within many of our institutions, it is not merely recognised as one of a variety of possible ways of producing knowledge, but it is still identified with knowledge itself. In consequence, anything not based on detached observation using systematic and replicable observations and measurement is rejected: the 'hard/soft' stereotypes with which we began our discussion still persist strongly in such milieux. The situation may be improved to some degree by the development of alternative criteria for the assessment of the quality of

non-positivistic forms of knowledge (Lincoln & Guba, 1987).

Finally, we nead to acknowledge that the social context within which research is carried out is also and always a political context. Accordingly the criteria for the affirmation and acceptance of a method are not necessarily tied to its heuristic capacities, but may lie elsewhere.

Conclusion

The study of organisations and the behaviour of those within them is important in understanding many of the key institutions of modern societies: business, the armed forces, civil service administrations and so on. Clearly, organisational studies deal with serious matters. We began by asking why those engaged in such self-evidently serious business should be bothering themselves with qualitative inquiries and gathering non-standardised, unmeasured data. Why, in terms of the stereotypes with which we started, was such a 'hard' topic being infiltrated by 'soft' methods?

Our answer, in essence, is that this question, if taken at face value, is a foolish one. It takes an oversimple view of the nature of scientific and of social scientific investigations, it embraces the orderliness of texts on methods in preference to the surprising quirkiness of the world outside the textbooks, it neglects the substratum of tacit methods, perceptions, skills and expectations which pervade all in-

vestigations, however systematically organised, and it overvalues the importance of number and measurement for their own sakes, whilst undervaluing a through understanding of the nature of number and of those mathematical characteristics which sometimes make valuable numerical assessments possible. These obscurities and oversimplifications become blended with the practical interests of established institutions, with a nostalgia for the promise of positivism and with a dichotomous view of the world which makes it all too easy to characterise such complex matters in terms of sexist stereotypes.

But behind this foolish question lies a more sensible one: the reasons for an increasing interest in qualitative methods are many, but they include: a recognition that work of quality in the past has commonly drawn upon quantitative and qualitative approaches as appropriate; a disappointment with the results which are to be obtained from approaches which seek large-scale generalisations drawn from investigations underpinned by the uncritical acceptance of conventional modes of investigation; and a growth in self-awareness about such matters in the organisational arena. As criticisms of existing orthodoxies have surfaced, existing qualitative methodologies and other broader alternative approaches have been re-evaluated and new approaches have been imported. As a result, we now have a wider variety of potential methods for approaching the investigation of orga-

nisations and for the exploration of problems which occur within them. This variety is prompted by and feeds upon a growing recognition of the complexities of behaviour in organisations - social life displays a remarkable subtlety wherever we examine it, even in organisational settings. Consequently any of the array of methods, approaches and modes of understanding used in the humanities or the social sciences can be used to illuminate some aspect of organisational behaviour. And, in recognition of this complexity, we are coming to understand that in organisational studies, as in our other life activities, we have to learn to live with diversity, uncertainty and incompleteness.

We can see, therefore, in our review of the field, how the divisions created by criticisms of the dominant paradigm have opened the way to a plurality of methods. Although these are not yet consolidated and though some of them still seek to acquire legitimacy, they do appear to have some fundamental characteristics in common. In stating this, however, we would not wish to suggest that they constitute a prelude to a new paradigm: we would rather see them as signalling the acceptance of a plurality of possible modes of analysis, all potentially valid, choice between them often being determinable on pragmatic grounds. A plurality of different disciplinary forms of knowledge converge in organisational studies, and this cross-disciplinary background may help to encourage the individual

researcher «to admit that his her own paradigm does not possess a monopoly of truth ... and to adopt an existential approach to research» (Gagliardi, 1986). The awareness of being able to choose among several approaches or methods of analysis according to the particular problem being faced may mean that we have to accept that we are becoming 'epistemological chameleons' (Martin, 1983) although our ability to change at will cannot extend to an adjustment of our most deeply held paradigmatic assumptions.

Nonetheless, we now realise that, just as 'hard' and 'soft', or 'qualitative' and 'quantitative' are not inalienable oppositions, so we do not need to choose once and for all between the various methods of organisational analysis. For many tasks, it may be beneficial to use several methods or approaches at the same time, though we should be wary of claims that such multiple activities actually produce a synthesis of views, or even a translation of one paradigm into another (Bednarz, 1987).

At the level of methods, there is little fundamental difficulty in taking up Denzin's (1978) call for 'triangulation' - «the combination of methodologies in the study of the same phenomenon», and this suggestion has been adopted in the study of organisations (Jick, 1979). The triangulation metaphor comes from the world of cartography and navigation and refers to the three sets of measurements from different points which are necessary to establish a new point or to take a

bearing. When three different perspectives on a given organisational phenomenon are reconcilable or translatable, the method of triangulation can be used. But the approaches set out in Tables A and B are not all mutually compatible, so that it remains open to question whether any coherent account of aspects of an organisation could be produced by trying to adopt several of these different perspectives at once: because of the efforts which are now being made to absorb all qualitative modes of analysis into the still powerful and positivistically influenced world view, it is important to realise that there are limits to such syncretism.

We can, however, use the general characteristics discussed above to locate approaches to organisations within one kind of unifying property-space, if we consider modes of inquiry according to their location with regard to the following three characteristics:

1. The degree of distance which the observer attempts to maintain from the field of observation;

2. The extent to which the analysis concerns itself with facts as against meanings;

3. The accessibility of the given problem to nomothetic as against idiographic modes of knowledge acquisition.

We recognise that our two Tables must contain elements of caricature, and we would not wish to be understood as urging the rejection of the approaches set out in the first in favour of those set out in the second. Our division has a strong element of convenience about it, as we have already acknowledged, with the recent discussions of crisis offering a useful conventional marker. While acknowledging that at the deepest level, paradigms cannot be selected, we would want to argue, at levels above this epistemological base, for a recognition of the complexity of the subject matter of organisational studies and an acceptance of the usefulness of the growing variety of modes of examining and understanding this complexity. Hegemony in inquiry eliminates choice, whilst the «exploration of assumptions involves the exploration of choice».

References

AGAR M., 1986, *Speaking of Ethnography*, Qualitative Method Series, 2, 1985, Beverly Hill, CA: Sage.

ARGYRIS C. & SCHÖN D., 1978, *Organizational learning*, Mass.: Addison Wesley.

BALDAMUS W., 1976, *Inference and Substance in Sociology*, London: Martin Robertson.

BARZUN J. & GRAFF H.F., 1977, *The Modern Researcher*, 3rd ed., New York: Harcourt Brace Jovanovich.

BASOUX J., 1987, «Women's contribution and the new vogue», *Times Higher Educational Supp.*

BATESON G., 1972, *Steps to an ecology of mind*, San Francisco: Chandler.

BELL C. & NEWBY R.,

BELL C. & ROBERTS H., eds., 1984, *Social Researching: Politics, Problems, Practice*, London: Routledge, Kegan Paul.

BROMS H., GAHMBERG H., 1987, *Semiotics of management*, Helsinki: Helsinki School of Economics Pubblications.

BRYMAN A., ed., 1988, 'Connoisseurship in the study of organisational culture', *Doing Research in Organisations*, London: Routledge, Kegan Paul.

BURGESS R.G., ed., 1982, *Frield Research*, London: Allen and Unwin.

BURGESS R., 1984, *In the Field*, London: Allen and Unwin.

BURKE K., 1969, *A grammar of motives*, Berkeley: University of California Press.

CASTANEDA C., 1973, *Journey to Ixtlan: the lessons of Don Juan*, London: Bodley Head.

CIBORRA C., 'Metodi di analisi organizzativa', Bontadini P., *Manuale di organizzazione*, Milano: Isedi.

COLEMAN J.S., 1965, *Introduction to Mathematical Sociology*, New York: Basic Books.

DAS H., 1984, 'Portmanteau Ideas for organisational theorising', *Organisation Studies*, 5 (3), 261-8 and discussion by Turner, Mrela & Warriner, 269-276.

DENZIN N.K., 1978, *The research act*, New York: McGraw Hill.

DENZIN N., 1983, «Interpretative interactionism, Morgan G. (ed.), *Beyond Method*, London: Sage.

EDEN C., JONES S., SIMS D., 1983, *Messing about in problems*. Elmsford: Pergamon.

ELBOW P., 1981, *Writing with Power*, New York: Oxford Univ. Press.

FEYERABEND P., 1975, *Against Method: outline of an anarchistic theory of knowledge*, London: N.L.B..

FOSTER M., 1972, 'An introduction to the theory and practice of action research in work organization', *Human Relations*, 25 (6), 529-556.

FUNTOWICZ S.O. & RAVETZ J.R., 1986, 'Policy-related research: a notational scheme for the espression of quantitative technical information', *J. Operational Research Soc.,* 37 (3), 243-247.

GAGLIARDI P., 1986, *Le imprese come culture,* Milano: Isedi.

GHERARDI S., 1985, *Sociologia delle decisioni organizzative,* Bologna: Il Mulino.

GHERARDI S. & STRATI A., 1988 (forthcoming), The temporal dimension in organizational studies, *Organization Studies,* (9), 2.

GHERARDI S., STRATI A., TURBER B., 1988 (forthcoming), «Industrial democracy and organizational symbolism», *Handbook of Participation in Organization,* (1).

GHERARDI S., STRATI A., TURNER B., 1988 (forthcoming), 'Organizational symbolism and industrial democracy'.

GLASER B., 1978, *Theoretical Sensitivity: Advances in the methodology of grounded theory,* Mill Valley, CA: Sociology Press.

GLASER B. & STRAUSS A., 1967, *The Discovery of Grounded Theory,* New York: Aldine.

GINZBURG C., 1979, 'Clues: roots of a scientific paradigm', *Theory and Society,* 7 (3), 273-288.

GOFFMAN I., 1975, *Frame analysis,* Harmondsworth: Penguin Book.

HAGE J., 1972, *Techniques and Problems of Theory Construction in Sociology,* New York: Wiley.

HAMMOND P., ed., 1964, *Sociologists at Work,* New York: Basic Books.

HEJZLAR J., 1987, *Chinese Watercolours,* London: Galley Press.

HERBST P.G., 1974, *Socio-technical design: strategies in multidisciplinary research,* London: Tavistock.

HOFSTADTER D.R., 1979, *Gödel, Escher, Bach: an eternal golden braid,* New York: Basic Book.

IRVINE J., MILES I. & EVANS J., eds., 1979, *Demystifying Social Statistics,* London: Pluto Press.

JAQUES E., 1951, *The changing culture of a factory,* London: Tavistock.

JICK T.D., 1979, 'Mixing qualitative and quantitative methods: triangulation in action', *Administrative Science Quarterly,* 24.

JONES G., 1983, «Life History methodology», Morgan G. (ed.), *Beyond Method,* London: Sage.

KAPFERER B., 1979, 'Ritual process and the transformation of context', *Social Analysis,* 1 (Feb), 3-19.

LAPASSADE G., 1974, *Groupes, organisations et institutions,* Paris: Gauthier-Villars.

LAWRENCE P. & LORSCH J., 1967, *Organization and environment,* Boston: Harvard University.

LINCOLN Y. & GUBA E.G., 1985, *Naturalistic Inquiry,* Beverly Hill: Sage.

LOWER A., 1977, 'Facts and frameworks: aspects of the research process', Unpublished MA thesis, University of Birmingham.

McGREGOR D., 1967, *The Professional Manager,* New York: McGraw Hill.

MANGHAM I. & OVERINGTON M., 1987, *Organizations as Theatre,* Chichister: Wiley.

MARTIN J. & POWERS M.E., 1983, 'Truth or corporate propaganda: the value of a good war story', Pondy L. et al. (eds.) *Organizational Symbolism,* Greenwich: Jay Press.

MARTIN P. & TURNER B.A., 1987, 'Grounded theory and organisational research', *J. Applied Behavioral Science,* 22 (2), 141-157.

MILES M.B. & HUBERMAN A.M., 1984, *Qualitative Data Analysis,* Beverly Hill: Sage.

MORGAN G., 1983, *Beyond method,* Beverly Hill: Sage.

MULLINS C.J., 1977, *A Guide to Writing and Publishing in the Social and Behavioural Sciences,* New York: Wiley.

NAIPAUL, V.S., 1987, 'The enigman of arrival', *New Yorker,* 11 (Aug.) 26-62.

OUCHI W.C. & WILKINS A., 1985, 'Organizational Culture', *Annual Review of Sociology.*

PARSONS T., 1959, *The social system,* Glencoe Ill.: Free Press.

PETERS T. & WATERMAN R., 1982, *In search of excellence,* New York: Harper & Row.

PLEDGE H.T., 1939, *Science since 1500,* London: H.M.S.O..

POLANYI M., 1959, *Personal Knowledge: towards a post-critical philosphy,* London: Routledge, Kegan Paul.

RAVETZ J.R., 1971, *Scientific Knowledge and its Social Problems,* Oxford: Clarendon Press.

RAVETZ J.R. & FUNTOWICZ S., 1986, See Funtowicz.

REASON P. & ROWEN J., 1981, *Human Inquiry: a sourcebook of new paradigm research,* Chichester: Wiley.

ROSENBERG M., 1968, *The Logic of Survey Analysis,* New York: Basic Books.

SELYE H., 1964, *From Dream to Discovery: on being a scientist,* New York: McGraw Hill.

SCHWARTZ H. & JACOBS J., 1974, *Qualitative sociology,* New York, The Free Press.

SILVERMAN D., 1975, *Reading Castanada,* London: Routledge, Kegan Paul.

SILVERMAN D., 1985, *Qualitative Methodology and Sociology,* Aldershot: Gower.

SUTTLES G.D., 1978, *The Social Order of the Slum: ethnicity and territory in the inner city,* Chicago: Univ. of Chicago Press.

STRAUSS A., 1987, *Qualitative Analysis for Social Scientists,* Cambridge: C.U.P.

STRUNK W. & WHITE E.B., 1979, *The elements of Style,* 3rd ed., New York: Collier Macmillan.

TURNER B.A., 1977, 'Research note: a comment on the nature of information in channels of observation', *Cybernetica* XX (1), 39-42.

TURNER B.A., 1978, *Man Made Disasters,* London: Wykeham Press.

TURNER B.A., 1981, 'Some practical aspects of qualitative data analysis: one way of organising some of the cognitive processes associated with the generation of grounded theory', *Quality and Quantity,* 15, 225-247.

TURNER B.A., 1987, 'Grounded theory and knowledge elicitation', Seminar presented at the Department of Civil Engineering, Bristol University, November, 1987.

TURNER B.A., 1988, 'Connoisseurship in the study of organisational culture', to appear in A. Bryman, ed., *op. cit.*

WILLER D.E. & WILLER J., 1973, *Pseudo-empiricism: a critique of a pseudo-science,* Englewood Cliffs, N.J.: Preston-Hall.

WITKIN R.W., 1971, *The Intelligence of Feeling,* London: Heinemann.

ZAN S., 1986, «L'analisi longitudinale delle organizzazioni», *Rivista trimestrale di scienza dell'Amministrazione,* 1.

ZETTERBERG H., 1954, *On theory and verification in Sociology,* Stockholm: Almqvist & Wisksell.

ZEY-FERREL M. & AIKEN M., 1981, *Complex organizations: critical perspectives,* Glenview: Scott, Foresman and Company.