

Discovery and dialectics: Gerhard Kleining's methodology of qualitative research

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[\[Biographical information\]](#)

See [this page](#) and [this one](#) for more material (in German) on the approaches pioneered by Gerhard Kleining.

"[N]either Method nor Theory alone can be taken as part of the actual work of the social studies. In fact, both are often just the opposite: they are statesmanlike withdrawals from the problems of social science. Usually, we have seen, they are based on some grand model of inquiry with which other people are beat on the head. That this grand model is not capable of altogether full use is not, perhaps, too important, for it may still be used ritualistically."

(Mills 1970 (originally 1959): 136).

This paper aims to present the Hamburg sociologist Gerhard Kleining's perspective on the methodology of qualitative social research to an English-speaking audience. In a context where methodological discussion seems to have become more or less concentrated on specific fields there may be some value in opening up otherwise neglected issues and in presenting unfamiliar perspectives on familiar problems, even if that means seeming a bit naïf. By exploring Kleining's central argument, that direct discovery (and not simply interpretation) of social reality can be made possible by a strategy of openness (rather than closure) in research, I want to examine some of the destinations that we might reach by following alternative paths [\(1\)](#).

In this paper I will try to bracket as far as possible the question of whether Kleining is right in his arguments. This is partly a "post-methodological" move: I think that some of the issues raised by the question are ultimately unresolvable ones, at least within the social sciences. The argument between materialist and idealist approaches is a case in point: we can cite philosophers and psychologists to support our own points of view, but it is not clear to me that as social scientists we can resolve these issues (if in fact a resolution is possible). Similarly, the argument between relational conceptions of humanity and what has become known as "methodological individualism" (2) (Elster 1985: 5-8) may not be open to a genuine resolution except insofar as so many qualifiers are built in to the models as to render them indistinguishable in practice. The argument between rational and explicitly non-rational modes of thought is by definition not one which can be resolved through discussion (3). Lastly, I think it may be impossible to devise a method to demonstrate the validity of any statement about how human beings investigate the validity of statements about themselves. "Human beings", says Marx, "have to demonstrate the truth, i.e. the reality and power, the this-worldliness of their thought, in practice" (Marx n.d.: 113), and this is as far as I want to take the matter of ultimate validity.

My aim in this paper, then, will be simply to demonstrate the interest of Kleining's approach; I will first give an account of it in its own terms. Following this, I will attempt to isolate some important theoretical points about the approach, particularly in relation to a Marxist tradition. Lastly, I will try to identify some useful insights into problems of practical research which are offered or opened up by this approach.

I. Social research as discovery: the methodology proper

Kleining's approach derives from four main sources. The first is a particular German philosophical and sociological tradition associated with Marx and Marxism, for which dialectic modes of thought, rationalist argument and the criticism of appearances are central points of reference. The second is the

sociological tradition of qualitative research, in particular the work of the Chicago School and of Glaser and Strauss (1967). The third appears to relate to Gestalt psychology, Piaget's study of cognitive development and psychological methods of qualitative research. The last is a rereading of the methodological writings of the turn-of-the century physicists: in particular, Kleining argues that Ernst Mach should be reclaimed from the positivists (see Kleining 1986: 730 for an appreciation of Mach's importance). It is also important to be aware of the relative isolation of qualitative research in contemporary German sociology; this has advantages (as well as drawbacks), most notably in weakening the pressures which may be exerted by dominant approaches to understanding and practicing qualitative methods.

Kleining takes as his central methodological question the issue of how we, as human beings, find out about the (natural, social, constructed) world we live in. His answer, derived inter alia from Piaget's cognitive psychology, is that we observe and interact with the world and thus come to a better understanding of the organisation of reality. Hence, rather than a rigid and immobile dichotomy between subject and object, the two are placed in a dialectic relationship: the individual is discovering the world, not simply interpreting it from an immobile standpoint; and "the world" is itself also an active partner in this relationship. Kleining argues that this view of human cognitive development does not leave any room for a concept of scientific research derived elsewhere than from those methods which are open to ordinary individuals trying to make sense of the everyday world: "The social scientific methods for the knowledge (Erkenntnis) of the environment grow out of everyday methods ... The everyday methods are the reservoir for all social scientific methods. They are developed from them by exclusion, by separation from their everyday context, [and] by abstraction." (1982: 225). In other words, scientific methods represent a particular selection of everyday methods and a particular approach to using these methods: in Kleining's approach, the difference between scientific and everyday methods is precisely the systematic concern for openness to experience and the systematic attempt to understand that reality [\(4\)](#). Kleining thus proceeds from a descriptive methodology - an account of what methods of discovery are - to a normative

one in which methods are distilled out of the everyday methods to serve a particular purpose - furthering the processes of openness and discovery rather than closure and interpretation. (5) This strategy does not rest on a claim that all human thought follows strategies of openness, but rather on the claim that processes of learning or discovery do so; effective research methodology is then a matter of development from the everyday strategies of openness rather than from those of closure, so that we are driven to discover more about the social world rather than to confirm our own previously-held views of it.

Any discovery of the social world, then, scientific or otherwise, is a matter of increasingly good approximations of the structure of our own understanding to the structure of social reality: alternatively, we could say that any understanding is a better or worse approximation, a more or less partial perspective on reality. In other words, our concepts and theories, as well as our own practical experience, are the starting point in a way that they are not, for example, in Glaser and Strauss's (1967) "grounded theory"; we interact with the world and in so doing learn about it. Hence strategies for maximising openness are fundamental.

The rules of qualitative research

Kleining offers four rules for a scientific and qualitative process of approaching understanding to reality, which aim at maximising the effectiveness of the process and avoiding premature closure. Each rule relates to one of the elements of the more general process, which is seen as a "question and answer" relationship between subject (the individual) and object (external reality): the subject acts (observes, experiments with) the object and evaluates the information thus gained, but the object will itself (if it is social) act and observe in its turn, and may also (if it is given the chance to do so) offer unexpected information of various kinds, which can in turn lead to new questions. This is close to the concept of experience elaborated by E.P. Thompson in *The Poverty of Theory*:

"[E]vidence does not stand compliantly like a table for interrogation: it stirs, in the medium of time, before our eyes. These stirrings, these events, if they are within 'social being' seem often to impinge upon, thrust into, break against, existent social consciousness. They propose new problems, and, above all, they continually give rise to experience - a category which, however imperfect it may be, is indispensable to the historian, since it comprises the mental and emotional response, whether of an individual or of a social group, to many inter-related events or to many repetitions of the same kind of event ...

[C]hanges take place within social being, which give rise to changed experience: and this experience is determining, in the sense that it exerts pressures upon existent social consciousness, proposes new questions, and affords much of the material which the more elaborated intellectual exercises are about ... " (Thompson 1978: 7-8).

In other words, the "dialogue between social being and social consciousness" goes on in social research as much as in everyday life. This dialogue has unmistakable overtones in terms of the power relationships between researcher and research subject: "a qualitative dialogue is not one of authoritarian criticism (authoritativ-kritizistisch), but an egalitarian one." (Kleining 1986: 734). It may also be worth distinguishing this experience from interpretation or construction of reality in the usual sense:

"Through interpretative social research the social appears as being in movement. Since [this research] reconstructs meaning which is produced by actors and actions, it also comprises changes. These are, however, limited to the actors and their modes of action; they show changes in the individual, concrete social field, and not in the macro-social area except insofar as meaning can be ascribed to it. Economic dynamics, for example, cannot be grasped by this approach." (Kleining 1988: 247).

The social researcher explicitly sets out to be open to experience, to change their understanding of their field of research towards a greater approximation of the reality; in other words, they engage in a "searching process" (Suchverfahren). As the subject acts on and evaluates social reality, they learn things about it because of the nature of the relationship between the researcher and the researched: "All areas [of this relationship] stand in a close

relationship to each other, they determine each other and are dependent on each other, through action and reaction, activity and effect, generation and processing of information, even though the process of searching naturally proceeds from the subject and the object is found." (Kleining 1982: 231). The rules, then, are as follows:

Rule 1. (refers to subject / researcher)

"Prior understandings of the phenomenon to be researched should be seen as provisional and should be transcended with [the discovery of] new information with which they are not consistent." (Kleining 1982: 231)

This rule implies that the researcher should be more open to information related to or generated by the object of research than to their own opinions about it. This is obviously easier to say than to do; means for ensuring the researcher's readiness to accept new information include the refusal to give an initial definition of the point of view from which one will view the object and avoiding taking up positions which are already fully articulated in the literature. This does not, however, imply that the researcher can be a "clean sheet"; rather that the traditional academic path of "reading the literature" is only useful to the extent that "the study of the literature takes in positions whose differences are as extreme as possible, hence encouraging the uncertainty of the researcher" (Kleining 1982: 232). A further point is the importance of openness in the tools of research themselves: the methods used should be such as to enable the discovery of unexpected information and information which points to the limitations of the focus of research.

Rule 2. (refers to the object of study)

"The object is provisional; it is only fully known after the successful completion of the process of discovery." (Kleining 1982: 233)

The definition of the object of the research is itself subject to change; the question of the boundaries and structural unity of the phenomenon is thus itself one of the issues of research. The researcher's understanding, initially, should be as vague or as provisional as possible, to encourage an openness

to new information leading to the recognition of structural elements of the object of research. The "unity" of the object can itself change in a number of ways during research; indeed it may change to the extent of appearing dissolved into a wider context. "As the structure of an object is discovered, the boundaries of the structure are also determined, the 'system' appears" (Kleining 1982: 223). The final definition of the object of research, in other words, is only reached at the end of the research itself.

Rule 3. (refers to action in relation to the subject of research, hence to data collection)

"The object should be approached from "all" sides; rule of the maximum variation of perspectives." (Kleining 1982: 234)

The object cannot genuinely be viewed from all sides because it is not yet fully known; hence the important point is to gather the greatest possible variety of information by varying any factor in the collection and analysis of data which it is thought may make a difference to the ultimate result. Two particularly important forms of variation are those of method and of time (possibly also of space or culture). In other words, one should always use a variety of methods and always look for change over time. This variety also applies to the aspects of the object which are isolated for study; the qualitative sample in this context is either a total sample or, more commonly, an "extreme sample" consisting of the most radically differing aspects of the object of study. (Importantly, everyday experience, the ordinary, is itself treated as an extreme case.) "The sampling strategy is that of extreme group sampling on the basis of structural relevance ... [This] does not simply require that unusual, outlying, 'extreme' situations be studied, but also that specificity, what is characteristic for the object, connected to it in some way, should be discovered and brought into [the research]" (Kleining 1986: 734).

Rule 4. (refers to the evaluation of information gathered, hence to data analysis)

"Analysis of the data for common elements." (Kleining 1982: 237)

The structure of the object of research is found by the discovery of common elements (Gemeinsamkeiten) or of structuring dichotomies. There are two important moves here: firstly, the research focus is shifted from an examination of differences within the object of research to a focus on the common elements which make it possible to speak of the object of research as forming a whole or as being a single phenomenon or process. Thus what is being asked here is what are the defining structural elements of the object of study; these are discovered at the end of the research process rather than defined by researcher's fiat at the start of the process. Secondly, common elements are understood to include opposition, contradiction and negation; "oppositions make particularly visible the dimensions from which they draw their opposition" (Kleining 1982: 239). In less philosophical language, we might say that dichotomies, contradictions and oppositions can themselves be structuring elements. I will try to show what this means in practice in a discussion of Kleining's method of textual analysis later. What may be worth pointing out here is the central importance ascribed to data analysis in this perspective, an issue which is often in practice treated as secondary to problems of data collection.

Research methods

This methodology is not simply compatible with a large variety of methods, but in fact treats the use of a number of differing approaches as a requirement for effective research (see 3 above). It also opens up two specific possibilities in terms of methods.

The qualitative experiment

The qualitative experiment is made possible by Kleining's analysis of scientific methods as derived from those of everyday life and as based on the interaction of the researcher with the object of research: while both everyday methods and quantitative ones leave place for more active and more passive means of interaction as a means to discovery, there is relatively little appreciation of the place of active methods - stressing experiment as opposed

to observation - in qualitative research [\(6\)](#). The qualitative experiment is defined as "the intervention with relation to a (social) subject which is taken following scientific rules and towards the exploration (Erforschung) of its structure" (Kleining 1986: 724) [\(7\)](#). Kleining mentions three strategies within the experiment:

- **Maximising and minimising:** this relates to the selection of extremes as subjects of study. Since "extremes are of course extremes 'of something', [they are] structurally characterised relationships" (Kleining 1986: 735), the opposite poles of these relationships are structurally relevant. For example, one could attempt to find out how the researcher can have a maximum of effect on the context being studied with a minimum of effort - and in what contexts a maximum of effort will bring a minimum of effect. Either a single relationship or a number of relationships can be "extremised" at a time, depending on the level of knowledge and hence on the competence of the researcher.
- **Testing limits:** "Qualitative methods aim at determining the limits of the object, the areas in which structure becomes arbitrariness, figure becomes background, what is meant becomes what is not meant, influence becomes lack of effect, meaning becomes nonsense." (Kleining 1986: 735). Experimentation here involves changes that make boundaries visible or bring about ambiguity. Paradoxes are a special case of this: "borderline cases where contradictions present themselves so that their contradictory nature becomes visible" (Kleining 1986: 736).
- **Adaptation:** This involves developing a multiplicity of techniques which are relevant to the subject of study, keeping these techniques flexible, and (in Kleining's archaeological metaphor) protecting the structure which is being studied.

Natural experiments

Beyond these forms of experimentation, Kleining also mentions two forms of "natural experiments": the thought experiment and the ex-post-facto experiment.

In relation to the thought experiment, Kleining mentions its use by Mach, Bohr, Einstein, Heisenberg and Weizsäcker. He argues that the same problems of disentangling interpretation and reality hold in thought as in our

experience of the world: "[mental objects] (gedachte Gegenstände) are also the transformed (verarbeitete) external world: representation, lived, experienced, known reality." (1986: 742) Reflection involves both observation of and changes in our thinking. The same rules of openness, provisionality, variation of perspectives and analysis for common elements hold. The thought experiment could then perhaps form one potential part of a research process, one way of approaching our articulated understanding to our actual experience of the subject of research.

The ex-post-facto experiment consists in examining a process which has already finished as if it had been planned in advance: this is of course made possible by the absence of the need to "hold all other factors constant" and so on. Kleining points out that ex-post-facto methods are natural in that they are "quite normal contemplations and absolutely necessary for the understanding of everyday processes: what happened, how and why?" There are obvious links here with historical approaches: "ex-post-facto experiments thematise specifically historical change" (Kleining 1986: 744).

In both cases, in other words, one is taking what are very often "everyday methods" used by social scientists when thinking about things: imagining how they could change or what the results of particular changes would be, and examining once-off processes for information; but in a number of contexts they are still used unproblematically in their everyday form. Part at least of what Kleining is saying here is that we can take reflections which in any case are part of our thinking as social researchers and make them more explicit, practice them in more rigorous, more "scientific" ways, and so on - rather than leaving them at the level of "common-sense" and taken-for-granted ways of thinking.

Documentary analysis

I will now attempt to show something of the implications both of the general methodology and of this experimental approach in the context of documentary analysis.

The issue of data analysis as opposed to data collection appears to be somewhat under-theorised in the literature. Kleining offers a model of analysis

which is both qualitative, discovery-oriented and systematic. In particular, this general method is offered in a specialised form as a method of documentary analysis. I will mention the main points of the procedure for documentary analysis; the more general approach to data analysis is an extended and abstracted version of the same approach (see 4 above).

There are two stages in the process: in the first stage, either all the information gathered, or an "extreme sample", is analysed for characteristic elements and grouped on the basis of similarity or of difference. Importantly, this grouping is not on the basis of a priori categories, but in terms of responses to "questions" which are asked of the documents and "experiments" which are performed on the documents. The kinds of questions and experiments which are used should be as varied as possible but also as open as possible to unexpected information. What follows are examples taken from Kleining's analyses of Helmut Kohl's New Year speeches and of a chain letter (Kleining 1990a, 1990c).

Some examples of appropriate questions and possible answers to the text are:

(Kohl speeches):

In what kinds of contexts does the speaker use "I" and where does he use "we"?

"I" is used in contexts which stress the similarity with the listeners - "The speaker is one of all of us" (1990a: 5) -, "we" is used to underline this unity; whenever possible "I" is transformed into "we" (8).

(Chain letter):

What kinds of things am I expected to do?

Do like the others (like yourself) who have sent on the chain letter; wait for happiness to arrive.

What is said about the "chain"?

I am part of a happy community spanning the globe, linked by irrationality and religion.

Some examples of appropriate experiments with the document are:

(Kohl speeches):

What happens if I reduce the text to its logical semantic content?

It shrinks to about a third of its original size without any loss of substance; the redundancies in the original text become visible, as does its more trivial character.

(Chain letter):

What happens if I substitute different numbers?

Translating hours into days gives a vaguer and less urgent deadline (4 days vs. 96 hours).

Substituting the values gives an appearance of rational causality (20 copies = 20 million marks instead of 20 copies = 9 million marks).

(General):

What happens if I change the order of different parts; what makes most difference, what makes least difference?

What happens if I change all the actors, if I change the linguistic register, if I change the form?

This process is repeated along different dimensions, following different lines of questioning, until no new elements are being discovered. In the second stage, the groups are themselves grouped on the basis of their own common elements or shared oppositions; this may require a transformation of the original groups and hence a return to the first stage. If it is likely to affect the result, this analysis should be performed by more than one researcher or participant. At the endpoint, "we have discovered the common elements in the multiplicity of forms of appearance and the fragments of data. In this way we have discovered the sought-after structure of the object." The operative rule is that all the data must be shown to be structurally related; this can also be expressed as the rule that no data may contradict the analysis (9). An important feature of this approach is the ever-present possibility of having to return to the starting-point if the analysis is flawed, rather than writing off anomalous information as irrelevant. In relation to the general problems of data analysis, the possibility of generalising this method to any type of data is important; it means that we can avoid the often-present pressure to inflate the significance of printed documents and the representativity of more articulated or intellectual documents, as well as the pressure towards "texts" of particular kinds and the sorts of assumptions that often accompany this as opposed to documents of other kinds. Kleining comments:

"Today we have to stress that both whole texts, content and form, as well as all kinds of texts are subjects of social scientific research, because they are created and used socially, in other words [they] are social products: everyday

texts, diaries, everyday stories, documents of every kind, scientific texts, trivial literature and also literary works of art ..." (1986: 740)

II Theoretical affinities and implications

I have attempted to show something of the importance of discovery and dialectics as categories within Kleining's work; before discussing the practical results of the methodology, I want to examine the intermediate categories of structure and experience, which are, I think, fundamental points of juncture in this kind of thought as well as central to everyday research. In this section I will use E.P. Thompson's (1978) discussion of these issues to illuminate some of the points in Kleining's argument.

The concept of structure

I have already said that there may be issues in methodology which are not capable of resolution within social science. This applies in particular to the concept of the social reality which is the object of research (for example, as between relational and individualist approaches); hence my aim in discussing structure here is simply to discuss how, if we were looking for it, we might set about finding it.

The concept of structure in Kleining is an explicitly relational, but not a structuralist, one: a significant point of difference is the emphasis placed on change over time, on process (and, presumably, causality) as forms of structural relations. The focus, then, is on the relations between elements rather than on the elements themselves; this is because the elements are not approached as isolated units, but in their quality of elements of relationships. I have already indicated how Kleining approaches structure within the research process - that is, as dimensions of which the aspects studied are extremes, and as common elements or dichotomies which are discovered by the process of analysis. A potential problem is the argument that this structure is an artefact of the researcher's own making; Kleining's argument for discovery, however, needs to be able to find structure "out there". Here I think Thompson may be of help: firstly, because of the important point that we tend to find

ourselves dealing with grouped and multiple information unless we deliberately set up methodologies to cut up social reality into what appear to us to be individual parts (is a magazine an individual item, or do we need to isolate individual articles?), and in this case we find relationships, groupings and common elements already present in that given multiplicity - it is not a case of building bridges between islands so much as looking at the whole archipelago (see, for example, Thompson 1978: 7). Thompson also has this to say about the ways in which historical evidence may be interrogated, including:

"As links in a linear series of occurrences, or contingent events ... such a reconstruction ... being an essential constituent of the historical discipline, a pre-requisite and premise of all historical knowledge, the ground of any objective (as distinct from theoretic) notion of causation, and the indispensable preliminary to the construction of an analytic or structured account (which identifies structural and causative relations)";

"As links in a lateral series of social /ideological / economic / political relations ... ";

"It may follow from this, if we press the point a little further, that even discrete facts may be interrogated for 'structure-bearing' evidence ... A historical materialist may argue that the structural organisation of given societies may be inferred not only from larger evidences ... but may be inferred, in some part, from certain kinds of seemingly discrete facts themselves. Thus a tenure exists as 'fact' as some Latin formula inscribed upon a court roll; but what that tenure 'meant' cannot be understood independently of an entire structure of tenorial occupancy and attendant law: that is, within a tenorial system: hence this 'fact' ... carries within it some 'index' towards that system, or, at least, it should propose to the interrogator an indicative question." Equally, a "soldier" (apparently an individual) cannot be a soldier without an army (Thompson 1978: 29-30).

The category of experience

So we may not be entirely stuck in relation to the problem of discovering structure and relationship; this still leaves us with the question of how we understand the ontological status of the information we gather in our research,

and the category of experience is central here. If we are to accept that the world as we experience it is at least partially constructed by ourselves (our perception of the unity of phenomena and of relationships such as time, space or causality cannot be derived simply from our partial and discontinuous sense impressions of reality (Kleining 1991: 5-6; see also Pirsig 1989: 133-138), but nevertheless argue for discovery rather than simply interpretation, we have to indicate in what ways the world is not constructed by ourselves - in other words, the points at which social reality can make itself felt, if necessary against our constructions [\(10\)](#). Otherwise we run the risk of collapsing entirely into solipsism. One obvious move is to identify ourselves as social beings and our own constructive activity as itself socially determined. This is important, but it may not be enough; it runs the risk of ultimately being a strategy of closure. If it were possible to indicate ways in which the external world could make its presence felt against our constructive activity, we might be able to argue for the possibility of something more [\(11\)](#).

I think Thompson does provide such an indication, in terms of experience, or the determination of interpretation by reality. In this perspective a scientific methodology would be characterised inter alia by not being an arbitrary, "anything goes" approach. The material (social reality, and not simply the world of meanings) should determine (set limits to and exert pressures on) the constructions which can be placed on that reality more closely than in everyday methods (Kleining's method, of course, aims at making this determination an absolute one):

"To be an object, to be 'null or inert', does not remove that object from being a determining party within a subject - object relation. No piece of timber has ever been known to make itself into a table: no joiner has ever been known to make a table out of air, or sawdust. The joiner appropriates that timber, and, in working it up into a table, he is governed both by his skill (theoretical practice, itself arising from a history, or 'experience', of making tables, as well as a history of the evolution of appropriate tools and by the qualities (size, grain, seasoning, etc.) of the timber itself. The wood imposes its properties and its 'logic' upon the joiner as the joiner imposes his tools, skills and his ideal conception of tables upon the wood ... [T]he object remains, within limits,

determinant: the wood cannot determine what is made, nor whether it is made well or badly, but it can certainly determine what can not be made, the limits (size, strength, etc.) of what is made, and the skills and tools appropriate to the making. In such an equation 'thought' (if it is 'true') can only represent what is appropriate to the determined properties of its real object, and must operate within this determined field." (Thompson 1978: 209-210)

And, we might add in a slightly different language, the more detailed information we have to work with, the less scope we have for handling that information just as we please: the determination, in other words, can be stronger or weaker depending on how systematic our information is and how systematically we handle it. A rejection of pure empiricism, in other words, does not imply that we have to reject any possibility that particular understandings of reality may not be closer or further away from the reality itself. This relationship between effective understanding of reality and the reality itself is of course fundamental to everyday survival in the social (and natural) world.

If this initial move is made, Kleining's claim for the possibility of discovery starts to seem less strange, and we can proceed to questions such as that of how we can maximise the openness of the individual and their willingness to change their understanding. Rather than assuming a rigid dichotomy between subject and object or a world in which there is nothing outside the subject and their interpretations, in other words, this allows a move towards overcoming the division between subject and object. When the subject has brought their structure of understanding to correspond to the structure of the social world, this is not simply a matter of coming closer to the structure of other people's understandings (we could ask ourselves whether it always happens that people understand their own lives, for example). It also represents some scope for what might be an important notion: the concept that the researcher might themselves learn something and change during the course of their research. One is not then either attempting to dissolve the object in the subject or to assert the primacy of subject over object. Rather, the subject is seen as capable of change because of their relational (social) and historical nature; scientific methodology becomes the attempt to find ways of allowing

the social relations entered into in research to determine the researcher's understanding of the object.

III Perspectives on practical research

I said at the outset that I was more interested in seeing where Kleining's perspective took us than in arguing for its ultimate validity; in this final section I want to indicate some points at which I found resolutions of some of the problems in my own research on counter cultures.

Systematic research

The first relates to the selection of objects of study, types of information and methods of data collection. For researching the kinds of complex, contradictory and often extremely disparate social contexts I am interested in, the possibility that a systematic approach can simultaneously be a highly selective one is obviously enormously useful, as a method of getting past mental blocks, as well as being a means to generate an intentional and workable research procedure.

Kleining's methodology is, however, still quite a demanding one. Some aspects of this are perhaps easy enough to come to terms with: the requirements of variation of methods and of variation over time are, I think, defensible requirements, even if they might exceed the limits of postgraduate research projects in some contexts [\(12\)](#). Others are less straightforward; to the extent that a number of analysts are required for example, this would seem to conflict quite straightforwardly with the usual requirements for dissertations, as would the attempt to avoid starting from positions which are already firmly established within "the literature". Presumably the question of manageability in relation to the size of the topic would come into play at some point as well; the basic requirement is that of the "maximum variation of perspectives", in other words of covering all the extremes of structurally relevant difference. Kleining suggests that in some contexts the research needed could be reduced to 60-80 interviews covering 20-25 (open-ended) questions (Kleining 1982: 236), but this, and the necessary analysis, would

not be a short exercise in itself. The question could be asked as to how far in terms of (structural, numerical, chronological, spatial) scale the method could be taken before it became unworkable. This might not be as great a stumbling-block as it seems; the method lends itself to the independent generation of analyses of relatively limited areas (and the collection of corresponding data) which can then be legitimately subjected to a "second-generation" analysis which would integrate them (including, if necessary, the re-analysis of the material gathered for the "first-generation" analyses). Alternatively, one might make a partial retreat on Kleining's aims of definitive discovery as the goal of every piece of research: this would involve focussing on the processual nature of the research process and the significance played by the researcher's initial understanding of the subject of research. It might be possible to argue, in other words, that a less systematic and less final version of Kleining's methodology could leave us with a result which, although still representing a partial perspective on the subject and perhaps misinterpreting central structural relations, would nevertheless represent a building block for further work. This would entail making all the material used in the analysis available to other researchers, because of the requirement to return to the stage of initial (re-)grouping under certain conditions. Such an approach might enable a more collective and longer-term approach to the research of a particular topic, still aiming at a definitive analysis, but applying Kleining's perspective to the results of a number of individual, albeit related, research projects.

Whether or not this is possible, the humanisation of the research process that is implicit in Kleining's emphasis on the everyday and on totality has its attractions, and the method does offer some pointers in this direction, in a number of contexts: in terms of how we bring ourselves into the research process, in terms of how we work, and in terms of how we involve the "research subjects" in the research.

On the first point, Kleining's location of the researcher's initial understanding as a starting point is important in allowing us to see not just our own academic reading and research experience of the subject as relevant, but more generally the totality of our experience, whether it is direct (personal) or

indirect (through books, for example), and whatever kind of context it appears in: the researcher as person would then appear fully within the process rather than being denied, but without being encouraged to dominate it.

The idea of working in small teams of researchers using qualitative methods in particular seems an appealing one; if we are to adapt our structures of understanding to the structures of extremely complex social realities (rather than attempt to impose our understanding on these structures) as well as interact with the people who inhabit these realities, in all their differences, particularities and conflicts, this will impose strains on researchers which the group context could go at least some way towards making bearable. On a related point, Kleining comments that the notion of the "maximum variation of perspectives" offers a rare theoretical legitimation for the traditionally attractive idea of using a multiplicity of methods.

That the "subjects" of research should be enabled to participate more fully in determining the research process is not of course a new idea. Kleining's ideas, however, open up three specific possibilities in this area. The first, based on the rule of the maximum variation of perspectives, is the possibility of offering the participants a place within the scientific research process itself which does not depend either on circumscribing their freedom of action or on extensive training. The variation of the individuals collecting and analysing the data is a necessary requirement of Kleining's methodology and not something imported from outside, and the kind of activity which is involved is not one which requires "discipline" in a particular form of action so much as a question of bringing an open-minded attitude to looking for extreme difference and analysing it for common elements. Secondly, the politics of the research move onto a rather different plane: one is not now arguing about the imposition or acceptance of rigid views of the world, but rather talking about people learning (in the case of the researcher) or thinking (in the case of the participants) about a shared reality. Incidentally, I think we are likely to find that by asking people to think about their own lives in different ways we are less likely to bore them; this is certainly the case within my own field of study, that of counter cultural consciousness. Thirdly, given that not all of those we interact with in our research will be interested or able to join in as full researchers, this

methodology offers natural points for returning to them - at the point in particular of the first and second stages of the process of grouping and analysis, where stock is taken of the extent to which the statements made conflict with or describe the information collected. The value at these points of involving "local experts" should be fairly evident: if somebody can say to us "That leaves out X", this is a very strong piece of information coming out of the heart of social reality which we would want to take into account.

Incidentally, if the need to involve the research subjects as active and autonomous participants is built into the research process in this way, it opens a scope for a greater determination of the focus of social research by the participants than is present otherwise: at its simplest, we will need to interest those we want to involve, and that means finding topics that are relevant to them.

The researcher as active participant

Lastly, I want to raise three related issues, which may bring the discussion back towards the other papers in this collection. The first is that of the active role of the researcher. In research on areas such as counter cultures this is a continuous problem, and one which I think is not adequately dealt with by the traditional approach, which focuses on participant observation where the participation is only for research purposes. In the kind of research I am interested in the researcher is often an active participant prior to being a researcher; in other words, they already have a position within the power structures of the context they are researching, and they are then likely to have a prior history of conflict as well as of cooperation with other members of the group. The knowledge gained in their research can also be a source of power within the group, independently of the power relationships within the research process proper. This is particularly true where the kinds of resources which are available to the researcher (spare cash, a work telephone, access to printing and photocopying, contacts, social status, access to particular kinds of legitimations and skills) are not available to those being researched.

Furthermore, such groups are very often small enough and flexible enough for

the interviewer, even if the above did not hold, to exert a significant influence on the direction of the group. In research on "new social movements", the case has been made for the researcher to take on an explicitly organising role: in Touraine the researcher brings the group to an understanding of its own true consciousness (see Touraine (1981) for a full discussion of what is of course a much more complex viewpoint) and in Melucci extremely contrived situations are organised by the researcher and, presumably, their team of assistants. (See the discussion in Melucci (1989: 235-261).) Neither this nor an entirely passive role for the researcher seems entirely credible nor entirely desirable.

What Kleining's work and in particular the discussion of the qualitative experiment opens up as a possibility is that we could see the researcher's activity within the group - of whatever kind - as a means of information-gathering, one form of "asking questions of social reality". The response, both of individuals and of the social context (obviously these are two very different types of responses) can then be treated as itself a form of data. This is another form of involving the whole person within the research process, and (since this happens anyway) there is no harm in making it explicit. There is then, however, a potential conflict between the motivations of the researcher-as-participant and those of the participant-as-researcher. There is a real risk that we fall into the trap of thinking that we can most effectively demonstrate "the reality and power, the this-worldliness of one's thought", to quote Marx (n.d.: 113) by dominating the group itself. This kind of move is all too easy to make, involving as it does a linkage between two versions of what Raymond Williams describes as "a basic orientation to the world as available raw material ... [which] necessarily includes an attitude to people as raw material" (1985: 261) - a linkage between a version of this which relates to our personal lives and a version of this which relates to our research activity. Rather than attempting to understand the world in order to change it in a given direction, in other words, we would be changing it in an arbitrary direction to demonstrate that we understood it. Avoiding this, I think, means tackling the question of the research ethic head-on, and this is my second point.

Ethical issues

Ethical solutions which depend on exogenous ethics appear as both intellectually and practically unsatisfying (see Kleining (1988: 236-239) and Thompson (1993) for alternative statements of the problem). Such solutions appear to suggest in practice (in other words, if we were to take them seriously enough to attempt to institutionalise them) either that all researchers should be nice people or that it would be possible to formulate rules which were strict enough to be enforced by some professional body and yet which covered all the relevant cases; either seems to me unlikely. It would be far better to identify a way in which ethical imperatives could be discovered within relevant cognitive approaches.

Kleining's perspective opens up the possibility that one could formulate an ethical approach which identified the researcher's openness to the experience of research and the experience of the participants as defining characteristics of good research. As one case of this, Kleining argues that in qualitative experiments, the most active form of qualitative research, "the explorative aims [of qualitative experiments] should prevent the destruction [of the subject of research].... Their methodology involves their careful use: the adaptation of methods to the object, the testing of boundaries, the gradual maximisation or minimisation of aspects in the object of research, the general question-answer game in the principle of dialogue, if possible with the direct participation of those affected (der Betroffenen) ... " (Kleining 1986: 744-5).

If it becomes possible to argue that this kind of attitude is a precondition of good research, I think we might see a lot more ethically acceptable research. This would lead us in a sense away from the dominant exogenous moralities of our societies to an endogenous morality which we can find in some counter cultural thought as well as in some very ordinary modes of thought: the idea, most explicit in Buddhism, that one should abstain from particular kinds of behaviour because of the effect they have on our own consciousness.

Relationships of understandings

The last issue I want to raise also deals with the relationship between the researcher and the researched, in this case in terms of the status we ascribe to the (self-)understandings of the participants. The understandings of reality and the self-understandings to be found within the kind of contexts I am researching are highly contradictory ones: this excludes a simple acceptance of the participants' points of view as an option. If one can understand differences of particular kinds as relating to structuring dichotomies, we may be able to make a certain number of steps forward. An example of the kind of point at issue is the question of power relationships. We can reasonably expect that in relationships of domination - within political organisations, for example - it is unlikely that all participants will have the same story to tell about decision-making (13); in other words, we can not simply produce an analysis on the basis of taking the accounts of the participants as straightforwardly true, among other things because of their contradiction. Thus for one group, the problem may be the centralisation of power in the hands of a small elite; for another group, the problem may be the way in which the making of necessary decisions is frustrated by an unworkable structure and an unrealistic internal opposition. Given sufficient evidence, a researcher could be justified in identifying common elements such as the identification of opposing groups, the existence of conflict over the ways in which decisions are taken, and the perception of a relationship between structures of decision-making and the location of effective power. The analysis is a result of comparing the self-understandings of the participants (how they locate themselves within a given social context), but at the same time it is not identical with these self-understandings.

In this perspective, the researcher on counter cultures is reproducing the processes of observation and experiment, as well as the attitude of openness and provisional understanding, which are characteristic of much of the counter culture, which is notably undogmatic and evidences a high level of practical consciousness. The ideas and the perception of the movement itself can be taken seriously, in other words, without being set up as canonical truth. In some ways it would be odd for the researcher to be the only individual in this "semi-shared reality" not to be attempting to make sense of it for themselves -

to be the only member of the group who did not have their own idiosyncratic ideas about what was going on. This obviously does not resolve the question of power in research results, but (as I have argued) the ultimate source of resistance to power in this context is the cognitive requirement of the openness of the researcher. A researcher who imposed their own interpretations on the movement would simply be a bad researcher; but the opposite to this is not the acceptance of the movement's (contradictory) self-image - it is the attempt to understand the movement and to change one's own understanding of it.

Conclusion: research as supportive and critical

Can this still be supportive research? I think it can. It is true that the method, when directed towards the dominant institutions, is very clearly a critical one; appearances are criticised in terms of the underlying structure, which thus presents the individual phenomena to us in a new light. As with the ethical approach, this is an immanent concept of criticism: the real is ultimately criticised in terms of the real and not on externally-derived terms. This concept of criticism, which we find in Marx, is one of global social rationality; but it can also be brought to groups with which we are in greater sympathy. Conflict and criticism are, after all, not foreign to everyday life, let alone to counter cultural groups, for example. What Kleining says of qualitative experiments can perhaps be generalised:

"[Qualitative experiments] have a kind of immanent morality, if by this we can understand the legitimacy of the illumination (Aufklärung) of structures and conditions of social relations. The reason for this is of course to be found in their relatively limited reification [of others] and in their compatibility with everyday situations" (Kleining 1986: 745).

In other words, we can act as human beings, as participants and as researchers without needing to be false to our relations with others, without destroying the context we are researching, and without denying the value of sociology: rather than simply reworking the self-images of the group and confirming its world-pictures we can offer a clearer self-understanding and a

clearer understanding of the wider society - which are necessary, if not sufficient, conditions of conscious change.

Footnotes

(Note 1) I wish to thank Hilary Tovey (Dept. of Sociology, TCD) for her comments on an earlier version of this paper and Brian Torode, Anne Good and Carol MacKeogh for their careful editing. I also wish to record my indebtedness to Prof. Kleining for the opportunity to participate in his courses and his discussion group on qualitative methodology during my research in Hamburg in 1990-91 and for his advice on and encouragement of that research. [\[Back\]](#)

(Note 2) I find Raymond Williams' discussion (Williams 1976: 133-136) of the history of the concept of the individual illuminating in this respect. [\[Back\]](#)

(Note 3) However, it is an important fact that in practice most writers and thinkers, whatever their stated position, present what is recognisably a rational argument of some form, in the sense of attempting to convince others by the communication of an (implicitly or explicitly) systematic or structured argument; irrational perspectives are much more frequent than genuinely irrational modes of presentation. [\[Back\]](#)

(Note 4) Obviously "scientific" is used normatively rather than descriptively here: not all methods used by people describing themselves as scientists are necessarily scientific ones, in this usage. [\[Back\]](#)

(Note 5) These everyday methods include experimentation, observation and questioning as well as simpler techniques such as spontaneous activity and comparison. In this perspective, qualitative and quantitative methods are not located in different directions; rather they represent different degrees of abstraction from everyday methods. Specifically, quantitative methods deal with the measurement of concepts which they cannot themselves generate: "If [qualitative research] explains a subject, a quantification of this does not help; if it does not explain it, quantitative research cannot make good the mistake." (Kleining 1982: 228) At a later point (Kleining 1990c: I.8, I.9) Kleining placed hermeneutic (interpretative) approaches at a higher level of abstraction than discovery-oriented approaches. [\[Back\]](#)

(Note 6) Kleining's discussion of the history of qualitative experimentation draws mainly on the work of natural scientists and psychologists. He sees the qualitative experiment as largely absent from the social sciences, with the partial exception of ethnomethodology. On Garfinkel's experiments he comments: "They were, however, not used for the purposes of discovery, but for the demonstration of previously known states of affairs (Sachverhalte)." He sees this as less advanced from the point of view of a discovery-oriented methodology than contemporary natural scientific and psychological approaches (Kleining 1986: 728-733). [[Back](#)]

(Note 7) Note that in contrast to the quantitative experiment, the qualitative experiment aims at discovering which relationships are relevant, rather than testing pre-given variables (Kleining 1986: 725). [[Back](#)]

(Note 8) These are my own summaries of Kleining's more detailed and analytic results. [[Back](#)]

(Note 9) This "100% rule" is "holistic" in the sense that it is based on the position that nothing definitive can be said about isolated areas of the data until one has reached a definitive analysis of all the data. Nevertheless, this does not prevent provisional statements about the data (since the bulk of the analysis in fact consists of generating such statements); and it is important to note that this 100% rule applies to a highly selective collection of information. [[Back](#)]

(Note 10) I think there are weaknesses in the apparent identification of all social reality with explicit meaning alone. There is also, for example, physical existence; there are also subconscious psychological events. Polemically, these issues could be identified as the most straightforward contributions of Marx and Freud to modern thought. "A brave man once convinced himself that people only drowned in water because they were obsessed with the thought of gravity. If they struck these ideas out of their heads, for example by declaring them to be superstitious and religious notions, they would be safe from any danger in water." (Marx n.d.: 118) [[Back](#)]

(Note 11) Bryan Turner argues that "it appears to be logically important to distinguish between three separate issues: (1) are beliefs socially determined? (2) are beliefs true or false? and (3) are beliefs rationally held?"

(1991: xxxix). What I want to argue is that it may be possible to identify kinds of social relation (research situations) where we can say that the social determination of our beliefs about that situation is of such a kind as to lead us to hold true beliefs; in such a case our beliefs would be indeed rationally held.

[\[Back\]](#)

(Note 12) This may be an overstatement of the case: Kleining himself offers an analysis of three New Year speeches by Chancellor Kohl (Kleining 1990a) and restricts himself to the analysis of these texts alone; the variation of method is limited to variant methods of textual analysis (observation and experiment) and the variation over time to three years. However, the claim is not made that the analysis has any wider significance than the analysis of three speeches; the divergence would arise at the point where the results of the analysis were said to tell us something about the fundamental nature of the West German state, for example. Such a claim could only be made if we were to make such an analysis of a group of types and sources of evidence which between them offered a "maximum variation of perspectives" on that state. [\[Back\]](#)

(Note 13) This is particularly true if we take a complex notion of power such as that offered by Steven Lukes, which accepts that "the domination of defenders of the status quo may be so secure and pervasive that they are unaware of any potential challengers to their position and thus of any alternative to the existing political process, whose bias they work to maintain"; on the other hand, if we are to include, as Lukes does, manipulation and authority as cases where conflicts of interest may be latent (not politically articulated), in our conception of power, we will have to find ways of discovering relations which are not explicitly visible to the dominated groups (Lukes 1974: 21, 32). [\[Back\]](#)

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I have tried as far as possible to limit references in the text to Kleining's published and accessible articles on methodology; however, for the sake of

completion, I have listed here other relevant pieces by Kleinig of varying status. As far as I am aware, none of Kleinig's work has been translated into English.

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