TOWARDS AN EPISTEMOLOGY FOR ACTION-RESEARCH

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"Action-Research and the Nature of Social Inquiry"

Abstract

Action-research's epistemological problem is that it proposes
an opposition to positivist forms of social inquiry while implicitly
using a positivist epistemology to justify its own procedures.
This work is an attempt to formulate an alternative epistemology for
action-research. A selection of action-research writing is
critically reviewed in order to show the lacunae and inconsistencies
which necessitate a more thoroughly argued theoretical framework.
This alternative epistemology is based on a reflexive theory of
consciousness and language, and on a dialectical theory of the
self-other relationship. In this way it proposes the possibility
of a theorizing Subject, and in particular its specific autonomy
in relation to theories of ideology and of societal and psycho-
analytic determinism.

The argument has the following stages. Chapter One introduces
the general theme. Chapter Two analyzes the relationship between
action and research, not as a process of evaluation or prescription,
but as a dialectic of reflexive and critical questioning. Chapter
Three critically considers theories of the self and of the unconscious
in order to formulate the possibility of critical self-reflection.
Chapter Four analyzes the social relationships of the research process,
criticizing the Habermasian notion of "emancipation", and analyzing
the relationship between criteria for the improvement of professional
practice and the criteria for adequate research, including a
consideration of how action-research might relate to the processes
of professionalized institutions. This section involves an analysis
of theories of professionalism and bureaucracy. In Chapter Five
the argument turns more generally to the forms of validity to which
action-research might aspire, criticizing such notions as "natural-
istic theory" and "illumination", and formulating validity in terms
of reflexivity and dialectics. In this context, also, action-research'
attempts to invoke "aesthetic" modes of understanding are considered,
and a contrast is drawn between action-research's reliance on forms of
representational realism and reflexive theories of textual structure
and response. Chapter Six, the conclusion, draws together the fore-
going arguments in order to present six critical propositions, as a
set of implications for the renewed practice of action-research.

Contents

CHAPTER ONE

INTRODUCTION: ACTION-RESEARCH AND THE INTELLIGIBILITY
OF SOCIAL INQUIRY

Preliminary

This work is about action-research.   And it is
also about the nature of inquiry in the social sciences.
The pairing of these themes was originally a biographical
necessity, the result of many years of engagement with
action-research - both that which I initiated for myself
and that of others which I have attempted to guide.
This experience of action-research raised the problems of
inquiry itself as a set of immediate worries concerning
not only what to do but also one's grounds for such decision,
their aims, their criteria, their mode of justification -
in short: their intelligibility. Complex activities always
pose questions of what to do; in this respect experimental
natural scientists are not unlike their own technicians.
However, neither of them is necessarily challenged by their
activities to provide grounds for the intelligibility of
those activities. Yet, here precisely is action-research's
specificity - its challenge, its vulnerability, and its
interest. For action-research claims to reject both
institutional traditions which propose grounds for its
activities: action-research rejects the tradition of
scientific research by invoking as a central principle, as a
criterion, the need for practical effectiveness at the level
of mundane activity; and it rejects the tradition of
mundane practice by invoking as a central principle the
scrutiny of practical judgements by means of research.

It is highly significant therefore that action-research has arisen in certain contexts of <u>professional</u> work, namely social administration, management, and (in particular) in education, which is the biographical context and the overall emphasis of this work. "Professional practice" in these contexts conventionally exhibits a fundamental contradiction which action-research seeks to address: professional expertise represents a cognitive authority based in "science", an authoritative formulation of knowledge as <u>generally</u> valid, such that clients' experiences can be conceived as predictable specific instances of prior generalities, ie. as "cases". And yet the corpuses of knowledge appropriate to the contexts where action-researchers have been active (the "theory of" administration, or management, or education) is precisely <u>not</u> of that authoritative and general form: the individual "case" is acknowledged to present a degree of "uniqueness" which threatens the relevance of experts' prior understanding and thus the authority by which they are "expert". Action-research has not, therefore, arisen in such "professions" as engineering or agriculture, nor even in what might have been expected to be the interesting "intermediate" case of medicine. Essentially, then, action-research seeks to re-cast the authority of the professional practitioner in "people-processing" organizations, by means of a version of inquiry which aims to mediate between the prescriptive authority of science and the unique experience of the individual case for which such prescription conspicuously cannot provide.

The intelligibility of action-research's project depends, therefore, on the <u>problematic</u> nature of knowledge in the social sciences. Hence the double significance of "education" in this work. Not only have professional educators espoused the notion of action-research to a greater extent than most other professionals, as a version of thier practice, but action-research writers in other contexts have proposed "education" or "training" as their version of the action-research process itself. (See Krech and Crutchfield, 1948, pp. 523-4; Lewin, 1946, p. 42; Lippett, 1948, p. 240ff.) The meaning of both "education" and "training", however, conventionally depends on an authoritative version of knowledge as the prescription for action, whereas action-research is concerned to question, if not - indeed - to reverse, such prescriptions. Action-research poses the question of the nature of inquiry both by its challenges and by its claims: it <u>challenges</u> a scientific method of inquiry based on the authority of the "outside" observer and the "independent" experimenter, and it <u>claims</u> to reconstruct both practical expertise and theoretical insight on the basis of its own inquiry procedures.

Action-research thus plays both ends against the middle, with inevitable consequences: "scientists" are scornful of action-research's claims to validity (action-research is <u>muddled</u> science); and practitioners are scornful of action-research's claims to feasibility (action-research <u>undermines</u> practical skills). The problem is that the "middle", for which action-research

wishes to speak, lacks theoretical definition, and
is thus (with rare exceptions) defended by action-research
only rhetorically or pragmatically.   Such defences,
lacking principles and grounds, are open to immediate
refutation, as noted above.   Thus it is in order to
provide a theoretical elaboration of action-research's
own mode of inquiry that this work is proposed.   This
will entail both a critique of the current state of
writing on action-research, and the provision of
theoretical resources for establishing in principle its
intelligibility.

In the remainder of this Introduction, I shall
first of all introduce the parameters of the main
discussion, in order to show how they constitute action-
research's central issues, and how the central issues
for action-research are also central issues for the
general project of inquiry in the social sciences.
Secondly, as a preliminary to the following chapters, I
shall argue for a number of general epistemological and
ontological positions, as theoretically necessary
presuppositions, not only for action-research but for
sociology in general.

Towards a Problematic for Action Research
"Analytic Grounds"

My argument could not begin by tracing a "history"
nor by reviewing "the literature" of action-research,
since that would be to presuppose a definition and a

coherence for action-research whose absence, precisely,
is the occasion for the work.   Admittedly, my initial
step was to consult the range of writing in which a
mode of inquiry was described as "action-research" (since
I had no theoretical basis for rejecting any examples as
cases where the label was either claimed or avoided
"illegitimately") but the "legitimate" nature of action-
research is a crucial topic for the analysis, and
indeed this initial step merely raised the question as
to what form of analysis could address "legitimacy";
or in other words: how could "action-research" be created
as an object of theory?

Kant provides us with an indication of the requisite
level of approach when he describes the "transcendental
exposition" of a concept as an exposition which shows the
necessary assumptions for the concept to be "possible"
(Kant, 1933, p. 70).   Hence his famous a priori categories
of space and time as the conditions for the possibility
of conceptualizing consciousness (ibid., pp. 72-8).
However, Kant also draws attention to the limits (as well
as the necessity and the possibility) of such transcenden-
tal knowledge: it is concerned "not with objects but
with the mode of our knowledge of objects in so far as
this mode of knowledge is to be possible a priori".
(ibid., p. 59).   Kant makes an important distinction
between "analytic" thought, whose "highest principle"
is that of non-contradiction (ibid., p. 189) since it
is concerned with single concepts (ibid., p. 48), and
"synthetic" thought, which is concerned with the nature

of the connections between concepts (ibid., p. 51),
and whose highest principle is the unity necessary for
the possibility of experience (ibid., p. 192).

Now, given the complexity and thus the conceptual
multiplicity of lived experience - the starting point
for a social science - the form of its unity must remain
high problematic, and thus it is important to note that
Kant's emphasis upon the provision of a priori grounds is
qualified by his emphasis upon the role of "imagination"
(ibid., p. 112), by a rejection of the "sophistical"
pretensions of prescriptive methods such as classical
logic (ibid., p. 99) and by a reminder of the inevitability
of the "illusion" by which "we take the subjective
necessity of a connection in our concepts...for an objective
necessity in the determination of things in themselves"
(ibid., p. 299). In other words, this work is not
intended to prescribe a priori grounds for identifying
an object ("action-research"), but to provide grounds
for identifying a mode of knowing such an object.

It is at this level and in this spirit, then, that
I have sought to elaborate a general problematic for
action-research. The work represents what certain
sociologists have called an "analysis" - in which they
perhaps follow Kant rather loosely, given the distinction
above.

> "Analysis is the concern not with anything said or
> written but with the grounds of whatever is said -
> the foundations that make what is said possible,
> sensible, conceivable".
> (McHugh et al., 1974, p. 2)

For action-research: what assumptions seem to underly
its claims and self-descriptions? What principles

might unify the concrete diversity of its experiences,
as themselves, in their diversity, nevertheless constituting
that unity to which the self-proclaimed label "action-
research" seems to aspire?   It is as answers to such
questions that the four main chapters of the work are
presented: Chapter Two, on action and research,
articulates the central meaning of the term itself and
hence its implicit problematic, and Chapter Three, on
self-reflection, attempts to provide for the theoretical
possibility of this problematic.   Chapters Four and
Five are concerned with criteria by which examples may
be judged to be adequate examples: Chapter Four approaches
this question concretely, from the starting-point of a
proclaimed aspiration, and Chapter Five more generally
and comprehensively.   These four chapters thus present
four constitutive themes for an analytical theory of
action-research, a set of conditions for its theoretic
possibility.   The four themes are summarily introduced
in the following section.

Four Constitutive Themes for Action-Research

A)   Action and Rēsearch

In proclaiming the intelligibility of a coupling
between "action" and "research", action-research by its
own self-categorization challenges a conventional
distinction between one type of act whose rational
properties are necessarily taken for granted as culturally
institutionalized glosses upon meaning, motive and
communication; and another type of act whose necessary

claim is that it "brackets out" precisely such features
in order to question the nature of meaning, motive, etc.
The question therefore is: what form of unity could
provide the site for such a challenge? Does action-
research propose that research is necessarily a form of
action (and thereby question science's claim to be
independent of its context) or that action is a form of
research (and thereby support the claims of the mundane
social actor to create adequate innovative understanding)?
If the distinction action / research is to be transcended,
what mediating category or categories could provide
grounds for such a transcendence?

B) Critical Self-reflection

In order to realise the aspiration noted in Theme A,
action-research must reject the separate roles of an actor
who is merely the object of research and a researcher
who merely observes; instead, action-research proposes
both researchers who participate in the action under
inquiry and actors who inquire into the actions they
engage in. For such a proposal to be intelligible, it
requires the possibility of a potential _theoretic_ competence
among social actors. This in turn requires the
possibility of formulating consciousness in terms of a
specific independence of both its cultural context and its
psychic history. Analytically, action-research's
problematic may be formulated as a search for a coherent
mediation between its necessary denial of determinism and
its equally necessary historical and cultural situatedness.

C) The Improvement of Professional Practice

The concrete version of its unity which action-research presents as reconciling its originary disparity (see Theme A) is that of professional practice. Professional practice is that form of action which claims explicitly to be guided by the discursive elaboration of theory, where actors invoke as their auspices the findings of "research", and thus present a mundane rationality in the form of a scientific expertise. In this way criteria for practice and for knowledge can be made to coincide: the improvement of professional practice becomes a criterion for research, since for action-research this dichotomy is precisely what has, supposedly, been removed. However, this "professional" knowledge derives its authority over action from the scientific auspices of positivism, which action-research wishes to challenge. Action-research therefore finds itself simultaneously and from the same intrinsic principle both supporting and opposing the cognitive authority of professional work, and, in order to resolve this contradiction, seeks a criterion by which an analytically justifiable formulation of validity may be distinguished from the conventional authority of institutionalized roles. The analytical question posed by action-research here is: can theory (or "research") be related to the institutional life of practical action except in the form of an always unfulfilled - because "idealistic" - promise?

D)   The Problem of Validity

Following directly from the considerations raised
in Themes A and C, it must be a condition for action-
research's intelligibility that it should address the
possibility of its own validity: by insisting on the
association between research and action, action-research
claims to achieve particularized relevance and effective-
ness (ie. for "practice"), but appears by the same token
to forfeit the possibility of generalizing any of its
outcomes.   In this respect action-research reverses the
familiar paradox generated when a natural science model
of inquiry is applied to social situations: namely, that
the validity of a general conclusion always lacks
relevance for particular contexts - to the extent that the
"significance" of the results of an investigation
becomes a pun between statistical and interpretive
meaning.   Action-research's reversal converts the pun of
positivist significance into an anxiety and thus into an
issue.   Its analytically necessary form is: what
formulation of validity (of "significance") could inquiry
aspire to which might "unify" the disparate notions
of adequacy which inspire action (on the one hand) and
science (on the other)?

Action-Research and the Nature of Inquiry in the Social Sciences

It must be clear from the above set of themes that
the analytical requirements of a coherent theoretical
basis for action-research's problematic are of the deepest

relevance for the social sciences. Each theme raises anew long-standing questions concerning the conditions under which social inquiry itself is possible as a theoretical enterprise (as opposed to its relatively clear-cut status as a sophisticated elaboration of mundane management). Thus, Theme A raises the issue of the relation between theory and practice, and Theme B poses the epistemological dilemma concerning the apparent interpretive freedom of consciousness, yet its (equally apparent) constitution within a specific biographical and cultural (ie. historical) matrix. Theme C poses the problematic relation between theoretic and institutional-ized authority, and Theme D challenges once more the widespread acceptance of analogies between the natural and the social sciences.

In the main part of this work these four themes will be elaborated in relation to action-research. However the four themes themselves can be seen as exemplifying two principles which are even more fundamental in providing grounds for the possibility of social inquiry in general, as well as action-research in particular, namely the epistemological and ontological principles of 1) reflexivity and 2) dialectics. Thus, it will be argued that the issues of theory and practice (Theme A) and freedom and determinism (Theme B) can only be grasped in terms of a dialectical relationship; and that only in terms of the reflexivity of inquiry can the issue of its theoretic as opposed to its institutional authority (Theme C) be satisfactorily addressed without recourse

to spurious analogies with natural science's positivist auspices (Theme D).

These two principles (reflexivity and dialectics) will progressively unify the series of arguments concerning action-research, and it is in their terms (it will be argued) that action-research must seek to resolve its constitutive dilemmas. Both terms suggest their potential status as grounding principles for inquiry by their fundamental significance as principles for the understanding of both language and consciousness, which must indeed be presupposed as conditions for the possibility of inquiry, if not for social life itself. Hence the remainder of this chapter will be concerned with the introduction of these two principles.

Reflexivity, Dialectics, and the Intelligibility of Inquiry

Reflexivity: Language, Theory, Self, and Other

Language is indexical: the sense of verbal expressions is decided by a hearer (reader) in the light of his or her elaborated understandings of the speaker's (writer's) relevancies, the situation in which the expression is used, and the verbal and cultural system of which the particular expression forms an element. The "indexicality" of language is thus "irremediable" except for the "practical purposes" of particular cases (Garfinkel, 1967, pp. 6-7). Communication therefore is not merely the transmission of a message. The words of a telegram may be "transmitted" between two pieces of

electrical equipment, but its properties as a communication
of meanings are created by the interpretive activities
of the sender and the receiver (at either end, as it were).
(A single index is related to a plurality of books in
a library or to a plurality of references in a single
book in the same way as a single signifier is related to
a plurality of signifieds, ie. concepts in the
consciousnesses of the members of a semiotic community
(Saussure, 1974, p. 67). Hence the intelligibility of
language is a potentiality: language users always have
before them the task of realizing this potential by
means of their own interpretive procedures, and a number
of these practical interpretive procedures for realizing
the potential intelligibility of language have been
specified (Cicourel, 1973, pp. 52-6). What they all
share is a quality of reflexivity.

A reflexive action is one which is "bent back" so
that it affects the doer: in doing the action to
Another, I necessarily do it myself. This to be under-
stood as follows. Given the indexical quality of
language, I can only communicate by presupposing the
intelligibility of my speech for the Other, and I can
only make this presupposition because I decide its
intelligibility for myself by envisaging its intelligibi-
lity for the Other. In other words, intelligibility
resides in the presupposition of the interchangeability
of perspectives between speaker and hearer, writer and
reader. The shifting structure of this intersubjectivity
is handled by means of the accomplishments of practical

reasoning ("etc.", "ad-hocing","retrospective-
prospective sense creation" - see Cicourel: loc. cit.)
which remain unnoticed by members, as do so many other
routinized cultural skills. Communication, then, is
always a formulation of the Self (Blum, 1971, p. 313) in
the light of the Self's mastery of the language, the
setting, and (above all) through the Self's awareness
of its own nature as the grounds for its assumptions
about how this speech or writing will be received by
others, and hence what this speech or writing should or
might mean. This is why educators and therapists
think that people learn by talking and writing: talking
and writing (including this writing!) are not descriptions
of an already existing state of awareness, but a means
for the self-reflective formation of awareness.

Now these taken-for-granted reflexive competencies
required by mundane interaction may be "uninteresting" to
lay members of a signifying community (Garfinkel, 1967,
p. 7), and indeed the repression of an awareness of such
reflexivity may well be a condition for the routine
accomplishment of social intercourse in a culture where
"knowledge" is taken to be a descriptive grasp of an
external object-world through the supposedly transparent
medium of a referential language. But for sociology
to be a "science" it needs to theorize adequately both
its objects and its methods (see Husserl's critique of
"Naturalism" - Husserl, 1965, pp. 80-3).[*] Thus,

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[*] Otherwise, as Rutherford is supposed to have put it:
"There is only Physics - everything else is stamp-
collecting".

sociology must formulate the relation between lay
members' methods for managing intelligibility and
sociology's own methods for managing lay members'
management, and this relation must in turn be formulated
in relation to the culture, in which both sociologist
and lay members are engaged, where the reflexivity of
language and consciousness is energetically denied by
a positivist ideology of facts and labels. There is,
in other words, an inescapably sociological relationship
between sociologists and the social world they purport
to account for.

This is the sense in which reflexivity is an
analytical issue for sociology - a condition of sociology's
intelligibility as a general project, and a condition it
frequently ignores. For example, whereas Garfinkel
reveals lay members' reflexivity as a feature of their
speech which is uninteresting to them but interesting
to a sociologist, Garfinkel's own speech, like theirs,
relies on reflexive features which are tacitly used as
an "uninteresting" resource for his revelation. For
Garfinkel, the "task" for sociology is that members'
reflexive procedures for constructing mundane
intelligibility may and should be treated as "data"
for "empirical research" (Garfinkel, 1967, pp. 281-2),
but Garfinkel has already argued at length that the
substitution of objective for indexical expressions
cannot be other than an "unsatisfied programme" (ibid.,
p. 4) except as a matter of practical social management

"in every <u>particular</u> case" (ibid., p. 6), and thus his
own "data" and "research" could be no more than yet
another practical and reflexive management of an indexical
communication.   In more general terms, Garfinkel's
own text <u>glosses</u> (in its writing) the glosses of members
which are its topic.   His own text is itself an example
of "practical social management" within the highly inter-
esting institutionalized speech community of "sociology".
As Garfinkel himself says (quoted by Filmer, 1976,
p. 80): sociologies are "made to happen as events in
the same ordinary affairs that in organizing they
describe".

Filmer's article makes clear the general significance
of reflexivity as an analytical principle.   Sociology's
conventional tradition is one in which the "essential
reflexivity" of sociology's "authorship" of its own
speech is <u>denied</u> by invoking instead "the generalized and
generalizable authority of science".   Whereas for
"science" the "essential reflexivity of accounts ... is
uninteresting", a "reflexive sociology" takes this
essential reflexivity as its central interest.   (Filmer,
1976, pp. 82-3).   In other words, to be "interested"
in the reflexivity of accounts is an analytical
requirement for social inquiry.   To propose an "objective"
description of lay members' practices is to reproduce
a process of mundane stratification, which denies the
reflexive socio-linguistic processes of the social
relation by which it is accomplished, in the same way
as do lay members' practices themselves (see Phillipson,

1975, p. 165). The analytical requirement for sociology
is to address the reflexive practices by which alone
it is possible: a sociology which fails to do this is
merely an instance of cultural stratification, an example
of what Becker calls a "hierarchy of credibility"
(Becker, 1970, p. 126), a legitimizing agency within a
functionally differentiated institutional order, such
as a "sociology of knowledge" (for example) would
necessarily wish to investigate. Thus, only in taking
its own essential reflexivity as its topic does sociology
differentiate itself from the mundane practices which
are its object, as an analytical, a theoretical enterprise.

The recognition of reflexivity is, generally, a
basic mode of ordering and presenting communicative
adequacy, a claim to grasp the symbolic process by
which communication is accomplished. For sociology
this is a central task, but in a different but analogous
way it is also a substantial and widespread feature of
other cultural forms which aim at a high degree of
persuasiveness. Thus: novels are written about
writers, films about film-makers, and musicals about
musicals; poems are written about language, plays include
dramatic representations and multiple disguise, and
paintings are made of rooms hung with mirrors. At
another level, jokes may be thought of as sudden
reflexive turns, showing that the expectations on
which mundane communication depends are indeed merely
expectations, by suddenly thwarting them as expectations
and revealing instead possibilities that had previously

been concealed by their un-expectedness.*

In other words, mundane reflexivity embodies the fragility of communication: it is by addressing this very fragility, by noting explicitly the art-fulness, the art-ificiality of the signifying process whose fragility is currently in question, that fragility can be survived, and communicability reassured and achieved. As a move against the fragility of the sign, the recognition of reflexivity reassures and disarms: by aligning the writer and the reader, the speaker and the hearer together, as it were, in complicity against the sign, it renews the very intersubjectivity on which the effectiveness of the sign depends. The recognition of reflexivity, then, is a source of rhetorical power, and hence a dimension of persuasiveness, of both aesthetic and theoretical adequacy.

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* The aesthetic power of music is often attributed to the way in which its "abstract" signifiers allow evocative reference to a realm of signifieds which is universal because it is completely individualized - the "emotions" of Everyman. But perhaps the lack of a specific referent for the musical sign allows scope not so much for universal evocation but for unimpeded self-reference. Music is in this sense always "about" itself. Development sections in sonata form, variations on a theme, fugues, key modulations, shifts in orchestration (often, in Haydn, for example, quite consciously jokes - see above): these all represent explicit demonstrations of the transformative power, the effectful work of the musical art itself. And perhaps it is this expressiveness of its own reflexivity which makes the musical text such a powerful and inexhaustibly repeatable utterance.

Now, as a requirement for sociological theorizing, this "analytic" reflexivity has one important consequence which will be of great importance throughout the discussion of action-research: it denies the possibility that theory ("research") can achieve a final or a legislative relation to social action; rather, it presupposes a relationship between theorist and social actor which must be continuous and unending, because it is both irremediably particularized and endlessly problematic. This is because the theorist <u>requires</u> an Other, not as an object but, in some sense, as a "collaborator" in that intersubjectivity where meaning itself resides.

The general point is made by McHugh et al., (even though for them "collaboration" is <u>between</u> theorists - see Chapter Two, p.75). Their argument is that there can be no finality to speaking, since to speak is always to assume (and hence, at that moment at least, to forget) the grounds of that particular speaking. Writing can thus never be <u>complete</u> for the basic reason that it needs to be read: "The papers in this book should be conceived of as displays which require alters. This is where readers come in. Readers are asked to treat our papers reflexively. They are asked to become our collaborators. That is our version of how to read". (McHugh et al., 1974, p. 8). This emphasis on the central theoretical importance of the creativity of the reader is found in the work of Barthes, as is the corollory that such a notion of creative reading

must involve abjuring or abolishing the unchallengeable
authority of the author as a source for meaning.
(Barthes, 1977).   Similarly Alan Blum (1974, p. 252)
empahsizes that "Speaking is controlled by its Rational
relationship to hearing".

Hence what McHugh et al. refer to as "the exemplary
character" of their work (op.cit. p.12).   The theorizer
formulates an example not to provide an exhaustive
description (impossible) nor to attempt a complete
description which is regrettably doomed to incompleteness
because of inadequate methods or funds (positivism):
rather, the example stands as a provisional accomplish-
ment and an invitation to the reader to continue
theorizing.   (What sort of an example is this?
What would be a further or a contrasted example?   How
was it chosen?)   Examples only exist as such insofar
as they are elaborately embedded, and thus are only to
be understood by an act of constructive responsiveness:

> "Speech, except by example, would have to be
> perfect speech ...   Example is to say the
> speech is imperfect because it does not speak its
> own auspices, but usable because it allows alter
> (the reader) to formulate its auspices".
> (ibid., p. 10).

Hence, speech can only proceed on the assumption
that there is Another, whose rational being can only
be postulated by analogy with, in reference to, the
self-concept of the speaker.   Whilst the irremediable
indexicality of speech ensures that no speech can claim
the finality of correctness (as in "logic" or "science"),
this is not a regrettable, defeated lapse into solipsism;
rather, the reflexivity of speech, by anchoring the

reality of the Other in the reality of the Self, through the processes inherent in acts of communication, allows (indeed _requires_) us to regard the Other and the communicative process as equally real along with the Self.   (See McHugh, et al., 1974, final paragraph). In other words, the reflexivity of language entails a dialectical ontology of consciousness, a dialectic between Self, Other, and Symbol, to which the argument now turns.

Dialectics: Self, Other, Language, Being, Time

The general project of theorizing proposes a relationship between self and world such that the _develop-_ _ment_ of understanding is possible; the first question, then, is: what conceptions of consciousness are compatible with knowledge as a reflexive and developmental project?   It follows from the considerations in the previous section that we cannot formulate a merely receptive subjectivity which simply _registers_ the existence of an external object, since this would return us to a version of language as a system of descriptive labels for their external referents.   Sartre, for example, rejects this simple dichotomization of consciousness and its objects as an "abstraction" (Sartre, 1969, p. 3) on the simple phenomenological grounds that "all consciousness is consciousness of something" (ibid., p. xxxvi).

In this he follows Hegel, and Hegel's elaboration of the point begins to display the sense of a dialectical (rather than a dichotomous) relation between Subject and Object, within which a knowledge-constitutive process may be formulated. Hegel observes:

> "Consciousness knows SOMETHING; this object is the essence of the IN-ITSELF; but it is for consciousness the in-itself. This is where the ambiguity of this truth enters. We see that consciousness now has two objects: one is the first IN-ITSELF, the second is the being for consciousness of this in-itself ... The first object, in being known, is altered for consciousness".
>
> (Hegel, 1977, p. 55).

This self-consiousness of the act of perception leads each perceived quality to be surrounded by the awareness of its potential variants, and hence to have a necessary dimension of incipient self-transcendence, a sort of spontaneous disunity. From this starting point (see Hegel, 1977, pp. 58-64) Hegel traces an ontology of "The Thing" as a "manifold" of contradictions, experienced in almost instantaneous succession as a single essence and a plurality of qualities, as universal and specific, as self-defined and as defined-in-relation-to-another" (ibid., pp. 67-71), in short as "a whirling circle" (ibid., p. 79).

Further dialectical complexities follow. Firstly, since the consciousness which perceives the thing cannot distinguish between the thing, consciousness itself, and the act of perception, the constitution of the thing in consciousness becomes an act of self-definition: the complexity of the thing (the manifold of contradictions)

is reflected back into the structure of consciousness itself. (Ibid., pp. 73-5). Secondly, this complex structure of the process of experience can never be directly grasped by language, since language can only utter the general: our unique and fleeting experience of, for example, "this tree here" or "that house now" is swept away in our uttering of "tree" (referring to any tree), "here" (referring to any place), and "this" (a reference from anywhere), and so on. As Hegel says: "It is just not possible for us ever to say, or express in words, a sensuous being that we MEAN" (ibid., p. 60).

With a similar purpose, Heidegger criticizes the equivocation which secretly undermines the pretension to unity and integration of classical logic. Logic, he says, is based on the distinction between subject and predicate, so that rules concerning the compatibility of the two can be devised, as a way of evaluating propositions (eg. the rule of no self-contradiction (Heidegger, 1968, p. 155)). But since propositions contain these two elements - subject and predicate - the main emphasis can be on one or the other, so that meaning has an inherent ambiguity, which cannot be accommodated in a hierarchically unified rule system, and thus, of necessity, "Logic becomes dialectic" (ibid., p. 156).

The rejection of classical logic is a refrain throughout What is Called Thinking? and indeed, the second series of lectures in that volume may be seen as an elaborate struggle against the structures of

conventional syntax in order to rescue the freedom
and creativity of thought ("Thinking") from the
stultifying restrictions of routinized linguistic
usage: "Every dialogue becomes halting and fruitless
if it confines itself obdurately to nothing but what
is directly said". (Heidegger, 1968, p. 178).   In
other words, Heidegger's argument is that understanding
cannot be accomplished simply by using language itself
as a set of tools, to be carefully honed and skilfully
manipulated as a descriptive ordering of reality
(ibid., p. 153).

Thus, since language cannot simply "label" exper-
ience, perception is never a uni-directional process but
always "a reciprocal interplay" (Hegel, 1977, p. 84)
in which successive stages of awareness "are themselves
self-superceding aspects" (ibid., p. 81).   It is
interesting that Lenin founds his notion of a dialectical
cognition in a theory of language which he explicitly
derives from Hegel:

> "Dialectics is general as a method since, as Hegel
> noted, every proposition itself contains the
> notion of the contradiction of the relation
> between universal and individual".
> (Lenin, 1972, p. 361).

This "contradiction" would, for Lenin, exemplify a
"unity of opposites" which is "the condition for the
knowledge of all processes in the world in their self-
movement, in their spontaneous development" (ibid., p. 360).
In this way, we may formulate a dialectical knowledge-
generating process, in which Subject and Object, individual
and universal, are inseparably bound up in a process of

reprocity and self-transcendence which constitutes the
mode of being of human consciousness itself. However,
"self-transcendence" is a rather elusive way of
formulating this active principle of cognition. How
might it be elucidated?

The most famous version is Descartes', which may
be summarized as: "I doubt the world; therefore I
think; therefore I (doubtless) exist". Sartre
explains in more detail. When consciousness registers
the presence of an object, the acceptance of an idea,
it is caught up by, and causally determined by "the
positivity of Being" (Sartre, 1969, p. 23). This is
consciousness without consciousness of itself as
consciousness, and as such it is incomplete or at
least untypical (cf. the state of being "lost in"
contemplation). For Sartre consciousness is always
conscious of itself as not identical with its object,
which he terms consciousness's "negative" aspect. For
example: "To believe is to know that one believes,
and to know that one believes is no longer to believe".
(ibid., p. 69). Or, concerning objects: "absence
appears necessarily as a pre-conditon of presence".
(ibid., p. xxxvi), ie. objects are present to conscious-
nessnot as ineluctable causes of their appearance, but
as contingently present, always potentially absent.
The intelligibility of the experience of being is thus
founded upon a sense of the discontinuity between
consciousness and its objects; otherwise, for example,

"my present state would be (determined as - RW) a prolong-
ation of my prior state" (ibid., p. 28) and thus identity
would lose its temporal dimension, which is a precondition
of the experience of identity (see Kant, 1933, p. 79).
Similarly, for the asking of a question to be intelligible,
the questioner must "have the permanent possibility of
dissociating himself from the causal series which
constitutes being" (as unquestionable - RW) (Sartre,
1969, p. 23).   And because of this "impalpable fissure"
(ibid., p. 77) which constitutes the experience of self
in relation to world as one of possibility (it happens
to seem thus now, but it could, has been, and will be
different) consciousness cannot help but exist as a
questioning state: "The being of consciousness is a
being such that in its being, its being is in question"
(ibid., p. 74).   And this takes us back to Descartes,
with "doubt" now established not as a technique or a
choice, but as the very condition of being, as
constitutive of consciousness itself.   It also enables
us to ground ontologically the competences whereby the
reflexive procedures which render experience intelligible
may be submitted to the analytical questioning of the
theoretic subject.

This last point suggests the structural parallel
whereby "reflexivity" and "dialectics" analytically
require each other at the levels of both epistemology
and ontology: the intelligibility of theorizing entails
theorizing (reflexively) with others (see p.23 above),
and the intelligibility of Being entails Being-with-Others.

Thus, Heidegger says: "The world is always the one that I share with Others" (Heidegger, 1962, p. 155). And: "Dasein's being is Being-with, its understanding of Being already implies the understanding of Others" (ibid., p. 161). Hegel's general argument also clearly links the two themes: it is through the dialectic between Self and Other that consciousness can develop towards "self-consciousness", ie. the comprehension of its own reflexive nature. Hegel phrases it as follows: "Self-consciousness ... is the native realm of truth". (Hegel, 1977, p. 104). It "exists in and for itself when and by the fact that it so exists for another". (ibid., p. 111). This is because "being-for-itself" can only attain certainty of itself (ie. "its truth") by "confronting" itself, and this is only possible when, between two self-conscious beings, "each is for the other what the other is for it" (ibid., p. 113). The true nature of one's own being is thus only achieved by confronting another (as the representative of oneself) in a "struggle" whose prize is simultaneously freedom and truth (ibid., p. 114).

Although Hegel goes on to speak of the winners and losers of this ontological struggle in terms of "the history of Spirit", it is clear that at another level he is formulating what might be called an "interactive" dialectics of ("truthful") understanding, which he embodies in a sequence of "ideal types" of increasing complexity:

a) the "bondsman" conceives himself as an object;
   (ibid., pp. 117-8);

b) the "lord" conceives the other as an object; (ibid.,
   p. 118);

c) the "stoic" achieves a fragile freedom through
   withdrawal from the other - unity at the cost of
   isolation (ibid., p. 122);

d) the "Sceptic" transcends isolation at the cost of
   internal contradiction, <u>oscillating</u> between conceiving
   himself as free and as contingent: he experiences
   the dialectic of Self and Other as a struggle, but
   does not recognise its structure (ibid., pp. 123-6);

e) the "unhappy consciousness", which suffers the sense
   of its own contradictory <u>and yet</u> unified (ie., in the
   strictest sense, "dialectical") structure.   (ibid.,
   pp. 126-32).

Without following through Hegel's evocation of the
progressive history of "Spirit", one can note the
analytic value of a dialectic which thus concretely
protrays the self-transcendental development of <u>self</u>con-
sciousness, and which progressively reveals the conditions
of its own possibility to be its own dialectical
structure.   Following on from previous arguments (see
p. 20 above), it is notable that Hegel's dialectic moves
progressively from "stratifying" towards increasingly
reflexive forms of consciousness.

There is one further theme which is essential in
a discussion of a dialectical formulation of consciousness
for theorizing: that of its <u>temporal</u> dimension, within

which theorizing and consciousness must be situated

for either to be intelligible, and which in particular

seems to be implicit in action-research's coupling of

action AND research.   If action is linked with research,

then it must be more than an instantaneous - and thus

a-temporal - response; and if research is linked with

action, then it must be more than an instantaneous -

and thus a-temporal - observation.   Or, in other

words: only if "meaning" must always be negotiated

within the temporality of experience before it is

imputed, can the possibility of "other meanings"

conceivably be explored within a (temporal) process of

inquiry.

Although Kant himself posits Time as a constituent

category of experience (see p. 9 above), Heidegger is

concerned to rescue understanding from the abstract

instant of Kantian "intuition" (Heidegger, 1962, p. 410),

and to rescue truth from "the superficial theory of

propositions and judgements" (ibid., p. 401), by locating

both understanding and truth within the temporality

of Being:

> "Only in terms of the temporality of discourse -
> that is of Dasein in general - can we clarify how
> 'signification' 'arises', and make the possibility
> of concept-formation ontologically intelligible".
> (Ibid., p. 401)

Time, here, is not the chronological sequence of discrete

"perceptions" by which a determinist model of cause and

effect is erected, but the temporal dimension within

which the Self is grounded in a structure of potential

"authenticity", ie. of "Care" (ibid., p. 370) with respect to its past and its future (ibid., p. 390). This Care-ful sense of responsibility towards truthful understanding is differentiated from "everyday interpretation" (ibid., p. 358) and from that mere "curiosity" which, says Heidegger, seeks the future only in order to make it into the present (ibid., p. 397); which, in other words, by seeking to make discoveries which are both "new" and final, thereby aspires to convert the temporality of future possibilities into a time-less present of unchanging certainties. In contrast, Heideggerian "Care" may be seen as that overarching principle under whose aegis the process of inquiry proceeds when it addresses the grounds of its speech in the necessarily temporal structures of experience, symbolization, a nd understanding, and thereby provides grounds for its implicit commitment to an unending dialectic of developmental and reflexive understanding.

Commentary: Grounds, Texts, Possibilities, and Resources

In this Introduction I have tried to provide grounds for this writing. It is important to stress that these "grounds" are not intended as an origin, a set of principles prescribing the remainder of the work as necessary, nor as a declaration of allegiance to a school (of theory), showing how a few general beliefs subsume a variety of specific interpretations. Rather,

since this is being written <u>after</u> the main text, it
attempts to show the <u>possibility</u> of the work, now that
it has been explored through, and now that the text has
been reviewed and revised in the light of the inevitable
first question: having written this, what made it
possible? More elaborately: what notions of
subjectivity, object-world, and language are consistent
with the activity by which the subjectivity currently
wielding this retractable pencil is assembling this
text now? Hence, a brief comment on "texts" seems
required.

Since my text seems to display a Subject re-viewing
a range of cultural resources (the published texts
of McHugh, Hegel, Lenin, Krech and Crutchfield, and
Kant, for example) in order to assemble its own speech
relating to a current concern (action-research), it
would be Quixotic (<u>Another</u> resource!) to propose this
text except as a resource whereby its readers can review
concerns: texts do not prescribe meanings for readers;
they set meanings in "play":

> "Writing (does not) designate an operation of
> recording, notation, representation, 'depiction' ...
> (it) traces a field ... which, at least, has no
> other origin than language itself, language
> which ceaselessly calls into question all origins".
> (Barthes, 1977, pp. 145-6)

Reflexivity predicates the intelligibility of writing
upon the reality of the reader, upon the inevitable
<u>question</u> of language's origin in the Other. In this
sense, the theoretic text can be seen in the light of

Wolfgang Iser's comment on the act of reading a literary
text: "Textual repertoires and strategies simply offer
a frame within which the reader must construct for
himself the aesthetic object" (Iser, 1978, p. 107).
The "theoretical object" also is constructed by writers
and readers through the repertoires and strategies
of language, as organized provisionally in a "text".

In that a text is predicated upon the reality of
a reader, as the necessary presumption of the intelligi-
bility of the act of writing, a text is essentially
constituted not as a legislative declaration, but
as a horizon of possibilities. It is this set of
possibilities which is given by the "play" of the
text: a ball "in play" is open to the unpredictable,
skilful, improvisatory contingencies of the game-process
and its idiosyncratic players; only "out of play"
does it become subject to a single prescriptive rule.
"Play", here, is therefore used in the sense of creative
exploration (cf. theories of child development - see
Jenks, C., 1982, p. 22). "Play" is a metaphor for the
creation of meaning, and thus a metaphor for metaphoricity
itself, that metaphoricity of language which maintains
the interpretive open-ness of texts, and thus makes possible
the creation of this text. In contrast, a non-playful
text, in which, say, a prescriptive logic claims to
organize correctly a system of propositions, in which
language is supposed not to play but to work within a
framework of tautologically valid definitions and object-
ively accurate references; such a legislative text must

propose to annihilate the legislations of its predecessors and thus fears its readers as potential executioners: since it proposes its own finality, its own existence as a text becomes an anomaly, a hopeful exception to the very rule whereby it supercedes its predecessors.

But "possibilities" are presented here only as preliminaries, as providing an _intelligibility_ for theorizing as a project: "possibilities" do not remain "an open horizon"; they are culturally located as a set of _limits_ and resources. As Alan Blum says (1971, pp. 301-2): "Theorizing ... constitutes a particular method for treating and reconstructing one's biography as a practically conceived corpus of knowledge". Or, as John O'Neill quotes from Merleau-Ponty: "Expression is always an act of self-improvisation in which we borrow from the world, from others, and from our own past efforts" (O'Neill, 1972, p. 95). Now, my resources and thus my limits for theorizing are inevitably i) my membership of a societal community and of a number of epistemic "sub-communities" - professional, academic, political, domestic, etc., and ii) my conception of theorizing itself as my most complete and most fully grounded and articulated response to i). For _me_ to theorize must involve me in seeking to integrate, transform, and transcend _my_ actual resources, and similarly for others. Hence, in a sense, knowledge and cultural tradition are biographically contingent and thus mutually "limiting".

However, this is merely to say that self and
cultural tradition define each other: the self as a
creative and changing "improvisation" constitutes and
is constituted by culture as membership in a plurality
of possibilities. Hence, theoretical resources and
limits may be conceived without exclusivity or legislation,
and not merely in an academic dimension: in that
theorizing is above all an engagement with the reflexive
processes of language and its relation to truth and
experience, its resources are not just those of "sociology"
and "philosophy"; rather, its resources are potentially
as wide as the social world which is so overwhelmingly
constituted by linguistic practices. The linguistic
practices of the social world include not only the
"mundanely reflexive" talk whereby members accomplish
practical interaction (Garfinkel's topic), but also the
playful, creative practices of joking, word-games,
crosswords, satire, and parody - practices which implicitly
but with varying degrees of elaboration begin to take
mundane intelligibility as their topic - and also the
widespread aesthetic practices of songs, films, dance,
and narrative fiction (in print and on radio and TV),
where again the mundane world is framed, re-structured,
and thus - in varying degrees - made available for
re-formulation (see p. 21 above). Thus, just as
"possibilities" do not mean that any formulation is possible,
nor that one correct possibility can be selected from a
universal series, "limits" do not mean that cultural
tradition imposes itself as a final closure, since culture

and tradition have their own constitutive play-fulness.

This relationship between possibilities and limits
applies to the ensuing work in two ways. Firstly,
it evokes the sense in which the present text consults
(and yet is constituted in) a series of resources for
the conducting of its argument: its process is not the
construction of an authoritative corpus which might
then be evoked as a legislation, since this would be
to deny the interpretive status of the meanings these
textual resources can have for my argument. Resources
are consulted to set meanings "in play", as the only
rigorous and self-consistent sense for a reflexive
and dialectical theorizing. Secondly, and consequently,
the following attempts to provide theoretic grounds
for action-research do not seek a site outside the
culture in which action-research operates from which to
impose upon action-research a legislative framework.
Rather, in each chapter begins with the set of possibili-
ties and limits which action-research itself invokes,
and proceeds to assemble and explore the further
resources which action-research's self-proclaimed aspira-
tions and problematic seem to require. This introduction
has sought to delineate the analytic limits within which
such resources may be conceived to be on the one hand
required and on the other hand available.

CHAPTER TWO

ACTION-RESEARCH, ACTION, AND RESEARCH

"Action-research"

What is the fundamental problematic within which "action-research" attempts to formulate its aspiration? Simply and basically, the impulse towards "action-research" originates in the attempt to question what is taken to be a conventional differentiation between "action" and "research".   Thus: "A realistic view of both action and research reduces the difference between them" (Halsey, 1972, p. 178).   Alfred Clarke makes the point in slightly different terms: "Action-research ... follows Popper's idea ... that all social administration should be conducted as experimentation" (Clarke A, 1976, p. 1) and thereby "combined discovery and implementation in one process" (p. 2).   By formulating discovery ("research") and implementation ("action") as one process rather than as two distinct processes, Clarke argues, action-research will be able to ensure that the "findings" of research will be "applied" to action, and thereby also ensure that the research efforts of "social science" will be able to claim "relevance" (ibid., p. 2).   Thus "action-research" poses for social science the challenge of relevance.   Jon Nixon, for example, begins, crudely:

> "What is educational research?   Disputation on
> irrelevant issues in impossibly esoteric journals ...?"

before going on to formulate "action-research" as "research ... initiated, conducted, and disseminated from the inside" (Nixon, 1981a, p. 5) - ie. from a vantage point where

"relevance" could not be questioned.

Clarke's version of action-research's question takes the form of a paradoxical juxtaposition: "social administration" evokes a form of life in which bureaucratic procedures require a specific emphasis on the predictability of rule-guided actions, where knowledge of general rules is invoked (deductively) as the authority for particular actions; "experimentation" in contrast conventionally suggests an attempt to derive the authority for general rules (inductively) from the knowledge of particular actions - ie. action-guided rules. Action-research asserts the unity underlying this distinction in order to assert an ambiguity underlying a simple polarization, an ambiguity which must be faced at the level of epistemology, in theorizing the cognitive practices which relate the rules of knowledge and action. Hence Clark's parallel rejection of a dichotomy between discovery and implementation: action-research desires an epistemology which will in principle transcend the terminology of journeys of discovery (where "truth" is "somewhere else") and of implements (which exist in themselves and may or may not be utilized) - cf Heidegger's various arguments against language and artefacts as "tools" (Heidegger, 1971). The nature of this proposed epistemology, the nature of the principles which underly the invocation of the "one process", is the theme of this work.

But in spite of its bold speaking for a problematic unity against established separations, action-research

also recognizes its fragility: as Halsey observes:
"Bringing together what is normally conceived as separate
has caused the confusion over the nature of action-research"
(Halsey, 1972, p. 178), a confusion created in part by
the attempt to integrate procedures and professional
roles with different traditions, methods, styles, and
interests (ibid., p. 165). The challenge of action-
research is thus not only epistemological but political
(and indeed also a challenge to that very distinction)
(see Chapter Five, p.241). In institutional terms action-
research wishes also to recover unity, the unity which
a "division of labour" in the production of knowledge
has fragmented and thereby lost. Action-research speaks
for the possibility of a set of social relations in
which "Theory" and "Practice" are no longer institutionally
segregated around a dichotomy which fractures the coherence
and rationality of social inquiry and creates a "problem
of relevance". Hence it is central to Clark's assertion
that in order for action-research to avoid the problem
of "relevance" the researcher must act "in collaboration"
with the subjects of the research, so that his problems
are also their problems. In this way, he claims, the
experimental "additions" to the situation do not need
"partialling out or controlling" (p. 1). Thus a concensual
politics of inquiry is proposed as a resolution of a
methodological dilemma.

This theme of a collaborative relationship underlies
many formulations of action-research's ideal. For example,
Cory (1953) observes:

"(Action-research is) a cooperative activity:
interested parties to the action proposed need to be
collaborators ..." (p. 18).

Similarly, Eric Midwinter (1972) explains:

"So we had to join in dialogue with the schools ...
By these means we endeavoured to relate real and
ideal, or, to put it another way, theory and
practice". (p. 56).

And Brown, Henry et al. (1982):

"Action-research is distinguished by its adherence
to a collaborative ethic". (p. 4).

In putting forward this "collaborative" principle,

action-research explicitly adopts from Habermas a problem-

atic of concensus formation as a basis for truth

(see discussion in chapter three, pp.   below) and thereby

opposes what Brian Fay (1975) describes as the "control"

problematic of positivist social science.   The quotation

from Clarke (above) uses "control" in a methodological

context, but Fay himself makes clear that he sees the

political sense of "control" as a significant metaphor,

even a systematic isomorphism between a conception of

inquiry and a conception of political order (Fay,

1975, p. 58).   A similar line of argument is implicit

when action-research denies the claims to cognitive privileg

made by institutionalized "science", ranging from

Midwinter's characterization of "University research"

as "wishing to stop life in order to measure static,

that is, unreal siutations" (Midwinter, 1972, p. 50)

to Jon Nixon's presentation of his book A Teachers' Guide

to Action-Research as "a radical alternative to the

paternalism of traditional research" (Nixon, 1981a, p. 9).

In order to provide a theoretical basis for action-research, therefore, the nature of the relation between research and action must also be analyzed as a social relation: how can the theoretical <u>authority</u> of a research stance be distinguished (in its relation to action) from the institutionalized <u>power</u> of research initiatives, in terms of political, economic, and ideological features?   What forms of interpersonal collaboration could possibly address "the division of labour' between action and research as a <u>problem</u>?   And there is a further question: Nixon's "radical alternative" is exemplified largely by cases where practitioners' research efforts take the form of <u>self</u>-evaluation; and so we must ask: what version of subjectivity could enable research and action to be carried out as a dialectic of self-transcendence by one person?

Underlying all these questions is one question: how could "action" and "research" possibly be separate, and, conversely, how could they possibly NOT be separate? In asking the question, one is not seeking to arbitrate between two potential <u>answers</u> ("action and research ARE separate" versus "action and research ARE NOT separate"): rather the asking is an attempt to recover the complexities implicit in the possibility of the question itself. (See Heidegger: 1968, p. 159).

To note action and research as a difference is to note that action proceeds on a basis which must always fall short of a theoretically conceivable certainty. The knowledge which guides action can always provisionally

be deemed to be sufficient for that course of action at
that time, but it can also always be deemed insufficient,
in the light of a notion of "greater understanding", which
not-action-but-research could possibly create. The
separation of action and research is thus one articulation
of a faith in the possibility of change: action is con-
ceived as meshed (however loosely) into a social system,
whereas research is the process whereby the self-perpetuat-
ing processes of that system might be interrupted.
However, although the possibility of change is grounded
in the distinction between action and research, it requires
equally an intimate and principled linkage between the
two, in order that the "findings" of research can be
translatable back into the world of action: indeed the
intelligibility of the metaphor of translation requires
both difference and similarity.   In this way action-
research's question is revealed as an insight into a
complexity: a conception of the rational development
of the social world and the possibility of inquiry into
the nature of that rationality require that action and
research be both distinct and mutually required.   This
mutual relationship between the two, as elements in a
dialectical progression, is what is glimpsed in the
action-research literature, but what is not recognized
is the theoretical necessity of a reflexive conception
of research's relation to action, so that their relation-
ship may be theorized in ways which (as action-research
also urges) preserves the authenticity of both, ie.
which preserves research's capacity for achieving a

critical distance from action AND preserves action's
intelligibility, as a creative, rather than a causally
determined response to social meanings.*

Action-research thus renews a long-standing debate
in sociology concerning the proper relationship between
social action, the "common-sense rationality" of mundane
social action and the "scientific rationality" of
social investigation; and it renews its question in
the light of a commitment to the possibility of rational
procedures both for valid critique and for justifiable
change.

Action and Research - The Evaluative Relation

One of action-research's central formulations of
the unity and intelligibility of its project is that research
can be the _evaluation_ of action: "Action-research is ...
the study of a social situation with a view to improving
the quality of action within it" (John Elliott: 1981,
p. 1). In this respect, action-research is not different
from other attempts by sociologists to formulate the
link between research and action, for example Wilkins
(1967, p. 109): "Social action ... should be evaluated

* For example, R. Lees (1975) writes, concerning "The
Action-Research relationship":

"The Community Development Project anticipates an intimate
and productive working relationship between administrators,
field workers, research-oriented social scientists and
local residents ... The aim of promoting participation
is in itself seen as a desirable goal ... The research
problem in this situation is to define participation ..."
(pp. 59 and 61).

and this means research". Thus a central question
becomes: from which site might research be carried
out from which it could claim to judge the "quality" of
action? Many action-research theorists have been
content to follow sociologists such as Wilkins and to
answer: the objectivity of scientific method. Thus
Alfred Clarke (1976) begins: "At the centre of action-
research lies the traditional scientific paradigm of
experimental manipulation and observation of effects", (p. 1
For this to be possible it must be assumed that the
researcher is able to be both present to the action (in
order to manipulate a phenomenon which really is the action
under investigation) and absent from the action (in order
to observe the action without affecting it). (The
importance of the complexity underlying this proposal
is brought out in Phillipson: "Sociological Practice
and Language", 1981).

This complexity is focussed by Clarke when he
goes on to suggest that researchers and their subjects
should "collaborate" in the formulation of "problems"
(op. cit., p. 1). In this way, for Clarke and others,
research's distance from action does not take the form
of a different set of questions, and thus action-research
appears to abandon one plausible way of formulating
the site from which evaluation could be carried out;
namely, that research has its own specific interests -
a formula which underpins much writing on social science.
Norman Denzin for example defines the research act as
"those endeavours of the sociologist that take him or

her from theory to the empirical world and back again"
and he elaborates: "I assume that the only justification
for an empirical observation is the refinement, develop-
ment, or refocussing of social theory" (Denzin, 1978,
p. ix). In contrast, the "action-research act" would
wish to be an endeavour leading from the empirical
world to theory and back again: "The research needed
for social practice can best be characterized as research
for social management or social engineering. It is a
type of action-research, a comparative research on the
conditions and effects of various forms of social action,
and research leading to social action". (Lewin,
1946, p. 35).

If, then, for action-research, research shares with
action a cognitive interest in managing or constructing
that same social world which is the arena of action,
could it be that research's independence from that
world rests in its methods of understanding, and if so,
in what respect? Cory (1953) suggests that "the
most important characteristic that differentiates action-
research from more casual inquiry is that evidence is
systematically sought, recorded, and interpreted"
(p. 26). But in what sense could action be thought of
as based on "casual" evidence? Not, surely, that
action is careless about its outcomes or its grounds.
On the contrary, as Garfinkel's work has shown (Garfinkel,
1967) action is always most care-ful to construct its
rational basis. Garfinkel's theme is precisely that
social actors seek, record, and interpret evidence in

ways which are elaborated, flexible, interrelated, & acceptabl

coherent.   In what sense could these procedures

be identified as <u>not</u> "systematic"?   Could it perhaps

be suggested that action is systematic in its improvisa-

tion of methods in a <u>particular</u> case, whereas perhaps

research (for Cory: action-research) wishes to specify

the system of relations between evidence and action

separately from and in advance of any particular case?

But the impossibility of achieving this uncontexted

version of "being systematic" has been argued at length

by Cicourel (1964).   For example:

> "The logic of everyday activities in which the
> social object under study is embedded must be
> related to the logic of the observer's theory
> (but) the transformations which relate one system
> to another and the language which describes each
> system taken separately and both systems taken
> together will never be perfect.   There can be
> general congruence but not perfect correspondence".
> (p. 186).

In other words, language's attempts to be "systematic"

would become enmeshed in the inextricable embeddedness

of its processes in the reflexive interpretive procedures

by which alone it "means":   even the modest sufficiency

of "a general congruence" would have to be decided upon

in each situation as "sufficiently congruent" for this

here-and-now purpose.   The theoretical impossibility

of ever "being systematic" in an absolute sense only

serves therefore to raise yet more sharply the question:

why might research wish to <u>claim</u> such a possibility?

And thus one returns to the basic concern of research

to subject action to a form of judgement which, research

claims, action itself avoids.

We have already seen that Lewin characterizes the aim of action-research as "social management": later in that paper he says that the evaluation of action programmes will occur as a result of "fact finding" (Lewin, 1946, p. 58). Elsewhere he states: "the aims of action research are to bring about certain changes under sufficiently controlled conditions in order to understand the laws which govern the nature of the phenomenon under study" (quoted in Foster, 1971, p. 3). In other words, the method of "controlling" variables will achieve the aim of creating the "factual" basis on which the effectiveness or otherwise of "social management" may be evaluated. In this way Lewin takes over, for action-research, the evaluative site of natural science: research can treat action as behaviour, as phenomena which are governed by laws-of-nature and thus may be managed by being understood. The unacknowledged complexity of Lewin's version of the research / action relation is that which is noted by Brian Fay: the metaphor of "control"— which evokes a critical stance towards the evidential basis of social action but an acquiescent stance towards the purposes of social action. Analytically the problem is that Lewin does not address the ambiguity of presence / absence, of critique and acquiescence in the relationship between action and research as he himself formulates it. So we must ask: on what basis may research take action's purpose of social management as an unquestioned resource rather than as a topic for critical inquiry? And we can only answer: by failing to notice the reflexive basis of its own activities in those socially defined purposes.

Further: on what basis may research formulate its difference from the presupposition of mutual inter-subjectivity which governs social action by treating the actions of members as observable behaviour ("facts"), while not addressing the apparent consequence that (in order to be consistent) research's own activities would need to be treated in the same way - as behavioural data obeying **interesting general** laws? On this basis research's claim to have good grounds for distinguishing itself from other actions would seem to be annihilated by the very form of the claim, by the form of those grounds.

In this way, Lewin's version of the research / action relation seems to embrace an ambiguity without analyzing its terms, and in this respect its analysis seems, if anything, weaker than the conventional "applied social science" action-research wishes to oppose. Wilkins (op. cit.) for example addresses in more detail both the unity and the difference of purpose which relate action and research. On the one hand, the unity of purpose which enables collaboration:

> "Those who wish to evaluate social action and test the effectiveness of social agencies want to do so for the very same reasons as those who plan the work of such agencies ... wish to do the action part. The work of social rehabilitation, reconstruction, preservation, and preventive action is a joint enterprise for action and research" (pp. 9 -10).

On the other hand, the difference which requires collaboration: the world of social action is in principle conservative, muddled, and defensively obscure

(pp. 26-7) so that "scientific method" can operate as a "court of appeal" (p. 27), using "measurement" as its tool (p. 10), enabling knowledge to be tested, ignorance to be admitted, and "meaningful" questions to be asked (p. 27). Wilkins has two striking metaphors for the principled difference between action and research. The first is concrete: research is a "raiding operation" upon the world of action ("enemy territory") (pp. 108 ff.), which adds a dramatic dimension to Brian Fay's image of science as <u>control</u>, and casts an ironic light on the "joint enterprise" in which both raiders and raided are said to be engaged. The second is highly abstract: research and action, like ends and means, science and ethics, are related as "possibly orthogonal dimensions" (p. 25), suggesting a principle of unlimited independent variation between them. Nevertheless:

> "the scientist should be integrated into the system (ie. of action, of social administration - R.W.). Both social research and social action are concerned, for essentially the same reasons, with the same objectives ... If we believe in democracy, then we should not seek to apply autocratic or dictatorship methods in the sub-world of social action, social policy, or social research" (p. 34).

Here we can see once more the political metaphor underlying the epistemology: the liberal "separation of powers" in opposition to the "monolithic" social unity of dictatorship: only through its independence ("orthogonality"), even to the point of a principled hostility to its environment ("raiding"), can research act as an incorruptible court of appeal for action, and thereby guarantee <u>not</u> (as Brian Fay would have us believe) an effectively <u>controlled</u> society, but a

democratically open, progressive society, based on the possibiloty of effective evaluation, ie. the asking of "meaningful" questions in order to subject action's presumed knowledge to research's objective testing.

We may agree with Wilkins in wishing to establish the possibility that research should have a site from which to subject action to critique, under the aegis of canons of Reason which are distinguishable from those of action; and that on such a possibility depends the notion of the democratic conduct of social affairs. What must be placed at issue, however, is whether Wilkins, and other sociologists invoking the quantifying rigour of "scientific method" have formulated adequately the basis for such a site. The following objections start from Wilkins's own formulations, but implicitly refer to the general stance, of which, in this respect he may be taken to be representative.

First, in constructing "science" as a source of prescriptive social authority, Wilkins does not explain why science itself might not become yet another social institution characterized by the defensive conservatism which - he says - is typical of other institutions.* Secondly, Wilkins constructs the authority of science as a prescription precisely by ignoring the ambiguities implicit in the metaphorical basis of his formulation, metaphors whose inevitable ambiguity renders problematic the very authority Wilkins claims they assert. How can the difference between action and research, science

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* This is indeed the point urgently at issue between
  Kuhn and Popper in their contributions to Musgrave and
  Lakatos's volume: Criticism and the Growth of
  Knowledge (1970).

and ethics, be presented in terms of the geometric
construction of a perpendicular ("orthogonal")?
Only by the prior assumption of mathematics as a realm
of simple essences which may be invoked to order the
complexities of the social world.   This is the myth of
geometry, the aspiration of man towards a divine
abstraction, the aspiration of knowledge to move from
Garfinkel's obscure jury-room on to Plato's sunlit
hillside;   a poetic image which enacts the actual
complexity, the hubristic risk, of analysis, while
describing its apparent simplicity as a manageable
technical accomplishment.   (The nature of such contra-
dictions as the underlying structure of myth is of
course Levi-Strauss's theme.   (See Chapter Four, pp.   )
Again: Wilkins elaborates at great length the imagery
of wartime operations against an enemy in order to
evoke the contribution of research to a consensually
agreed project of "social rehabilitation, reconstruction",
etc.:   the metaphor both affirms and denies the taken-
for-grantedness of the social values at stake - it
affirms the necessity for prior commitment, but denies
that the shared commitment is shared: the researcher
is both enemy and partner.   Wilkins affirms science's
independence and clarity of analysis while exemplifying
the dependence of his own analysis of that clarity on
complex ambiguities which he treats as not requiring
analysis.

Third, in the same way as he treats language as
merely "conceptual" by denying its metaphoricity, he
asserts the possibility of treating measurement as mere

quantification, by denying its indexicality - its reliance on those interpretive judgements by which phenomena are chosen to be counted - or not, as the case may be. Again, this ignores Cicourel's well-known critique of measurement's claims to mechanical replicability, claims which Wilkins rehearses fully, by means of a "philosophy of measurement" derived from a model of language as the transmission of messages between coders and decoders (pp. 183-4). Again we have a highly evocative metaphor, which imposes the intelligibility of quantification upon language, as the myth of science's authoritative method, and which needs to be explicated through an analysis of the activity of language which would formulate the relation in language between acts of numerical awareness and acts of metaphorical generalization.

Such versions of the possibility of research as the authoritative evaluation of action thus rest on a number of crucial simplifications and impositions concerning the research / action relationship, which result in a prescriptiveness of method and a restrict-iveness of truth criteria which action-research wishes precisely to avoid:

> "Action-research is nothing if not eclectic. This eclecticism may prove to be a stumbling block to the reader who has too narrow a view of educational research. A conscious effort should be made to bracket any preconceived ideas concerning the correctness or otherwise of a particular research model. What matters is the extent to which the model is appropriate; appropriate to the skills of the teacher, the constraints of the classroom, and the nature of the problem to be explored". (Nixon, 1981a, p. 7)

However, mere eclecticism opens up a whole range of crucial issues concerning criteria for validity (see Chapter Five), and is thus no remedy for the inadequacy of Lewin's attempt to preserve the framework of a positivist epistemology while minimizing the distinction between research and action on which such an epistemology depends. And we have also seen that, for example, Wilkins's more coherently positivist account of research's claim to possess evaluative authority over action also fails, in a number of ways, to address analytically the basis of that claim. My argument therefore turns to consider in detail those exponents of action-research who have attempted to free action-research from its involvement with positivist versions of the evaluative relation between research and action.

Action-Research: Beyond Evaluation?

Both Halsey and Midwinter claim that positivist forms of evaluation necessitate the subjection of the creativity of action to research's authoritative constraint. Midwinter says, "University research wishes to stop life in order to measure static ... situations" (Midwinter, 1972, p. 50). Halsey suggests that researchers' desire for "clear variables" tends to influence the naturally "exploratory" tendencies of administrators towards greater "conservatism" in the design of investigative strategies (Halsey, 1972, pp. 173, 177). Halsey implicitly agrees with Midwinter's claim that the

notion of studying static situations is "unreal"
(Midwinter, 1972, p. 50) when he recognizes that research
itself will have an impact on the action it researches
(p. 175-6).   More explicitly and elaborately, G. Smith
(1975) admits that his action-research project had
actually begun with a model of "discrete and self-
contained ... programmes of action" (p. 191), evaluated
by researchers whose authority was guaranteed by their
invisibility (p. 194), but that this hope had foundered
in the "turbulence" (p. 193) of social arenas
characterized by conflicting social interests (p. 195),
in which action programmes had no clear boundaries and
were thus always vulnerable to invasion by the "sudden
effects" of massive social forces (p. 193), and where
evaluation could thus be neither final nor non-
controversial.   As an action-researcher, Smith thus
accuses positivist evaluation of being "unrealistic" in
its characterization of the social world and therefore
inevitably ineffective: "The conventional weapons of
research are cumbersome; heavy field-pieces dragged
slowly into position - hardly suitable for the swift-
moving, rapidly changing targets of an action
programme" (Smith: op. cit., p. 194).

Hence, rather than attempting to capture the swift-
moving target of action in order to subject it to
controlled experimentation, action-research proposes
to observe action's complex movements in its habitat:
it will be through the analysis of the occurrence of
change that action will be understood.   Thus, whereas
Smith (op. cit.) sees the absence of boundaries to

action as a problem for action-research, Halsey claims,
optimistically, that action-research can resolve the
longstanding disputes concerning holistic OR piecemeal
approaches to reform by "adopting an open-minded approach
to scale" (Halsey, 1972, pp. 4-5), and can thereby treat
conventional institutional boundaries (such as that
which separates the school from its community) as a
starting point for innovative action whose ramifications
will be both a topic and a resource for research.
Using a metaphor from economic theory, he explains:
"Unlike the planning model, (action-research) seeks
to use the social context of the project to increase its
own effects ... The function of the research here will
be largely a search for likely 'multiplier' effects
and an attempt to identify the outcomes that occur"
(p. 167). It is significant that here again the
theoretic basis for social research is grounded in a
metaphor (the "multiplier effect") which evokes authority -
the authority of a conceptually bounded system of
assumptions concerning the motives of "economic man", a
system in which variables are derived (almost literally)
from a "model" and given a mathematical value so that
its outcomes can be calculated from its presuppositions.
Halsey thus implies above all the ambiguity·of action-
research's ostensible willingness to follow action down
the ramifications of a process of open-ended change:
his metaphor suggests an action context whose parameters
are defined in advance and which is therefore in
principle predictable, even though Halsey's intention is

to evoke the un-predictability resulting from the number
of variables at work.

This ambiguity is indeed required by Halsey, since
he also notes that the action project is from the outset
conceived in "theoretical terms" (p. 172), which would
suggest that research leads rather than follows action,
although his list of "theoretical terms" ("disadvantage",
"power", "context-bound operations") in fact raises
once more the question as to whether such terms arise
from action, from research, or from both, and thus
reveals that the nature of the research / action
relationship still remains unaddressed.

Halsey's "multiplier" is a modification of a broadly
experimental approach to action-research (see Halsey,
op. cit., pp. 165-7); it does not address the problem
of how to conceive of research's procedures when action
is varying in accordance with other criteria than those
of research's requirement, ie. when the principle for
research is no longer a positivist epistemology relating
to evaluation by experiment. The characteristic response
of action-research exponents to this challenge is to
assert that action-research cannot determine its processes
in advance, since it cannot know which direction action
will take: thus Cory says: "the very nature of action-
research makes it highly improbable that the investigator
or investigators will know definitely and in advance
the exact pattern of the inquiry that will develop" (p. 13)
and Elliott (1981) invokes a procedure of cyclical
"shift" of the project as the successive phases of

action are evaluated.   Midwinter describes the process
as follows:

> "Action-research for us differed from research
> alone chiefly in its avoidance of the static,
> controlled, and contrived model and its emphasis
> on a fluent, on-going approach, one not afraid
> to attempt properly guarded assessments in
> unpropitious circumstances.   Action-research
> differed from action alone mainly in the constant
> feeding back of evaluation and the effect this had
> on crucial shifts of direction in the action". (p. 52)

This account immediately suggests a problem.

If research is "assessing" action while action is still

"on-going", this is indeed "unpropitious":   it will

not be clear what criteria might be appropriate, since

in principle there are neither origins nor outcomes to

be compared, and, unlike "social science research" (which

has its own "theoretical problems" - see Denzin, quoted

on p.   , above) we have not yet found action-research

making explicit any criteria of its own (beyond attempt-

ing to borrow the notion of "fact finding").   How

therefore could action-research know that the evaluation

it was "feeding back" to action was any different from

action's own evaluations (of its effectiveness and

appropriateness) which are action's perennial taken-for-

granted resources?   How, then, can we attempt to provide

an epistemology for the process in which action and

research are united by being modified through their

reciprocal relation?

Lawrence Stenhouse (often cited as an authority in

this respect by other writers) argues for "a particular

kind of professionalism ... research-based teaching"

(1975, p. 14) whereby research and action are both the

province of the practitioner, ensuring the relevance

of research to action and the improvement of action

by means of research. The theoretical framework

invoked to support his wish to abandon "the separation

of developer and evaluator ... in favour of integrated

curriculum research" (op. cit., p. 121) is Popper's model

of scientific rationality. Against the positivist

argument that the evaluator needs to be "independent",

Stenhouse proposes:

> "a more <u>scientific</u> procedure which builds action
> and criticism into an integrated whole. The
> dialectic between proposition and critique which
> is personified in the relation between artist
> and critic* is integrated in the scientific method.
> Conjectures and refutations (Popper, 1963) are
> woven into one logic". (p. 124)

Hence the need for what Stenhouse calls "a Popperian

view of policy" which means "treating current policies

as only tentatively established, always open to change,

admittedly imperfect, and thus necessarily in an important

sense 'experimental'" (p. 125). Stenhouse is not

alone in invoking Popper as a theoretic authority:

Clarke (1976, p. 1) says: "Action-research ... follows

Popper's idea (<u>Open Society and its Enemies</u>) ... that

all social administration should be conducted as

experimentation".

However, the recurrence of the term "experiment"

must alert us to the weaknesses in the claim that Popper's

work could be the basis for a non-positivist version

of science as dialectic and critique. Popper does

indeed assert the primacy of "critical discussion" in

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* See Chapter Five, pp.   below.

defining the nature of "science" (Popper, 1963, p. 127);
he opposes the positivist claim that knowledge can be
"positively" established (p. 29) and aligns himself
instead with the Presocratic philosophers' stance
that knowledge cannot arise from observation and must
remain irremediably uncertain (p. 153). And yet, as
Habermas observes (Habermas, 1974, p. 201), Popper
suggests that conjectures are "refuted" by being shown
to be "in contradiction with facts" (Popper 1963, p. 327)
and thus knowledge appears after all to be not conjectural
but positive. Consider also the implications of Popper's
statement, "Only the falsity of a theory can be
inferred from empirical evidence, and this inference
is a purely deductive one" (op. cit., p. 55): this
would leave Popper (and action-research) with a weak
model indeed of critique, since validity would remain
unaddressed, and falsity could not be shown either,
since refutation would depend upon "deductive" inferences
which, as suggested by Cicourel's arguments (see p.
above), must themselves depend on interpretive judge-
ments which are, once more, in Popper's terms "conjectures".
Hence, by not addressing the reflexivity by which alone
the imputation of meaning is accomplished, Popper
cannot prevent his "dialectic" lapsing into a circle:
"refutations" become indistinguishable from the conjec-
tures for which they are supposed to legislate.
Furthermore, in placing all his emphasis on the "testing"
phase of his cyclical process of scientific method,
Popper is content to formulate the nature of conjectures
in whimsical, non-rational terms such as "jumping to

conclusions (p. 53) or "trial and error" (p. 323),
in total contrast to the deductive rigour of "refuta-
tion". Action-research's hope that Popper could
provide a <u>unified</u> logic for inquiry is thus misplaced:
he either returns action-research to the dualist terms
of a planning evaluation cycle, or he provides no
basis for the authoritative, "experimental" form of
evaluation which Stenhouse and Clarke are seeking.

The key to Stenhouse's misplaced hope lies in his
use of the term "dialectic" to characterize the unified
logic of action-research. Dialectic does indeed provide
a mode of theorizing both unity and complexity, change
without randomness, but this is precisely what Popper's
epistemology lacks. For Popper, contradiction is a
symptom (indeed <u>the</u> symptom) of error; for dialectics
it is a condition of understanding: "The condition
for the knowledge of all processes of the world in
their 'self-movement', in their spontaneous development,
in their real life, is the knowledge of them as a unity
of opposites" (Lenin: "On the question of Dialectics",
1972, p. 360). Although Stenhouse recommended the
study of Mao Tse Tung's works for their illumination
of action-research (personal communication, and see also
Carr and Kemmis, 1983, p. 185) and although Midwinter
deemed Lenin to be "perhaps the master action-research
officer of all time" (Midwinter, 1972, p. 57),
action-research has invoked the rhetoric of dialectics'
complex unity, but has - on the whole - not sought to
base its activities on an epistemology actually derived

from dialectics.*

However, the reason why Popper dismisses dialectics
and uses contradiction as a simple procedure for
diagnosing error returns us finally to the problem of
evaluation and the relationship between the rational-
ities of action and those of research.

Action and Research: Towards a Reflexive Dialectic

For Popper the notion of contradiction is not a
complexity in phenomena but a rule for the construction
of valid propositions according to a canon of logic
(Popper, 1963, p. 320).   Contradiction offends against
the rule of scientific method, and it is the subjection
of social life to the rule of science which is the
defence against tyranny (op. cit., p. 52).   In other
words, the technical method of sceince (social phenomena
converted to empirical propositions and testable
within a deductive system of logic) can be, indeed must
be, politically, the evaluative criterion of social
action.   But Garfinkel observes ("The Rational Properties
of Scientific and Common Sense ACtivities" (in
Garfinkel, 1967) that for science to treat its own
rationalities as direct criteria for the evaluation of
social action is to prevent an understanding of the
complex rationality which action itself actually displays,

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* A rare exception is the author's own article on
  "Dilemma Analysis" (Winter R, 1982).   See Chapter
  Four for a discussion of its limitations.   See
  also Carr and Kemmis, loc. cit.

and also to prevent an understanding of the specificity
of science's own procedures and assumptions; instead
such arguments merely generate "ironic comparisons"
between the ideal of science and the "distortions" and
"inefficiency" of action processes, which are presumed
to be understandable as defective realizations of that
same ideal (Garfinkel, 1967, p. 280).

For action-research operating with a problematic
of "evaluation" this is a crucial problem. As we
have already noted in this chapter, action-research
wishes to install practitioners as researchers;
it wishes to install the improvement of professional
practice as a possible ideal for research, and scientific
experimentation as a possible ideal for institutionalized
action. Action-research thus would seem to be wholly
undermined by Garfinkel's suggestion that the attempt
to subject action to evaluation by science's ideal will
lead only to an irremediable irony. Nevertheless I
wish to argue that Garfinkel's argument does not disable
action-research's project but rather - at last -
clarifies it. The "scientism" criticized by Garfinkel
denies the authenticity of action by treating it as a
deficient version of research, and thereby legitimates
the hierarchical authority of research over action
which action-research would wish specifically to oppose.
But without a clear assertion of the difference between
research and action, which Garfinkel enunciates,
action-research cannot prevent research and action
defining each other in an ironic circle: action will be

judged by the canons of "experiment" and will thus always be judged "unrigorous"; and research will be judged by the canons of "improving practice" and will thus always be judged as "impractical". If action-research continues to <u>unite</u> action and research under one rule (the rule of "science") while dismantling the institutional and strategic separation between action and research which alone gives authority to the rule of science (the rule of experimental methodology), then action-research will merely be the disablement of both research and action: action may become absorbed into research (whereby action-research becomes merely "applied research" of dubious "validity") or research may become absorbed into action (whereby action-research becomes merely a portentous rhetoric for management's planning procedures or the common-sense thoughtfulness of practitioners' decision-making). In contrast it would be by <u>following</u> Garfinkel (by asserting clearly the <u>difference</u> between research and action) that action-research could then formulate the collaboration of action and research in the terms of that unified and constructive dialectic which action-research seeks, could abandon the model of the relation between research and action given by the scientific model of evaluation inherited from conventional social science, and could begin, finally, to formulate action-research's own ideal.

We may start this task by noting Garfinkel's list of the "rationalities" (Garfinkel, 1967, p. 263) which guide the actions of "daily life".

1) Categorizing and Comparing - the successful and frequent practice of and concern for seeing matters as "an instance of a type"

2) Tolerable Error - close attention to the varying degrees of precision required between observations and types of account, the attention which sometimes provides for a "literary allusion" and sometimes for "a mathematical model" as appropriate

3) Search for Means - the ability or inclination to review past actions in order to transfer successful procedures to current actions

4) Analysis of Alternatives and Consequences - care and attention paid to "rehearsing in imagination" the alternatives which different possible actions might produce

5) Strategy - the awareness that a number of alternative circumstances are hypothetically possible and that actions must be prepared "in case of" these hypothetical variations

6) Concern for Timing - a definite sense of the restricted possibilities for the scheduling of future events

7) Predictability - concern to restrict the unpredictability of events

8) Rules of Procedure - recognition that rules should be followed "without respect for persons" rather than in order to "respect ... certain interpersonal solidarities"

9) <u>Choice</u> - recognition that choices are actually
   possible

10) Grounds of Choice - are rational to the extent that
    they:

   a) involve inferences from a scientific corpus
      of knowledge

   b) involve references from empirical laws

   c) involve the strategies of 5) above

   d) involve constructing an account of a past
      action in order to render it coherent or
      publicly acceptable.

(In this summary of Garfinkel, 1967, pp. 263-7, I
have made clear the element in Garfinkel's own account
which stresses that "rationality" is a <u>normative</u>
<u>judgement</u>, (see, in particular the rather anomalous
statement at 10a) even though later (p. 270) Garfinkel
is concerned to distinguish between rationality as "a
stable property" and as "a sanctionable ideal").
Garfinkel then goes on to give an account of "the
scientific rationalities" (pp. 267-8) as a further set
of norms which govern the practices of "science" but
specifically do <u>not</u> govern the practices of "daily
life".

11) Compatibility of ends-means relationships with
    principles of formal logic

12) Semantic clarity and distinctness - as a criterion
    for practical judgements

13) Clarity and distinctness "for its own sake" (as well
    as for the purpose which "clarity" is intended to
    serve

14) Compatibility of the definition of a situation
    with scientific knowledge.

The first point about these two lists to which I
wish to draw attention is that "daily life" (or "action"
in the terms of the present discussion) possesses
its own elaborate series (1 - 10) of <u>norms</u> for rationality
norms which are always, for action itself "sanctionable
ideals", such that whether or not they are "stable
properties" will be a matter of interest to actors
themselves, as well as to "scientists".   Garfinkel has
here provided a resource for the formulation of
action's own grounds.

Secondly, even though Garfinkel's argument stresses
the <u>separation</u> of the two lists, such that the norms
for daily life may <u>not</u> be assimilated to those of science,
there is nevertheless an intimate relation between
them as follows: each of the norms for scientific
rationality (11 - 14) is constructed by taking one or
more of the norms for the rationality of daily life
and converting it into a topic, ie. by subjecting it
to a further elaboration according to science's own
norm.   For example, Strategy (norm 5) and Search for
Means (norm 3) would be scrutinized under the aegis of
"formal logic" (norm 11);   Tolerable Error in the
management of the appropriate precision of accounts (norm
2) becomes subject to an abstract notion of "semantic
clarity" (norm 12), and so on.   This relation between
the two lists appears at first sight to recreate (in
spite of Garfinkel's declared purpose to the contrary)
the subjection of common-sense to the rules of an

algorithmic version of rationality (unmotivated clarity,
formal logic, a corpus of findings, etc., pp. 267-8).
But this is to forget that Garfinkel is here listing
<u>norms</u>: the thrust of Garfinkel's work is that norms
are related to specific instances by means of procedures
for the construction of intelligibility ("the inter-
changability of standpoints", "ad hoc", "etc.", etc.)
which again common-sense takes as an available resource
but "science" must treat as its topic. And <u>this</u> is
where science must cease to be the authoritative
revelation of "the truth" about common-sense. For
in the same moment as it <u>topicalizes</u> the interpretive
procedures by which common-sense invokes its norms of
rationality, science <u>utilizes</u> those same interpretive
procedures, to invoke its own norms of rationality in
order to accomplish that topicalization; science itself
is charged by its own insights with addressing its
own irremediable reliance in its own activity AS science
upon the interpretive procedures it makes explicit
as features of common-sense intelligibility. (It is
Garfinkel's failure to follow through this argument
that Filmer notes in his article on Garfinkel(Filmer,
1976; see Chapter One, p. 20 above).

At the end of his paper, Garfinkel poses two helpful
and radical questions concerning the relation between
action and theory (even though his tacit desire to
exclude science from the rule of reflexivity which
otherwise governs the practices of communication leads
him to characterize the questions as "empirical"):

> "Why are the rationalities of scientific
> theorizing disruptive of the continuities of
> action governed by the attitude of daily life?
> What is there about social arrangements that
> makes it impossible to transform the two
> "attitudes" into each other without severe
> disruption of the continuous activity governed
> by each"?   (Garfinkel, 1967, p. 282)

Research, we can answer Garfinkel in analytical terms,

disrupts action's taken-for-granted reflexivity;

and action disrupts research's endless seeking for the

grounds of that reflexivity.   Action-research's ideal

and its challenge are that it seeks (and needs) to

formulate the nature of the mutual "disruption" of

research and action, so that this "disruptive" relation

can be creatively transformative of both action AND

research, as conventionally conceived.

How could this relation be formulated?   We can

make a preliminary statement as follows.   The possibility

of action being managed depends on its taking for

granted the interpretive basis on which, without remedy,

its intelligibility depends;   action is thus never

determined by the requirements of the situation:

rather it constitutes those requirements as requirements.

Hence "research" is always possible - as providing

an account of action's reflexive basis - a showing of the

conditions of its being produced as intelligible.

But research must then address its own possibility

- its own production as intelligible action.   Action

and research thus confront one another, but never finally.

Whereas positivist evaluation suggests that action CAN

in principle become experimental (only to lament action's

continual failure to be sufficiently rigorous in this

respect, leading to the clashes of principle and personnel described by Halsey, Midwinter, and others), research as reflexive analysis does not suggest that action can become reflexive, but that the moment of analytical reflexivity can clarify action's ultimately and necessarily unreflexive process and research's intimate but ultimately and necessarily non-directive involvement with the understanding of that process. Research as reflexivity cannot prescribe reflexivity for action since it (reflexively) knows that its own attempts at showing the non-reflexivity of action must, in themselves, finally lapse into non-reflexivity. Reflexivity and non-reflexivity are moments in the dialectic of analysis, a form of analysis which allows action and research to be moments in the dialectic of investigation.

But how might one specify the content of such a dialectic, so that it would be a "clarification" and a creative transformation, as well as a disruption? One approach would be to formulate action-research as a "questioning dialectic". This would be to see Garfinkel's work in terms of the ideas of Sartre and Hegel concerning the dialectics of consciousness and the "negativity" of thought and language, as noted in Chapter One. For example:

> "The being of consciousness is a being such that
> in its being, its being is in question".
>   (Sartre, 1969, p. 74, quoted on
>   p. 30 above.)

And it would link such arguments with Heidegger's "disruption" of literal syntax and propositional meaning in What Is Called Thinking?

"To understand a thinker is to take up his quest
and pursue to it the core of his thought's
problematic ... a way of questioning in which
the problematic alone is accepted as the unique
habitat and locus of thinking"
                              (Heidegger, 1968, p. 185).

However, for action-research (and following Garfinkel)

both actors and researchers are thinkers.   Thus

research _questions_ action: research's concern is the

irremediably question-able basis

of action's intelligibility.   Research will never cease

questioning action, for its rule is the question which

is always begged by action.   But action also _questions_

research: action questions the possibility, the

intelligibility, and the need for questioning;   for

action's rule is: for all practical purposes, this

(here and now) MUST go un-questioned.   And since

research will question reflexively, it will indeed

support the questioning of the question: action will

thus find in research both an ally and an interrogator.

This is the fundamental significance of Garfinkel's

list of the rational norms of "daily life" and of the

"interpretive procedures" by which they are applied.

They constitute action's _own_ ideal, being both theoretical

and always located in a particular action context.

"Reflexive research" is not a reminder to action that

action's rationalities must be seen in the light of

science's _other_ rationalities;   it would _not_ question

(for example) action's pragmatic assembly of strategies

and means by asking (for example) how far they "measured

up" to the canons of "formal logic".   Rather, "research"

would disrupt the assembling and operating of strategies
by posing the question: what are the reflexive
judgements by which these strategies (and not others)
are being assembled and operated as intelligible and
normative decisions? "Questioning" here is calling
upon actors to recover the grounds for action: the
"sanctionable ideals" for the rationality of actions
and the interpretive judgements whereby those ideals
are invoked. Thus, although the ideal of research
would indeed be theory-not-action, this would not be the
"external" ideal of scientific-theory-in-the-light-of-
which-action-seems-to-be-non-rational, which Garfinkel
rejects and which motivates the critical stance of
action-research writers towards "academic" research.
Instead the moment of research would be the moment
when action is summoned to recall its own ideal, ie.
when action's essential reflexivity is made explicit,
as a delicate set of judgemental procedures which
constitute an "acceptable" and situationally located
relation between subjectivity, consciousness of the
Other, and symbolized meaning. Research is the theore-
tical moment when action reviews its resources for
meaning construction, and thereby recollects its
unending question-ability, and in doing so recognizes
that surrounding action's here-and-now choices are an
array of possibilities, which so far have <u>all</u> been glossed
but <u>some</u> of which could, now, be formulated <u>as</u> indeed
possibilities.

This presents us with research's moment as the

theoretic formulation of action's possibilities:

> "Theorizing consists of the methods for producing
> a possible society. A possible society is the
> theorist's methods for re-forming his knowledge
> of society.   Since the theorist is engaged in
> re-forming his knowledge of society, he can be
> seen as re-forming his knowledge.   One who is
> re-forming his knowledge is re-forming his self:
> theorizing is then best described ... as a self-
> transforming operation, where what one operates
> upon is one's knowledge of the society as part
> of one's history, biography, and form of life".
> (Blum, 1971, p. 313).

But for the present argument, theorizing is formulated

as a moment in a dialectic between theory and action:

Blum's weakness is that he formulates possibilities

without formulating their analytically necessary limits.

The cultural context for theorizing is not merely an

initiating occasion for theorizing, but an ever-present

set of practical influences and symbolic resources,

which will always stand in an interesting sociological

relationship with theorizing's attempts to re-formulate

those influences and to re-order those resources (see

further discussion in Chapter Three, p. 128).   Hence,

although Blum's notion of "re-forming" suggests how

"research" questions action, we now need to consider the

other moment in the dialectic, which Blum ignores:

how action can question research.

Firstly, it must be stressed that the notion of

a dialectic between action and research is not intended

in principle to characterize a relationship between

personnel (as described by Halsey - see p.   above) but

the process of social inquiry.   Hence we are not making

an "optimistic" assumption about the "open-mindedness" of

individuals when we emphasize that the moment of

research, as outlined above, <u>anticipates</u> its counter-

moment. Rather: this is analytically required,

since the posing for action of the question of

reflexive grounds may not, without a disabling self-

contradiction, forget that, in turn, reflexive grounds

will also have to be given for the posing of the question,

as itself an interesting <u>action</u>. This is why theory

here is no longer prescriptive: analytically reflexive

theory is in principle formulated as that form of speaking

which makes explicit its own transience and limit, as

a moment (only) in a trajectory between two points at

which reflexivity must be taken for granted and meaning

glossed as "sufficient". In providing for its own

always <u>unfinished</u> status, theory provides for the

recurrence of the moment of action, since theory

itself once more becomes question-able concerning the

point at which theory <u>chooses</u> to finish, and in that

choice has to rely once more on the pragmatic rationalities

which theory <u>shares</u> with action.

For example, when action has been challenged by

theory to ground a set of here-and-now strategies and

meanings in a set of possibilities, and thus to re-formulate

the knowledge on which the initial stragegies and meanings

were selected, action thereby acquires resources for

conducting a <u>practical</u> review of the decisions on which

the strategies and meanings were based. Such a review,

in the light of reformulated knowledge, may lead to

amendment but this again will be a practical, here-and-

now decision - though of course both "here" and "now"

are changed. Such practical decisions could <u>not</u>

conceivably be a direct carrying out of theory's
injunction (as an "implementation"), nor is it condemned
to be an ignoring of theory's request (as "irrelevant").
Rather, action's response to the moment of theory will
be its own (practical) counter-question: which of
these possibilities is a here-and-now-feasibility?
Which of these reflexively elaborated rationalities
and intersubjectively constituted meanings must once
more be glossed and treated as "adequate-for-the-purpose-
at-hand)   Action, after all, must "go on".   But, of
course, as soon as it does so, theory's reflexive question-
ing (now concerning "amended" strategies and meanings)
will once more be become possible and necessary.

This, then, would be the form which inquiry
would take as a questioning dialectic between action
and research.   It is a dialectic in a strict sense.
Both terms ("action" and "research") are enabled to
interact by their own internal contradictions and inherent
instability (see the quotation from Lenin on p.   above).
The complexity of the process is embodied in the image
of the dialectical "moment": in physics there is a
moment of counteracting forces which constitutes for
a given structure its temporary equilibrium, and the
analytic necessity that this equilibrium is temporary
and thus will change is given by the inevitability
with which each "moment-in-time" will be succeeded
by the next.   Thus, on the one hand action is formulated
as, of course, pragmatic, but also as constituted by

its own elaborate set of normative rational ideals
and interpretive procedures, and thus as anticipating
its own reasearchability; on the other hand research
is formulated as, of course, theoretic, but also as
constituted by its location in the procedural rules
for mundane intelligibility, and thus as anticipating
its own inevitable incompleteness, its reliance upon
the recurrence of action for its own continuation.

Such a dialectical formulation provides for action-
research's requirement of an intimate connection between
action and research.   It locates research in action's
process and problematic, and it formulates a mode
in which action could respond to research without that
response being one of action's subjection to research's
prescription, a subjection which of course could never
be "sufficient" for action to gain recognition as
"having put theory into practice".   In this way, by
reformulating (as a dialectic) action-research's
proposal to "unite" action and research, we can formulate
as intelligible action-research's fundamental aspiration,
by enabling action-research to cast aside a model of
"evaluation-by-experimentation" which presupposes
precisely the methodological and hierarchical separation
of research and action to which the very idea of
"action-research" is opposed.

CHAPTER THREE

ACTION-RESEARCH AND CRITICAL REFLECTION: THEORIZING AND THE SELF

"Critical Reflection"

In the previous chapter it was argued that action-research is founded upon an implicit challenge to positivism's version of the relation between theory and practice, and theoretic resources were presented for making that challenge explicit. The argument now becomes: that action-research's challenge to positivism rests upon a conception of the theoretic competence of the social actor, which action-research writers present as a process of "critical reflection". This chapter begins by collecting the questions raised by action-research writers' attempts to evoke the possibility of "critical reflection" and then presents theoretic resources for addressing these questions within an analytically rigorous conception of the relation between theorizing, cultural authority, and the self.

It is the hubristic claim of positivist science that it possesses a methodology for inducing Nature "herself" to speak. In one version Nature's data are "collected" to provide grounds for the scientist's interpretation; in Popper's more sophisticated version Nature either refutes the scientist's conjectures, or - by not offering a refutation when called upon to do so - provides a quasi-corroboration, of typical, Delphic ambiguity.

In both cases Nature is summoned by the power of
methodical logic.   Action-research, in contrast, has
no such firmly articulated logic: its invocations of the
logic of positivism are always ambivalent, and this is
both a strength and a weakness.   Its strength is that
its ambiguity suggests the spuriousness of claims to
speak with the voice of Nature;   ie. there is an
ambiguous recognition that investigators can only speak
for themselves: their speech is not "findings" from
Nature but "reflection upon" Nature, and indeed "critical
reflection".   (For example: "To bring together theory
and practice it is necessary to view educational theory
as a 'critical and systematic reflection on practice'" -
Whitehead and Foster, 1984, p. 41).   However, action-
research's weakness is that, still haunted by an illusion
of Nature's own speech and thus of Nature's authority
for speech, investigators note the need to speak
independently of Nature but do not analyze their grounds
for doing so, so that conditions for the possibility of
"critical reflection" are ignored by being presupposed.

For example, Brown et al. propose that the sequence
of action-research is as follows:

Strategic planning $\longrightarrow$ Action $\longrightarrow$ Observation $\longrightarrow$ Reflection
(1982, p. 2) together with the suggestion that "reflection"
will lead back cyclically to further strategic planning.
Similarly Elliott, (1981) presents the process as:

Review $\longrightarrow$ Diagnosis $\longrightarrow$ Planning $\longrightarrow$ Implementation $\longrightarrow$
Monitoring effects (p. ii) and also goes on later to

suggest a continuous "spiral" (p. 2). But what is
not addressed is how, in either version, the process
manages to be developmental rather than merely repetitive.
How does a "view" become a "re-view", such that "diagnosis"
becomes more than a prelude to a repeat prescription?
What sort of reflection upon observation will be, as
Brown et al. go on to suggest, a "critique" leading to
"self-renewal" (p. 3)? And how may the Self be
envisaged such that its "renewal" is a transcendence rather
than a reproduction? In both sequences of terms the
possibility for an _increase_ in understanding is silently
inscribed in the _space_ between the investigator and the
world investigated. The emphasis on innovation means
that, for action-research, it is _not_ Nature who speaks,
since Nature would merely reproduce itself; rather it is
in that space between self and Nature that the investi-
gator can find resources for creative insight.

But here precisely is the problem with such merely
cyclical formulations of the cognitive process: in both
formulations (above) the possibility of developing insight
depends in principle upon their vagueness. The separate
terms are merely evocations, and the process which
links them has the vacuity of a dialectic without a
thesis. Such formulations are a parody of positivism's
formulations of _its_ methods. As such they merely fail
to enforce the prescriptivism of positivism, and they do
not of course provide an alternative: having established,
negatively, that Nature itself could _not_ be the origin
of action-research's innovative insights, we are left with

a mere question-mark as to how insight occurs.

Clearly, more is required than a space. Rather, what is

required is a formulation of the Self which can provide

for the possibility of self-transcendent theorizing,

and a formulation of culture which will provide for

the limits of theorizing without falling into determin-

ism; a formulation, in other words, of "critical reflection"

which is neither determined by the world nor assumes a

freedom (from the world) which seems "spacious" only

because it is empty.

Action-research's attempts to formulate critical reflection

Action-Research and Individual Self-reflection

At its simplest, action-research seems to suggest

that individual consciousness has a spontaneous and un-

problematic capacity for self-transcendence. Thus,

Jon Nixon, in his introduction to A Teachers Guide to

Action Research, says that the action-research practitioners

whose work he is presenting "have started from their

own skills and inclinations and from their own enthusiasms

... from a simple desire to learn, and progressed, some-

times by hints and guesses, towards the development of

a research style which suited their own particular needs

and circumstances ... The single most important point

to be taken from this book is the necessity of developing

one's own unique way of looking" (Nixon, 1981a, p. 7).

However, the word "necessity" suggests that the development

of "uniqueness" may encounter resistances, and these

resistances are what Nixon ignores. Similarly, else-
where (Nixon J, 1981b) Nixon cites Foucault as presenting
an "optimistic" view of the possibilities for "specific"
intellectuals working theoretically at their own
professional contexts (p. 31), whereas the article
which Nixon invokes (Foucault, 1977) emphasizes the
political struggle of the intellectual against "the
forms of hegemony (social, economic, and cultural) within
which it operates" (p. 14).

Admittedly, most formulations of the action-research
process recognize the contribution of an Other to the
subject's capacity for thorizing, and this will be noted
in a later section (see p. 109). But the nature of
this contribution is often left open. Thus, Brown, et
al. (op. cit.) suggest that "practitioners ... may be
emancipated from ... institutional assumptions and
habitual ways of thinking ... through the processes of
collaborative effort, rigorous critique, and self-
reflection" (p. 3). The point to be made here is
simply that the list seems to identify separable processes,
and thus the implication is that critique and self-
reflection are conceivably not dependent on any form
of self-other dialectic. Similarly, the conference
report "Action-research in schools - some guidelines"
(Elliott, 1978), suggests baldly that "teacher /
researchers" should "deepen" their understanding by
"adopting a critical, questioning stance" (p. 2),
as though such a "stance" might be the result of an
individual decision. And it is significant that Elliot

entitles one of his papers: "Action-research, a frame-
work for <u>self-evaluation</u> in schools" (Elliott, 1981 - my
emphasis).

Hence the question arises: what would be meant
by a "critical" stance? What would differentiate
"self-reflection" and "self-evaluation" as processes of
creative cognition from the complex yet routine "practical
reasoning" by which consciousness achieves its mundane
purposes? Action-research certainly <u>intends</u> to address
such questions, since much of the writing considered
here aims at creating a "practical" methodology for
individual innovation. However, the very concreteness
of the practical suggestions presented seems to conceal
the question of how particular activities could achieve
the transcendental effect to which they aspire. The
central quality of innovative thought, by which the
whole project of practitioner action-research stands or
falls, remains both merely a hope and merely a pre-
supposition, rather than an elaborated possibility
grounded in a theoretical analysis of its possibility.

Elliott clearly exemplifies the nature and scope
of this inadequacy. He lists a number of "practical"
procedures which an action-researcher may undertake -
making lists (of potential issues and methods) (1977, p. 8),
keeping a diary, producing a "profile" (eg. of a lesson),
conducting a "shadow study", making a "running commentary"
or a "document analysis" (1981, pp. 16-17) - and in
each case the outcome is described in terms of creating
"information" or "facts". But no explanation is given
of how this process of constructing a factuality might

make available <u>new</u> conceptions as opposed to merely

documenting (and thus reinforcing) the basis of

previously held interpretations.   Elliott himself shows

the need for such an explanation when he describes the

procedure of writing "analytic memos" as follows:

> "Analytic memos contain one's systematic thinking
> about the evidence one has collected ...   These
> memos may record such things as new ways of
> conceptualizing the situation under investigation
> which have emerged; hypotheses which have emerged
> ... statements about emerging problems and issues".
> (1981, p. 10)

The recurrent metaphor of "emergence" here encapsulates

the central feature of innovative thought during the

process of investigation: what was originally "hidden"

gradually "comes out" and finally stands revealed.

"Emergence" presents the difference between the beginning

and the end of inquiry as a difference between ignorance

and knowledge, between the dark cavern of illusion and

the sunlit vista of truth.   But the metaphor itself

suggests the crucial questions which Elliott ignores:

what was the nature of the concealment, and what induced

the emergence from concealment?   What are the differ-

ences which create the intelligibility of the metaphor,

and what processes of thinking, acting, and writing

would enable "profiles", "shadow studies", "memos"

and the rest to address analytically that difference

which is embodied in their purpose of inducing knowledge

to "emerge"?

At one level such issues are perhaps implied.
Each of the procedures suggests the possibility of
constituting differently the central relation between
experience and language in   the process of
"reflection" and "interpretation " (p. 16) whereby
experience is routinely assimilated into current practices;
documents once "analyzed" may be compared across the
contexts which produced them; even lists and diaries
make explicit and review-able what is normally implicit
and irrecoverably transient.   Indeed a strong argument
could be made that it is the process of writing itself
which in each of these procedures "interrupts" mundane
intersubjectivity (cf. Silverman and Torode, 1980)
and thus constitutes that differentiation which theorizing
requires.   Yet Elliott does not argue that the process
of undertaking these procedures will be a process of
theorizing, but that the product of the procedures will
be "evidence".   In this way Elliott shows how action-
research remains haunted by the voice of Nature as the
auspices of inquiry.   There is one interesting exception.
Elliott observes that it is the transcription of tape-
recorded interaction which "enables (the researcher) to
move backwards and forwards through an episode" (1981,
p. 14), ie. to deconstruct the temporal dimension of
language and experience, so that (to complete an argument
that Elliott merely hints at) their elements and relation-
ships can be explored and reordered.   .

In general then, action-research's proposal that
inquiry could differentiate itself from mundane practical

reasoning as a process of "individual self-reflection"
requires a theory which would permit multiple relation-
ships between experience and language, relationships
(in other words) which would allow for exploration,
play, ambiguity, and transformation.   More concretely,
as presented by Elliott's listing of practical procedures,
one might perceive action-research as beginning to
suggest a theory of writing as the central process in
establishing between experience and language a critical
and self-transcendental relation.

Meanwhile it is clear that, without any explicit
theory of language's inevitable distance from experience,
investigation along the lines such as Elliott puts
forward cannot articulate that theoretical space
which it must presuppose;   in the end it merely
articulates a subjectivity determined by the "facts"
of experience, and thus denies the possibility of that
self-transformative innovation which it nevertheless
wishes to urge as a practical programme.

Action-research's notion of the Subject

A subjectivity thus determined by its cognition of
a factualized experience is a subject in a rationalized
relation to its context, ie. a subject with no internal
processes but only externally oriented relationships
of perception;   and indeed much action-research writing
does seem to treat the subjectivity of the investigator
as a taken-for-granted, instrumental rationality.   (See
Chapter 4).   Underlying Elliott's emphasis on

investigation as "information gathering" (1981, p. ii)
is Nixon's confidence that a research style can be
grounded in practitioners' "unique way of looking",
their "simple desire to learn", their "needs and
circumstances" (1981a, p. 7).

However, questions concerning the limitations of
this view of subjectivity are raised by other writers
on action-research. For example, Lippett (1948)
suggests that one of the problems for "action-research"
(p. 6) is that "the backlog of knowledge about more
effective skills of living and working" has not
been "communicated into action" (p. 7) because of the
specific resistance to change derived from our "ego-
investments in the present way of doing things" (p. 8).
One of Lippett's co-workers on the Connecticut training
programme he describes was Kurt Lewin, and although
Lewin's best known article on action-research (1946)
presents investigation as based upon "fact-finding"
(p. 37), he presents a radically more problematic version
in a slightly earlier article (Lewin and Grabbe, 1945),
a version which casts doubt on the efficacy of fact-
finding by suggesting that changes in belief "cannot
be merely a rational process" (p. 56). The authors
continue: "As a rule the possessing of correct
knowledge does not suffice to rectify false perception"
(p. 57). Rather, what is required is a change in the
"culture" (p. 55) of the individual, which is alternatively
formulated as "a change in social perception, namely the
position in which we perceive ourselves and others within
the total social setting" (p. 57), as "a change of

(the individual's) superego" (p. 59), and as a change in the individual's "system of values" (p. 60).

Clearly, Lippett, Lewin, and Grabbe wish to draw attention to cultural and unconscious structures which pose the rationality of the subject as a problem. But how could one understand the problematic which leads them to align so crudely and without explanation the notions of values, perceptions, culture, and Freudian categories of the psyche? A suggestive starting point is the contradiction encapsulated in Lewin and Grabbe's use of the notion of "change". On the one hand they emphasize that individual change is difficult because individuals are determined by their culture/social perceptions/systems of values/superego; on the other hand they formulate as though it were unproblematic their intention (as action-researchers) to change just that - the individual's culture/social perceptions ... superego. In other words it seems that their account of the non-rational determination of the subject is not intended epistemologically as a general theory, since such a theory would necessarily apply to the investigators as well as to the investigated. Rather, their account draws attention to the technical problems in changing the subjectivity of others, ie. the "trainees" enrolled in their "training programmes", whose imperviousness to the presentation of "correct knowledge" requires the manipulation of group pressures (p. 62) by those whose own possible determination by group pressures is ignored. The curious list of cultural and unconscious

determinations is thus not a theory of the subject
but a pragmatic model of the manipulation process.
In this respect it is likely that neither Lippett's
reference to "ego-involvement" nor Lewin and Grabbe's
reference to "superego" is intended to invoke the
Freudian notion of the unconscious, but are merely grandiose
terms for "emotions" and "beliefs".

But Pandora's box is not so easily closed. Lippett,
Lewin, and Grabbe have, for their own purposes, noted
that the subject is constituted in a matrix of cultural
and psychic forces, so that consciousness cannot be
conceived simply as a retina upon which data impinge,
nor as a container in which facts are gathered.
To take this point seriously is to raise profound questions
concerning the investigative process as a biographically
located exploration of cultural resources, and as a
necessarily reflexive analysis of the nature of those
resources.   And not all action-research writers are
unaware of the seriousness of the issue of reflexivity.
Nevitt Sanford ("Whatever happened to action-research?"
(in Clarke A, 1976) provides a fitting comment on the
pretensions of Lewin and Grabbe: as sociologists, we
should, rather than "disseminating a monstrous image
of researchable man ... demonstrate our willingness to
study ourselves, which in turn would hlep to restore
trust in our competence to study others" (pp. 29, 31).

But if this complex theme, of the relation between
determination, rationality, and reflexivity, and between
subjectivity and culture, is indeed a central issue for

action-research, it is essential that action-research should face the challenge posed by institutionalzed authority systems to the possibility of individual critical reflection, and thus the next section examines how far action-research writers have been aware of the scope of this challenge.

Action-research, Reflection, and Institutionalized Authority Systems

At the end of his article "Action-research and minority problems" (Lewin, 1946) Lewin notes the significance of "the relation between the local, the national, and the international scenes" and he goes on to state:"Intergroup relations in this country (ie. the USA) will be formed to a large degree by events on the international scene and particularly by the fate of the colonial peoples" (p. 45). But how would "relations" between local groups be "formed" by international "events"? Lewin's theme of course is racial prejudice, and his phrasing at this point seems to plead for a coherent formulation in terms of a linkage between societal authority and individual experience, ie. a theory of culture as the resource for the self-representation of the subject. And yet other passages demonstrate the incoherence of Lewin's approach to this issue: on the one hand he elaborates "the international scene" of race relations at the most general level of historical forces ("the policy of exploitation which has made colonial imperialism the most hated institution the world over" - p. 46); on the other hand he elaborates the

"local" problem of race in terms of a rationalized
individual consciousness, namely the creation of "the
same level" of "self-esteem" and "group loyalty" for
members of different racial groups (p. 45). Thus
Lewin's presentation of the need to relate history and
biography, institutionalized authority and subjective
experience, renders such a relationship un-thinkable:
historical and political racism are constituted as a
moralistic demonology, while the interpersonal experience
of racism is constituted as an individualized phenomenon,
presumably so that "levels" may be measured as collections
of "facts". What is missing is an awareness of the
embodiment of history and politics at the level of
intersubjective relationships, either among the social
actors whom Lewin wishes to study, or between such
actors and Lewin himself, as a necessary reflexive
dimension of the process of the study.

Both of these dimensions are implicitly present in
John Collier's study of the US Indian Administration
(Collier, 1945), invoked by Lewin himself at the end of
his 1946 article. Collier specifically criticises
"the dead hand of an absolutist and unlearning bureau-
cracy" (p. 272) whose desire for control "atomized"
the Indian by "destroying the tribal and community
organizations" (p. 272). As a result the Indian
service failed to understand Navajo culture, and Navajo
culture had "no mechanism for translating ... insights
and impulses into tribal decisions and actions" (pp. 288-9).
Thus Collier's theme is precisely the relation between

knowledge and social authority, and the problematic

for "action-research" (p. 294) is how understanding may

transcend the effects of such authority relationships.

In contrast to many writers on action-research, Collier's

account of this is admirably complex. He articulates

clearly action-research's central tenet that "research"

requires the transformation of institutional relation-

ships, ie. by substituting "participation" for super-

ordination (pp. 276, 294). But he does not argue that

understanding is determined by its social relationships,

and thereby preserves the possibility of action-research's

transformative effect. For example, he does not claim

that the Indian's subordination to colonial power results

in the destruction of the Indian's capacity for insight,

only of the means for translating such insight into

practice. And he suggests that if only the "unlearning"

bureaucrat is "faithful to the spirit of science, to the

spirit of that knowledge which he has not yet mastered"

(p. 298), he will recognize that "what (the Indians) are

must be known in relation to what they must become"

(p. 297) and thus be able to "encounter (the Indian's)

ample capacity to think" and their specific "sentiment

of responsibility" (p. 289). Thus the bureaucrat may

transcend his readiness to appropriate "thinking"

and "responsibility" as his own prerogative, and come,

finally, to "learn".

What Collier presents here is a formulation of

knowledge as inherently reflexive, of understanding as

grounded in a sense of its own incompleteness and of its

developmental ideal, and thus in the possibility of
critical reflection. He is clear that institutional
authority relations leave intact the possibility for
such critical reflection; but he is less clear about
the specific inhibition of this possibility created by
the hierarchical relationships he describes, and thus he
does not address in detail the procedures by which such
inhibitions might be removed. Hence his implication
that the capacity for insight on the part of the dominated
simply survives intact seems over-optimistic, if by
"insight" more than "some interpretation or other" is
meant. And equally optimistic is his suggestion that
the "bureaucrat" can achieve a reflexive understanding
if he merely "goes quietly there, to the homes and
little neighbourhoods (of the Navajo) and stays a while ..."
(p. 289) as though the state of truthful understanding
were available as a sort of pastoral refuge away from the
"noise" of institutionalized authority. But pastoral
versions of truth are profoundly ambivalent, presenting
a way of life as valid because of the apparent absence
of the very sophistication by which in fact it achieves
its expression. This disables Collier's ethnography.
He emphasizes that the relationships and the possibilities
for understanding between the bureaucrat and the Navajo
are constituted within an authority relation: he cannot
then propose that valid understanding could take the form
of a pastoral absence, ie. a simple rejection or denial
of the effect of the authority relationship by one or
both of the parties to that relationship. What is
needed, rather, is an account of the specific resources

for meaning as these are constituted dialectically
and reflexively within the authority relationship - as
symbols and myths, as ambiguities and contradictions.

Analogous with Collier's optimism concerning action-
research's capacity simply to "reject" institutionalized
authority is that of Nixon. He presents action-research
by teachers as an attempt "to reject the paternalism of
traditional research", namely the subjection of
practitioners' creative analytical potential to the
prescriptiveness of "academic" methodologies (1981a,
p. 9). We have already seen (p. 82, above) that Nixon's
view of action-research rests precisely upon a libertarian
principle. But this of course immediately undermines
itself. If teachers are "free" to choose a research
style in accordance with their "needs" or enthusiasms,
they are quite likely to choose ("freely"?) to adopt a
style derived from that "paternalist" tradition which
strongly influences the awareness within which they
make such a choice. And indeed a number of writers
in Nixon's book proclaim their reliance on the theoretical
perspectives and methods of conventional positivist
social science (see for example pp. 17, 92, 155).

This ambivalence in Nixon's work raises directly
the question which so far has only been hinted at:
how far forms of understanding themselves may be
recognized as adequate (or inadequate) only within a
cultural order which is structured by institutionalized
authority relations. Nixon's argument is presented

more elaborately by Elliott (1982a). He begins by
claiming that teachers do not see themselves as
potentially competent theorists because of a "doctrine"
which separates theory and practice, and makes truth
a matter of having access to an "objective" reality
"external to (people's) minds" (p. 3). This (widespread)
epistemological position, whose conventional status
is conveyed by the word "doctrine", leads to a set of
perspectives on the part of teachers which Elliott
sums up as professional practitioners' "assumption of
intellectual dependence" on the personnel and practices
of "the kind of higher education institutions which
qualified them" (as professional practitioners)
(pp. 2-3). In other words, positivist epistemology
is institutionally embodied in a division of labour which
segregates the clarification of ideals by "philosophers",
the clarification of means-ends relationships by
"scientists", and the "application" of the results
of such work by practitioners who thus see themselves
as "technologists" (pp. 4-5). (The hierarchical
relationship here is dramatized and biographically
located in the process of "qualification"). This
general argument is derived, of course, from Habermas's
criticism of the notions of science and technology as
"ideology" (Habermas, 1971), as legitimated forms of
knowledge expressing a political oppression, which
requires, as a response, "the determination to take up
the struggle against the stabilization of a nature-like
social system over the heads of its citizens ..."

(the closing sentence of <u>Legitimation Crisis</u>, Habermas, 1976). For Elliott action-research <u>is</u> this struggle, a struggle to reunite theory and practice, action and research, which the cultural and occupational order of industrial society systematically strives to put asunder.

However, there is a general irony surrounding the notion of "epistemological" ideology. If a way of conceptualizing knowledge is ideological in a <u>determinist</u> sense, then indeed one has an urgent sense of the cultural constraints upon understanding, but one then needs analytical grounds for differentiating an alternative form of cognition, in order that theorizing itself may be intelligible. Otherwise the theory of ideology is self-engulfing: <u>all</u> knowledge would become the outcome of an authority system, including of course the assertions of a theory of ideology; in this way theorizing would simultaneously say that theorizing is impossible and also say that it has no grounds for saying so. It would have grounds neither for speech nor for silence.

Now Elliott's theory of positivism as an ideology is certainly phrased <u>non</u>-deterministically - as a "doctrine" (which therefore one might reject) and as an "assumption" (which therefore one might renounce) - and thus Elliott can go on to claim, in response to criticisms of action-research by structuralist Marxism, that action-research "did not assume that the process of schooling was not constrained by its political and economic context, but did assume that teachers could become aware of such constraints, and in doing so increase their capacity to devise strategies for overcoming them" (p. 28).

But notice that at this point Elliott is no longer considering the epistemological level: having posed the question in terms of the ideological separation of theory and practice, means and values, a question which radically threatens the cognitive capacity of the individual, his answer is in terms of the determination of _role_ relationships. A theory of ideology, of the relation between social authority and the investigative process, challenges action-research _not_ by suggesting that (for example) the process of _schooling_ is constrained by its cultural context, but that the process of _understanding_ schooling is thus constrained. Hence, although Elliott is right to reject a determinist social theory, his formulation of the individual's response to institutional- ized authority as merely "becoming aware" is apparently incompatible with his previous account of how that awareness is itself institutionally constructed and biographically effective. In order to remedy this dichotomous tendency, Elliott's theory requires a formulation both of consciousness and of social structure in terms of their mutually constitutive dialectical contradictions.

In contrast to Elliott's argument, Moser (1978) formulates the issue of action-research's relation to the authority structures of its context by specifically denying that there can be any question of the _practitioner-_ _as-researcher_. The researcher and the researched are defined in terms of "different life-situations" which in turn means that the former has a theoretical competence

which the latter lacks (p. 141). Consequently problems
are derived by the researcher, and he has the task of
"convincing" participatory groups of   "relevance"
(p. 148). It is thus the researcher's special
capacity for analysis which enables him or her to show,
"in concrete social situations ... that gap between
claim and reality, between formulated democratic
principles and real domination, in which we are
constituted by late capitalism" (p. 78).*   It is the
researcher's analysis which explores contradictions,
reveals mystifications, and leads first to "enlightenment"
("Aufklärung") and thence to "Praxis" (p. 78).
But what Moser does not address is the grounds for the
researcher's understanding, given the reality of
"domination". Conversely, given the researcher's
understanding, what are the grounds for the social
actor's ignorance? Hence in his own final formulation
of the possible achievement of action-research, Moser
preserves precisely the hierarchical relation which
Elliott, Nixon, and other action-research writers wish
to subvert. On the one hand he envisages the creation
of a form of consciousness "which can differentiate
between being and appearance, essence and phenomenon, is
and ought ... which transcends the merely given
situation and includes in discussion the overarching

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* "in konkreten gesellschaftlichen Situationen ...
   welche Kluft zwischen Anspruch and Realität,
   zwischen formulierten demokratischen Prinzipen und
   faktischer Herrschaft im Spätkapitalismus uns besteht".
        (All translations from Moser are by R. Winter)

set of relationships which determine that situation

(p. 169)* (In other words we are given a <u>determined</u>

context within which individuals can transcend those

determinations only insofar as they can discuss them).

On the other hand, in the next (and final) paragraph

he suggests that action-research will enable "those

concerned to take their <u>destinies</u> into their own

hands" (p. 169).** However, by this he explicitly

means merely that social members ("Feldpersonen")

who have been involved in an action-research project

will be able to pursue the aims of the project even

after the scientists ("Wissenschaftler") have eventually

withdrawn. Thus, in complete contrast to Elliott and

Habermas, Moser envisages that the citizen will be

freed from the internalization of societally enforced

norms only at the price of internalizing the norms

of the action-researcher. Whereas for Elliott

intellectual dependency was the problem, for Moser it

is the beginnings of a solution - a necessary prologue

to liberation. Theory is envisaged in a prescriptive,

rather than a dialectical relation to action, and thus

critical reflection is constituted <u>unreflexively</u>, as

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* "Das Sein und Schein, Wesen und Erscheinung, Sein
und Sollen unterscheiden kann ... welche über die
bloss vorgegebene Situation hinausgeht und den
übergreifenden Zusammenhang, welcher diese Situation
bestimmt, mit in die Diskussion nimmt".

** "Die Betroffenen selbst ihr Schiksal in die Hand
nehmen".

an intellectual procedure which removes _itself_

from the institutionalized relationships which are its

object.   Although this is a conventional position

for critical theory, it denies the aims and the specific

contribution of action-research, which Moser elsewhere

urges as a necessary intervention in order to remedy

critical theory's own inability to relate theory and

practice (see Moser, p. 40).

The question thus becomes: how can the relationship

between the intellectual authority of theory be

formulated as analytically different from the legitimating

relationships of an institutional order, without falling

back into that prescriptivism which action-research

wishes particularly to avoid?   What is required

is the formulation of an analytical relation between

ideology and reflexivity, such that a recognition of

the challenge of ideology to valid understanding can be

assimilated to action-research's dialectical relation

between theory and practice.   (see below, p.128 ff.).

Meanwhile, Moser has raised in an urgent form the question

of the nature of the _social_ relationships, between

those involved in action-research investigation, which

might facilitate the development of critical reflection.

Action-research, critical reflection, and the social
relationships of the research process

We have seen that for Moser the citizens' understand-

ing can only be emancipated from subjection to institution-

alized authority on condition that they apprentice

themselves to the theorist: the theorist hopes in the

end to be able to withdraw from the scene, his/her

work completed, but the theorist's authority is presented

as the necessary condition for the achievement by others

of autonomy. This tension, between authority and

liberation from authority, constituted in action-research's

double aspiration that action may be both informed by

"research" and yet remain free from determination by

"theory", is central to the action-research problematic.

It is perfectly expressed, for example, in the following

passage from D. Krech and R. Crutchfield (1948),

articulating Lippett's notion "the community or

organizational self-survey" (Lippett, 1948, p. 2):

> "A community self-survey can be described as
> action research in which the members of the community
> themselves, under the expert guidance of applied
> social psychologists, are responsible for the
> collection and analysis of community data".
> (p. 524) (emphases in the original).

Notice the specific stress on the "members of the community"

in contrast to the tacit ambiguity with which their

"responsibility" is undercut by the "expertise" of

the scientist, and hence the crucial need for an

explication of the contradictions within "guidance" as

the mediating category. Krech and Crutchfield, however,

avoid such issues. For them the principles of their

approach are 1) to obtain "the facts", 2) "facts thus
uncovered by the citizens of the community will be
more readily accepted by the community", and
3) the process of carying out the survey will "have a
powerful motivating effect" upon the citizen-surveyors.
(pp. 524-5).   In other words, the "expert social
psychologists" are engaged in what the authors term
"the educational process", which they define as "any
measure designed to change the motivational structure
or perception of an individual (through) manipulation
of the person's environment for specific ends" (p. 519).
There is a double authority here.   Firstly, experts
have "specific ends" in mind for the community, and
"design" manipulative means to achieve them;   secondly,
community situations are constituted as "facts" which
communities must be induced to "accept".   The two
bases for authority are linked: the unquestioned
authority of the expert scientists may be presumed
to rest on their unquestioned access to the authoritative
facts of Nature.   Once more, by failing to question
a positivist epistemology, action-research presents
theory as an unreflexive authority borrowed from the
same scientific practices it wishes to oppose, and lapses
into the manipulative devices of managerialism.   _And
yet_ the original passage italicized the members of the
community and wished to make them "responsible" ...

A similar tension is expressed in the work of Cory
(1953).   Cory stresses that studies of educational
practice "must be undertaken by those who may have to
change the way they do things as a result of the studies ...

Teachers, pupils, supervisors, administrators and school patrons (must) continuously examine what they are doing ... use their imaginations creatively and ' constructively ... etc. This is the process I call action-research". (p. viii) And yet on p. 18 we read that the reason why action-research must be a "co-operative" activity is that, unless "interested" parties to the action become collaborators, they may well become an opposition. Thus action-research gives autonomy to practitioners-as-researchers but only in order to subject them to the authority of the action-research process, which itself creates auspices for cooperators and (by the same token) for opponents.

But it is precisely these auspices which will always need to be theorized even, for example, when the social relations of the research process are formulated in accordance with the Habermasian "ideal speech community", in which, alone, the integration of concensus and emancipation enables freedom to be inscribed within authority itself.

It is the Habermasian ideal of a speech situation, in which possibilities for initiative and critique are "symmetrically" distributed (Habermas J, 1970, p. 143), which underlies the work of Elliott, and thus enables him to present the social relations of action-research as in principle those of an anti-hierarchical collective. The ideal of "dialogue" in which "participants must have equal freedom" for interpretation

and criticism explicates "a procedure for determining
the objectivity of practical judgements" (Elliott:
1982a, p. 19) and this constitutes "Habermas's
reconstruction of the interpretative model" for social
science (ibid.). Elliott continues: "In my view ...
educational action-research constitutes the concrete
expression of a reconstructed interpretative paradigm
with respect to the study of schooling". (p. 22).
The ideal of a "symmetrical" discourse leads Elliott
to describe the interaction between the participants
of an action-research project very differently from
the rhetoric of manipulative management discussed
above. For example "interviewing" is presented as
"a good way of finding out what the situation looks
like from other points of view" (Elliott, 1981, p. 15);
researchers are told to "use the experience of other
teachers/researchers" and to seek "access to varying
interpretations" (Elliott, 1978, p. 8). The symmetry
of the interactional process is embodied in the idea
of "triangulation", namely the comparing of different
accounts in order to "mount discussions on points of
disagreement between the various parties involved,
preferably under the chairmanship of a 'neutral' party"
(Elliott, 1981, p. 19). And in order to protect
the symmetrical interaction of the investigative process
from the hierarchical interactional norms of its
institutional setting, it is necessary to negotiate an
"ethical framework" concerning "confidentiality", in
which participants retain "control" over information
concerning their activities and opinions: "they have

the final say" (ibid. p. 9).

For Elliott, then, action-research's possibilities for the development of understanding seem to rest upon an analytical difference between mundane inter-action, beset by institutionalized role norms, and the symmetrical interaction of the investigative process, in which the Self-Other relationship is freed to become an explorative dialectic. "Theory" then would implicitly reside in such a difference. From this point of view the notion of a need for a "neutral" chairman and for a defensive framework of confidentiality embodies an awareness of the inevitable fragility of the Habermasian ideal.

But there is a crucial problem here. Elliott does not say that action-research requires the implementation of a non-authoritarian dialogue. The article from which most of the above quotations are taken is subtitled "A Framework for Self-evaluation ..." And the other article cited, ("Action-research in schools, some guidelines") begins as follows:

> "Basically classroom action-research relates to
> any teacher who is concerned with his own teaching;
> the teacher who is prepared to question his own
> approach in order to improve its quality.
> Therefore the teacher is involved in looking at
> what is actually going on in the classroom ...
> This research may be extended to include other
> individuals". (1978, p. 1) (my emphasis).

The formulation of theorizing in terms of a symmetrical dialogue is thus only an option, depending on "the scope of the research" (ibid.). In the absence of such absence of such dialogic possibilities, action-research can still, apparently, rely on the individual's solitary capacity to "question" and to find access to a Natural world of facts -

"what is actually going on". The central question of the analytical relation between the authority of theorizing and the authority of Nature remains unaddressed by this tacit juxtaposition, which reduces a principle to an option. Until it has been addressed, one can have no confidence that Elliott's "neutral" chairman will not burst out of his inverted commas and begin to adjudicate between interpretative differences in the name of Nature, rather than calling upon all interpretations to recollect their irremediable reflexivity.

Whereas Elliott's use of Habermas is undermined by a residual empiricism, the work of Brown et al. seeks to assimilate Habermas to an activist epistemology. Thus, they invoke Habermas's notion of a "critical social science which is conditioned by the explicitly political emancipatory knowledge-constitutive interest" (p. 14) and continue immediately to describe it as "a science specifically oriented to the development of improvement and understanding through the strategic action of participants in social situations through action-research" (ibid.). This contrasts strongly with Habermas's own statements that "the emancipatory cognitive interest aims at the pursuit of reflection as such" (Habermas, 1978, p. 314) and that there is a inevitable disjuncture between self-reflection and strategy (Habermas, 1974, p. 39). Perhaps it is this attempt by Brown et al. to short-circuit Habermas's highly complex formulation of the theory-practice relation which leads them into an interesting ambiguity,

reminiscent of the problems in Heinz Moser's work.

Firstly, Brown and his co-authors present a notion of

"collaboration" among action-research participants

which seems distantly to evoke the Habermasian ideal

of self-reflection through symmetrical dialogue:

> "Action-research is distinguished by its adherence
> to a collaborative ethic. Action-research is a
> collaborative endeavour in which groups of
> practitioners work together to understand better
> their own practice, to increase their awareness
> of the effects of their practice, and of their
> control over the situation in which they work".
> (Brown et al., 1982, p. 4).

But in practice, they go on to say, action-research by

practitioners requires the "assistance" of "a facilitator

from outside the immediate situation being studied"

(ibid.) and it is towards the explication of the nature

of this role, rather than the possibility of "colla-

boration" that they devote the remainder of their article.

Their account of the role of the facilitator

embodies many of the tensions which are the topic of

this section. The facilitator is "a supportive

friend ... providing a sympathetic ear in times of

doubt" (p. 6), and a "group recorder", who, by circulating

notes on meetings generates "a sense of group identity

and history" (p. 6). In contrast, the facilitator

provides "an outsider perspective", providing criticism

and the challenge of "alternative" viewpoints, or acts

as a "Devil's advocate" (pp. 5-6). The tension

between challenge and support embodies the facilitator's

commitment both to the possibilities for transcendence

and to those activities on the part of practitioners in

which such possibilities must (for action-research) be
grounded.   This is focussed in a complex notion of
discourse: the arrival of the facilitator requires
practitioners to "articulate their respective points of
view", ie. to explain what has previously been "taken
for granted", and thus to challenge the "history" of
communication within the social situation (p. 4).
This seems to suggest a notion of the facilitator as
the Other of discourse, who precipitates a realization
of the space between language and action and thus
enables critical self-reflection (in Habermasian terms)
to extricate itself as a moment of consciousness distinct
from consciousness's otherwise inevitable commitment to
mundane action.   This, again, might be taken as a
starting point for the formulation of the "authority"
of theory as residing in its analytic difference from
action, and as such to be enacted in a reflexive
discourse between a practitioner and one-who-is-not-a-
practitioner:

> "By their own openness, sharing questions and
> doubts, facilitators seek to engender an environment
> where obstacles to progress can be frankly
> examined" (Brown et al., 1982, p. 4)

But Brown et al. are not content to present such
a model of authority: they also present the facilitator
as "a teacher of action-research" (p. 5).   As a teacher,
the facilitator "frames the principles" of the work and
"clarifies the process".   But, for strategic reasons,
he does not "explain the entire rationale for each
practical step" (p. 5).   The facilitator is no longer
the Other of self-reflective discourse but the one who

<u>tells</u>, who prescribes principles and defines clarity, who possesses (and decides when to provide) "a rationale". Rather than the <u>analytic</u> authority of theorizing as a moment in the dialectic between action and language, which must be action-research's underlying aspiration, we have here, once more, the <u>social</u> authority of the expert, whose procedure once more, seems to be a form of manipulation which profoundly contradicts the ideal of collaborative, reflexive discourse. Another familiar problem re-surfaces here, when we are told that the facilitator-as-teacher should possess expert knowledge concerning "data-gathering techniques", so that authority for inquiry is once more reinforced by the supposition of a methodological access to Nature on the part of a rationalized consciousness constituted by the factuality of its context.

But Brown and his colleagues do hint at the limits of such a version of rationality when they refer to the facilitator's reliance upon and need to generate "an atmosphere of trust ... (among) a sympathetic audience" (p. 5) and indeed by the general suggestion that the facilitator should be "supportive". Such mundane comments in themselves of course hardly add to their account of the social relations of facilitation: rather they merely serve further to undermine its theoretical coherence. However, they do serve as reminders of an awareness among writers on action-research that the rationality of the subject is constituted within a complex psychic structure, and hence of the inherent <u>fragility</u> of self-reflection.

Within this perspective, Brown et al.'s

fragmentary references may be seen in the light of

Michael Foster's statement, in his article "An intro-

duction to the theory and practice of action-research

in work organizations" (1971):

> "This brief review of early action-research
> thinking could not be complete without reference
> to the development of 'T-groups', which are often
> part of the repertoire of the change agent" (p. 7)

Foster then goes on to quote various "streams of develop-

ment" which have characterized action-research, including

"an integrative psycho-analytical ... approach" and a

"group dynamics approach" (p. 8), and admits that

there is a necessary debate concerning the relation-

ship within research between "interchange at the

cognitive level" (p. 29) and "interpersonal feeling"

(p. 30).   Similarly Lippett (1948, p. 254 ff) works

towards a triangular framework consisting of "action,

research, and training", and Moser (p. 53) cites

with approval Lewin's comparable association.   By

references such as these, action-research writers begin

to acknowledge a possible contribution from psycho-

analysis in formulating the possibility of developing

self-reflection through the dialectic of action and theory.

And thus, in formulating the social relationship through

which this might be achieved, an adequate theory for

action-research's problematic would need to consider not

only the nautre of "collaboration" and of "facilitation",

but also of "therapy".

The notion of therapy makes explicit and central
ideas which action-research writers like those I have
discussed present merely as peripheral hints: that
inquiry faces a challenge from the resistances created
by its own interactional process; that rationality's
problem is that it seems to be both an outcome and a
presupposed resource for inquiry; that the authority
for interpretation is precariously balanced between
the investigator and the object of investigation, and
that (indeed) perhaps this very distinction is itself
questionable; that inquiry must enact its problematic
in order to grasp its object; that the understanding
of specific situations can only be grasped as inter-
sections of symbolic structures which ramify afar, both
in time and place, both in culture and biography.
It is interesting to note that Habermas, who inspires
action-research's ideal of "collaboration", and who is
invoked as an authority for "facilitation", explicitly
raises the possibility of psycho-analysis as a formulation
of the relation between authority, rationality, and
discourse (Habermas, 1970, p. 116 ff.), and even more
interesting that the action-research writers who cite
Habermas ignore this aspect of his work while also
citing other action-research writers whose pages refer
to "changing people's superego's"!   Hence, even though
the work of Freud is significantly ignored by action-
research, it will figure quite substantially in the
following pages, in which I shall outline the
theoretical resources necessitated by action-research's
failure to articulate coherently its self-proclaimed
problematic of "critical self-reflection".

Theoretical Resources for the Formulation of Critical Self-reflection

Critical Self-reflection and Theories of the Self

A version of the inquiring subject simply as a "rational" consciousness leaves action-research open to a charge of naive idealism, which would disable action-research's fundamental commitment to theorizing above all the links between theory and practice. On the other hand, a determinist version of the subject as a product of its action context would undermine action-research's need to formulate an innovative, theorizing subject which (through action-research) can <u>transcend</u> its context. How can this polarity be reformulated into a coherent complexity?

Freud's work is often seen as exemplifying a crude determinism, at the level of biological drives ("the Id"), neurological processes (the theory of memory in <u>The Interpretation of Dreams</u>, Freud, 1976a, p. 687), and the "universal" Oedipus complex. However, in many ways Freud's work is ambiguous with respect to determinism. Certainly, he tends throughout his work to preserve an "instinctual" explanation of mental acts, which Habermas criticizes (Habermas, 1978, pp. 253-4): for example, Freud's analysis of jokes explains that "the comic" builds up "a surplus of psychic energy" which then needs to be "discharged" (<u>Jokes and their Relation to the Unconscious</u>, Freud, 1976b, pp. 254-6)*

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* From an analytical point of view, one would wish to see jokes as sudden reflexive twists, revealing both limitations of their own conditions <u>and</u>the hitherto concealed possibilities which these conditions might otherwise facilitate or provoke. (See Chapter One, p. 21).

On the other hand he presents other accounts of mental life in terms of a cultural system structured like a language: there is a <u>vocabulary</u> of images, derived from "folklore, popular myths, legends, linguistic idioms, proverbial wisdom, and current jokes" (1976a, p. 468); and there is a <u>syntactical process</u> of condensation and displacement, which constitutes mental productions in a form which is representative in function, metaphorical in texture, and mythic in structure. (Even at the biological level Freud moved towards a dialectic based on the "dualistic" principle of mutually opposed instincts -(Freud, 1961, p. 47). And Freud's "linguistic" model of subjectivity does not have at its centre the determinism and fixity of a dictionary but the open horizon of words themselves, "which, since they are nodal points of numerous ideas, may be regarded as predestined to ambiguity" (1976a, p. 465).

From this point of view, the Freudian unconscious with its web of symbolic metaphor and ambiguity is not simply an obstacle to rationality but rather rationality's own resource. Conventional "philosophy of science" is embarrassed to admit that the instrumental model of rationality can explain only the <u>post facto</u> <u>checking</u> of theory; theoretical insight itself remains anecdotal and sentimentally mysterious - dreams or accidents, contingencies without principle, except as Fate's reward for "genius". Hence the importance for my argument of works such as Koestler's <u>The Act of</u> <u>Creation</u> (Koestler, 1969) which argue systematically

for the metaphorical processes of unconscious mentality

as the resources for theoretical work. In this way

the simple polarity between Self and Rationality is

mediated by the complexity of the symbol, as constitutive

of both Self <u>and</u> Rationality. This, then, is the

significance of Freud for the present argument: the

unconscious not simply as a limitation upon the subject's

capacity to respond "rationally" to its context (ie.

as the origin of "neurosis"), but as the resource for the

subject's capacity to respond "creatively", ie. not merely

to <u>respond</u>, but to transform its context from an

experiential <u>given</u> into a range of symbolic <u>possibilities</u>.

The unconscious, in this view, adds to the logical

constituents of mind, posited by Kant, both desire and

metaphor, and thereby formulates for creative conscious-

ness both motives and pathways.

Jung formulates this view of the unconscious as

a <u>resource</u> quite directly:

> "The same psychic material which is the stuff of
> psychosis is the <u>fund</u> of unconscious images,
> which fatally confuse the mental patient but ...
> is also the <u>matrix</u> of a mythopoeic imagination
> which has vanished from our rational age".
> <div align="right">(Jung, 1967, p. 213 - my emphases)</div>

The mythopoeic imagination has vanished because reason

has abandoned the resources of the unconscious - the

"archetypes" - in favour of various reductionist

versions of thought, such as "concepts of averages"

(Jung, op. cit.: p. 17) or "systems of concepts" (p.

154). "Archetype" attempts directly to formulate

thought as structured but not determined. Thus, when

Jung makes a _parallel_ between the "mythological motif
(of) the hostile brothers" for the human psyche and
the "instinct" of nest-building in birds (Jung, 1977,
p. 228) one can hear this as a riposte to Freud's
reduction of psychic phenomena _to_ "instincts": Freud is,
as it were, implicitly accused of making a "category
error": for man the instinctual IS the mythic.

In other words, archetypes are an extension of
arguments concerning the constitutive categories of
thought.   Kant presents _consciousness_ as structured
by the constitutive categories of _perception_ (Space,
Time, Subject, Object); Piaget makes a similar argument
at a more specific level when he presents _instrumental_
rationality as structured by the constitutive categories
of _purposeful action_ (relations of part/whole, cause/
effect) (see for example Piaget, 1977, p. 727);
and Jung presents _subjectivity_ as a whole as structured
by constitutive categories of _experience_, structured,
that is, at that level where the apparently physical
("hunger, disease, old-age, death") is mediated as
the cultural ("war, the hero"), and thus structured
above all in ambivalence (Jung, 1977, pp. 238, 443):

> "Symbols, by their very nature, can so unite
> the opposites that these no longer diverge or
> clash, but mutually supplement one another
> and give meaningful shape to life".
> (Jung, 1967, p. 370)

In a curious way, then, the Jungian notion of archetype
seems to take Levi-Strauss's mythic structures of
metaphor and contradiction and install them alongside
Garfinkel's procedures for "practical reasoning" - as
the unnoticed conditions for intelligibility, communica-

tion, and interpretation. To follow through this
suggestion would be to show what a "mythopoeic
imagination" might achieve as a current theoretic
practice (rather than as an exotic reference). It
would be tempting to "de-mythologize", as Barthes does
(see Barthes, 1976) and to forget the requirement that
such "mythopoeic" theorizing must, like any form of
analysis, be reflexive: Garfinkel forgets that his own
writings exemplify in practice "etc." and "ad hoc" (see
Filmer, 1976); does Jung forget that "archetype" is an
archetype?

This is an important question. There is a real
danger that one might attempt to <u>collect</u> archetypes as
a "fund" or dictionary of "real" meanings prescribed
with the authority of a universal unconscious.
But Jung's emphasis is <u>both</u> on an archaic and universal
inheritance (Jung, 1977, p. 228) and on the irremediable
uniqueness of the personal:

> "Interpretation cannot be a method based on
> rules: it requires a study of the wholeness of
> the symbol-producing individual"
> (ibid., p. 250)

The Self is not merely a repository of resources nor
a product of their combination, but is "the principle
and archetype of orientation and meaning ... a personal
myth" (Jung, 1967, p. 224). The individual Self can
grasp the universal because it is not a simple component
of a complex totality, but rather a complex microcosm
of that totality. The reflexive self is thus inevitably
a theorist of meaning in general, which again suggests
that "meaning" is made possible not by the symbol as a

clarified label for an element in an external reality,
but by the inherent metaphoricity of the symbol itself.
The strength with which the Jungian version of myth
holds both to the universal and to the personal can be
related to Hegel's view of this aspect of symbolism
in general (Hegel, 1977, p. 62). It is also profoundly
significant for action-research's problem concerning how
the individual instance can "achieve" generality. The
question for theoretical adequacy ceases to be: how
can the individual find an external pattern in which it
can take its place as an element in a system (of
objects), but rather: how can the individual grasp
its own inherent complexity (as a symbolic structure)?

At one level, the problematic is engaged by
"ego-analysts" who preserve the complex dynamics
of the Freudian psychic structure but remove the Freudian
theme of a determining and relatively inaccessible
unconscious, and thus envisage such procedures as
"self-analysis" (Horney, 1962), "self-discovery" (Rogers,
1983), and "transactional analysis" (Berne, 1967, Harris,
1973) as intelligible (if lengthy and difficult)
processes whereby the subject can achieve self-under-
standing, ie. achieve a measure of autonomy with regard
to its own complexities. But these accounts would
themselves need to be grounded in a theory of the relation-
ship between a self-reflective subjectivity and the
symbolic order in which it is constituted as potentially
self-reflective, rather than as determined.

This is Lacan's problematic. The self-representa-
tions of the subject may not be understood by tracing
them to an experiential origin (which would of-course
formulate consciousness as determined by its antecedents).
Rather, the "signifying chain" pursued by an interpreter
of the "meaning" of the subject is a sequence of
symbolic substitutions (metaphor) and combinations
(metonymy) (Lacan, 1977, p. 258). Hence (for example)
Lacan re-formulates the Freudian phallus not as an
objective feature of a traumatic experience, but as
a "signifier" (ibid., p. 285). In emphasizing that
the subject is constituted as a structure of symbols,
rather than a structure of motives, Lacan's major theme
becomes the "alienation" of the Self, following, in
many ways, Sartre's view of language as "negativity",
and attempting to provide developmental grounds for this
view. Thus, from the moment at which the infant
finds a problematic image with which to unify its
experience of its own body (a phase necessitated by
the relative "prematurity" of the human neonate -
Lacan, 1977, p. 4), consciousness of self is mediated
by the symbol, which embodies the dependence of the
subject on the Other (p. 5) and thus converts primary
narcissism into a sense of perennial threat, and hence
into aggression (pp. 5-6), reconstituting all objects
(including the self) in a "paranoid mode" (p. 17).
Hence the game which the infant plays with its own
identity, his baffling presence AND absence in its
image and its name (Freud's "Fort ... da" game - see
Freud, 1961, p. 9)- the game which is the "point of

insertion of a symbolic order that pre-exists the
infantile subject, and in accordance with which he will
have to structure himself" - (Lacan, op. cit., p. 234)
is fraught with an inescapable danger: the symbolic
order is one in which "the Other is the locus of the
deployment of speech" (p. 264), and thus the Self-Other
dialectic of reflexive speech which constitutes
consciousness (p. 86) is structured as the Hegelian
struggle between Master and Slave (Lacan, op. cit., pp. 80,
305).   In this way, through its originary constitution
in the symbolic, the subject is from the outset in
a perpetual state of "discord with its own reality" (p.2).
And it is through this "primordial" discord and self-
alienation that metaphor and metonymy, displacement and
fictionalization are subjectivity's constitutive modes
of being.

In this way, Lacan argues for the "intellectual"
potential of the self (p. 171) while maintaining a
sense of its tragic limitations: its very creativity
resides in a specific and irremediable fragility.
Thus Lacan's theme can be added to those of Kant, Piaget,
and Jung noted above (p.116): he gives us <u>language</u>
as a further constitutive structure of subjectivity,
but language neither as a determining system of concepts
and rules, nor as an available mechanism for uncon-
strained external reference: "language" here is the
"parole" which Saussure neglected, that discursive
reality in which the self is located, biographically
and with Others (Lacan, 1977, p. 86), in which meaning

is continuously created, and yet (condemned to reflexivity
by the instability of the Self-Other dialectic)
continuously transformed.

But there seems to be a discrepancy in Lacan's
work precisely between versions of the self as constituted
in the structures of reflexive discourse and of the self
as structured in developmental phases, eg. the "mirror
phase", and the "Oedipal stage" (p. 282). In one we have
a matrix of metaphoricity (as a theory of speech) and
in the other we have a causal sequence (as a theory of
biography). Lacan does not address this tension as
possibly inevitable, but rather attempts a radical
fusion, for example in his theory of the phallus as the
"signifier" of the Self-Other dialectic in general
(p. 289) and of "the Name-of-the-father" as "the law of
the signifier" (p. 217), of which the very least that
can be said is that it suggests a reductionist and
determined version of discourse, and one which would
undermine Lacan's own emphasis on its ramifying
metaphoricity.

The question raised by this aspect of Lacan's work
is crucial: if the self is indeed and inevitably
structured in time as well as in language, how can these
two be related so that a determining causal chronology
does not threaten the explorative, innovative quality
of self-reflection? The nature of the problem can
be seen if we once more consider the ambiguities of
Freud's work. On the one hand Freud seems to propose
a precisely causal relationship between symbol and
origin: the structure of the individual is a set of

repressions which conceal infantile traumas by preserving
instead a symbolic "memory trace" (Freud, 1976a, p. 687).
Hence to understand is a process of re-tracing a linear
chronology of cause and effect.   And in the therapeutic
process, re-tracing is made possible by re-enactment.
But "re-enactment" suggests a one-to-one relationship
between symbol and experience - otherwise re-enactment
could not be known to be a "genuine" re-enactment as
distinct from, say, a distortion , a variation, or even
an antithesis; and hence therapy could not be a
re-tracing.   But, as we have seen (p. 114 above) Freud
is at pains to deny that symbols are unified labels
that can be thus mechanically manipulated: symbols,
for Freud, as for Jung and for Levi-Strauss, are
inherently ambiguous, and it is precisely their ambiguity
(in their relation to experience and to each other)
that constitutes their effect as symbols.

How, then,can the structure of symbolization be
related to the structure of experience, such that the
subject may, without self-contradiction or self-reduction,
engage in theoretic reflection upon experience?
This must be a central concern, since the subject's
theorizing must be reflexive, ie. grounded in the
processes of its experience, and (particularly) in
the processes whereby experience is conceived as available
to interpretation.   For example: theorizing may not
"make use of" symbolic structures as though they were
"equipment" with which an external reality might be
(accurately) "described" (see Heidegger, 1968).

It is Derrida's work which is helpful at this point. Derrida observes:

> There are two interpretations of interpretation, of structure, of sign, of play. The one seeks to decipher, dreams of deciphering a truth or an origin which escapes play and the order of the sign, and which lives the necessity of interpretation as an exile ... (and thus) throughout (its) entire history has dreamed of full presence, the reassuring foundations, the origin and the end of play ... The other ... no longer turned towards the origin, affirms play ..."   (Derrida, 1978, p. 292)

"History" here is the history of western culture, which has constructed for itself "linear" self-representations (in terms of "origins" and "foundations") as "enigmatic models" in such various forms as "scientific economy ... technics, ... ideology ... hierarchy ... graphic phoneticism (in writing) ... and the mundane concept of temporality", each of which depends in different ways on "the repression of pluri-dimensional symbolic thought" (Derrida, 1976, p. 86).

The link between these two passages from Derrida is as follows: it is the linear model of thought which creates the notion of truth as a re-tracable origin and the mundane model of temporality as a linear chronology which suggests that investigation can, by "reversing" chronology, re-enter the presence of a past cause of a current effect. But linear thought knows that it is only a model, that it operates by denying the pluri-dimensionality (the metaphoricity) of the symbols it claims to reduce to its own linearity, and hence that the "full presence" of truth is always only a dream: linear thought is thus condemned to approximate interpretations which it cannot theorize

except as always lamentable failures (in "exile").

In terms of the original question, then: linear thought
(the ideal of logic, of positivism) cannot be reflexive,
since it does not correspond to the structure of
experience nor to the structure of reflective thought.

It is within this general orientation that Derrida
wishes to liberate Freud from the linear model of the
symbol as a translation from an original experience,
by quoting Freud's own awareness of the "pluri-dimension-
ality" of the symbol: symbols "frequently have more
than one or even several meanings, and, as with Chinese
script, the correct interpretation can only be arrived
at on each occasion from the context" (Derrida, 1978,
p. 209, quoting Freud). In Derrida's revision of
Freud there is no "origin", only a plurality of symbolic
structures: "The unconscious text is already a weave
of pure traces, everything begins with reproduction"
(Derrida, 1978, p. 211). Hence the process of
repression is no longer the "forgetting" of an experience
but the creation of the meaning of experience through
the "deferral" of its interpretation. Indeed, only
"deferral" makes interpretation possible. "Différance
is originary and indeed it is the essence of life"
(ibid., p. 203).

This argument is thus not merely a revision of Freud
but a general theory of meaning. How might its generality
be grasped? If metaphoricity is the essence of the
symbol, then it must be conceived in terms of an
essential Difference. (Without Difference a symbol
lapses into the mere unity of a label). But the symbols

in which the subject represents experience to itself
partake of the temporal dimension of experience and
of subjectivity, since the subject is analytically
formulated (for Lacan quite explicitly) as discursively
constituted in the Self-Other <u>dialectic</u> (rather than in
the a-temporal moment, of individual perception or
intuition - see Heidegger's critique of Kant in
<u>Being and Time</u> (Heidegger, 1962, p. 410).   Derrida's
"Différance/Différence" thus evokes the dialectical
structure of consciousness as temporal as well as
analytical;   indeed his double formulation is intended
to cancel precisely such a distinction* in order to
express the conditions, the possibility, and the
effectivity of consciousness as a symbolic process.
Without Difference the symbol loses its metaphoricity
and thus its interpretive effect.   Without Deferral
("différance") experience loses its biographical
structure and thus its meaning.   Hence we can remedy
Lacan's failure (see p.   above):   difference and
deferral together make up the dialectical conditions
for the possibility of discourse - both the discursive
consciousness in which subjective experience is
constituted as intelligible, and the discourse of
theorizing by means of which subjects can formulate
their Being and re-formulate reflexively the possibility
of their so doing.

For Derrida this is particularly the case with
<u>writing</u>.   (See Derrida, 1976, p. 9).   Through the

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* by reminding us of an earlier etymological unity.

pluridimensionality of writing, and through the playful, critical interpretation of writing's pluridimensional text, the "ideologies" of linear expression, linear thought, and linear time may be "deconstructed" and hence transcended. Such arguments have important analogies with dialectics. Lenin indeed observed that "rectilinearity" of thought was a feature of "obscurantism" (Lenin, 1972, p. 363). From this point of view, dialectical thought is characterized by not having an origin or a final truth "at the end of the line" - but rather the endless "play" of contradiction, the ceaseless deferral of difference, by which each synthesis becomes the thesis for further antithetical questioning. "Writing bears within itself the necessity of its own critique" (Deridda, 1978, p. 284). The "play" of dialectic allows for the possibility of the development of a discursive subject and of the theoretical, critical comprehension of its development.

With such considerations we may formulate intelligibility for action-research's desire to disrupt the linearity of positivism. Action-research requires the possibility of a self-transcendent subject and the possibility of inquiry founded in interaction and in a non-determined dialectic between action and interpretation, ie. a form of inquiry located in biographical experience and hence in time, but not based on a determinist chronology in which a naturalized factuality provides a warrant for observation as "having-been accurate". It was earlier pointed out (in the

introduction to this chapter) that action-research,
finding no coherent alternative to such a chronology,
lodged its hopes in its "spaces", which we may now
interpret as action-research's awareness of positivism's
"exiled" status. This section has tried to provide
theoretic grounds for action-research (in this respect)
through a notion of dialectical play, which constitutes
both Self and Theory, Action and Research, and thus
can articulate their possible mutuality as well as
their difference.

However, although the rejection of determinism
is a necessary beginning to the formulation of a
competent theoretic subject, there are other important
stages in the argument. It is disquieting to read:
"The domain of play ... of signification henceforth
has no limit" (Derrida, 1978, p. 281), since such a
lack of "limit" seems to imply a theoretic subject
without a context and thus without cultural resources:
a symbolic order is indeed an "arena" for "play", but
an arena is defined by its limits - hence the value of
the contributions of Freud and Jung to the foregoing
discussion. Indeed action-research is particularly
concerned to theorize contextually, and thus part of
its central problematic is the relation between the
possibility of theory and the institutional context
(and above all the professional practices) to which
theory addresses its possibilities as possibilities.
Action-research could have no interest in a form of
theorizing in which "play" is taken not merely as the

openness of the dialectic but, concretely, as "a positive affirmation of a world without fault, without truth, without origin" (Derrida, 1979, p. 292) since action-research is defined by its need to address the limits of its institutional possibilities. In this, action-research is wiser than Derrida, whose playfulness fails to be reflexive when it ignores its relationship with the linear culture whose resources it is forced to use at one level even while opposing it at another. Derridean play risks being either intimidated, when it transgresses limits whose existence it denies, or being issued with a license to affirm the faultlessness of a world whose reasons for issuing such a license are of the deepest sociological interest. In other words, theorizing as a social practice must, in order to be reflexive, address the relation between the authority of theory itself and the distribution of institutionalized authority among different social practices. We need, then, a theory of "ideology".

Reflexivity and Theories of Ideology

The problem with the notion of ideology (already noted on p. 97 above) is that it articulates theory within the contingencies of cultural authority; in order to accomplish this, however, the theory of ideology must articulate itself as not subject to such contingencies. This leads to two further problems.

Firstly the theory of ideology demands from itself an

account of its own possibility (as non-ideological) -

this is the general problem of reflexivity.   Secondly,

if the theory of ideology were able to differentiate

itself in principle from its cultural contingencies

it would have at its disposal an absolute and thus

a prescriptive science of human action, which would be

a resource for positivism, not for action-research,

since action-research bases its claim for a necessarily

unending dialectic of action and research upon the

presumed failure of positivism in this respect.

Hence the aim of this section will be to question the

claims of "science" to prescribe remedies for "ideology",

and to consider how theorizing might recognize its

institutional context in ways which are both reflexive

and self-consistent.

Self-consistency is an issue which Althusser

explicitly raises for himself.   Having presented

as his "central thesis" that "ideology constitutes

individuals as subjects" ("Ideology and Ideological

State Apparatuses", in Althusser, 1971, p. 160), he then

admits:

> "Both he who is writing these lines and the
> reader who reads them are ... ideological
> subjects"
>   (ibid., p. 160)

and also that:

> "The author, insofar as he writes the lines of
> discourse which claims to be scientific is
> completely absent as a 'subject' from 'his'
> scientific discourse (for all scientific
> discourse is by definition a subject-less discourse ..."
>   (ibid.)

Another irony is added when he says: "(this) is a different question which I shall leave on one side for the moment" (ibid.), without specifying whether the "I" who does the leaving on one side is the ideological subject who is writing the text, or whether the phrase "I leave on one side" could claim to be part of a scientific discourse and thus "subject-less". If so, then what theoretic status can be attributed to a decision to leave aside the question of whether a text concerning the relation between ideology and science itself exemplifies ideology or science?) The questions could be multiplied at this point: they would all focus on the issue of Althusser's reflexive awareness as a theorist; on the unaddressed issue of the relation between what the text asserts ("subjectless-ness") and what the possibility of writing the text requires (an "I").

There is a further irony. Two years later Althusser wrote his Eléments d'autocritique (Althusser, 1974), in which the author confesses as an error (not as a superceded moment in a dialectical development) his "theoreticist tendency"* (p. 50) and withdrew important propositions, including his treatment of the notion of error (p. 42) and his "theory of the difference between science and ideology in general" ** (pp. 50-1), original emphasis).

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* "ma tendence théoriciste" (translations from Eléments d'Autocritique by R. Winter)

** "théorie de la différence entre la science et l'idéologie en général".

How is this to be understood? One starting point
would be to consider Althusser as writing within a
problematic of functionalism, so that all elements
are explained by their necessary contribution to a
material totality.   Hence, for Althusser, ideology is
a set of "practices" and not a set of ideas (Althusser,
1971, p. 159-60), and the framework within which these
ideological practices are presented is the "reproduction
of the relations of production" (ibid., p. 141) through
the medium of "ideological state apparatuses".   Now:
"there is no ideology except by the subject and for
subjects" (ibid., p. 159).   Indeed there is a "double
constitution" where by "the category of the subject is ...
constitutive of all ideology" and at the same time "all
ideology has the function (which defines it) of
'constituting' concrete individuals as subjects" (ibid.,
p. 160).   Hence "there is no practice except by and in
an ideology" and the various cultural formations become
"apparatuses" whereby the state determines subjectivity.
The general argument is thus one in which Parsons's
monolithic integration of institutions and role
expectations is extended to the level of consciousness
itself.   This is accomplished, according to Ernesto
Laclau (1979, p. 100) by Althusser's reading of Lacan's
formulation of the "Mirror phase" in the development of
the subject's self-representation.

Now, functionalism integrates all social actors
by installing Society-in-general in each one, and
Althusser is no exception: every subject is
"interpellated" by a Subject ("Unique, Absolute" -

Althusser, 1971, p. 166) and this Subject is (in various "ideological" guises) the state. Althusser's problem then is clear: he cannot be a subject, because all subjects are <u>determined</u> by the state, whereas in Althusser's writing the state, precisely, is his object. And yet he cannot <u>not</u> be a subject, because <u>all</u> subjects are determined by the state! Only through the "scientificity" of his writing could he be free of ideology (and hence of this dilemma) since "scientific writing" is the only practice which is not ideological. He must thus insist on his authorship (which is indeed continuously asserted by a marked professorial tone) while denying the subjectivity of his authorship, thereby illustrating perfectly the contradictions of "logo-centric" language as noted by Derrida (1976, p. 4, p. 12). (Clearly action-research's purposes are in no way served by a professorial, prescriptive science, nor by a reproductionist theory of consciousness: indeed it is precisely against such notions that much of Elliott's writing is directed, see Elliott, 1982a, p. 2, p. 25 ff.)

But Althusser himself, in his <u>Eléments d'autocritique</u>, thematizes the instability of the functionalist argument in ways which are relevant to action-research's needs. First of all he admits and regrets "the absence of contradiction" in his theory of ideology (pp. 81-2), which had led him into a regrettable "entanglement" with structuralism* (p. 53), which he rejects as "mechanistic" (p. 61):

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* "le jeune chiot du structuralisme nous a filé entre les jambes"

> "Marxism is not a form of structuralism, not
> because it affirms the primacy of process over
> structure, but because it affirms the primacy
> of contradiction over process". (ibid., p. 64)*

Indeed, Althusser elsewhere presents a theory of

"the materialist dialectic" in terms of "overdetermined

contradiction" (Althusser, 1977, p. 113) derived from his

reading of Freud and Lacan on the nature of symbolization

as a process of "displacement and condensation" (Althusser

1971, p. 191). And at both levels (the psycho-analytic

and the historical) Althusser is arguing against a

simplistic notion of determinism, either by "the economic

level" or by "the unconscious". Althusser wishes to

speak on behalf of the "metonymy and metaphor of language"

(ibid., p. 191) and the "dislocation of discourse"

(p. 192), and on behalf of philosophical thought as the

play of difference between metaphors (1974, pp. 18-19,

footnote). Each point here, however, would undermine

his own attempt at the literal description of a reproductive

apparatus for consciousness in his theory of ideology,

and thus weaken the challenge that such a theory

would pose for action-research.

Secondly, Althusser is concerned to remedy his former

"theoreticism" (1974, p. 13) by arguing that the practice

of philosophy is "class struggle at the level of theory"

(ibid., p. 86)**, and thus he asserts "the primacy of

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* "Le marxisme n'est pas un structuralisme, non
   pas parce qu'il affirme le primat du procès sur la
   structure, mais parce qu'il affirme le primat de
   la contradiction sur le procès".

** "La lutte des classes dans la théorie".

the practical function over the theoretical function
in philosophy itself" (p. 88).*   Hence "the particular
dialectic which is at work in the practice of philosophy"
(p. 86)** returns us once more to a formulation consonant
with action-research's dialectic of theory and practice,
since it is in both cases (as a dialectic) a formulation
concerned with the theorizing of change, whereas
functionalism (without a notion of contradiction or of
dialectic) cannot articulate change except through "deviance"
(see Talcott Parsons, 1951, p. 321) - ie. a failure of
functionalism's own theoretical framework.   Hence the
paradox of a Marxist functionalism, and hence the
inevitability of Althusser's recantation.

What remains after the recantation, is presented in
the work of Chantal Mouffe and Ernesto Laclau.   Within
a theory of ideologizing of "the subject" they present
a theory of "articulations" (Laclau, 1979, p. 7 and
Mouffe, 1981, p. 174), which denies that the "elements"
of thought and cultural practice serve specific class
interests, even where specific combinations of these
elements do so.   Thus ideological systems may be
"disarticulated" into their elements, so that these
elements may then be re-grouped ("re-articulated") so
as to transcend the class interests served by the
original combination.   Instead of "ideology" as a
unified structure imposed upon thought in general, we have
a level of "ideological struggle" as a process of

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* "Le primat de la fonction pratique sur la fonction
  théorique dans la philosophie même"

** "La dialectique très particulière qui est a l'oeuvre
  dans la pratique de la philosophie".

<u>contested</u> dis-articulation / re-articulation, in which

social classes struggle for the appropriation of "the

fundamental elements of a given society ... its 'social

imagery'" (Mouffe: op. cit., p. 175). In particular

there is a continuous contest between social classes

for an articulation of <u>its</u> class interests with the idea

of "The People" in general (Laclau, op. cit., p. 109).

This emphasis has points of contact with Foucault's

suggestion (cited with approval by Heinz Moser (Moser,

1978, p. 96) that although "knowledge" and "power"

are intimately conjoined, this does not imply a

structure of centralized imposition, but rather that

power is a ubiquitous and immanent feature of the

relationships within which knowledge is constituted

(Foucault, 1981, pp. 91-8-9).

How, then, might these various suggestions find

a place within the previous arguments concerning the

nature of the theoretic self and its theoretic

resources?

The first step is to note the link between the

ideological and the mythic. Paul Ricoeur (1981) seems

to open up this possibility by his description of the

features of ideology within a:

> "fundamental thesis ... that ideology is an
> unsurpassable phenomenon of social existence
> insofar as social reality always has a symbolic
> constitution and incorporates an interpretation,
> in images and representations, of the social
> bond itself". (p. 231)

According to Ricoeur, ideology presents "the social

bond itself" in terms of justifying and rationalizing

the originary basis (the "founding act" - p. 225) of

the particular social group; its tendencies are thus to
simplify and to reduce to a taken-for-granted orthodoxy,
to justify the authority which pervades and preserves
the group, and to treat its own symbolic representa-
tion not as a representation but as reality itself
(pp. 225-31). The crucial point for Ricoeur is
that ideology does not thematize (p. 227)

But, if ideology does not thematize the social bond,
why does ideology continue? Why cannot a taken-for-granted
social bond simply be taken for granted? If ideology
justifies authority, why is authority not simply accepted?
Whence, in other words, the apparently auto-destructive
quality of "justification"? The point is, that a
formulation of ideology needs to recognize ideology's
own fragility as a justification, in order to explain
ideology's necessity, as a widespread and endlessly
repeated feature of the cultural process. This is perhaps
the final irony for Althusser's functionalist theory:
the notion of a successful imposition of an ideologized
consciousness explains the reproduction of social relations
but fails to address the reproduction of ideology
itself. Conversely, only an ideological effort whose
effectiveness was always in question would "explain"
in functionalist terms) the necessity for a continuous
ideological process, and would also explain why social
relations are not simply reproduced but (slowly) develop.
Which is, after all, what Marxism would wish to argue.

Now, Ricoeur suggests that the Other of ideology
is thematization, leading to his general proposal of

"science" as <u>critique</u> (p. 235), but a simple opposition

of this type would lead to a confrontation which could

be resolved only by a capitulation - and one typical

outcome would then be the <u>prescription</u> of "critical

science" for a defeated "ideology" newly revealed as

error.   But Ricoeur wishes, on the contrary, to

establish "an intimately dialectical relation between

science and ideology" (p. 224), and for this (I would

argue) it is necessary to establish the possibilities

for thematization <u>within</u> ideology, as an "intimate" and

inherent contradiction which would sustain Ricoeur's

dialectic.   And this is where the notion of mythic

thought, as founded in contradiction, becomes crucial.

Thus, Levi-Strauss argues:

> "For a myth to be engendered by thought and for
> it to turn to engender other myths, it is necessary
> and sufficient that an initial opposition should
> be injected into experience ...   This inherent
> disparity of the world sets mythic thought in
> motion, but it does so because ... it conditions
> the existence of every object of thought".
> (1981, pp. 603-4).

Elsewhere he says:

> "The purpose of myth is to provide a logical
> model capable of overcoming a contradiction ...
> it therefore grows spiral-wise until the intell-
> ectual impulse which produced it is exhausted"
> (1972, p. 229).

It is for this reason that Levi-Strauss's analyses of

myth work towards structures of "binary opposites"

(1981, pp. 559, 692).   Hence, for example, myth does

not present a hero as heroic except in a dynamic,

dangerous, and problematic relation with a force for

villainy (a dragon or a "traitor").   Myth is indeed

the <u>exciting</u> story of their struggle.   Myth <u>justifies</u>

the original social bond but always at the "political"
cost, as it were, of thematizing it, by showing it
to be endangered.

My argument, then, is that ideology is not opposed
to thematization. Rather, there is an inevitable
tension within ideology, as part of ideology's process:
any elaborated justification implicitly thematizes the
questionability of the justification, the possibility of
alternatives. (Hence the possibility of critical
readings of even children's fairy stories)* However,
it is not possible to thematize everything at the same
time (Ricoeur, op. cit., p. 227). Thus, on the one
hand science can never find a position where it could
stand "apart" (as "pure" thematization) and criticize
ideology; and on the other hand ideology always evokes
the possibility of science at the very moment that it
denies science's accomplishment. This is the
dialectical relation desired by Ricoeur, a relation
founded ultimately on the reflexivity of consciousness
and on the "negativity" of language's relation to the
reality it speaks: to utter "justification" is to
recollect the possibility of non-justification.
Ideology and science thus constitute a mythic pairing;
hero and villain, locked in a necessary, mutually
constitutive, and unending combat, rehearsing the possibi-
lity but also the fragility of self-knowledge on the part

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* This ambiguity inherent in justification underlies
    Habermas's thesis of the current "legitimation
    crisis" of the modern state.
                        (Habermas, 1976, p. 71)

of the social group, and, by the dialectical progress of their struggle, transforming the parameters of that self-knowledge.

Such a framework can accommodate the proposal, by Laclau and Mouffe, of ideology as a <u>political</u> struggle for a particular articulation of social imagery, and also Althusser's suggestion that the practice of philosophy is "class struggle within theory". Both proposals can be seen as evoking the "interests" of knowledge (cf. Habermas, 1972) within a dialectical formulation both of political power and of the understanding of that power, ie. a dialectics of "interest" and a dialectics of knowledge.

In this way, the theory of myth-as-contradiction can help to formulate dialectical and reflexive possibilities for ideology. Can ideology perhaps help to reformulate myth? Levi-Strauss's argument concerning myth is that an "inherent disparity of the world (which) conditions the existence of every object of thought ... sets mythic thought in motion", and he gives examples such as the "disparity ... between the high and the low, the sky and the earth, land and water, the near and the far, left and right, male and female, etc." (1981, p. 603). For Levi-Strauss the universalization of mythic themes was an important project, but for the argument here the generality of his examples is a limitation, if myth is to be considered as a resource for theory in an <u>industrialized</u> society. In other words, we need to consider the concrete binary operations

of the "social imagery" (cf. Mouffe, loc. cit.) with which the members of an <u>industrial</u> society represent to themselves the "social bond" (cf. Ricoeur) in which specifically they are constituted.

Durkheim argues precisely the point that in industrial society the articulation of the social bond is analytically different in nature from that which characterizes the type of "primitive" society observed by Levi-Strauss, and the same argument poses a problem concerning the relevance of the Jungian "archetypes", which point always towards the archaic as the resource whereby modern society might recover its "mythopoeic imagination". Hence also the "pastoral" theme which underlies Jung's view of non-industrial cultures, so that he refers, for example, to "arab culture" as "the paradise of childhood" (1967, p. 272). In a sense Jung seems to view primitive man as the unconscious for the European, but not to envisage the possibility that the European could embody the unconscious for a primitive civilisation, as in "cargo cults". The "mythopoeic imagination" seems to be not so much a formulation of the process whereby the members of a modern society might recover an intelligible sense of self-reflection (which is the problematic for this study) but a nostalgic invocation of <u>direct</u> access to a concensual social meaning, which (Durkheim would argue) the differentiating structures of industrialization have for ever banished.

This, then, is what "ideology" could contribute
to myth-ology: the social imagery of a society structured
by hierarchical difference and contested power relations,
including, therefore, the imagery of class, bureaucracy,
and the state.   As a resource, therefore, ideology
as it were "updates" the vocabulary of myth.   (See
Barthes, 1976).   This "updating" is, I would argue,
a necessary process, given the complex relationship
in industrialized societies between their past and
their present, as mythic locations for their ideals.
While still haunted, as was the Greece of Homer and
Sophocles, by the imagery of past "golden ages"
inhabited by gods and heroes, we also glamorize the
contemporary, as part of the myth / ideology of
industrialization's progressive achievement: thus,
alongside the mythic images of Helen of Troy and Mary
Queen-of-Scots, we also install "Princess Di" and,
in capitalism's own dynastic Olympus: Joan Collins.
And in the institutional context of the educational
action-research worker, symbolic possibilities are also
articulated in terms of mythic figures both from the
past (Socrates - "the thinker", Archimedes - "practical
innovation", Einstein - "genius") and also in the present
(A.S. Neill - "creative freedom", Keith Joseph - "restrictive
power").   Even without being personalized, the imagery
of the institutional forces which make up the educational
context (the DES, the MSC, the LEA, the NUT, "the
school") do not form a unified and self-legitimating

system of norms which determine our comprehension of
them, any more than, at the political level itself,
they form a stable, static, and integrated set of
practices.   If, on the contrary, the institutional
context is seen as structured by political conflict
(rather than as a pacified "system") then our thinking
of that context can also, without inconsistency, be
characterized, not as a determined reflection of that
context, but as a dialectical process, both within the
oppositions of ideological imagery and within the
reflexive dialectic between ideology and theoretic
analysis.

To sum up, then: in this section I have argued
that action-research's ideal of critical reflection
upon its institutional context is not to be undermined
by a theory of ideology: on the contrary, theories
which would assimilate reflection ot its context are
themselves undermined by the overwhelming irony of
their non-reflexivity.   Rather, I would argue that
a dialectical formulation of institutional processes
and of ideological processes preserves the possibility
of critical theorizing and gives precision to its
aspirations.   At the same time, the unending dialectic
between ideology and theory is another formulation of
action-research's grounds for proposing the specific
intimacy of theory and practice, of research and action.
However, the characterization of analysis as a dialectical
process returns us to its basis in the Self-Other
relation and thus to the question of the form of the
social interaction within which theorizing may be
constituted.

The Social Relationships of Theoretic Reflection

Having established that theory is not necessarily encapsulated by a merely institutionalized authority (see previous section), the question becomes: wherein lies the authority for theorizing itself, or indeed: what is the relationship between theory and authority in a relationship constituted in a search for enlightenment? It has already been argued that for action-research this question has a specific urgency, since action-research wishes to refuse the theoretic authority of Nature, as uttered by a would-be oracular positivism. In wishing to locate theory within the domain of practitioners, to locate research as action-research, action-research must reject a simple polarized authority relation between the theorist who knows and the actor who is known. Otherwise, if it tries to theorize itself within such a relationship - as, indeed (as we have seen) it often does - action-research would be assimilated either to a ramshackle format for applied social science or to the apologetics for a sophisticated version of management theory - as indeed (as we have seen) it often is. For this reason this section will only be concerned with theory's authority within relationships where this authority is already rendered problematic by the axiomatic counter-authority of the knowledge available to the object of theory, the "social actor". Only a genuinely "balanced" opposition of this type (in which the problematic of action-research is constituted) could originate a

sustainable dialectic between its two aspects, ie. an
unending, because self-sustaining dialectics of theoriz-
ing, which would not rapidly "come to a standstill" at
one pole or other: "factual truth revealed, for ever" or
(alternatively) "pragmatic strategy agreed, for now".
This necessary and poised ambiguity, which action-research
writers have presented, for example, through the notion
of the "facilitation" (of actors by theorists) will be
approached by comparing it on the one hand with "therapy"
(for actors by theorists) and on the other hand with
"emancipation" (of actors into theorists). It is for
this reason that I begin once more with Freud, and with
Habermas's criticism of Freud.

Although Freud's theory of the object of inquiry
(the patient) aspires always to a determinism based
on causal origins which are positively known within
the structure of a general descriptive theory, these
causes and origins are manifested in wasy which are
irremediably personal. It is for this reason that
the "training" of an analyst must take the form of the
psycho-analysis of the would-be therapist (see "The
Question of Lay Analysis" Freud, 1962, p. 109).
Hence the ambiguity of the process of inquiry into
those causes and origins: inquiry cannot be "a causal
therapy in the literal sense" - that a cause identified
by the therapist thereby becomes available to remedy
by the therapist (Freud, 1952, p. 443). Rather, the
therapy depends on the contribution of the patient
(the "resistance", ibid., p. 445), and indeed the process
of inquiry becomes its own object (the "transference"

- pp. 449, 452), ie. inquiry is forced to become
inherently <u>reflexive</u>.   Hence:

> "The labour of overcoming resistance is the
> essential achievement of the analytic treatment:
> <u>the patient has to accomplish it</u>, and the
> physician makes it possible for him to do so by
> <u>suggestions</u> which are in the nature of an
> <u>education</u> ...   Psycho-analytic treatment is a
> kind of <u>re-education</u>".   (ibid., p. 459, my emphases)

But in this passage we have a statement of the basic

ambiguity at the level which Freud did <u>not</u> address:

the accomplishment is the patient's, but the terms

of that accomplishment are "suggested" by one in the

role of "physician";   the <u>accomplishment</u> is thus the

<u>acceptance</u> of a re-education, whose content is conceived

by Freud in terms of a biologically based causal theory.

It is precisely to this contradiction that Habermas

points, in a chapter called "The scientistic self-

misunderstanding of metapsychology" (Habermas, 1978,

p. 246 ff.).   Habermas's basic argument is that Freud

confuses the different epistemological bases underlying

respectively a natural, empirical science, with its

knowledge created by experimentation, and a hermeneutic

science "embedded in the context of self-reflection",

namely a dialogue "involving both partners, doctor and

patient" (ibid., p. 252-3).   Freud, says Habermas,

wishes to claim that <u>his</u> science is simultaneously both,

"because he considered the analytic situation of

dialogue quasi-experimental in character" (ibid., p. 253),

while Habermas emphasizes the incompatibility between

"the controlled observation of predicted results"

and "the intersubjectivity of mutual understanding"

(ibid.).   For Habermas, and for my argument here, it
is important to disentangle from Freud's "scientistic"
claim a formulation of analysis in which the authority
for "enlightenment" arising "intersubjectively" is
not obscured by the prescription of a supposedly prior
scientific expertise.   This would of course entail
questioning the basis of a relationship based on the
role of "the physician", whose "training", once it
is "completed" allows him to "re-educate" others.

        Jung's criticisms of Freud are helpful at this
juncture in the argument.   Jung emphasizes that there
is no set of "rules for interpretation" which a therapist
can bring to particular cases: he claims to abandon
what he terms "so-called 'methods'" and "theoretical
assumptions" (Jung, 1967, p. 153) and instead locates
the authority of the therapist in a rigorously pursued
self-understanding which (more unambiguously than for
Freud) is <u>not</u> the acquiring of "a set of concepts"
but learning "to know (one's) own psyche and to take
it seriously" (ibid., p. 154).   Further, this
"seriousness" is not the familiar "scientific"
seriousness of "systematic rigour":   quite the contrary
(see Jung, ibid., p. 153).   Rather, in more Heideggerian
terms it is a willingness to make a <u>commitment</u> to the
situation.   Therapists must bring their whole
personality "into play" along with that of the patient.
They cannot "cloak themselves in authority";   they are
"part of the drama" (ibid., p. 154).   The seriousness
of the therapist is thus not a defence, but on the
contrary a specifically recollected vulnerability: the

"play" of the drama is its unpredictability; uncloaked, the therapist is <u>at risk</u>; he is, indeed "effective only when he himself is affected. 'Only the wounded healer heals'." (ibid., p. 155). And - the final reflexive twist of the argument - "the healer heals himself" (ibid., p. 242). The condition of the therapeutic relationship, then, ceases to be the <u>resistance</u> of patients (Freud's version of reflexivity) but therapists' <u>lack</u> of resistance to their own sense of need. Jung thus introduces the notion of a "counter-trans-ference", whereby not only the patient but the therapist also focusses unconscious themes into the interaction (see Fordham, F, 1978, p. 80 ff.)*.

Underlying this radical abandonment of a cognitive authority are two important strands in Jung's thought. One is his emphasis on the uniqueness of the individual: "Interpretation ... requires a study of the whole symbol-producing individual" (Jung, 1977, p. 250). Each case requires "a new language", the grasping of a separate "personal myth" (1967, pp. 153, 224). Secondly, the sense of the unique individual is in turn grounded in what might be termed a theological epistemology, in which a radical protestantism - the direct access of the individual psyche to the divine - is constituted in an equally radical deism - the divine manifested in both nature and culture - hence Jung's interest in both alchemy and archetypes, as points of

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* For this last point I am indebted to my colleague
  Steve Decker.

intersection between the apparent availability of human symbolism and the numinous mystery of the grounds of that symbolism. For Jung, therefore, there is a ready and potent analogy between the therapist as a wounded healer and Christ as "the suffering servant of God" (1967, p. 243). But is it possible to interpret these ideas within the analytical limits of social inquiry itself, to "bracket out", as it were, the theological problematic in which indeed inquiry might be seen in terms of "the care of souls" (ibid., p. 242) and to understand in analytical terms what Jung seems to present as the necessary vulnerability of the would-be theorist?

In The Elementary Forms of the Religious Life Durkheim says: "The exceptional authority of ... Reason" is "the very authority of society ... transferring itself to a certain manner of thought which is the indispensable condition of all common action" (Durkheim, 1915, p. 17). And in "The Determination of Moral Facts" (in Durkheim, 1974) he says that he is "indifferent" as to whether the basic formulation of "collective being" is taken to be God or Society, since he sees "in the Divinity only society transfigured and symbolically expressed". (p. 52). Durkheim's sociological "indifference" in this respect permits the following analogy: the prophet of the divine "serves God" by recollecting the basis of the relationship between the divine and the earthly; the prophet is condemned to call for the sacrifice of earthly

advantage, as a manifestation of the seriousness of
its grounds in the divine. Such a sacrifice cannot
merely be preached - it must be exemplified: the
prophet's destiny is martyrdom, an earthly death
signifying the life of the divine. Similarly, the
social theorist "serves Society" by recollecting the
basis of the relationship between the theoretic
and the pragmatic; the theorist is condemned to call
for the sacrifice (the "interruption") of the pragmatic
as a manifestation of the seriousness of its grounds
in the theoretic. Such a recollection of the theoretic
grounds for social action cannot merely be preached
to others, but must be exemplified: the theorist must
suffer the demonstration that his/her own social action
(of providing theoretic grounds for others) is grounded
in the same theoretic basis. The theorist's destiny
is the unremitting recollection of reflexivity, a
pragmatic death, signifying the life of theory.

There is however, a further important step to
the analogy. For the conventional religious
"believer" (such as Jung's father, whose views he
rejected - see Jung, 1967) the martyrdom of the prophet
is not so much a tragic irony as a triumph. Similarly,
conventional social science proclaims theory as a
triumph - not a wound but a weapon, not an awareness but
a method. In contrast, the analytical reflexivity
of the theorist-as-wounded-healer is not a theory-<u>of</u>-
action, but a <u>meta-theory</u> - a theory, that is, of the
relationship between theory <u>and</u> action, of their

mutual neediness within a constitutive dialectic.

The recollection of the "life" of theory does not, then,

signify the _final_ "death" of pragmatic action;   rather,

both are moments in an unending cycle, that cycle of

theory and action to which action-research aspires.

The theoretic "authority" of the meta-theoretical

analysis of reflexivity does not create a relationship

whereby the theorist-as-physician provides diagnosis

and prescription for the social practitioner: rather,

it is a continuously self-cancelling, self-questioning

authority which constitutes a mutual dependence between

practitioner and theorist, as a "questioning dialectic"

(see previous chapter, final section).

A further way of grasping the mutuality of theory

and research, of theorist and practitioner, is in

terms of Kirkegaardian _irony_.

> "Constantly engaged in leading the phenomenon
> up to the Idea (the dialectical activity) the
> individual is thrust back, or rather flees back
> into actuality. But actuality itself has no
> other validity than to be the constant occasion
> for wanting to go beyond actuality - except that
> this never occurs. Whereupon the individual
> draws these exertions of subjectivity back into
> himself, terminates them in himself in personal
> satisfaction. Such is the standpoint of Irony".
>   (Kirkegaard, 1966, The Concept
>   of Irony, p. 183).

Irony itself therefore is the apparent triumph of

analytical reflexivity over action.   But Kirkegaard

continues:

> "Only insofar as irony is mastered ... does
> irony acquire its proper significance and true
> validity" (ibid., p. 338).

> "Irony as a mastered moment ... teaches us to
> actualize actuality ...   Actuality acquires
> its validity ... as a history wherein consciousness
> successively outlives itself ... actuality
> acquires its validity through action".
> (ibid., pp. 340-1)

In other words, although irony "establishes the disparity

between Idea and actuality ... and between possibility

and actuality" (ibid., p. 302), mastered irony does

not allow this disparity to be formulated as an

infinite negativity, which would not only ironize

action, at a distance, but would itself in turn be

rendered helplessly ironic by action, precisely in

terms of that distance.   Instead, the mastery of the

ironic moment formulates the disparity as a constitutive

relationship - between Idea and actuality, between

theory and practice.   This relationship would always

subject action to the irony of theory, but would also

ceaselessly challenge theory to transcend its negativity,

to "outlive itself" by formulating its necessary

relation to action.   Thus, through submission to irony,

the irony of the "healer" can itself (temporarily)

be "healed".

These lines of argument enable us to make two

vital points concerning action-research.   Firstly,

that action-research's desire to reject the prescriptive

theory of positivism makes a meta-theoretical framework

absolutely essential.   Moser (1978, p. 140) dismisses

meta-theory,arguing that since discourse is a

"practice in the life-world" ("Lebenspraxis") questions

of validity can be settled only within a given discourse

and therefore not "in general".   On the contrary,

I have argued that unless discourse is rigorously
formulated in meta-theoretical terms, ie. within an
analytically reflexive dialectic between action and
theory, then action-research's epistemology will be
engulfed by positivism, and action-research's investi-
gative relationships will be cast once more in the
authoritarian mould of the expert and the client,
the physician and the patient.

Secondly, we can challenge action-researchers
who suggest that they can act as "catalytic agents"
(Halsey, 1972, p. 198). The catalyst is a metaphor for
an "unchanged agent of change". Hence both Halsey
(op. cit., p. 58) and Moser (op. cit., p. 169)
anticipate a situation where action-research personnel
transform social actors, by endowing them with previously
absent qualities (theoretic autonomy), and then withdraw -
themselves apparently untransformed. Similarly,
Brown, et al. (1972) list the one-way traffic of
facilitation between "facilitator" and "participants",
but do not consider what might be facilitated in and for
the facilitator. The facilitator is a teacher, but
he/she is not taught. But, from Marx onwards, a
question for all sociology of educational processes
must be: how is the educator educated? ("Theses
on Feuerbach", III, Marx, 1970) and it is this issue
which has been reformulated in this section: the
dialectic of analytical reflexivity challenges the
theorist along with the practitioner, and denies
the direct authority of one over the other by submitting
both to the necessity of commitment and irony, and
providing a set of meta-theoretical auspices under

which "change agency" and "facilitation" are mutually

transformative processes.   Hence: what remains

unchanged by the process of inquiry is neither the

theories nor the practices of either theorist or

practitioner, but rather the auspices of reflexivity

and commitment, dialectical question and play,

under which the interaction of the inquiry proceeds.

We have thus reformulated the problem of the relation

between authority and inquiry.   Inquiry's desire for

critical enlightenment contests the authority given

by institutional roles (and the previous section

attempted to provide for the analytic possibility of

this contestation).   Similarly inquiry (in the form

required by action-research) denies the authority

derived from specific corpuses of knowledge located

externally to the situation under inquiry.   (This has

been the theme of this section).   The relationships

of inquiry thus exclude "scientists" and "catalysts",

and include only "participants-at-risk".

But authority is now embodied in the auspices

whereby inquiry _requires_, precisely, risk.   What form

of authority could sustain these auspices?

Habermas presents an answer at the requisite

level - that of the meta-theory of discourse - and

one that has appealed to action-research's aspiration

to formulate the relationships of inquiry as a non-

hierarchical "collaboration".   In a paper first

published in 1981* I presented Habermas's argument as
follows:

> "In his paper "Towards a Theory of Communicative
> Competence" (1970), Habermas presents "the
> dialogue constitutive universals ... of the ideal
> speech situation" as a <u>symmetrical</u> distribution
> of control by dialogue participants of the
> following dimensions of discourse:
>
> 1) The personal pronoun system;
>
> 2) The system of logical expressions of space,
>   time, identity, and causality;
>
> 3) The system of speech acts concerning speech
>   itself, eg. questions, answers, forms of
>   introduction and closure;
>
> 4) The system of speech acts concerning truth
>   and value, ie.
>
>   (a) being and appearance (claims and disputes)
>
>   (b) being and essence (revelations and
>   concealments)
>
>   (c) being and ought to be (prescriptions
>   and refusals)
>
> Habermas summarizes the significance of these as
> follows:
>
>> 'Pure intersubjectivity is determined by
>> a symmetrical relation between I and You
>> (We and You), I and He (We and They).
>> An unlimited interchangeability of dialogue
>> roles demands that no side be privileged
>> in the performance of these roles: pure
>> intersubjectivity exists only when there is
>> complete symmetry in the distribution of
>> assertion and disputation, revelation and
>> hiding, prescription and following among the
>> partners of a communication'. (p. 143)
>
> Only under the conditions of such "pure inter-
> subjectivity" can there be a discourse about

---

* "Social Research as Emancipatory Discourse", in
  P. Holly and D. Whitehead (eds): <u>Action-Research in
  Schools</u>, Cambridge Institute of Education, 1984;
  first published as Occasional Paper No. 1, Essex
  Institute of Higher Education, 1981.

> truth in which all can share who are affected,
> in which the absence of constraint allows only
> genuinely common interests to be agreed, in
> which there is no deception because each individual's
> interpretation is subjected to scrutiny, and in
> which no force except that of the better argument
> is used. (Habermas, 1976, pp. 108-110)"

(The paper continues by arguing directly from this account

of Habermas to the possibility of "emancipatory"

dialogue and collaboration as the essential format for

inquiry).

Now, if the ideal of speech is taken to be the

"unlimited interchangeability of dialogue roles",

then it is possible to say, as Habermas does: "With

the very first sentence the intention of a general

and voluntary concensus is unmistakably enunciated"

(Habermas, 1974, p. 17). Hence the ideal of

"dialogue" provides the authoritative auspices for the

search for enlightenment, since it provides analytically

for agreement that the outcome of dialogue is enlighten-

ment, and it also provides auspices for the necessary

relationships, ie. egalitarian mutuality. Hence

Habermas's appeal for action-research writers.

But this argument moves through a series of un-

reflexive abstractions. To begin with, language is

equated on the one hand with the form of utterances

("pronoun system") and on the other hand with its social

function ("speech acts") and thus ignores the reflexive

processes by which utterances and social interactions

are mutually constitutive - the processes described,

for example, by Lacan and Garfinkel. Then, having

thus dismantled the complexity of symbolization into its notional "components", Habermas can then reassemble them back into an "intersubjectivity" whose "purity" seems to represent the final cessation of dialectic, and is postulated precisely so that it will offer no ressitance to "concensus", as a static moment of changeless unity. Finally, the invocation of "logic" in terms of Kantian universals is significant: an unproblematically valid "logic" would indeed prescribe the "better" argument and thereby endow it with "force". My argument against Habermas here parallels Heidegger's criticism of Kant's abstraction of the cognitive subject and of the a-temporal moment of "intuition" from the dialectics of Being (Heidegger, 1962, pp. 366-8, p. 410 - see Chapter One, p. 33 above). Habermas's argument is "utopian": the auspices for theorizing which it proposes are illusory: it provides a theory of language which cannot enter into relation-ship with the practice of language, but remains as a disabling irony. Unlike Kirkegaardian irony it does not ironize itself with an awareness of its own self-contradiction: for Kirkegaard, irony itself must, as it were, be seen ironically, and hence provide for the mastery of its own moment. The inert unity of Habermas's ideal of speech simply "makes a mockery" of actual speech, just as the ideal of science, says Garfinkel, mocks the actual practices of comprehension.

And yet this discursive ideal rendered inert by the purity of its intersubjectivity and the finality of its concensus is far from what Habermas himself requires.

On the contrary, he wishes to formulate a "universal
pragmatics of speech" as an ideal which can guide the
adjudication of validity claims in practical life
(Habermas, 1979, pp. 2-5).   Elsewhere he states that
he rejects "pure theory" in order to urge that knowledge
and interest are related in a "dialectic" (1978,
pp. 314-5) and that "it is not unconstrained inter-
subjectivity itself that we call dialectic, but the
history of its repression and reestablishment" (ibid.,
p. 59).

A meta-theory of the auspices for a reflexive
dialectics of inquiry, then, must provide for its own
history, of which concensual intersubjectivity would be
an evanescent and self-cancelling moment, rather than
a teleology or a prescription.   The relationship
whereby these auspices might be maintained remain
problematic: my argument against Habermas is that
auspices for theorizing cannot be equated with an
egalitarian dialogue formulated as the "interchangeability
of roles".   As Habermas himself says, enlightenment
could only be equated with agreement (among those whose
roles have ceased to be differentiated) in a society
which was "already emancipated" (Habermas, 1978,
p. 315), and where, in consequence, inquiry's own
interest and necessity would seem to be in question.

But does not the requirement of a reflexive
dialectic also imply a symmetrical distribution of
initiative and responsibility in a sense which
preserves implicitly at least something of Habermas's
egalitarian ideal?   Are not all at risk?   Are not

all to be transformed?   Certainly, analytical reflexivity
is not a corpus of knowledge which one might authoritatively
possess and transmit;   rather it would be exemplified,
and its exemplification would begin with a question
addressed to its own possibility.   Can anything be
said concerning the distribution, symmetrical or other-
wise, of the initiatives for such questioning among
the participants of inquiry?   Are we perhaps even
talking of a theoretic practice which requires a
specific prior "education" and thus reconstitutes a
hierarchical relation between "theorists", for whom
the reflexive dialectic is a _familiar_ risk, and
"practitioners", to whom its challenges must be
unfamiliar, and whose attempts at reflexive analysis
will be, initially at least, "unskilful"?   Our
question has thus become: the relationship between
the mundane reflexivity of interaction and the
analytic reflexivity of theory.

At this point we must consider more carefully
the familiar dichotomy between hierarchy and equality.
At the level of role relationships hierarchy and
equality are related in a dimension of power, and
present their Difference in the form of a question:
what might be the legitimate grounds for a difference
in power?   In contrast, the difference between theory
and action is not a question but a necessity: to
abandon their Difference is to annihilate both (see
previous Chapter).   However this Difference does not
organize a power dimension except within the positivist

version of theory, which presumes to dictate its
interpretations and explanations as the real "nature"
of action.   Thus, analytic reflexivity is related
to mundane reflexivity as an irremediable Difference,
yet not within a power relation, but as an analytic
necessity.   Indeed, the very problematic of this
section - what is the appropriate social relation for
an investigative enterprise? - must be queried, since
it presupposes the possibility that a theoretic
stance could be equated with a social relationship,
which is indeed Habermas's suggestion, but which previous
arguments enable us now to reject in principle.   This
in turn helps to justify action-research's claim that
investigation can be an individual process of self-
transcendence, that collaboration or facilitation are
"helpful", but not necessary.   Within a Habermasian
frame of reference this is not intelligible.

There are two main dimensions to the suggestion
that there is no power dimension constructed by the
Difference with which analytical reflexivity confronts
the mundane reflexivity of action as the auspices of
theoretic work.   First, as we have noted previously
in this section, it is a challenge to its own grounds
as well as to the grounds of action.   Secondly, this
very Difference is constructed within a particular
intimacy   between analytic and mundane reflexivity.
Analytical reflexivity always has as its occasion and
potential starting point social members' implicit
grasp (within a dialectic of consciousness and action)

of the ambiguities and contradictions which characterize

the symbolic processes of familiar cultural forms (see

Chapter One, p.   )   The most vivid examples of this

would be in the realm of the aesthetic: jokes,

advertisements, soap operas, films, novels: all play

with the crucial meanings which structure collective

self-representation, and thereby constitute their massive

appeal, as "entertainment".   Often, their "play" is

explicitly quite "risky": only "in the end" do they

turn aside from their subversive course and gloss

their own challenges as within the realm of mundane order.

At one level, mundane reflexivity is, as Garfinkel

says, "uninteresting": what is, on the contrary,

of absorbing interest (and thus "entertaining"),

is to play with the limits of mundane reflexivity - to

move "thrillingly" close to an analytic rupture.

The Difference of analytic reflexivity, however, is that

its auspices are actually to make that rupture, to suffer

the fall from the high wire, to plunge into symbolization's

own reflexive abyss.   (Earlier in the discussion - of

ideology as "justification" - I noted the inherent

link between play and risk at the level of language

itself, see p. 138 above).   My argument, then, is

that although only theorists do it, everyone could do

it, because everyone can envisage it, and does so, all

the time.

In other words, the resources for analytic reflexivity

are provided for as possibilities in the ambiguous

meaning structures of culture itself, ie. in their

mythic, metaphorical, dialectical, and ideological

features.   As Blum and McHugh would say: everyone

can be a theorist;   any experience can be the occasion

for theorizing (see McHugh et al., 1974, Introduction).

It is this tension between the familiar and the

different, between play as entertainment and play as

critical analysis, that the auspices of analytical

reflexivity can provide for a dialectic between theorist

and practitioner which calls upon the contribution of

both - a dialectic which formulates an epistemological

relationship quite precisely, but which has no

necessary implications for the institutionalized

interaction in which it might be embodied.

In this way, then, "critical reflection" may be

formulated as a possible stance: it may be established,

as action-research requires, independently of specific

role-relationships and independently of specific

corpuses of academic knowledge;   in other words, it

establishes Subjectivity in a resource-ful, rather than

a determined, relation to the symbolizing processes of

both psyche and culture.

CHAPTER FOUR

ACTION-RESEARCH AND THE IMPROVEMENT OF PROFESSIONAL PRACTICE

"Improving Professional Practice"

So far we have pursued action-research's aspirations down two avenues: the intelligible unification of theory and practice (Chapter Two) and the theoretic competence, resources, and independence of the reflective individual (Chapter Three).   This chapter is concerned with a further theme which is widely invoked as a basis for action-research investigation - both as a defining characteristic and even as an implicit criterion of adequacy - and which thus serves to provide a further elaboration of action-research's inherent problematic:-action-research, it is almost universally claimed, is founded upon "the improvement of professional practice".

The initial problem is illustrated by the following statement by Brown, et al. (1982, p. 2):

> "Action-research ... has as its central feature
> the use of changes in practice as a way of
> inducing improvement in the practice itself, the
> situation in which it occurs, the rationale for
> the work, and in the understanding of all these.
> Action-research uses strategic action as a probe
> for improvement and understanding.   In fact
> the action-researcher selects a particular
> variation of practice with these two criteria
> uppermost".

The argument of the passage enacts a crucial ambiguity: on the one hand it seems as though "changes" in practice will, by "probing" a situation, disclose

hitherto concealed possibilities, and thereby <u>lead</u>
(in an open-ended sense) to an improved understanding
of the situation; on the other hand such changes
are presented as "strategic", ie. as <u>planned</u> with a
clearly envisaged end in view, so that improvement in
both understanding <u>and</u> in practice will be <u>induced</u>,
as though the range of outcomes were predictable.
Hence the direct question becomes: will <u>any</u> changes
in practice lead to an improved understanding, or
only a special type of change? The answer suggested
(only those changes which lead to an "improvement"
in practice) begs the question: how are criteria for
practice grounded, such that those leading to
"improvement" may be distinguished from those leading
to "deterioration"? Unless this question is addressed,
the recommendation that "improved practice" can generate
"understanding" becomes a form of unexplicated
prescription: whereas positivist evaluation failed
(inevitably) to explicate its grounds for prescribing
methods for investigation (see Chapter Two), action-research
(in this formulation) threatens to allow the methods
and outcomes of investigation to remain open merely
at the cost of failing to give grounds for its prescrip-
tion of methods for professional practice. Thus the
relationship between Brown et al.'s "two criteria"
("improvement" of practice and "understanding" practice)
becomes a central issue, one which their juxtaposition
above tacitly avoids.

The difficulty is <u>not</u> lack of elaboration of the
relationship, as may be seen by a summary of the argument
which the writers present: practices, they argue, can
only be understood in conjunction with their rationale,
and thus they are open to critical self-reflection.
This in turn requires collaboration with other practi-
tioners, who may otherwise stand in a hierarchical
relation to each other, and such relationships must
be "suspended" so that an "unconstrained" critical
self-reflection can take place. Action-research
is thus an expression of Habermas's "emancipatory
knowledge-constitutive interest", a form of "critical
theory", and "fundamentally concerned with democratic
values as these are expressed in the idea of a self-
reflective community" (Brown et al., op. cit., p. 3).
In this way the development of understanding and the
development of social practices are encapsulated within
a notion of "emancipation" (see final section of
Chapter Three, above).

At one level this line of argument may be seen
as the justificatory invocation of a rhetorical
tradition, mobilizing well-known notions in a heroic
scenario: rational action, critical theory, self-
reflection, collaboration, democracy, emancipation,
autonomy, and community triumph together over
"hierarchy" and "the constraints of habit, tradition,
and institutional intransigence" (Brown et al., op. cit.,
p. 14); and one important way of analyzing it would
be to "deconstruct" the various elements into their
constitutive metaphors, ambiguities, and contradictions,

as Levi-Strauss and Marx analyze the structure of other

heroic scenarios.

However, a more direct problem with the argument

as presented is its central lacuna: changes in practice

are envisaged in terms of teachers' professional work,

but the scenario of "emancipation" concerns only the

teachers' freedom to reflect and to innovate; it is

not seen as constituting the nature of the work

itself, namely the practice of teaching.   The question

therefore becomes: on what basis is such a boundary

for the principle of "emancipation" constructed?

In other words, the problem concerns the relationship

between authority and emancipation in the research

activity and that same tension in the professional

practices which the research activity claims to be

able to use both as a topic and as a resource.   It is

this relationship on which Brown et al. are silent.

Suppose, for example, that a group of teachers

(including a head of department and a scale I staff

member) reflected upon their "habitual and traditional"

practices, and determined to liberate themselves from

the "constraints" imposed upon their work by a taken-

for-granted professional ideology of active contributions

to lessons by pupils, and decided instead - mounting

a thorough critique of institutional policy - to

translate an area of the curriculum into predetermined

behavioural objectives for which massive and carefully

planned practice would be given, reinforced by a

calculated system of symbolic rewards and punishments,

in order that the curriculum should be "more effectively learned". Such an example would exploit the lacuna in the argument, since the hypothetical case describes the liberation from "constraint" of a group of teachers (as a Habermasian "speech community") in order to enable them to increase the constraints upon the learners within their classrooms. Thus: the innovative discourse would deconstruct the hierarchical relation between the Head of Department and the Scale I teacher, freeing them to interact on the basis only of "the better argument" and of their "common interests" (Habermas, 1976, p. 108), but as a result, the hierarchical relationship of the classroom would be intensified by an increased didacticism: a more prescriptive curriculum backed up by a more intensive application of external sanctions, which would reduce the opportunity for pupils to present "arguments" concerning the curriculum and would necessitate that pupils' "interests" be defined by teachers (cf. Brown et al.'s quotation from Habermas above). So the question is: upon what grounds is staff discourse to be considered in relation to the Habermasian ideal, but not classroom discourse?

Certainly, this exclusion is not explicitly intended by action-research writers. For example Holly (1984) indicates the "emancipatory" thrust of action-research by means of the following diagram:

The Institution	Action-research
Hierarchical; vertical relationships	Horizontal relationships
Divided; compartmental-ized (subject-based)	Unified; collaborative across subject boundaries
Bureaucratic; "Top-down" management style	Democratic; "Bottom-up" management style
Closed	Open
Formal (teacher-pupil relationships)	Informal

Holly's argument is that action-research generates
relationships among staff which are incompatible
with the requirements of the school as a societally
determined institution.   His vocabulary suggests
a clear debt to Bernstein (Bernstein, 1971a, 1971b) and
hence, indirectly, to Durkheimian arguments concerning
social order in general, the wider implications of
which will be explored in more detail later (see p. 202 ff.
below).   This echo makes it clear that for Holly
action-research will challenge institutional structures
inside the classroom as well as outside, so that, for
example, "informal" teacher-pupil relationships may
be thought of as "horizontal", and as a "democratic"
style of classroom management.

But these are of course metaphors for a supposedly
dichotomous mutual exclusion which itself evokes
one of the central difficulties of an "emancipatory"
problematic for action-research: if the opposition
between action-research and its institutional setting
is of the categorical nature suggested by such pairings

as vertical/horizontal, open/closed etc., then it
becomes difficult to see in what sense Holly recommends
"caution" (op. cit., p. 100): is he suggesting that
by means of "caution" action-research might conceivably
succeed in "overthrowing" institutions into a diametrically
opposite form? If so, how could this be understood?
In fact Holly's pairs of terms are schematic, evoking
rather than analyzing the sense of the contrast they
present: "open" and "closed", for example, are the
most crudely ideological formulation for the issue of
"democratic" relationships, begging every question of
their meaning, and every student of "organizational
theory" knows that a "formal" structure generates an
"informal" structure as part of the inevitable texture
of institutional life (see Selznick, 1964).

Heinz Moser (1978) argues even more explicitly
than Holly for the intimate relation between the
processes of investigation and the aims and criteria
for professional practices, by making clear that for him
the notion of a critical social science of education
must also in principle inform pedagogical practices,
ie. he explicitly does not make the separation which
is so problematic in the work of Brown et al. Indeed,
he says: "... Pedagogy, in the sense of a critical
theory of education, is taken to be a theory of society".
(p. 12)* This "critical theory of education" is

---

* "Pädagogik wird ... im Sinne einer kritischen
  Erziehungswissenschaft als Gesellschaftwissenschaft
  verstanden". (Translations from Moser by R. Winter).

conceived as requiring the autonomy of youth and as

tending to render the educational relation itself

superfluous (p. 14), and it is therefore also in this

sense that Moser claims, "A science of education which

is not devoted to the maintenance of the status quo

but which includes in its programme this process of

substantial liberation must therefore identify with the

emancipatory interest which characterizes science

as the enablement of a liberating praxis". (p. 13)*

In other words, for Moser, a theory of research which

liberates the teacher is inseparable from a theory

of teaching which liberates the learner. Hence Moser

contrasts education for the progressive liberation

of youth _from_ education _into_ autonomy, with education

as the instrumental practice of the educating Subject

upon the child Object (p. 14), and generalizes from

this: "Education therefore becomes the sine qua non

of any (social) scientific programme, which must

first of all 'create' self-reflective Subjects" (p. 19)**

However, this argument - although more sophisticated

than Holly's diagram - by its abstract and programmatic

form seems to neglect action-research's specific

concern: the creation of a principled relation between

---

* "Erziehungswissenschaft, die sich nicht dem bestehenden
  status quo verpflichtet, sondern jenem Prozess
  substantieller Befreiung in ihr Programm aufnimmt,
  hat sich deshalb jenes emanzipatorische Interesse
  zu eigen zu machen, dass Wissenschaft als
  Ermöglichung befreiender Praxis charakterisiert".

** "Erziehung wird damit sine qua non für eine
  Wissenschaftsprogrammatik, welche die sie (sic.
  "sich"? - RW) anerkennenden Subjekte erst 'schaffen'
  muss".

theory and <u>practice</u>, between research and professional work <u>within</u> an institutional context.

In contrast, Elliott (1975) is much more specific about what he envisages as action-research's programme for improving professional practices. His Ford Teaching Project is an action-research investigation devoted to the implementation of "Inquiry/Discovery Learning" in the classroom, ie. to a specific pedagogy which is explicitly described as the removal by teachers of the "constraints" upon their teaching method fostered by their own institutionalized authority, such as their general tendencies to impose "preconceived problems", to reformulate problems in their own words, (op. cit., p. 7) to impose changes in the direction of discussion, to probe "too deeply" into pupils' personal lives (ibid., p. 12), etc. In each case "the principles of Inquiry / Discovery Learning" involve recommending to teachers "constraint removing strategies" (ibid., p. 6). The generality of the principle of "constraint removal" in the classroom is shown in one of Elliott's recent papers (Elliott, 1982a) where he suggests that Stenhouse's <u>Humanities Curriculum Project</u>, in which the teacher's role is reformulated as that of the "neutral chairman", was an attempt to set up in the classroom a Habermasian "ideal speech situation" (op. cit., p. 22), and Elliott describes his own work as located within a similar problematic (ibid., p. 23).

For Elliott also, then, "emancipation" provides
a criterion which guides both the relationships of
the action-research investigative process and the
professional / client relationships of the "practice"
which forms both a topic and a resource for action-
research. What Elliott fails to consider, however,
is the irony implicit in proposals to realize "ideals"
in practical situations. This is a version of
Garfinkel's issue (see Chapter Two, p*p*. 64-5): a
presentation of practical "failures" to realize ideals
merely directs attention from actual processes.
Thus, given that teachers cannot simply "remove
constraints" without abandoning the fundamental
parameters of their institutionalized roles, we are
left wondering what sort of judgements action-researching
teachers actually make: action-research must formulate
(as the essence of its proposal to unify theory and
practice) what "emancipation" could mean as a form of
action within an institutional context.

Elliott structures his argument around two suggest-
ive pairs of terms: a contrast between Habermas's
"ideal speech situation" and "Bureaucracy" (Elliott,
1982a, pp. 22-4), and between "ethical" and "technical"
theories of teaching (Elliott, 1982b, p. 20) where
he argues that criteria for the "validity" of an
educational process reside in the values guiding the activi-
ty rather than in measurable qualities of the outcome.
But the work of this latter project makes clear the
nature of the problem when action-research attempts

to enact its "emancipatory" aspiration.   The first

aim of the project is defined as:

> "To help teachers monitor the extent to which
> the higher level understanding tasks they plan
> for pupils ... qualitatively differ from those
> which pupils actually come to work on in class-
> room settings" (op. cit., p. 12)

Later Elliott writes:

> "I would claim that the idea of 'teaching for
> understanding' is entailed by educational action-
> research.   The latter has as its general focus
> educational action, but what makes action
> educational is not the production of extrinsic
> end states but the intrinsic qualities expressed
> in the manner of its performance ...   The general
> idea of teaching for understanding simply
> specifies a quality of educational action, and as
> such guides, rather than directs, teacher
> deliberations about how to improve the educational
> quality of their teaching" (op. cit., pp. 21-1).

Elliott's argument is that "understanding" is entailed

as a pragmatic consequence of the educational enter-

prise;   hence to be engaged in educational action-research

is ipse facto to be engaged in teaching for understanding

and thus in "improving the quality" of educational

practice.   But this is merely to take for granted the

conventional normative form of the term "education",

in the same way as "the philosophy of education" used

to justify current practices by unexplicated appeals to

normative linguistic usage, a form of philosophic

practice whose theoretic weaknesses are cogently

analyzed by Ernest Gellner in Words and Things (Gellner,

1968).   Elliott wishes to attenuate the prescriptive-

ness of his appeal to usage ("guides rather than directs")

in accordance with action-research's desire to preserve

practitioners' autonomy, but the residual prescriptive-

ness remains unaddressed. "Education" is taken as a
unitary meaning which can be insisted upon, rather
than as a moment in a dialectic, whose contradictions
(say: authority and autonomy) can be explored but
not resolved: the educational practitioner as reflexive
theorist would need to pose the nature of "education"
as a question, rather than utilize it as an authoritative
usage.

The notion of "levels of understanding" seems
to be an even more concrete instance of taking for
granted precisely what should be an issue (the question-
able relationship between the rationalities of teachers'
plans on the one hand and of pupils' "work" on the
other) in order to invoke a cognitive hierarchy quite
at variance with action-research's proclaimed desire
to ground criteria for action in an ideal of
emancipation.

Elliott's paper is programmatic for the "TIQL"
(Teacher-Pupil Interaction and the Quality of Learning)
project. When one turns to some of the reports
produced by the teachers involved, one finds even
clearer examples of the failure to address the grounds
of the professional practices which have been
"researched" or the grounds of the research process
itself. Thus Ingham (1984) reports:

> "I very soon became aware through my observation
> that children often return to lower order concepts
> when acquiring those of a higher order, if they
> consider it relevant to the situation. I was
> able to show that if there is a deficiency in
> the lower order network, then it will be
> difficult for pupils to attain a higher level
> until this has been made good". (pp. 5-6)

The claims to <u>observe</u> "higher" and "lower" orders of
conceptualization represents an unreflexive prescriptive-
ness towards pupils' meanings which demonstrates quite
dramatically the dangers of action-researcher's failure
to articulate an alternative to positivism: Ingham
here comes close to exemplifying the hypothetical
siutation described on p. 165 above: her own research
stance is accomplished by assuming the "instrumental"
stance of "the educating Subject upon the child Object"
which, as Moser suggests (p. 169 above), is the antithesis
of the desire on the part of action-research and of a
critical social science to found their practices upon
the constitutive possibility of "self-reflective Subjects".

Not surprizingly, Ingham does not point to the
system of unexplicated norms in the following recommenda-
tions which she quotes from another professional
practitioner / action-researcher in the project:

> "The desire of children to re-negotiate and
> simplify tasks is widespread. Children will
> frequently go against given instructions if
> they can see a short-cut to the answer.
> Work should be scrutinized when set to avoid
> leaving these short-cuts open"
> (Ingham, op. cit., p. 7).

Only by questioning the notions of higher and lower
"levels of understanding" would Ingham have been able
to consider what children as well as teachers see as
reelvant as rational features of a situation which
children will inevitably be formulating <u>for themselves</u>.
Classroom practices are constituted in a mutual
difference between teacher and taught which action-
research aspires to address as a <u>problematic</u> inter-

subjectivity between "reflective Subjects": if educational

practice is conceived as the <u>setting</u> of a task and as

a requirement of <u>the answer</u>, this Difference will be

glossed by an authoritarian imposition, presented

nevertheless as that concensus which (as Elliott implies

above, p. 172) education's norm of autonomous rationality

always necessarily invokes. In other words, criteria

for the "improvement" of practice must be theorized

independently of the institutional authority by which

practices are routinely evaluated, namely ideologies

of higher and lower levels of understanding, and of

obedience to instructions which gloss their constitutive

contradictions. Otherwise action-research lapses

into a managerial rhetoric which takes for granted

precisely the judgements it should be questioning.

However, Ingham's paper, though significant, is

exceptional: on the whole action-research work has

indeed been concerned to formulate the improvement of

practice by questioning its prescriptive version.

Thus, Michael Armstrong says, at the beginning of the

article which Nixon uses to open his collection of

action-research writing (Nixon, 1981a):

> "Part of the art of teaching consists ... in
> asking children questions, discussing their ideas,
> exchanging experiences with them... There is a
> self-consciousness implicit in this aspect of
> a teacher's activity that makes those teachers who
> manage it successfully - however fitful and
> fragmentary their success - students of those
> they teach as well as their teachers".
> (Armstrong M., 1981, p. 15).

Even more explicitly, Stephen Rowland says:

"It is only by committing ourselves to ... a
process of self-education that we can fully
appreciate the endeavours of the children we
teach as they strive to make sense of their world"
(Rowland S., 1983, Introduction)

This may be construed as a specific denial of the

authoritative separation of teacher and taught, of

professional and client: the educator's resource for

a reflexively conceived educational enterprise is not

a realization of his or her knowledge and of the children's

ignorance, but rather of his or her own ignorance and of

the children's understanding.   In general terms: a

reflexive analysis of professionalism would render

problematic a series of normative definitions and their

attendant systems of authoritative decision-making:

a reflexive social-work profession would problematize its

basis for distinguishing between and responding to

"deviant" and "normal" ways of life, a reflexive medical

profession would question its conceptions of health

and treatment, and reflexive journalism its conceptions

of newsworthiness and reportage.

A concrete example of how an action-research

project might begin to work towards such an awareness

of the problematic nature of professional practice is

given in John Crookes's paper (Crookes J., 1983) in

Stephen Rowland's collection.   Crookes tape-recorded

a conversation during a science lesson with two 15

year old pupils concerning why crystals grow, and what

relationship there might be between the growth of

crystals and the growth of human beings.   He observes:

> "My early reactions on transcribing the tape
> were confused, as I had expected the conversation
> to turn largely on the differing mechanisms
> each boy employed to explain growth" (p. 9)

In fact the two boys kept "digressing" onto the topic of

destiny and hence to their own identities and futures.

He analyzes his sense of confusion as follows:

> "The importance of the learner's own knowledge
> in the growth of understanding ... can be viewed
> as largely one-way ...   The teacher encourages
> the learner to tell of what he knows already
> (so that) the learner's own knowledge is a resource
> to be used by the teacher.   (But) the learner's
> knowledge cannot be circumscribed in this fashion ...
> For as well as using his own knowledge to interpret
> and understand new events, the learner also uses
> these phenomena as vehicles for the interpretation
> and understanding of his own preoccupations and
> concerns ...   (Hence) one of the reasons for
> my initial confusion was an inability to see
> Anthony's using the crystal as a starting point to
> re-explore an issue that continued to perplex him"
> (p. 10)

The first step in Crookes's analysis, then, is

his recognition that what an educational practice is

about (ie. "the curriculum") is constituted as problematic

within that practice: the teacher's definition of the

nature of his professional practice ("a lesson about

crystals") is challenged by pupils to be simultaneously

intelligible as a discussion about their own destinies.

The second step is to recognize that the problematic

nature of this intersubjectivity, its "digressiveness"

is not an inadequate realization of a normative "pure"

intersubjectivity (as Habermas might argue - see Chapter

Three, p.154), in which an "improvement" might be

to avoid such "digressions", but on the contrary a

condition of the creative process of understanding.

To this end, Crookes cites the "digressions" in the work of Kepler, with its "analogies from every phase of life ... exhortations ... textual quibbles ... personal anecdotes ... delighted exclamations" (p. 10) and observes that Rob and Anthony "stand in an analogous position to an early scientist" and that "their interest and concerns spread more widely and deeply than the usual mundane events of the classroom". (p. 11)   In this way Crookes questions his own teacherly authority by providing as it were a counter-authority for Rob and Anthony to play an autonomous role in the constitution of their education: education itself becomes no longer a professional practice "carried out" by teachers but the achievement (between teachers and pupils) of an imaginative intersubjectivity which enables the exploration of the metaphors ("growth" in this instance)   whereby such intersubjectivity accomplishes its communicative process.

However, a pointer to an interesting limitation in Crookes's paper is contained in his reference to Kepler as an "early" scientist.   Elsewhere he develops a contrasting account of the procedures of "modern" science in highly normative terms.   This suggests the difficulty of adopting a reflexive stance towards one's professional authority.   Crookes seems to have succeeded in retreating from his conventional authority as a teacher by taking up a defensive position behind his authority as a scientist! Nevertheless Crookes's analysis of this "science lesson" does suggest how a

reflexively conceived professionalism might begin to
point beyond what Crookes himself refers to as the "mundane"
realizations of its practices to its own inherent
possibilities, without having recourse to a normative
ideal for those practices which would render such
possibilities "only theoretical".

What Crookes fails to do, however, is to consider
how such insights could be related back to the institution-
alized practice of education.   In a sense he illustrates
only the theoretical moment in the dialectic articulated
at the end of Chapter Two.   Thus, in a different way,
like the other writers discussed so far, he evokes
but does not address the issue of how the improvement
of professional practice <u>within</u> an institution is related
to action-research's problematic.   We have seen how
this relationship is generally enacted as the adoption
of a consciously "progressive" stance on such questions
as ethical v. technical rationality, instrumentality
v. the self-reflective Subject, and teachers' v. pupils'
versions of relevance.   In other words we have seen
how action-research writers have tended to present
these various issues in terms of an overarching
"liberationist" dichotomy: ideal speech v. bureaucracy,
emancipation v. constraint, democracy v. hierarchy.
Such formulations evoke rhetorically and metaphorically
action-research's <u>challenge</u> to its institutional context,
the general dimension on which professional improvement
is sought, but it fails to formulate action-research's

possibility except <u>as</u> a challenge, ie. as an "idealistic"

aspiration whose practicability is always potentially

undermined by the unaddressed ironic relation between

ideal and actuality, between the individual and the

institution. To do otherwise, _{in order} to formulate action-

research's constructive relation between theory and

practice, the inert ironies of the dichotomies presented

so far must be reformulated in dialectical terms, in

order to provide analytically not only for opposition

but also for resolution, transformation, and thus for

change. It is to this task that the argument now

turns, by considering the nature of professional

practices and the sense in which they themselves offer

opportunities for the improvements which action-research

seeks.

Professionalism and Bureaucracy: Myths of Normative Rationality

Professionalism is presented by Talcott Parsons

as the historically achieved resolution of the principles

of rationality and morality, the application of objective

<u>science</u> to everyday experience (Parsons, 1954). Hence

the institutional power of the professional over the

client is immediately legitimated by the form it takes,

namely expertise, authority derived from scientific

knowledge and structured by Weberian notions of bureau-

cratic form: functional specificity, systematic

disinterest, and the universalistic rules.* The enormous

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* What Parsons's presentation fails to address, of
  course, is the gap between action and explanation,
  between description and rationalization, between
  actuality and ideal type.

<u>mythic</u> appeal of this fusion of science and morality
into an axiomatically authoritative rationalism may
be seen in the genre of professional-as-hero Radio and
TV series, films, novels, etc., which endorses simultaneously
the righteousness and the expertise of such professions
as the doctor, the nurse, the vet., the pathologist,
the solicitor, the journalist, and (most of all) the
policeman, and the detective.   Where there was error
and hence injustice, there shall be truth and justice.
But mythic structures are created by contradictions:
Levi-Strauss says (1981, p. 604):   "For a myth to be
engendered by thought and for it in turn to engender
other myths, it is necessary and sufficient that an
initial opposition should be injected into experience".
And (on p. 603):   "This inherent disparity of the world
sets mythic thought in motion, but it does so because ...
it conditions the existence of every object of thought".
The "initial oppositions" within professional work
may be thought of as those between individualized
authority and universalized truth, and between science
and morality.   Both oppositions are mythically resolved
in the figure of the <u>hero</u>, who reveals the objective
error of <u>other</u> professionals, whose expertise is
axiomatically on the side of the Good, and whose version
of "the case" is thus by definition "the Truth".   The
Good of the hero is both highly idiosyncratic (hence
the emotionalism of Quincy, the rudeness of Kojak,
the vanity of Poirot, the privacy of Sherlock Holmes) <u>and</u>
universal (scientifically expert).

Perhaps it is an echo of the myth of the professional-as-hero which enables Brown et al. to see the emancipation of the professional from bureaucratic authority as a sufficient formulation of "improvement": the forces of error against whom hero-professionals score their triumphs are very often their own professional superiors. And in this element also we have as it were a mythic treatment of the contradictions of a cultural form: the Weberian bureaucracy and the Parsonian profession both express the progressive rationalization of institutionalized action, the rationalized format for authority. Yet this authority is in contradiction with itself: bureaucracy creates a hierarchy of jurisdictions in which practitioners at each level can decide the means but not the ends of action; whereas the status of "professional" gives the practitioner precisely that principled autonomy which bureaucracy withholds, ie. the autonomy which comes from possessing a moral as well as a technical jurisdiction. Since professional practitioners are also members of more or less bureau-cratized institutions, the authority by which they practise is enmeshed in ambiguity; and this ambiguity is mythically opened out into a confrontation between apparently dichotomous principles (autonomy / constraint; professional / bureaucrat; "red-tape" / "what justice demands") which is worked out in the adventurous confrontations of the professional as Rebel.

But a myth reveals its fragility at the same
time as it asserts its possibility - hence the need
for continual repetition, hence, indeed its "appeal":
the hero only just triumphs, by means of the "arduous
journey" (Levi-Strauss, 1981, p. 659) through the series
of "dangerous" confrontations (on the street, in court,
in the lab.) with the forces of error or injustice
by which professional work is always threatened. That
professional work will always sense this threat is
guaranteed by its origin in the ambiguity, the instability,
of professionalism's own auspices: the rationality
by which it claims authoritative jurisdiction is the
same authority by which, in the name of bureaucracy,
such jurisdiction is circumscribed. Hence the powerful
appeal of the figure of the action-researching
professional, who is continuously aware that his or her
authority possibly might not correspond with the practice
of justice and truth, while sensing a general requirement
that it should.

But although this version of professionalism shows
the inherent possibilities for action-research's
"heroic" calls for "improvement", there is a sense in
which action-research merely subscribes to the myth
which it should be examining, namely the struggle for
the emancipation of the authority of the individual
professional against the constraints upon that authority
provided by his or her institutional context. We need
therefore to look more closely at the contradictions
which constitute the form of professional life, to

establish an analytic rather than a mythic formulation of "improvement". We may begin by considering the crucial role of the notion of "rationality" in Parsons's presentation of professionalism, as the point at which the unproblematic authority of the professional is established.

Weber's "Zweckrationalität" and "Wertrationalität" were originally conceived as analytical devices, so that for Weber the formulation of an instrumental rationality was not descriptive but was rather constituted analytically in contrast to "the great bulk of everyday action" which approximates to tradition-orientated "almost automatic reaction to habitual stimuli" which "lie very close to the borderline of what can justifiably be called meaningfully oriented action" (Weber M., 1971, p. 139). However, it is this contrast between rationality as an analytic norm and as an empirical norm which Parsons often seems to lose: "The starting point, both historical and logical (my emphasis - RW) is the conception of the intrinsic rationality of action ... The rationality of action ... is measured by the conformity of choice of means" (Parsons, 1968, p. 698-9). When the historical and the logical are thus elided, we have a metaphysics of instrumentality, in which action's rationality is "intrinsic" and axiomatic because it is merely the rule of subjective purpose. Thus for Parsons a "system of action" is a "set of variable relationships "between an organism" and its "objects" (Parsons et al., 1962, p. 6). But such a conception of

the subjective instrumental rationality of action makes
it difficult to conceive of everyday interaction except
as either authoritative, in which the Other is success-
fully manipulated (as an _object_) or as irrational, in
which the Other (as an obstinate _subject_) undermines the
possibility of reliable control or prediction, and thus
leads to Weber's concession that the ideal of instrumental
rationality relegates everyday action to the borderline
of the meaningless.   Hence the authoritative option,
the subject/object model, reminiscent of Hegel's primal
Master / Slave relation, cannot be challenged by any
thoroughly _intersubjective_ articulation of rational
action.

   At this point in the argument the metaphor of
social action as the selection of an appropriate
instrument for a subjective purpose joins the myth of
the professional-as-hero.   Professional practices are
conceived as fusing the moral authority of Society
with the technical authority of Science: the professional
as Subject thus possesses a _knowledge_ of the client,
as an Object of science, of expertise, to which the
client's own life-world offers no challenge, since it
appears to have no theoretic resources, being indeed
merely an "almost automatic reaction to habitual
stimuli".   Hence the cultural mandate for action-
research's unreflexive call for the improvement of
professional practice in terms of greater diagnostic
or therapeutic discretion _for_ the professional _over_
the client, in terms of emancipation from bureaucratic

constraint or from the residues of "unscientific"
common-sense. The authoritative stance of the professional
towards the client's life-world is thus closely linked
to the social scientist's deficit theory of the common-
sense social actor: we have frequently noted Garfinkel's
critique of this, his assertion of the rational properties
of the life-world, and the need to theorize the nature
of science's Difference, rather than (by positing
only one form of rationality - the instrumental) imposing
science upon the life-world as an unproblematic authority.

In other words, in "professionalism", in "bureau-
cracy" and in "instrumental rationality" we have normative
principles which gloss the conditions for their production
as norms. But to explicate the requirements of practice
in terms of normative ideals is to present concensus
as what can theoretically be envisaged but never
achieved at the level of practice. Normative usages (of
"education" or "understanding", of "bureaucracy" or
"professional practice") attempt to prescribe for action
but cannot provide for the processes whereby their own
prescriptions could possibly be acted upon. To speak
for a normative version of concensus is immediately
to enact its opposite: in any actual situation such
speaking makes a prescription which is inevitably
open to contestation on an unpredictable variety of
dimensions. What the action-research writers reviewed
in the early part of this chapter have done is to embrace
the norm of professionalism and to contest the norm of
bureaucracy without realizing the intimate relation
between the two, constituted by the normative version

of rationality which underlies both. The analysis so
far shows how both action-research's embrace and its
contestation represent mythic responses to a set of
contradictions surrounding all three concepts. The
final argument in this chapter will be a consideration
of how these contradictions may be addressed in terms
which sidestep the invitation to mythic identification
with a "maverick" professional, since this threatens to
lead action-research into an "idealistic" confrontation
with its institutional context, and this in turn under-
mines action-research's aspiration to be a form of
investigation which can <u>unite</u> a theoretical stance
with practitioner activity. Meanwhile it is important
to look (at last) in detail at Habermas's theory of
emancipatory discourse, which authorizes the self-
mythologizing stance adopted by so many writers on
educational action-research when they attempt to make
a "critical" move against their positivist inheritance
(see Carr and Kemmis, 1983, for the most elaborated
version).

Habermas and the Theory of Emancipation

The appeal of Habermas's work for action-research
in an educational context lies perhaps in that it
addresses directly one of action-research's central
concerns — the problematic relationship between emancipation
and authority — while the proposed solution, in terms of
an ideal fusion of Reason, Truth, and communicative
participation, articulates one of the deepest ideals and

hopes of the professional educator. A further

"attractive" feature of Habermas's ideal is that it

appears to relate both to theory and to action; it

combines a communicative possibility (rational discourse)

and a political possibility (interaction freed from

contingent power relations):

> "Only in an emancipated society, whose members'
> autonomy and responsibility had been realized,
> would communication have developed into the
> non-authoritarian and universally practised
> dialogue from which both our model of recipro-
> cally constituted ego-identity and our idea of
> true concensus are always implicitly derived.
> To this extent the truth of statements is based
> on anticipating the realization of the good
> life".   (Habermas, 1978, p. 314).

Only in such an emancipated society would an "ideal

speech situation" allow "an actually attained concensus

the claim of a rational concensus" and constitute

"a critical standard against which every actually

realized concensus can be called into question and

tested" (Habermas, 1976, p. xviii). Thus, by means of

the perfectly free and symmetrical procedures of

Critical Reason, interaction could be <u>both</u> emancipated

(from any constraint other than its own constitutive

features ("Reason")) <u>and</u> authoritative (grounded in

concensus).

At one level this is a restatement of the liberal

concept of the constitutive relationship between freedom,

reason, and truth, which stems from Kant and J.S. Mill:

> "Reason has no dictatorial authority; its
> verdict is always the agreement of free citizens".
>   (Kant: Critique of Pure Reason,
>   1933, p. 593).

and:

"Complete liberty of contradicting and disproving
our opinion, is the very condition which justifies
us in assuming its truth for purposes of action;
and on no other terms can (we) have any rational
assurance of being right".
(J.S. Mill, "On Liberty",
1961, p. 271).

But Habermas's argument also derives a particular

strength from its specification at the level of language

(Habermas, 1970, p. 141-3) and also from a commitment

to an intersubjective conception of consciousness and

of the constitution of knowledge:

"The subject of the process of inquiry forms
itself on the foundation of intersubjectivity ...
Every dialogue develops on (the) basis ... of
the reciprocal recognition of subjects ... (and
thus) investigators are always already situated ...
on the ground of intersubjectivity".
(Habermas, 1978, pp. 137-9).

An ideal for inquiry is thus formulated as an ideal for

dialogue:

"Pure intersubjectivity is determined by a
symmetrical relation between I and You (We and
You), I and He (We and They). An unlimited
interchangeability of dialogue roles demands
that no side be privileged in the performance
of these roles: pure intersubjectivity exists
only when there is complete symmetry in the
distribution of assertion and disputation,
revelation and hiding, prescription and follow-
ing among the partners of communication".
(Habermas, J, 1970, p. 143).

It is this ideal which action-research wishes to

interpret in directly practical terms as the formulation

of a feasible mode of interaction between investigator

and investigated, and between educator and educated.

Now, Habermas is indeed concerned with the practical:

his whole argument in Legitimation Crisis (Habermas, 1976)

is that the increasing dependence of social authority

upon the technical rationality of "science", by removing

the choice of goals from the citizen, presents a
political choice between submitting to the imposition
of values through contingent power relations and developing
universal discursive forms for testing the validity
claims of moral norms (p. 105). The "ideal speech
situation" in other words is an ideal which can guide
action. But it is precisely the relation between an
"ideal" and any practice which it might "guide" which
is so problematic. Weber's concept of the ideal is
explicitly analytic: actual phenomena may be understood
in terms of their variable distance from a "logically
deduced" ideal type of that phenomenon. But the
possibility of such a deduction rests on a restricted,
instrumental view of rationality. Habermas's ideal,
in contrast, is presented as a development of a Kantian
imperative: from the constituent conditions for the
possibility of consciousness and inquiry arise the
political ideals of pure intersubjectivity, emancipated
speech, and hence the critical analysis of social norms.
The political arises directly from the analytic: to
question the possibility of the Habermasian ideal is
self-contradictory, since the question itself
presupposes and expresses the necessity of the ideal
whose necessity it purports to deny. Hence "the
transcendental character of ordinary language"
(Habermas, 1976, p. 110):

> "In taking up a practical discourse, we un-
> avoidably suppose an ideal speech situation that,
> on the strength of its formal properties,
> allows concensus only through generalizable

interests. A linguistic ethics has no need
of principles. It is based only on fundamental
forms of rational speech that we must always
presuppose if we discourse at all".

But does this <u>normative</u> ideal of emancipatory

reason arise from engaging in the reflexive practices

of theorizing or from the unreflexive communicative

practices of the life-world? This is a crucial question

for exponents of action-research, who make the latter

interpretation and thus claim that Habermas's ideal,

though unattainable, provides a criterion and a direct

ambition for the improvement of mundane professional

practices:

> "In the real world, discourse is to some extent
> distorted or biassed by assymetrical power
> relations between participants. But one can
> make progress towards the ideal situation by
> identifying and coping with negative instances
> of distorted discourse".
>   (Elliott, 1982a, p. 19).

If this is the relation between ideal and actuality,

between theory and practice, if theoretical ideals

are posited as states of affairs which one can intelligibly

but always unsuccessfully "progress towards", then social

action is forever condemned to lamentable deficiency:

theory will be conceived as normative, ideal types

will be treated as moral aspirations (cf. Parsons on

professionalism, see above, p. 180), and ironies

in the inevitable Difference between the theoretic

and the actual will not be "mastered" as an analytic

resource for grasping the contradictions of experience

(see Chapter Three, p. 151) but bemoaned as <u>lapses</u>

of experience.

The intrinsic weaknesses of Habermas's argument have already been presented (see Chapter Three, p. 155). The point to be made here, rather, is that the widespread invocation of Habermas by action-research writers rests on a misunderstanding. Habermas does not derive his ideal from everyday communicative practices among practitioners; when he uses the term "discourse", he specifies that he means the specific modes of talk in which the "naive" assumptions of everyday speech are critically topicalized (Habermas, 1974, p. 18). Thus, Habermas is concerned to articulate the emancipatory possibilities of critical analysis at the level of theory, which involves for example the recovery of unconscious determinants of (the) self-formative process" and the making explicit of general rule systems (Habermas, 1974, p. 22-3). Indeed he is explicitly dismissive of "the fashionable demand for a type of action-research" (ibid., p. 11). In other words, Habermas, unlike Elliott (see quotation on previous page) does not forget that symmetrical discourse is an ideal - in a Weberian sense - and thus a theoretical principle rather than an intelligible practical goal.

It is because of this implication in Habermas's work that Heinz Moser, wishing to argue that action-research makes a necessary contribution to "critical theory", is (apparently alone among writers on action-research) strongly critical of Habermas. For Moser, Habermas's notion of emancipated discourse rests on a rationalized notion of consciousness (Moser, 1978, p. 99) and of history (p. 95) and on an over-optimistic view

of the possibilities for unforced concensus and
individual autonomy (p. 100).   Moser urges, rather,
that "in discourse itself, power is still at work,
compelling us without our noticing". (p. 97*)
Hence, the discursive recognition of the "validity" of
norms may either conceal "overpowering and irrational
motives" or simply calculative tactics (p. 99).
Further:

> "If one considers ... how humanity is actually
> enmeshed by the coercive relationships of
> society, and the individual devalued ... it
> seems that Habermasian discourse overestimates
> itself.   It sees itself as a counter-force
> to the concrete power relations of late capitalist
> society, and thereby forgets that, by restricting
> itself to a mere willingness to cooperate, it
> yields up all possibility for building opposition ...
> For this reason, discourse itself needs criteria
> which might prevent those taking part in discourse
> from introducing the ideological arguments of
> false consciousness" (p. 100)**

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* "Im Diskurs (ist) selbst noch Gewalt am Werk,
die uns aufzwingt, ohne dass wir es bemerken".

** Bedenkt man ... die reale Verstrickung des
Menschen in die gesellschaftlichen Zwangszusammenhänge
und die Entwertung des Individuums ... so scheint
sich der Habermassche (sic) Diskurs selbst zu
überschätzen.   Er betrachtet sich als Gegenmacht
gegen die faktischen Herrschaftsverhältnisse in
der spätkapitalistischen Gesellschaft und übersieht
dabei, dass er selbst durch sein Beharren auf
blosser Kooperationsbereitschaft, alle Möglichkeiten
zum Aufbau eines Widerparts aus der Hand gibt ...
Aus diesem Grunde braucht der Diskurs selbst Kriterien,
welche verhindern, dass die Diskursteilnehmer
ideologische Argumente des falschen Bewusstseins
in den Diskurs aufnehmen".

Moser would thus not be surprised to find Elliott
unable to put into practice the Habermasian ideal,
and indeed he is critical of his own earlier attempts
to list practical procedures for inquiry (p. 131).
However, there are powerful ambiguities in Moser's
formulation, of which he hardly seems aware. His
presentation of individuals as "enmeshed" and "coerced"
by "concrete power relations" makes it difficult to
see how "ideology" could be avoided merely by framing
discourse criteria. And, conversely, if ideology
and false consciousness are embodied merely in
"arguments", which might be recognized and excluded
from discourse, then what possible meaning can be attached
to "coercion" and "power"? It seems as if Moser's
account rests upon precisely the purely rational
notion of historical and psychic processes he criticises
in Habermas. The explication of this ambivalence
takes us back once more to the ever-present irony of
determinist theories of the subjection of consciousness
to its politico-cultural context: Moser, like so many
other writers, wishes to present a strong version of
the cultural determination of the mundane social actor
and yet to exempt from this determination the social
theorist (see Chapter Three, p. 132).

The constructive relevance of Habermas's work
for action-research, I would argue, is that it presents
a metatheory of investigation. His arguments concern-
ing language, intersubjectivity, rationality, and the
unconscious present the theoretical possibility of theory

and of the autonomy of the theorizing Subject, which (as
was argued in Chapter Three) is indeed an analytical
requirement for action-research. Unlike the argument
of the present work, however, Habermas is neither
concerned with nor sympathetic to action-research's
project. When action-research writers attempt to
treat Habermas's metatheory as though it were (or
could be) directly programmatic for action-research
as a social practice, they are using Habermas's vocabulary
of emancipation and dialogue as _metaphors_ while claiming
that such a vocabulary can, for action-research, be
literal. Hence they fall into claims (for action-
research's "emancipatory" process, for example) which
seem both idealistic (concerning the possibility of
action-research's institutionalizability) and rationalistic
(in relation to the complexity of the psyche (see
Chapter Three, p. 110). It is this misuse of
Habermasian arguments concerning _ideals_ of speech,
role relationships, and rationality which frequently
leads action-research to oversimplify all three - to
treat speech as literally relatable to facts (rather
than as essentially reflexive and metaphoric), to
treat symmetrical role relationships as a necessary
concomitant of the process of theorizing (cf. Chapter
Three, p.154-5), and to treat "critical" rationality as
instrumental and prescriptive (rather than as dialectical
and playful - see Chapter Three, p.126). In short,
by taking Habermas's theoretic ideal as a practical
goal, action-research creates a mythic scenario for

emancipation rather than an analytic theory of investigation.*   Hence "the improvement of professional practice", as an essential dimension of action-research's format for investigation, is itself presented in mythic terms - as the "removal of constraints" imposed by bureaucraticized roles, for instance - and thus it is to an <u>analytical</u> account of professionalism and its relation to bureaucracy that the argument now turns.

<u>Professional and Bureaucratic Practices: Dialectical Possibilities</u>

How can professionalism be understood as a potentially self-transformative set of dialectically related contradictions, rather than as the inert and unitary ideal evoked by Parsons?   Following on from the contradictions noted earlier (p.   above) it is important to notice that professionalism regularly invokes not one but at least <u>two</u> "opposites".   Firstly, professional work is not "<u>trade</u>": professionals are not supposed to be motivated by profit (but by service);   they may not advertise for customers nor operate competitive pricing. Hence the professional's proclaimed commitment to the good of the client: the professional is the <u>servant</u> of the client's interests;   their interaction is

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*   This is the limitation of the work of Carr and Kemmis (1983):   their reliance on the general Habermasian framework pushes their version of "critical theory" towards an unreflexive political rhetoric whose grounds could easily be contested analytically by anyone who - unlike myself - found its conclusions unwelcome (see pp. 180-4).

confidential: the professional is trusted to protect the client from investigation by police, tax-collector, life-insurance company, or even (controversially) by parent. Secondly - and in marked contrast - the professionals are not "amateurs"; they may accept and will indeed require payment for making an appearance; they have a skill and a living to make; they are committed, in the sense of serious: they are not "half-heartedly" amateurish, and can thus be relied upon to do an expert and effective job under difficult conditions. Hence a professional relationship is not concerned with persons but techniques: amateurs will perform for (or give services to) friends and relations for free: professionals will refuse to do so on principle: their expertise is only available to anyone who will pay.

Thus, even without recourse to the Marxian critique of professionalism as an ideological disguise for the construction and exploitation of a cultural monopoly (see Larson, 1977, pp. 220-244) we have two very different versions of the professional authority (as an ethic or as an expertise) and of the professional relationship (as a commitment or as a technical service). This contrast is not one which needs to be denied (by the heroic stances described earlier in this chapter); nor, evidently, does it prevent the accomplishment of professional work with sufficient coherence for its mundane purposes. The argument is rather that to note the contradictions within the conventional auspices

of a mundane practice (given here as examples, with no
pretension to exhaustiveness) is to note the opportunity
for a questioning of the grounds of practice, ie. for
the instigation of the type of questioning dialectic
proposed at the end of Chapter Two as a general format
for action-research's process.   Such a process (as
was made clear, see p. 73) will not confront professional
practices with their errors, nor will it prescribe
an improvement on the basis of either an ethic or a
technical authority: rather it will install within
professional work a moment which topicalizes the
reflexivity by which alone the complexities of professional
judgements are handled.

Focussing specifically on the contradictions within
which professional judgements are carried out serves
to make explicit that the normative forms in which
judgements are presented as mundane accomplishments
cannot be taken as literally descriptive of the practice
of those judgements;   judgements such as "higher" and
"lower" orders of conceptualization (see p. 174 above)
would be recast as problematic by the elaboration of
the contradictory versions of the authority, and the
relationships in which they are grounded.   Similarly,
given the grounding of communicative competences in
the Self-Other dialectic (see Chapter One, p. 31)
the elaboration of the reflexivity of professional
judgements would render problematic a series of normative
definitions and their attendant systems of authoritative
decision-making, since the client's rationality would
be recognized as a constitutive element in the

formulation of adequate practice.   In this sense
"the improvement of practice" would be bound up with
an explicit grasp of the reflexive grounds for practice.
This would not be the proposal of a move from "constraint"
to "emancipation" (as Elliott would have it - see p. 170),
but rather the recollection that practices are in
principle grounded (as the condition of their intelligibi-
lity) in the _intersubjective_ dialectic between self
and other, between professional and client.   This
recollection would be a moment in the dialectic between
theory and practice, action and research (see Chapter
Two) and equally a moment in the dialectic between
ideology and theory (see Chapter Three):   in both
cases the reflexivity of each moment provides for a
dialectical _self_-transcendence, and thus prevents
"critique" becoming merely the assertion of an ideal
against practice.

Furthermore, we may recollect (from Chapter Two)
that practice itself is intrinsically guided by a
complex set of criteria for rationality and by a further
complex set of interpretive procedures for the enactment
of those criteria.   If this is true analytically of
action in general, then we will expect that professional
practice (as a set of actions whose discursive elaboration
is relatively accessible and widespread among practitioners)
will certainly have available its own resources for
"improving upon" the literal invocation of its general
rules ("higher" and "lower" concepts):   such resources
are mundanely presented by professionals as the
"discretionary" quality of their practice, whereby

professionalism denies that a normative rule can exhaust
the rational properties of professional work, but
rather welcomes the recognition of the complexities
which are glossed by such rules. More concretely,
professionals deny that a single prescriptive rule
can exhause the technical properties of the individual
case, which thus always remains in need of specific
diagnosis by the professional worker, within the
complexities of a) the dialectical contradictions
between different rules, and b) the reflexive process
by which any rule or combination of rules is applied.
(At this point we may note once more the significance
for arguments about the improvability of practice of
the analyses in Chapter Three concerning the Self, the
possibility of theorizing, and the intersubjectivity
of the therapeutic relationship).

However, in emphasizing at this point the
discretionary quality of professional work, as action's
own auspices for analysis, we are perhaps in danger
once more of formulating a "heroic" opposition between
the action-researching professional and his or her
"bureaucratic" role _definition_. It is thus important
to emphasize now that bureaucratic roles, like professional
practices, may be formulated in terms of a set of
dialectical contradictions rather than as a unitary
ideal type.

Clearly, for Weber, bureaucracy represents the
evolved institutional form for the ordering of social
decision-making according to the canons of reason,
justice, and authenticated knowledge. But Weber also

presents bureaucracy <u>historically</u> as the enforcement

of centralized control:

> "The triumph of princely power and the
> expropriation of particular prerogatives (ie. of
> local feudal "estates") has everywhere signified
> at least the possibility, and often the actual
> introduction, of a rational administration".
> (Weber, 1964a, p. 133)

Hence Weber emphasizes "a firmly ordered system of super-

and subordination in which there is a supervision of

the lower offices by the higher ones" (Weber, 1964b,

p. 465), and this is made possible because "the management

of the office follows general rules, which are more or

less stable, more or less exhaustive" (Weber, 1964b,

p. 467). This does indeed emphasize the oppressive

nature of institutional order, and in the end Weber

seems to forget his own principle of the analytic

status of ideal types, and finds himself in "despair"

at the vision of combined "timidity" and "mechanization"

in social affairs (Weber, 1964c, p. 473) which his own

theory conjures up, not merely as a heuristic device

but, apparently, as a description.

But Garfinkel would have comforted Weber by

reminding him that even if general rules are "stable"

in themselves, they can never be "exhaustive" of the

cases to which they purport to refer, and thus in

principle bureaucrats cannot be "timid" because their

work is <u>not</u> "mechanized": rather, they always require

a specific <u>confidence</u> in their capacity for <u>improvising</u>

the application of rules to cases. It is this sort

of awareness which leads, for example, Selznick to

argue that "Every organization creates an informal

structure" in which "professed goals" are substantially "modified" by the "operational goals" of groups of workers within the organization (Selznick, 1964, pp. 477-9).

However, this line of argument only serves to ameliorate the sense of bureaucracy as "constraint". In order to find an argument which establishes a clear contrast of principle, so that we may formulate bureaucracy itself in strictly dialectical terms, we can turn to Durkheim. Durkheim interprets the same historical processes of rationalization and industrialization which for Weber are the origin of "bureaucracy", as leading to the division of labour and thus to the development of "organic" social solidarity. For Durkheim this is the opposite of a historical move towards the subjugation of the individual to a centralized rule system: on the contrary, it represents the relative decline of the collective consciousness which a centralized rule system implies: under organic solidarity:

> "It is necessary ... that the 'conscience collective' leaves open a part of the individual consciousness in order that special functions may be established there, functions which it cannot regulate"
> (Durkheim, 1972, p. 140 - my emphasis).

Hence:

> "The 'conscience collective' ... comes to consist of very general and indeterminate ways of thought and sentiment, which leaves room open for a growing variety of individual differences".
> (Durkheim, 1972, p. 145)

In a sense Durkheim's theory of organic
solidarity itself presents a dialectic between individual-
ization and social coordination, in which interaction
becomes necessarily more intense as its basis becomes
more problematic.   This in turn provides an interpretation
of bureaucratic organizations as institutions where
opportunities for discretionary judgement are increased
by the specialization of functions, and thus where the
integration of such functions becomes necessarily more
and more a focus of concern as it becomes more questionable.
Hence, bureaucracy's principle of hierarchical juris-
dictions is in a dialectical contradiction with its
other principle of expertly qualified officials,
especially if expertise (as "knowledge") is no longer
taken to be a law-like corpus of warranted propositions
but rather as a capacity for and experience of essentially
reflexive interpretation.   If rationality is, as
Garfinkel argues, an inherently pluralistic set of
possible interpretations, then the very notion of
"legal-rational" authority immediately expounds a
contradiction, since social rationality denies the
possibility of general laws and thereby renders authority
subject to a continuous process of individual inter-
pretation.

In conclusion then, as with professionalism,
bureaucracy is not a monolithic format for authority-as-
oppression, with which action-research's project of
transformational development must needs do battle, but
rather a context with its own developmental dialectic,

which thus offers to action-research its own inherent
opportunity for the devolution of decision-making.

Now, all this is not new: Bernstein's well-known
work on education is explicitly presented as an inter-
pretation of largely bureaucratized institutions
according to Durkheim's problematic of organic solidarity;
and Bernstein ends the first of his papers on this theme
by emphasizing that he is not contrasting "order"
with "flux" nor lamenting "the weakening of authority"
but rather exploring "changes in the forms of social
integration" (Bernstein, 1971a, p. 169). It is in
this spirit that Bernstein presents a change in the
institutional order of the school from "closed" to
"open" (Bernstein, 1971a, p. 169), from subject-based
to across-subject teaching roles (ibid., p. 167),
from vertical to horizontal relationships between
teachers (Bernstein, 1971b, p. 62) and towards "increased
discretion of the pupils" (ibid., p. 60).

For Bernstein, following Durkheim, institutional
order itself has become a problematic pattern of inter-
action, not a hierarchy of prescriptions. How ironic,
then, that Peter Holly, in his diagrammatic representa-
tion quoted earlier (see p. 167), uses Bernstein's
vocabulary to articulate not a Durkheimian but a crudely
Weberian model of a prescriptive version of institutional
life, which action-research must "painfully" and
"cautiously" oppose. The particular irony is that
Holly's vocabulary for the principles of action-research
reproduces Bernstein's vocabulary for the basis of the
institutional order, thereby undermining the very

distinction which Holly wishes to put forward and thus implicitly and accidentally putting forward the counter-suggestion which is the theme of this section: that the institutional order is in itself available to action-research's project.

To avail itself of this opportunity, what action. research needs is not the oppositional "caution" recommended by Holly, but a grasp of the complex but ultimately enabling relationship between on the one hand the potentially reflexive interactive processes of institutional life and professional practice, and, on the other hand, the reflexive processes of action-research's own dialectic between theory and practice. In this way action-research "improves" institutionalized practices by exploring to their uttermost limits the discretionary possibilities within which they are (institutionally as well as epistemologically) constituted. In this way, also, action-researchers may differentiate between those dimensions of their professional and institutional lives which are amenable to concrete projects for "improvement" and others which - determined by political and economic forces beyond any influence from within their immediate institutional setting - must indeed be treated as "constraints" and thus as beyond the scope of the particular project. This argument is in its own way "cautiously" balanced between an emphasis on possibilities and an equal emphasis on limits: it would be beyond the scope of this work to attempt to envisage or to estimate the

likelihood of a world in which professional practitioners

in <u>all</u> institutions were <u>simultaneously</u> pressing to their

limits the possibilities inherent in their roles!

CHAPTER FIVE

ACTION-RESEARCH AND THE PROBLEM OF VALIDITY

Versions of "Validity"

The previous chapter began to engage with an issue which must be analytically necessary and indeed central for any project of formulating a mode of investigation, namely its criteria for validity. So far the argument has centred on only one aspect of action-research's problem of criteria - its aspiration to "improve" practice. In this chapter the argument will be broadened: it is concerned with action-research's general problem of how it might conceptualize "validity" in accordance with its own processes and inherent problematic, ie. independently of such echoes of positivism as: accounts which purport to correspond "accurately" to an external object world, and interpretations which aspire to be "generalizable" propositions.

Generalizability is of course the direct claim with which positivism challenges its rivals: its hypotheses are derived from "laws"; its experimental method produces statements of "significance" concerning "representative" populations, so that in turn its results can be claimed as potentially "law-like" or, at least, essentially "replicable". Action-research, by eschewing the axiomatic generality of empirical laws and statistical formats, opens itself to the charge that its validity

is limited to the concrete instances in which it is constituted. This is another way of accusing action-research of failing to be more than a mundane <u>action</u> strategy, rather than an alternative, non-positivist <u>research</u> strategy.

It is for this reason, perhaps, in order to authorize its validity claims, that action-research has claimed to draw upon the "established" methodological tradition of symbolic interactionism (see Elliott, 1982b, p. 31). Hence also the importance of the notion of "case study" as a format for action-research inquiry (Elliott, 1978b, p. 356), which also enables action-research to claim kinship with institutionalized social science, eg. "anthropology" (see Walker, 1980, p. 33). The purpose of this chapter then is to analyze the forms of general validity which may be conceived, or which action-research as claimed, for the interpretation of the specific action contexts with which action-research is concerned.

Elliott's article in the <u>Journal of Curriculum Studies</u> (Elliott, 1978b) presents action-research's claim, in a passage which raises many of the central questions, and it will thus serve as a starting point for the analysis of (in turn) "naturalistic theory", "concrete description", and "narrative form", as versions of "validity" for action-research accounts:

> "In explaining "what is going on", action-research
> tells a "story" about the event by relating it
> to a context of mutually interdependent
> contingencies, ie. events which "hang together"
> because they depend on each other for their
> occurrence. This "story" is sometimes called
> a case study. The mode of explanation in case

> study is naturalistic rather than formalistic.
> Relationships are "illuminated" by concrete
> description rather than by formal statements
> of causal laws and statistical correlations.
> Case study provides a theory of the situation
> but it is a naturalistic theory embodied in
> narrative form, rather than a formal theory
> stated in propositional form"
>   (op. cit., p. 356).

"Naturalistic Theory"

By the brevity of his presentation, Elliott seems

to suggest that this could be a taken-for-granted

category or a methodological device, rather than a

contentious assertion which proposes to annihilate

a central philosophical issue.   At the very least it

represents a grand epistemological irony and / or a

methodological dilemma: how could theory be natural?

How could nature be theoretical?   How could either claim

be grounded?   Nevertheless, the writers in the symbolic

interactionist tradition which Elliott seems here to be

invoking also treat the elision as achievable.   For

example Schatzman and Strauss, in Field Research:

strategies for a natural sociology (1973) - often used as

a methodological text by action-research practitioners -

claim that it is a basic property of "the human scene"

that social action is always an outcome of actors'

theories or "perspectives" (op. cit., p. 5) and that the

researcher is a naturalist" by direct analogy with the

researcher in "zoology, archeology, and geology"

(ibid., p. 14) in that he works by observing "the

natural properties of his field" (ibid., p. vii), namely

actors' perspectives.   This seems at first to be the

fairly simple point that it is the task of the researcher

to discover actor's rationalities, so that it is those
actors in their "natural" world who are the arbiters
of what is to count as an adequate understanding.
This would be a straightforward relativist argument, and
it would coincide with Elliott's suggestion, at another
point in the article quoted above, that action-research
"interprets 'what is going on' from the point of view
of those acting in the problem situation" and indeed
"in the same language as they used" (Elliott, op. cit.,
p. 356).

However, the apparently non-ironic invocation of
zoological parallels accomplished by the category
"naturalistic theory" is indicative of a larger and
even more problematic claim.   In their account of
"naturalistic" inquiry Schatzman and Strauss admit that
researchers will begin their work with concepts
("classes") presumed in advance on the basis of an
academic discipline, but that the process of "observation"
will make available the "classes" used by the members
of the situation under observation.   These two sets
of "classes" will be synthesized in the course of "the
experience of observation" (op. cit., p. 112) and it is
specifically this synthesized set of categories which
is termed "theoretical".   They conclude: "thus we
can anticipate the researcher will continue shifting
his grounds as he creates or changes his classes, until
all his presumed classes are displaced by those based
upon observation" (ibid., p. 113).   To suggest, in this
way, that concepts can be derivable from observation is

to present a metaphysics of naturalism, in which theory
is encapsulated by nature. The rhetoric of the naturalist
is used to suggest the possibility of an account which
has "nature's" authority, thereby implying metaphorically
what they analytically deny: that the human world is
a world of objects available to inspection. The
symbolic interactionist perspective (to which Schatzman
and Strauss ostensibly subscribe) is, on the contrary,
that the world of social actors is a world of subjects
and their interacting "perspectives": the further
interaction between actors' perspectives and researchers'
perspectives can thus in no way be reduced to "the
observation of nature", but is rather a central analytical
problem in formulating the category of "theory" itself,
and (as Becker himself says, in "Whose Side Are We On"? —
Becker, 1971) a dilemma in the social relations of
validity claims.

That symbolic interactionism and action-research
should thus use the positivist metaphor of nature's
passive open-ness to observation, when both wish also
to emphasize the independent interpretive competence
of the social actor, is highly suggestive. It relates
to a failure to articulate fully the relationship
between science and common-sense and, in particular, a
failure to come to terms with positivism's powerful
challenge in this respect, which is of crucial significance
for the issue of generalizable validity.

Zetterburg's argument (Zetterburg, 1962)
concerning the relationship between social theory and
social actors' relevancies offers an instructive

contrast.   For Zetterburg social theory is a set of
general laws, eg. "A person tends to modify his
communications ... so that they approximate those found
among his associates" (op. cit., p. 81), and specific
action contexts can be understood at the level of
theory by being classified under a "systematic"
combination of these laws (ibid., p. 132).   "Common-
sense" on the other hand is "unable to make the right
combination of ideas" (p. 132).   Thus for Zetterburg,
"case study" is merely "descriptive" and "intuitive":   it
lacks "analysis of the principles at work" (pp. 27-8).
But Zetterburg's analytical problem is that he treats
the relationship between law and instance as deductive -
the practitioner can deduce an understanding of the specific
from the lesson of the law (see Zetterburg, op. cit.,
p. 166 ff.:   "The Calculation of Solutions").   But
this is to ignore the process of inductive generalization
by which the laws were originally formulated.   This
process is acutely complex even for natural science:
for social science it is the problematic for the whole
enterprise since it raises the central theoretical and
methical issue of the relationship between observers'
categories and those of the social actors being observed.
In thus treating "induction" as an available procedure,
whose resources can be glossed as established, Zetterburg
ignores more or less every sense of social science's
specific challenge, and in particular - of course -
the issue of its inevitable reflexivity.

In formulating "grounded theory" or "naturalistic inquiry" (Denzin, 1978, p. 6) symbolic interactionism has tried to remedy Zetterburg's "problem", but has failed to do so because it has retained a positivist notion of theory construction as organized according to a classificatory logic.   We have already seen that for Schatzman and Strauss "analysis" is a process of "class-ification" of instances under concepts, as though "analysis" could be a process of reducing language's metaphoricity to literalness.   But such literalness could only be a prescription or a pragmatic interpretation (see Garfinkel, 1967, p. 192), and thus not an achievement of analysis, but itself the occasion for the analysis of that reductive process.   Denzin makes the issue even clearer.   He is "committed ... to theory that is grounded in the behaviours, languages, definitions, attitudes, and feelings of those studied" (Denzin, 1978, p. 6) and yet also to "processes of sampling, generalization, (ibid., p. 19) and measurement" (p. 24), and to providing "causal explanations (p. 16) which are "repeatable and reliable" (p. 22).   But if "languages, definitions, attitudes, and feelings" could be sampled and measured, they would have to be formulated as observable behaviours, and this would dramatize Denzin's lack of a reflexive awareness; for we would then need analytic grounds for the crucial differentiation between those "languages and definitions" which are to be measured and the "languages and definitions" by means of which the measurement would be accomplished.   Otherwise theory and the object of theory ("commonsense", say)

would remain undifferentiated.   Zetterburg himself

notes that each of the "theoretical" generalities he

adduces is itself "well known to common sense"

(Zetterburg, H., 1962, p. 132).   Thus the notions

of "system" and "law", in terms of which he presents

the analytic Difference at issue, are essentially unaddressed

metaphors for theory's claim to authority.   As metaphors

they evoke theory's Difference as a set of interesting

problems;   namely the relation between "law" as a social

prescription and "law" as a general truth.   As metaphors,

"law" and "system" evoke social science's aspiration, its

sense of its own Difference (from commonsense) as its

ideal of "validity";   however, as Zetterburg presents

them, they are proposed as rules-of-thumb, which could

operate the Difference to which they refer as though

it were a mere methodological device.

For symbolic interactionism and action-research

to address the irony inherent in "naturalistic theory",

the question of general validity would have to be

approached in terms quite other than as a process of

classification by progressive abstraction.   Such a

process denies in principle the need to address the

grounds for its own selectivity, since it presents

itself as having the warrant of an algorithm, and

denies the creative doubtfulness of the web of metaphors

which alone make classification possible.

Illumination by Concrete Description

Ironically, the notion of explanation as
"illumination" is taken by Elliott from a paper (Parlett
and Hamilton, 1977) in which "evaluation as illumination"
is presented as diametrically opposed to what the writers
call "the agricultural-botanical paradigm", ie. as a
rejection of the analogy between the human and the
biological sciences which informs the "naturalism"
of Schatzman and Strauss. The basis of the distinction
for Parlett and Hamilton is that, whereas innovatory
programmes in agriculture can utilize an "experimental
testing" format for evaluation, educational programmes
cannot do so (see Chapter Two above). Instead: "the
task is to provide a comprehensive understanding of the
complex reality (or realities) surrounding the programme,
in short to 'illuminate'" (Parlett and Hamilton, 1977,
p. 21). Illumination thus involves an account of the
"milieu" (op. cit., p. 11) surrounding the specific
programme, and how therefore the latter is affected
by "a network or nexus of cultural, social, institutional
and psychological variables" (ibid., p. 11). Hence
Elliott's emphasis (see above, pp.208-9) on relating
"the event" to "its context", and on the description of
these "relationships". Thus, whereas the single
variable focus of the experimental method leaves the
event "obscure" (to follow up the metaphor), light is
shed by tracing the "complexity" of which it is a part
(Parlett and Hamilton, op. cit., p. 11). Such complexity
cannot be tested or measured directly, so "the primary

concern (of illuminative evaluation) is with description
and interpretation" (ibid., p. 10). The question then
becomes: how can "description and interpretation" be
methods for the creation of valid accounts of this
complexity? This is a crucial theme for writers on
action-research in general and writers on case study in
particular.

For Midwinter "interpretative description" is
"the attempted medium for relating the results of the
(action-research) project" (Midwinter, 1972, p. 52), as
a way of meeting "the need to balance action and research"
(ibid., p. 54). For Midwinter the justification for
"interpretative description" is that it is compatible
with the rapidly changing, flexible, and interactive
procedures of action-research (ibid., p. 53): he
admits that this "is not often academic method research"
(sic) (p. 52) but his whole argument for action-research
is that inquiry is too urgent to be left to the slow
pace of "theory-based" research (p. 51). He goes on
to quote E.H. Carr on "the continuous process of
interaction and the unending dialogue between facts
and their interpreters" (p. 53). In other words,
"interpretation" can be "valid" precisely because it
allows the structure of experience to proceed uninterrupted.
But this would return us to our original problem (see
p. 208, above): what forms of reflection does action-
research add to the pragmatic reflection which is the
basis of mundane action? This is particularly important
for Midwinter, since his projects and the case studies
which report them are all predicated upon a specific

"theoretical" ideal of "community education", and thus
require a principled basis for evaluative judgement if
they are to constitute a form of inquiry at all, rather
than a managerial process of "implementation".

Midwinter's "anti-academic" emphasis is at
variance with Parlett and Hamilton, for whom the method
of "description and interpretation" places educational
evaluation "unambiguously within the ... anthropological
paradigm" (Parlett and Hamilton, op. cit., p. 10)
which also includes "participant observation research
in sociology" (ibid., p. 7). Now these traditions
of inquiry do have a basis for claims to general
validity, and this basis is (again in contrast to
Midwinter's emphasis on speed and non-intrusiveness)
the comprehensive, painstaking variety of the investi-
gative process. Thus Denzin emphasizes "triangulation
of methods" (Denzin, 1978, p. 21), Glazer and Strauss
(1967) emphasize the need for a continuously "comparative"
analysis and Becker stresses the importance of checking
interpretations against possible negative instances
(Becker, 1971, pp. 31-2). It is this emphasis which
is found in the work of Rob Walker, who is concerned
in general to relate the case study tradition in social
science to educational research with a direct commitment
to change professional practice. For example, he says
that anthropology succeeds in preserving complexity
of meaning through a research process which is highly
time consuming (Walker, 1977, p. 18), and, in another
paper: "Long term study is justified in terms of the
need to determine areas of significance and to check the

reliability and consistency of data" (Walker, 1980,
p. 30).

At this point we can see, however, that the notion
of validity being presented here presupposes a correspond-
ential conception of knowledge. If "validity"
resides in the "complexity" of the factors influencing
a situation, ie. if the aim of inquiry is to "describe"
this complexity, then the longer the time spent in doing
so, and the more varied the sources of information, the
greater the chance that the resulting "interpretation"
will correspond to the complexity it describes. But
this returns the problem of adequate understanding to
the infinite number of variables, which Parlett and
Hamilton recognized as undermining the feasibility of
the positivist paradigm they rejected, but which also under-
mines their own project of "description". Elliott
himself (see the quotation on pp.208-9, above) refers to
"a context ... of events which 'hang together' because
they depend on each other for their occurrence";
but how would such dependence be knowable except by
invoking those same "causal laws" which he rejects?
"Illumination by concrete ... description" evokes
the ancient metaphor of knowledge as light, but to
propose that by means of "description" the object of
knowledge is "illuminated" does not formulate the process
of knowledge; rather, it presupposes its accomplishment:
to call the process "illumination" presupposes that what
is being shed is, indeed, light.

It is particularly important that action-research
should be able to dissociate itself from a positivist
notion of correspondential description, since as
Walker himself goes on to argue (following Midwinter at
this point), the time constraints of an inquiry which
is intended to be of direct value to practitioners
mean that description which is adequate in positivist
terms can never be achieved before the situation itself
changes (Walker, 1980, pp. 31-2).   Underlying Walker's
argument is the general principle of <u>dialectical</u> under-
standing (see chapter one, p. 28) which would make
any project for the <u>exhaustive</u> description of phenomena
self-contradictory: its implicit ambition of achieving
finality is incompatible with the temporal, developmental
quality of its object.   Further: a recognition of
the reflexivity of language allows us to argue that
description cannot, in principle, merely "correspond"
with the phenomenon described.

How, then, have exponents of educational action-
research and educational case-study attempted to formulate
"description" in terms other than Elliott's implicitly
positivist version?   Robert Stake presents "description"
as a necessarily <u>intersubjective</u> process, with its own
inherent principle of generalizability: "Our methods
of studying human affairs need to capitalize upon the
natural powers of people to experience and to under-
stand" (Stake, 1980, p. 66).   Understanding and experience
involve "naturalistic generalization", which is a
process whereby "intuitive" expectations based on
"tacit knowledge" enable "a full and thorough knowledge
of the particular, recognizing it in new and foreign

contexts" (ibid., p. 69). Hence, if "the target case
is properly described (p. 70) ... readers recognize
essential similarities to cases of interest to them,
(and thus) they establish the basis for naturalistic
generalization" (p. 71). "Nature" here is no longer
the nature which the "naturalist" observes, but in which
he participates, as a member of a shared, culturally
and linguistically constituted reality. In this
sense Stake's argument has links with Hegel's analysis
of the generalizing property of language (quoted in
chapter one, see p. 27). However, whereas this
intersubjective and generalizing property of the symbol
is for Stake a methodical resoucrce, for Hegel it
presents an irreducibly problematic quality: concrete
objects cannot be referred to except through the
universalism of language; the ontology of the concrete
is thus a "whirling circle", and "it just is not possible
for us ever to ... express in words a sensuous being
that we MEAN" (Hegel, 2977, p. 79, p. 60). Indeed,
if the issue of generalization were as straightforward
as Stake suggests, then his argument would apply to any
descriptive communication, and we would still lack
grounds for inquiry's claim to be other than mundane
interaction. For Stake, the complexity of the
symbol is an affirmative answer to the question:
can concrete meanings be generalizable? For Hegel,
in contrast, this complexity poses the question:
how can generalization be related to the concrete?

Addressing this issue, Eisner presents the notion
of "thick description", which "aims at describing the

meaning or significance of behaviour as it occurs in
a cultural network saturated with meaning", and which
"also aims at using language in a way so vivid that it
enables the reader to participate vicariously in the
quality of life that characterizes the events being
described.   It is in this sense that educational
criticism is an art form" (Eisner, 1977, p. 97).
A similar argument (linking description with aesthetic
form and with an effect of "vicarious" experience) is
made by Whitehead and Foster (1984, p. 44).   The various
ways in which aesthetic qualities have been invoked
as part of a declaration against positivism will be
the topic of the next section;   meanwhile it is notable
that for Eisner, as for Stake, "description" is not
the transmission of exhaustive information, but involves
the dialectical participation of writer and reader
in a shared symbolic culture, and is thus constituted
in the transcendental properties of language.   However,
it is clear that these properties are much too super-
ficially presented by Eisner as "vividness", and that
"vicariousness" (as a claim for the effect of such
vividness) is either exaggerated or merely cryptic.
Both "vividness" and "vicariousness" are glosses for
the intersubjective dialectics of language's effectiveness:
how such effectiveness may be either sought or invoked
as a criterion for validity remains to be analyzed.

Theory Embodied as Narrative

In the passage originally quoted on pp.208-9 above, Elliott suggests that it is by constructing a "story" that the case studies of action research programmes find coherence, whereby contingencies "hang together" and "events" are related to their "context". In this way "theory is embodied in narrative form". Similarly Kemmis suggests "case studies work by example rather than by abstract argument ... just as Tolstoy's theory of history is embedded in the story of War and Peace" (Kemmis S., 1980, pp. 136-7). How might a theory be embedded in a story? McDonald and Walker declare: "Case study is the way of the artist, who achieves greatness when, through the portrayal of a single instance locked in time and circumstance, he communicates enduring truths about the human condition. For both scientist and artist, content and intent emerge in form". (McDonald and Walker, 1975, p. 3). The notion of an enduring truth within the specific instance is focussed in the idea of the "typical", and they cite Zola, the "naturalist", who achieved "scientific generalization" by "carefully researching the factual settings ... (and) ... creating characters to represent the social type" (ibid., p. 3). This would make of Zola a "documentary"novelist, and McDonald and Walker do indeed also cite the "documentary" as a possible format for the presentation of case studies (p. 9). But both the documentary and the naturalistic novel raise the question: how are certain events and charaters

deemed to be "typical"?   And this question is crucial

if we wish to consider how Zola's "factual" research

created social types   rather than concrete reportage,

and, in general, by what process either fiction or

documentary can structure particular experiences into

forms which might aspire to a validity beyond those

particulars.

Lukács begins to answer this question through a

distinction between "Naturalism" (as "mere" reportage)

and "Realism", as the selection of detail through

criteria of significance relating to an overall

perspective (Lukács, 2964, p. 51, p. 56).   This "perspective"

is embodied in a "typology" of significant, typical actors

ie. "characters", who thus act out the central meaning of the

narrative as its "plot" .   In other words: "Characters

are not in a novel;   they constitute it, just as a

typology   -   a range of hypothetical possibilities -

constitutes one form of sociological theory.   In both

cases we are presented with a series of hypotheses

set up in order to investigate the nature of the world"

(Winter R., 1975, p. 34).   For this argument the

theoretical problem then becomes the origin and the

grounds for the "perspective" which operates as the

criterion of relevance.   Lukács relates it to a

positively known "history", and he is in general

opposed to the reflexive turn of "modernist" fiction

which addresses the grounds of the writer's perspective

as a central issue.   On the other hand McDonald and

Walker point to the issue without engaging it:

"Clearly, representativeness is an important consideration ... instance and abstraction go hand in hand in an iterative process of cumulative growth" (McDonald and Walker, 1978, p. 4). "Hand in hand", "accumulation", and "growth" are metaphors for the desirability of a theoretical relationship between instance and abstraction but do not specify what this relationship might be.

The argument so far has presented a parallelism between positivist social science and realist fiction, a parallel which enables Becker to propose the valuable contribution of "Life Histories" to the "mosaic" of available "data" (Becker, 1971, pp. 70, 72) and to suggest that the sociologist's hypotheses can be inspired by reading novels as well as by reading sociological theory (ibid., pp. 21-3). But this parallel, although it rescues description from mere data collection, simply interposes a third term, "typology" or "perspective", to bridge the gap between "narrative" and "theory": the theoretic processes which might be involved remain unaddressed. In particular, it does not address the grounds of the analytical work carried out by the producer of a documentary in selecting interviewees, settings, and background "information", nor that of the fiction writer in devising a set of characters and their interaction in a narrative. Rather the notion of "typicality" is used as an unexplicated resource for generalization, a resource which can be treated as available for two reasons: 1) by reliance

upon what Stake calls "naturalistic generalization",
which the symbolic process itself seems to facilitate
as soon as the symbol is treated as non-problematic,
divorced from the reflexive issue of its invocation;
ii) by reliance on the rationalist model of action
invoked by both Weber and Schutz to create "ideal types"
for actors' perspectives.   In other words, the documentary
and realist fiction are examples of how generalization
from the concrete can be treated as achievable through
cultural convention - the "vivid" example, the "typical"
illustration.   It is precisely the grounds for these
conventions - the grounds for the possibility of
generalization - which are not addressed.

It is an indication of the significance of these
issues for action-research that Walker has attempted
to elaborate a methodological link between fiction
and research, in an article called "On the Uses of
Fiction in Educational Research", (Walker, 1981).
Walker suggests, following Terry Denny ("Story Telling
as a First Step in Educational Research"), that the
format of a story can "communicate the general spirit
of things" which is true to what people "mean" rather
than what they merely (according to a tape-recorder)
"say" (Walker, 1981, p. 155).   But how is this achieved,
asks Walker (op. cit., p. 157).   He suggests: "A story
sets limits, it controls what the writer lets the reader
see.   In this sense a story is analogous to a theory"
(ibid., p. 157).   But this is, of course, to use a

prescriptive version of theory, which is alien to
action-research and to "grounded theory", both of which
desire to generate theory from the actions of participants.
Walker's failure to consider any but a positivist
theory of knowledge or any but a realist form of fiction
finally leads him to say: "The attraction of fictional
forms ... is that they offer a license to go beyond
what, as an evaluator / researcher, you can be fairly
sure of knowing" (ibid., p. 163), and to propose that
fictional forms can be "adopted" by a case study researcher
as "a means of disguise" (ibid., p. 159), so that he
can report his data-gathering while preserving its
confidentiality. In this way, since he has no
principled basis for addressing the theoretic quality
of fiction, fiction's particular form of truth, Walker
cannot follow up his earlier statement that "a story
is analogous to a theory" except in the superficial sense
that a theory, like a story, is an observer's point
of view. Hence fiction is finally aligned in opposition
to "real" knowledge, as a form of "licensed" subjectivity.
Fiction is not itself a knowledge-constitutive form;
hence it can be "used" strategically in relation to
knowledge, which is constituted as "objective", presumably,
on other grounds. What is thus in urgent need of
consideration is the sense in which fiction constitutes
knowledge through its own forms, ie. fiction as a
structuring of reality, fiction as a reflexive structuring
of the relation between subject, object, and symbol.

This involves questioning precisely the conventions concerning art, science, reality, and knowledge, on which scientific positivism and aesthetic realism both rely.

Action-Research and the Validity of the Concrete

So far I have considered three aspects of action-research's quest for a principle of "validity" which might guide its accounts of social situations. The argument has been, that the notion of "naturalistic" theory needs to be recast in terms of the reflexivity of theory and the metaphoricity of language, that the notion of "concrete description" raises the issue of the relation between the general and the concrete in terms which necessitate a dialectical theory of inter-subjectivity, culture, and symbolization, and that the notion of "narrative-as-theory" cannot simply utilize the assumptions of realism, but requires also an awareness of the reflexivity of aesthetic structuring. In the final section of this chapter (p.   below) these arguments will be developed in a more positive and detailed form. But in order to prepare for that argument the next two sections will consider in general terms the relation between action-research's requirement of non-positivist formulations of validity and the principles of 1) reflexivity and 2) dialectics.

Action-Research, Validity, Reflexivity

In Chapter One the reflexive quality of symbolization was emphasized, and it was argued that it is by addressing (rather than glossing) this feature that acts of communication (ranging from the spoken comment and the anecdote to the novel and the social research project) can attain a form of "completeness" and thus of "adequacy".*   Reflexivity was taken to be the underlying structure of the relation between consciousness and its objects (including of course, and in particular, "other" consciousnesses).   Reflexivity (it was argued) is conventionally glossed, leaving communication open to the cultural contingencies of "bias", ie. the political and psychological pressures which socially distribute the plausibility and authority of interpretations.   Such pressures cannot be abolished, although it is precisely the claim of positivism to do so by means of methodology, and thereby to transform interpretation into scientific knowledge.   Rather it is by <u>analyzing</u> the irreducibly reflexive dimension of communicative acts (including such analyses themselves) that their grounds are revealed.   "Validity" is thereby approached by taking as a topic the form and nature of communication itself, ie. the "conditions of its possibility", (see Chapter One, p. 9).   "Bias" is thus neither glossed nor abolished but rather confronted, through an analytically

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* I am indebted to Paul Filmer for this point.

"complete" examination of the theoretical basis of the communicative act in the general (reflexive) structure of the relation between subject, symbol, and object. Validity, in other words, becomes a quality of the interpretive process whose grounds are adequately theorized, rather than a quality of a particular interpretation which itself can claim to be everyone's interpretation.

How does this relate to action-research? Action-research certainly recognizes the importance of its own process. Does that mean that it envisages the need for reflexive awareness?

Lippett says: "Probably the best resource every group has for studying the problems and techniques of human relations is the life of the group itself" (Lippett, 1948, p. 110). However, this seems merely to point to the group as a conveniently available "example": the "life" of the group is said to exemplify the problems of human relations: a reflexive analysis would note rather that in attempting to address "the problems of human relations" those same problems would manifest themselves which would then raise the topic: "the problems of attempting to address the problems of human relations".

Elliott, in the paper quoted at the beginning of this chapter, refers to criteria guiding the process of action-research by saying: "Action-research ... can only be validated in unconstrained dialogue" (Elliott, 1978b, p. 356). He goes on to specify:

> "The participants must have free access to the
> researcher's data, interpretations, accounts,
> etc. and "the researcher" must have free access
> to "what is going on" ...   Action-research
> cannot be undertaken properly in the absence
> of trust established by fidelity to a mutually
> agreed ethical framework governing the collection,
> use and release of data".   (pp. 356-7)

For McDonald and Walker (1975) the process is one of

negotiation: the case-study worker does not produce

one summative interpretation but rather engages in a

negotiating process: "the evaluator acts as broker in

exchanges of information between differing groups"

(op. cit., p. 7).   For Elliott the "process" is

constituted in an "ethical framework", whereas for McDonald

and Walker there is also a related political dimension:

the "process" they outline is termed "democratic"

evaluation, which they say is predicated on the notions

of "confidentiality", "negotiation", "accessibility", and

"the right to know" (ibid., p. 7).   In both cases the

process of investigation does indeed begin to be the

topic of grounding principles, in which the epistemological

adequacy of an account is described in terms of the

interpersonal conditions of its possible production.

However, both Elliott and McDonald and Walker formulate

the investigative process at the level of mundane and

rhetorical prescriptions which fail to consider the

further reflexive processes by which such mundane rules

would have to be interpreted.   How would an adequate

degree of "faithfulness to an ethical framework" be

decided?   How accessible is "accessible"?   What are

the structure and (inevitable) limits of "a right to know"?
In remaining at this level, these writers rely for
their intelligibility upon the glossing procedures which
a reflexive analysis would take as its topic. They
have attempted to provide concrete answers, injunctions,
and a method, where a reflexive analysis would provide
questions, dilemmas, and a redirected problematic.

However, another writer, Kemmis, does invoke the
principle of reflexivity more explicitly: "The insights
reached through case study are impermanent ... (they)
must therefore be treated historically. Any useful
social science is reflexive, and must be treated as
such" (Kemmis, 1980, p. 133). For this to be more
than an invocation, however, "history" itself would
have to be formulated reflexively, rather than being
treated (as it so often is within such arguments) as
a taken-for-granted causal origin. More precisely
Kemmis says:

> "In reporting the study, the case study worker
> demonstrates how, in his own case as a cognitive
> subject, the imagination of the case and the
> invention of the study have exerted controlling
> influences on one another" (ibid., p. 126).

Kemmis calls this a "dialectical process" involving
the subject, the object (ie. "the case"), and the method
(ie. "the study") (ibid., p. 124). This, he says,
is "a new perspective" which preserves "the interdetermin-
acy of knowledge" as a constructive alternative to the
untenable claims of positivism (p. 117-9).

However, Kemmis's formulation presents an
interesting ambiguity. On the one hand he proposes a
cognitive subject who "imagines" and "invents", and yet

the very activity of exerting the power of imagination
and invention seems to constitute the subject once
more in terms of "control" and "influence". This
could only be addressed in terms of a reflexive theory
of symbolization in general and of language in particular,
which is exactly what Kemmis's paper lacks. On the
contrary he oscillates between formulations of the
subject as a master of language ("In all knowing, the
knower ... brings to bear his language and perceptual
habits" - p. 108) and references to Wittgenstein (ibid.,
p. 101, p. 135) where language is an independent structure
which masters the subject, by means of "conventions"
and "games" (p. 135). By thus reducing the complex
reflexivity of language to an unaddressed dichotomy,
Kemmis can only imply the parameters of the reflexive
awareness which must underlie a non-positivist process
of inquiry, ie. imagination and control, indeterminacy
and validity, contingency and necessity. He leaves us
with the problem of how such a reflexive awareness
could be formulated: analytically, in order to conceive
of that form of validity which is compatible with the
indeterminacy of knowlege; and yet descriptively, as
a form of theorizing to which an action-research study
could aspire.

In previous Chapters I have made two suggestions
concerning a possible reflexive dimension to action-
research - both embodying the form of the question -
the mutual questioning of action and theory (Chapter Two)
and the mutual questioning of professional and client

(Chapter Four). In examining the nature of the
"validity" with which reflexivity might be concerned,
it is once more the possibilities of the questioning
mode of thought I wish to explore. Heidegger says
that to understand "a thinker" is "to take up his
thought's quest and pursue it to the core of his thought's
problematic". In this way, he continues, "we are taking
a way of questioning (Heidegger's emphasis) on which
the problematic alone is accepted as the unique habitat
and locus of thinking" (Heidegger, 1968, p. 185).
Now, whereas questioning is taken to be the quintessence
of "thinking", Heidegger's whole effort in the second
half of What is Called Thinking is an elaborate dismantling
of the syntactical structure of the assertion, in order
to reveal the thinking which asserting conceals and,
layer upon layer, glosses. In this he seems to be
engaging directly with Hegel's problem (already cited):
"It is not possible for us ever to say, or express
in words, a sensuous being that we MEAN". In this
respect both writers seem to suggest an argument that
the "performative" functions of language's indicative,
non-questioning mode (noted by Austin: How to Do Things
With Words, 1962) constitute the problematic nature of
language as an analytical means. To assert a meaning
is to take part in the mundane world of unexplicated
action (listed by Austin as: giving verdicts, exercising
power, making commitments, and, in general, adopting
roles). Extending this argument, then, one might
suggest that it is the question which can interrupt this

mundane interchange by addressing the grounds of its
intelligibility: since assertions can never give their
own grounds, they can always only address one problematic
by creating another; hence questioning alone is the
"habitat" of the problematic in general.

How, then, could "questioning" establish such a
habitat within action-research? Action-research studies
have frequently been described as "dialogue" between
participants and as "brokerage" between the multiple
viewpoints of those involved (eg. Elliott, 1978b,
p. 356; McDonald and Walker, 1975, p. 7). The image
of a "broker" neatly evokes, in a context of commercial
trafficking, the ambition of a format acceptable to
all "parties". But what would be its theoretic
equivalent?

Any set of viewpoints within a mundane situation
will manifest a range of tensions or even incompatibilities.
Merely to "exchange" the viewpoints among the parties,
as McDonald and Walker suggest (op. cit., p. 7) is
not necessarily more likely to generate a single
mutually acceptable interpretation than to reinforce
existing oppositions. And for the researcher to <u>adopt</u>
a viewpoint on the basis of an elaborately justified
adjudication between members' interpretations would
still be to operate within that set of oppositions.
To this extent, any justification of a particular
preferred viewpoint will be "polemical" and thus, according
to Heidegger, unlikely to constitute nor to develop
"clarification". As Heidegger says, "Any kind of polemic
fails from the outset to assume the attitude of thinking.

The opponent's role is not the thinking role. Thinking
is only thinking when it pursues whatever speaks <u>for</u> a
subject". Heidegger, 1968, p. 13). "Polemic" is the
language of assertion, the language of what one might
term "oppositional interpretation": it asserts the
adequacy of this interpretation and the inadequacy of
others. When this process is extrapolated one can
see that the justification of asserted interpretation
will merely serve to maintain the pressure of the mundane
power struggle, within which any claims to validity will
immediately be contested. The oppositional stance
justifies one interpretation by attempting to annihilate
the intelligibility of what it rejects; this is the
rhetorical mode of the law-courts, of parliament, of
wars, rows, and divorces.

In contrast, reflexive interpretation is the
language of questions: it questions my interpretation
along with others; its extrapolation poses as problematic
the origin, the coherence, the grounds, of <u>all</u>
perspectives; it is a form of question which attempts
to speak <u>for</u> not against its interlocutor (a formulation
conventionally espoused within "counselling" for example).
It creates a theoretic space by means of a <u>general</u>
withdrawal from interpretation to problematic. This is
a space therefore within which discourse can proceed
under the auspices of theoretic grounds, which may be
shared, and which thus may come to be agreed as <u>valid</u>
theoretic grounds for the whole set of interpretations
at issue. Further, and of crucial importance for

action-research's commitment to "change" and "improvement",
the withdrawal from interpretation to problematic may
create not only a theoretic space but also as it were
a potentially political space, allowing for at least
the possibility of a redefinition of the interpretations
themselves, and hence, in turn, of new possibilities
for action.

In this way, Heidegger's notion of "thinking"
as reflexive questioning can suggest a possible analytical
form for action-research's metaphors of "negotiation"
and "brokerage".   But then a further question arises:
if a reflexive questioning can constitute a theoretical
space which allows the possibility of change, what form
might this change take, such that change itself might
be formulated analytically, rather than as mere contingency?
It is in this context that I wish to examine the significance
of the dialectic as a basis for critique and thus for
transformation.   Can the dialectic be formulated
reflexively and thus constitute for action-research a
further dimension for the process of theorizing?

Action-Research, Validity, Dialectics

Action-research has frequently invoked the rhetoric
of dialectics as a way of presenting its commitment to
action and to change, and some of these presentations
were considered in Chapter Two in order to explore the
possible form, within action-research, for a dialectic
between action and theory: in this section I wish to
examine how far the form of the dialectic might allow

the action-research study itself to approach its
inherent problem: how can the study of a single
concrete situation claim a validity beyond that of a
possible interpretation, a mundane actor's perspective?
(see p. 208 above).

In one simple sense, "dialectic" can enable us
to address once again the problem of "grounded theory",
which (as I have argued earlier) is also action-research's
problem. Theorists of grounded theory suggest that
validity can be sought through "triangulation" of
methods and viewpoints (Denzin, 1978, p. 21; Becker,
1971, p. 58; Elliott, 1981, p. 19); but when they do
so, what are the grounds for the Difference which produces
the triangulation AS a triangulation, and thus creates
the force of the metaphor of validity derived from
trignometry? Problems in navigation can be solved
by invoking Euclid's theories of the forms of triangles,
but what are the equivalent theories and forms which
problems in social science might require? A straight
line identifies an infinite number of points: only
the Difference created by a triangular form enables
the One point to be identified. Similarly, the listing
of a multiplicity of interpretations does not generate
a basis for choice between them (nor for the construction
from them of a further transcendental interpretation)
until they are structured in terms of a principled
conception of Difference. In the previous section
this principled Difference separated reflexive from
assertive analysis; in this section Difference is

examined in terms of Contradiction, as a principle
which permits "dialectic" as a process of theorizing.

Underlying the image of triangulation is the desire
to create validity through the structure of inquiry,
rather than by the multiplication of the objects of
inquiry: for positivist social science validity can
be located in the replication of similarity (generalization
of the object-as-a-unity); for action-research and
case study the object itself is non-replicable - only
by comprehending the structure of the object as the
set of Differences which constitute it, can validity
be claimed in terms of a generalizable structure.
However, grounded theorists and action-researchers are
concerned that this structure sould be grounded in the
object of the inquiry, rather than in an independent
system of categories brought to the inquiry. Hence the
relevance at this point of one of the major questions
concerning the dialectic: where are contradictions
located?

Dabates within Marxist theory have attempted
at times to provide clear-cut answers to this. For
example, Colletti (1975) wishes to make a clear distinction
between conflicting forces in nature and logical
incompatibilities in thought, but finally recognizes
that such a dichotomy, resting as it does on a further
dichotomy between "science" and "philosophy" merely
leaves the social sciences "without a true foundation
of their own", awaiting a "reconciliation" (op. cit.,
p. 29). In reply, Edgley (1977) proposes such a
reconciliation by suggesting that social reality, being

a symbolic constellation, is therefore both "thought" and "nature", and hence in itself quite intelligibly constituted in contradictions which it is the task of analysis to expose and thus help to overcome. Yet, as McCarney says, "the realization of (Edgley's) science would be a society without contradiction. It is far from clear that such a state of affairs could be coherently described in any detail" (McCarney, 1979, p. 29).

Yet each of these proposals seems to be an attempt to resolve an issue which seems in principle to be not susceptible of resolution, namely the problem of the irremediable tension between theory's desire for clarity of exposition, and the complexity of - on the one hand - its object (the contradictions of social reality) and - on the other hand - its relation with that object (theory's essential reflexivity). Formulations of the role of "contradictions" in social analysis must - I would argue - embrace this complexity - a complexity which involves the symbol and the object, thought and reality, and indeed renders problematic those very categories. Hegel, for example, as we have seen, described "The Thing" as "a manifold" of contradictions: the One essence and the Many qualities, the universal and the concrete, the self-defined and the defined-in. relation-to-other (Hegel, 2977, pp. 67-71). Further: the consciousness which perceives the Thing cannot simply distinguish the Thing, Consciousness, and the act of perceiving: instead the act of perceiving

becomes "a complex assumption of responsibility"
(ibid., p. 74) whereby the constitution of the Thing
in consciousness becomes an act of self-definition,
and thus the complexity of the Thing becomes reflected
back as a structuring of consciousness itself (pp.
73-5).   The ontology of the Thing is thus a "whirling
circle" (p. 79) from which commonsense tries to escape
by means of such simple dichotomies as single/plural,
essence/qualities, concrete/universal, or - one might
add - the contradictory/the logical.

In this respect, as noted in Chapter One (see p.
Lenin follows Hegel:

> "Dialectics is general as a method since, as
> Hegel noted, every proposition itself contains
> the contradiction of the relation between universal
> and individual"   (Lenin, 1972, p. 361)

Thus, "The universal exists only in the individual
and through the individual" and conversely "Every individual
is connected by thousands of transitions with other <u>kinds</u>
of individuals (things, phenomena, processes, etc.)"
(ibid., p. 361) which seems to evoke not only a
connected world of "nature" but also, potentially at
least, that metaphorical aspect of thought which gives
the development of knowledge always the property of
a dialectic (ibid., p. 362) - a "spiral", so that for
Lenin "rectilinearity" of thought is equivalent to
"obscurantism" (p. 363).   This emphasis is followed
by Adorno for whom contradiction is that principle
of ontology "which indicates the untruth of identity,
the fact that the concept does not exhaust the thing
conceived" (op. cit., p. 5).   What must be avoided

therefore is the apparent self-sufficiency of the concept (ibid., p. 12) and the implicit claim to unity of "systems" (p. 20). Instead, "philosophy" must "adhere as closely as possible to the heterogeneous" (p. 13). For these writers, then, dialectics proposes a way of encompassing the complexity of social experience, and the complexity of attempts to understand social experience, within a general structural principle, while allowing for the essential heterogeneity of the concrete. In other words, dialectics offers the possibility of grounding validity in experience, by formulating a principle for the structure of inquiry which is at the same time a principle for the structure of experience itself.

Such seemed to be the value of dialectics for action-research, and, as such, inspired my article "Dilemma Analysis - A Contribution to Methodology for Action-Research", (Winter R., 1982) as an attempt to apply dialectical principles to action-research.* The following passage embodies the main line of the argument concerning validity and dialectics in an action-research context (in this example, a study of students on "teaching practice"):

The nature of the action-research task

A teaching practice, in common with many social situations, involves interaction between different parties who, as a consequence of their different roles in the situation have different aims, priorities, and definitions of reality. Also,

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* See also Whitehead and Foster (1984) p. 43.

the situation creates a hierarchy of power and
status between these roles, hence, <u>some</u> of the
problems typically encountered will rest on a
failure by one party to appreciate the point of
view of the other parties involved.   The task I
formulated for myself, as a teaching practice
supervisor/researcher, was to attempt to transcend
my view as a supervisor in order to create an

1)   account of the T.P. situation which would be
faithful to the views of students, classroom
teachers, and pupils, as well as those of
fellow supervisors.   This account had to gain
the assent of all parties so that it could be
used to illuminate for each party the point of
view of the others, as a practical contribution
to preparation for T.P.   The different views
therefore had to be presented plausibly as
parallel rationalities, without the hierarchical
valuation which conventionally discriminates

2)   between them.   In other words, the analysis had
to gain acceptance as "objective", evoking the
main areas of tension in the siutation without
generating immediate controversy by seeming
partisan, which would of course lead to its
being rejected in such terms as: "It's your
point of view as a supervisor" or alternatively:
"You've gone over to the other side".   The
action-research task then, in this case, and not
(I think) untypically, was precisely that of

2)   creating an account of a situation which would be
seen <u>by a variety of others</u> as convincing, ie.
as "valid".

The theoretical basis of the method

It was earlier argued that basing an interpretation
directly on social theory inevitably creates an
interpretation <u>imposed</u> by the researcher.

However, I suggest that this difficulty can be
overcome by providing a theoretical basis for the
method rather than the interpretation.   This
entails working with a different level of theory,
namely theory concerned not with patterns of
motives, ideologies, or institutional structures
and relationships, but with the most general
characteristics of social reality itself.
Hammersley M. (1980) uses the terms
"substantive" and "formal" theory to articulate
a similar distinction.   Roughly then, I wish to
distinguish between "substantive theory" which
guides the interpretation of specific data and
"formal theory" which guides the specific method
for interpreting any data appropriate for that
method.

The formal theory which guides the method
of Dilemma Analysis is what could loosely be
called the sociological conception of "contra-
diction", which is used here in the form of a
series of general, indeed all-embracing postulates:
that social organizations at all levels (from
the classroom to the State) are constellations
of (actual or potential) conflicts of interest;
that personality structures are split and
3)   convoluted;   that the individual's conceptualization
is systematically ambivalent or dislocated;
tat motives are mixed, purposes are contradictory,
and relationships are ambiguous;   and that the
formulation of practical action is unendingly
beset by dilemmas.   Hence a statement of an opinion in
interview is taken to be a marginal option which
conceals a larger awareness of the potential
appeal and validity of different and even opposed
points of view.   (This is an elaboration of
Winter, 1980b, p. 68).   On this basis, then,
it became intelligible to analyze the interview
transcripts not in terms of particular opinions,
but in terms of the issues about which various

opinions were held. The method is called "Dilemma Analysis" precisely to emphasize the systematic complexity of the situations within which those concerned have to adopt (provisionally at least) a strategy. Beneath the analysis lie the conceptual underpinnings of Marxian and Freudian theory; at the literal surface of the analysis is the relatively non-controversial notion of the paradoxical nature of social existence. (op. cit., pp. 167-8)

The article was written in 1980, before the present study was undertaken, and the extract above clearly reveals a number of weaknesses, some of which I have already criticized in other action-research work. Concerning the marginal numbers:

1)   Its own practice is presented as the articulation of viewpoints, a form of "brokerage" (see p. 230 above).

2)   Its version of validity is seen as a concensus, without any reference to a process by which such a concensus might be created, except through the presumption of spontaneous empathy (cf. Eisner's "vicariousness", see p. 221 above).

3)   Although there is a reference to the "ambivalence" of conceptualization, there is no specific reference to the reflexive problematics of language itself, and thus there is no awareness that the action-research worker is himself beset by the problematics he describes.

The last point gives rise to a crucial weakness of the article: it attempts to provide a quasi-mechanical "methodology" based on precisely the literal

specification of simple alternatives which Adorno

dubs "bureaucratic thinking" (Adorno, 2973, p. 31).

(See the "Teachers' Perspective Document" later in the

article, which presents the various issues in terms of

a repeated sequence of oppositions: "On the one hand ...

BUT on the other hand" - pp. 271-3). Thus "Dilemma

Analysis" attempts to be literal, where it should

recognize the inevitability of metaphor; it attempts

to be exhaustive, where it should recognize that it

must remain "inconclusive" (Adorno, op. cit., p. 33);

and it locates contradiction in an external world of

actors' perspectives, where it should recognize that its

own processes of cognition and expression are constituted

in those same contradictions. Hence it attempts to

prescribe a description by utilizing "contradiction"

as a resource which could provide a method, where it

should attempt to transcend description by reflexive

analysis of the problematics of that resource in

relation to its own process.

Finally, and most disabling of all, it denies

the temporal dimension required by its own theory.

Contradictions are constituted as such *as* terms in

a dialectical process of transformation. (As Lenin*

says: "The condition for the knowledge of all processes

of the world, in their 'self-movement', in their

spontaneous development, in their real life, is the

knowledge of them as a unity of opposites" (Lenin, 1972,

p. 360). In thus presenting contradictions as a

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* "Lenin ... the master action-research officer of all
time" (!) (Midwinter, 2972, p. 57).

series of static, if complex "perspectives", Dilemma
Analysis fails to provide for its own process of
inquiry which constitutes them, a further failure of
reflexive awareness, and, more curiously, a failure of
the basic spirit of action-research, whose ambition is
essentially to constitute its theorizing within the
developing action of its own process.

In principle, however, as the above critique
implies, the dialectic could provide a powerful theoretic
basis for the conduct of action-research.   It raises
the possibility of an analytical basis for presenting
the structure of concrete situations and thus for
grounding the study of such situations in a general
principle, a principle which would not be the pretext
for a prescriptive methodology, such as "Dilemma
Analysis", but an inherent epistemology which locates
theorizing in relation to its own cognitive processes
as well as to its apparent object.

In this way, one might begin to formulate a
constitutive relationship between the two principles
of reflexivity and dialectics in terms of which I have
tried to present "validity" in this chapter.   One
might suggest that there is an analogy between the
questions which reflexivity poses to interpretive
assertion - questions of grounds and possibility -
and the dialectical logic which, as Adorno says, is
"one of disintegration ... of the prepared and
objectified form of the concepts which the cognitive

subject faces" (Adorno, 1973, p. 145).   "Dialectics
is ... the resistance which otherness offers to identity"
(ibid., p. 161).   The important point is that investi-
gators are themselves "cognitive subjects", and their
own interpretations take on a quality of "identity"
as soon as they are expressed, so that their own work
must accept inevitable "disintegration"; it must itself
face the resistance of "otherness", and thus in the
end "enact its inconclusiveness" (ibid., p. 32).
It is such an emphasis that action-research requires,
since action-research by its very nature is constituted
in a dialectic between action and theory, and thus does
not wish its inquiry to provide a conclusive prescription
for action, but rather to allow action to open out
developmentally on the basis of such provisional
enlightenment as has been achieved by its inquiry, and
on the basis of that achievement always to invite and
require further phases of action-research itself.

It is on such a basis, I would argue, that action-
research could begin to formulate the "validity" of
its processes, and it is thus within this formulation
that we must now seek a sense of "validity" for action-
research's descriptive accounts of the situations
which are its topic and its occasion.

Dialectics, Reflexivity, and the Descriptive Text

It was noted at the very beginning of this chapter that action-research has often attempted to authorize its validity claims in terms of "anthropological" case-study methods, and it is with an anthropological approach to the issue that this section commences, namely an analysis of "thick description", so unsatisfactorily glossed by Eisner (see p. 220 above). The term originates with Gilbert Ryle, but its relevance for the present argument is elaborated by Clifford Geertz.

Positivism, he argues, seeks "valid" description by reducing phenomena to the "thin-ness" of "operational", ie. behavioural, terms (Geertz, 1973, p. 5), so that a social action such as "winking" becomes "rapidly contracting (the) right eyelids" (ibid., p. 7). Validity here is: what-could-not-possibly-be-contested-by-anyone. But this would be an entirely unhelpful formulation of validity in social inquiry, since it evades social inquiry's central task: to understand the significance of the action in question. The rapid eyelid contractor may have an involuntary muscular twitch (and behaviourists could indeed collect such instances, but he or she may be "winking", in which case the question is, whether the action is an enticement or a conspiracy, or even "practising a burlesque of a friend faking a wink to deceive an innocent into thinking a conspiracy is in motion" (Geertz, 1973, p. 7). Hence the description of social actions must be at least

as "thick" (ie. as complex, as multi-layered) as the
meaning of the actions described.   Now, it is clear
from Geertz's example of how "meaning" is structured into
layers of mutually imputed interpretation, that one
such layer must be the interpretation imputed by the
describer.   This in turn implies that accounts of
social meanings can never have the finality of a
behavioural definition: "ethnographic assertion is ...
essentially contestable" (Geertz, op. cit., p. 29).
But this does not mean that interpretation is therefore
merely a matter of private opinion or whim, which is so
often the despairing response to a recognition of the
impossibility of realizing positivism's ideal.   On
the contrary, meaning (says Geertz) is inherently
"public" (ibid., p. 12,, ie. it is constituted essentially
in the dialectical intersubjectivity and interplay of
cultural symbols (eg. "winks", "conspiracies",
"fakes",.   Thus, although interpretations can never
be finally "verified", they can always be "appraised"
(ibid., p. 16), and this appraisal itself, being a
further interpretation, is available for further
appraisal, in the endless dialectic of inquiry.

In other words, description may be considered
as a hermeneutic experience and accomplishment: "The
structure of the hermeneutical experience is ... the
dialectic of question and answer" (Gadamer, 1975, p. 340).
In more detail:

"The reconstruction of the question to which

> the text* is presumed to be the answer takes
> place itself within a process of questioning
> through which we seek the answer to the question
> that the text* asks us"
> (ibid., p. 337)

This dialectic interweaves with another, and one which

is particularly relevant to action-research:

> "Understanding (is) an event, and the task of
> hermeneutics ... consists in asking what kind of
> understanding, what kind of science it is, that
> is itself changed by historical change".
> (ibid., p. 276)

In a sense Gadamer begins to answer his own question when

he goes on to describe a third dialectical strand in

his presentation of the process of interpretative

description, "the great dialectical puzzle of the one

and the many, which fascinated Plato" (ibid., p. 415),

and which in Hegel's version has been cited frequently

in this work.   Gadamer presents it as follows:

> "The hermeneutical rule (is) that we must
> understand the whole in terms of the detail
> and the detail in terms of the whole"
> (ibid., p. 258)

a rule which would need some reformulation in the

context of social inquiry, of course, since social

situations do not have the finite boundaries of a "whole"

text.   In fact, Geertz's analysis of ethnographic

meaning construction (outlined above) could serve as

such a formulation of "hermeneutics" as applied to social

actions, and in particular to the process of action-research.

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* For "text" read "social action" in the context of
the present argument.   The possibility of this
equivalence is asserted by Ricoeur, in "The Model
of the text-meaningful action considered as a text",
in the same volume as Ricoeur (1981).   See p.
below.

Meaning is a relation between social actions and their cultural matrix; interpretations of this relation and appraisal of those interpretations must endlessly succeed one another, since interpreters are situated within the same process of historical change as the social actions they describe.

At this stage in the argument we have moved once more to the central role of reflexivity. How can we approach description in reflexive terms? We can begin to pursue this question by considering Levi-Strauss's essay "The Science of the Concrete" (in Levi-Strauss, 1966). Levi-Strauss approaches the issue of the relation between concrete experience and validity of meaning by distinguishing between "two strategic levels at which nature is accessible to scientific inquiry: one roughly adapted to that of perception and the imagination; the other at a remove from it". (op. cit., p. 15). These two strategies are labelled (with specific reference to man's interaction with the world of inanimate objects) "engineering" and "bricolage" (ibid., p. 17). The engineer operates with "concepts" whereas the bricoleur uses "signs", the distinction being that, "although either may be substituted for something else, concepts have an unlimited capacity in this respect, while signs have not" (p. 18). The bricoleur therefore "interrogates ... an already existent set made up of tools and materials, to consider or reconsider what it contains, and finally and above all to

engage in a sort of dialogue with it ... to widen the
possible answers ... to discover what each of them could
'signify' and so contribute to the definition of a set
which has yet to materialize but which will ultimately
differ from the instrumental set only in the disposition
of its parts". (p. 28). In other words this is an
essentially <u>reflexive</u> review of biographically
situated resources and their possibilities. The
engineer, armed with the unlimited referential scope
of "concepts" can interrogate "the universe": he can
claim to transcend his culture, while the bricoleur
knows he must stay within it (p. 29). In this way,
Levi-Strauss's account of the concrete science of the
bricoleur evokes a way of formulating the possible
achievement and the necessary limitation of the social
scientist's descriptive case-study. He allows us to
reformulate "concrete description" in terms of the
possibility of an analytical and reflexive strategy based
on the multiple meanings of the culturally defined <u>sign</u>,
leaving the larger claims of the logically constructed
<u>concept</u> to those who wish to define their social science
in positivist terms* (cf. Popper, whose apparent modesty
in restricting his claims to "social engineering" is
thus revealed as mock modesty indeed!)

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* There is, however, a tension in Levi-Strauss's work
  between passages where he clearly describes his own
  myth-ology as bricolage ("a precarious assemblage of
  odds and ends" - 1981, p. 562, and other passages
  where he seems to anticipate a <u>future</u> state of affairs
  when the human sciences will indeed transcend
  bricolage through an "absolute" methodology (ibid.,
  p. 686).

The notion of "concrete description" as carried out by "observers" (who could possibly - if they wished - choose to do "abstract" description instead) raises the unanswerable question of how such observers could <u>select</u> their concrete details from the infinite range available, and thus of how any such selection could be either replicable or representative. In contrast, Levi-Strauss's notion of "bricolage" as a science OF the concrete avoids the epistemological trap of the residually positivist formulation by treating "the concrete" as the inevitable habitat of social inquiry, a habitat which delimits cognitive resources as culturally constructed and contingently available, and constitutes "validity" as a provisional, essentially temporary achievement. As "bricoleurs", in other words, interpreters of the social world <u>know</u> the limitations of their resources and their achievements, as constituted by their situational availability: only self-styled "engineers" could consider themselves able to ignore the reflexivity of their work and thus to claim "universal" validity.

Levi-Strauss goes on to make the reflexive dimension of his work quite explicit, and in doing so addresses the other important theme raised by the action-research writing reviewed at the beginning of this chapter, namely the relationship between descriptive structuring and <u>aesthetic</u> form. He suggests that any symbolic process (science, myth, myth-ology, ritual, or art) can be considered as constituted in a relationship between "structure" and "event", between the contingent and the necessary (Levi-Strauss, 1966, p. 21 ff.). In general

original distinction: what event could be so simple
that it was not also a structure, and what structure
could be either so eternal or so instantaneous that it
did not also constitute a complex event?   However,
the value of Levi-Strauss's argument is that he provides
a level of analysis which can-encompass the aesthetic
as a mode of comprehension and expression in juxtaposition
to other forms of symbolization, and that it allows a
consideration of the nature of the aesthetic to be
formulated, which is precisely what the writers
previously discussed have merely glossed as an available
convention.   Levi-Strauss's proposal is that the
expression itself is the "structure", and that this
structure must be considered in relation to its three
constitutive contingencies: the occasion of the work,
the execution of the work, and the purpose of the work
(op. cit., p. 27).   Or: "The process of artistic
creation therefore consists in trying to communicate
(within the immutable framework of a mutual confrontation
of structure and accident) either with the _model_ (ie. the
reality-to-be-represented - RW) or with the _material_ or
with the future _use_". (ibid., p. 27).   (These are
not of course mutually exclusive alternatives.)

The importance of this argument for action-research
is two-fold. Firstly it enables us to envisage a
"_reflexive description_" as one which makes explicit
the relation between, on the one hand, its own structure
and, on the other hand, its symbolic resources, its
audience, and the events which are its topic.   Secondly,
it makes possible an analogy between social research

and a formulation of art which is not merely concerned with a _model_ (which realism and positivism might claim or admit - see above, p.223), but also with both the "material" and the audience for the symbolic process, ie. a reflexive formulation of the aesthetic, which would parallel action-research's own ambition to transcend positivism by addressing the principled relation between action and theory (the "materials" of its research process, and between research and its audience, namely its attempts to formulate such possibilities as i) the action-researching professional as one who is simultaneously artist and audience, or ii) the case study researcher whose work is a continuous negotiation with the practitiners whom he serves and in whose concerns he wishes to "ground" his theory.   In both cases there is a constitutive relation between expressive process, audience, and theoretic resource.

A reflexive formulation of the aesthetic would find support in, for example, Kenneth Burke's contention that (literary) "form would be the psychology of the audience ... the creation ... and the adequate satisfying ... of an appetite in the mind of the auditor" (Burke, 1968, p. 31), an emphasis which for Barthes leads to "the realization of the relation of writer, reader, and observer (critic)" (Barthes, 1977, p. 156), and transforms the closure of the author's descriptive _work_ into the open-ness of the reader's interpretive interaction with a _text_ (ibid., pp. 155-6).   Hence, whereas literature previously employed a supposedly

transparent language for the description of "Nature",
"Literature is (now) openly reduced to the problematics
of language" (Barthes, 1967, p. 8).   (However, this
"reduction" is better described - without nostalgia -
as a <u>principled recognition</u>).   The general thesis that
the essence of a modernist aesthetic is its reflexivity
is the theme of Gabriel Josipovici: <u>The World and the</u>
<u>Book</u>:   "The modern writer ... makes his art out of the
exploration of the relation between his unique life
and the body of literature, his book and the world".
(Josipovici, 1971, p. 291).   This argument applies not
only to such explicit and well-known exponents of
"modernism" as James Joyce, T.S. Eliot, and Samuel
Beckett, but to aesthetic form <u>in general</u>,(see Chapter
One, p. 21 and Chapter Three, p. 160).

The relevance of this for social science is taken
up by Michael Clarke, who contrasts literature's
<u>tradition</u> of reflexive self-questioning with science's
strategy of insulating the person of the scientist from
his data by means of his "methodology" (Clarke M.,
1975, p. 99).   In other words, whereas "artists"
have accepted (and indeed, latterly, <u>embraced</u>) a role
which casts them simultaneously as hero and as victim,
"scientists" persist in attempting to evade <u>any</u> destiny
whatever, by seeking a role of principled invisibility
through an <u>ideology</u> of technicism (cf. the analysis of
the "wounded healer", Chapter Three, p. 147).

The way in which action-research can learn from
literature is not, then, to borrow its "realist"

claims as an alternative approach to the generalizing
format of scientific positivism - which action-research
recognizes it must try to avoid.   Rather, by analogy
with a "modernist" aesthetic, its claims to a theoretic
status can be made through an explicit recognition of
the reflexive form of its own process.

The above argument constitutes the process and
the effect of art (and, by implication, of inquiry)
as essentially reflexive in terms of its confrontation
with its contingent conditions ("material", audience, and
"model").   What about the aesthetic structure itself,
which so far has only been referred to (within a realist
problematic) through the positivist metaphor of a
"typology" expressing a "perspective"?   At this point it
is once more helpful to invoke the principle of dialectics
as the inherent structure of social phenomena.   Even
here a lead is given by action-research writers them-
selves, namely McDonald and Walker.   Having said, "the
kind of case-studies which we believe education needs
have characteristics which call for a fusion of the style
of the artist and the scientist", they go on immediately
to quote Freud: "the case histories I write ... read like
short stories" (McDonald and Walker, 1975, p. 3).
Now a Freudian case history is a narrative rather like
a "whodunnit":   the questionable meaning of a dream
sequence, for example, is progressively "solved"
as a structure of "rationality".   Since dreams are
themselves narratives, this resolution is itself a
narrative of a narrative.   Freud's theory of the

representative strategies of the dream thus becomes
a possible entry to the question: how can narrative
"embody 'truth'"? His argument concerns the two
basic dimensions of symbolization, synchronicity and
diachronicity. The chronology of the narrative
transforms causal relations into a sequence (Freud,
1976a, p. 427), and logical relations into contiguity
(ibid., p. 424). The metaphoricity of the narrative
unifies opposites (p. 429) and fragments similarities
into contrasts (p. 431). In general, dreams constitute
a systematic distortion of an original reality, often
to the point of "reversal" (p. 441). To understand the
dream, by means of the case history, is to clarify
the distortion, to reverse (as it were) the reversal.

As with much of Freud's work, an ingenious insight
into symbolic process is limited by an ambition towards
a mechanical methodology (leading, in the present
argument, to the apparent implication that "rationality"
may be "decoded" unproblematically out of "distortion",,
but what Freud does seem to provide here is the notion
of narrative structure constituted in a dialectic
both of action and of meaning, such that one might
tentatively suggest that to understand the "truth"
of narrative is to grasp its structure as dialectic.

It is of course dialectical structure which
underlies Levi-Strauss's analysis of the meaning of
mythic narrative (see Chapter Three, p. 137). For
example:

"For a myth to be engendered by thought and for
it in turn to engender other myths, it is necessary
and sufficient that an initial opposition should
be injected into experience, and as a consequence,
other oppositions will spring into being".
(Levi-Strauss, 1981, p. 604).

In an analysis of mythic structures which has clear

parallels with Freud's previosly cited account of

the dream process, Levi-Strauss suggests (1979,

pp. 224-9) that opposites may be resolved into an

intermediate term, producing a "triad", that characters'

contradictory qualities involve them in relations

which gradually mediate an original opposition, and

that the events of the myth may "transpose" its original

semantic terms.   Levi-Strauss sums up: "The purpose

of myth is to provide a logical model capable of overcoming

a contradiction"(ibid., p. 229).   And we may follow

Northrop Frye in taking myth to be an underlying pattern

for aesthetic form in general (Frye, 1957, pp. 263-

225).

However, this "logic" of mythic or aesthetic form

is not a prescription nor a "typology" but a set of

possibilities made available by the ambiguities, meta-

phors, contradictions, reversals, etc. embodied in the

narrative itself.   This "logic" is embodied in the

narrative in the sense that its constituent units

are "bundles of relations" which are sufficiently large

as to function both synchronically and diachronically

(Levi-Strauss, 1972, 221-2), and thus it constitutes

meaning at the level of metaphorical structures

(protagonists, settings) and dialectical structures

(actions, transformations), or rather: at the level
where metaphor and dialectic are mutually constitutive,
namely, as Derrida notes, the level of symbolization's
intrinsic process of "Differencing".

In this way we can perhaps make sense of Elliott's
original suggestion that action-research can seek, as
part of a non-positivist approach to inquiry, to embody
"truth" in narrative (see pp.208-9 above).   Narrative
recognizes, in Levi-Strauss's original terms, the
analytic confrontation between the necessary and the
contingent, structure and event, general and particular.
This confrontation is expressed in narrative's under-
lying pattern of metaphor and dialectic, ie. its pattern
of transformation both at the level of meaning and of
action, which parallels action-research's own ultimate
ambition to transform meaning by means of action.
Positivist description seeks to dichotomize the necessary
and the contingent in terms of method and data: it seeks
to isolate data so that they have no inherent structural
or temporal properties, and so are purely available
to be gathered (by means of "method") into a literal
and ahistorical "truth"; the form of representation
accomplished by narrative allows truth to be metaphorical,
and dialectical: the narrative of action can show
action's own semantic transformations.

Finally, then, how could these various considerations
be related to a possible set of principles for action-
research's descriptive accounts.   Three of the central
ideas seem to be related in the following way:

1)   A <u>reflexive</u> description can only seek validity
through a structure which embodies a principled
recognition of the <u>problematics</u> of its own possibility
(see references to Barthes, on p.256 above).

2)   The <u>scope</u> of the problematic noted in 1) as
applied to social inquiry, is given by the relationship
between the descriptive account and a) its symbolic
resources, b) its audience, and c) its "model" - ie. the
experience(s) "described" (following Levi-Strauss's
formulation, see p.254 above).

3)   The <u>structure</u> of each of the problematic relation-
ships noted in 2, is dialectical, as follows:

a) <u>Symbolic resources</u> for a descriptive account
are, for example: actors' perspectives, institutional
documents, interpretive theories, and narrative structures
for specific events - which embody mythic/ideological
patternings of its semantic terms, such as its
constitutive "characters" and "plots". The relation
<u>between these elements</u> will be dialectical, ie. a
combination of intimacy AND incongruity, similarity AND
difference, between ideals and experiences, between
claims and actions, between long-term and short-term
rationalities, and between the ideals, ideologies, reported
experiences, and rationalized interpretations of different
social actors.   (This is an extension of the principle
behind "Dilemma Analysis" - see p.243 above).   The
relationship <u>between the description and such resources</u>
will be dialectical, ie. its coherence will take the
form of making explicit the dialectical play between

elements, in a structure whose unity is that of <u>irony</u>
rather than of resolution and negotiated concensus.
In this it may resemble the documentary or the news*
report which presents "different sides" in a studied
stance of abstention from authorial imposition" (cf.
Barthes on the "death of the author" in the reflexively
conceived text - see p.2.SS above).   In this respect, it
may also resemble a story with a complex plot and character
and with one or several protagonists but - following
the principle of ironic play - <u>without</u> a hero, taking
"hero" here as typically embodying a mythologized
elaboration of an authorial perspective.   (See Brown R.,
- 1977, chapter 5 - for an elaboration of sociological
accounts as structures of irony).

There is another sense in which description will
have a dialectical relation with its resources: it will
recognize the historically situated quality of its
collection, and will explicitly present its collection
as contingent and provisional, rather than as exhaustive
or final.   It will thus be structured by its principled
and necessary anticipation of a <u>continuation</u> (ie.
amendment and critique), since description will have
a dialectical relation with its audience.

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* In mundane examples of such reportage, this
  "abstaining" stance is, of course, merely a carefully
  presented illusion: it is the <u>textual structure</u>
  which is being considered at this point in my argument.

b) <u>The audience</u> for descriptive accounts will
have both necessary and contingent features (see the
presentation of Levi-Strauss's argument, p. 253 above).
At one level the rhetorical processes of writing are
structured by the requirements of an analytically
presupposed intelligibility to a readership. This is
the dialectical structure of intersubjectivity necessarily
required by acts of communication (see Chapter One
p. 32). "Validity", then, would be the achievement
of persuasiveness. But audiences are also historically
contingent. A description may anticipate a highly
<u>specific</u> audience, one which shares a particular stand-
point or set of relevancies, and may achieve a persuasive
validity <u>for</u> that limited audience, while other audiences -
with whom the description in question does <u>not</u> anticipate
a dialectically constitutive relationship - would
characterize such a description as, say, "tendentious",
and would note "inadequacies" in a variety of dimensions.
"Objectivity", within this argument, can then be seen
as the quality of a description which anticipates a
constitutive dialectic with a highly <u>varied</u> audience,
ie. a description which structures a dialectical relation
between a wide variety of its own heterogeneous elements,
and thereby achieves persuasiveness for audiences which
<u>begin</u> their reading of the description from a position of
provisional identification with only a limited range of
those elements. Hence the persuasive <u>task</u> of description
can be seen as establishing, through its own processes,
that the heterogeneity of phenomena <u>does</u> indeed represent

a dialectical (ie. a mutually transformative) relation, rather than that simple antagonism between "similar" and "different" in which they are constituted by the pragmatic requirements of daily life.

c) The "model" for description is a set of experiences, whereas description itself is, of course, constituted symbolically, and - in particular, linguistically.* It has already been argued at a number of points that linguistic representations cannot be seen simply as "labels". Rather their reference to experience must be seen as metaphorical, and thus as always located within the dialectic between reference and difference noted by Hegel (see p.$\mathbf{2\mathbf{1}0}$ and p.$\mathbf{23\mathbf{9}}$ above). Again, Richard Brown makes this point explicitly and at length: "A theory must be metaphorical: if it were literally identical with what it theorizes about, it would not, could not tell us anything new". (Brown R., 1977, p. 101). This would hold true for description, as a communication between One who-has-had-an-experience and Others, who have not had that identical experience but who could be brought to understand that experience in the light of different but potentially similar experiences which they have had. Hence the central function of metaphor's dialectic between similarity and difference.

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* In principle, much of the argument of this section could be applied to descriptions embodied as painting, film, ballet, music, marble, or papier-mâché, and indeed as combinations of these. But to do so would further complicate an already complex set of ideas, and the verbal sign plays a central and - arguably - indispensable role.

(Description is <u>not</u> engaged in between people who are
<u>both</u> present at an event.   Rather, one says to the other
for example: "Just look - isn't that a terrible/
beautiful ..."?   One might indeed say, "It reminds me
of ..." but that, precisely, would return us to the
principle of metaphoricity).   "Metaphor", then, is
itself a metaphor for the problematics of description
and interpretation, the problem of the general and the
parcicular, of description's always ambiguous ambition
to <u>be</u> description (rather than - say - a random association
or an eccentric vision).   Thus "validity" for description
must ultimately reside in its recognition of the very
ambiguity of its own aspiration; it must explicitly
recognize that its metaphorical structure, no matter
how densely and subtly woven, can never claim a literal
or final correspondence with its object.   For positivist
description this would be a matter for despair (as though
"validity" were to be given up as impossible); for a
reflexive and dialectically structured description it
marks a rigorous requirement for critical awareness,
and thus a dimension of validity itself.

But, and finally, what form might be taken by
description's "<u>recognition</u>" of its dialectical ambiguities
and limits?   In general terms we may remember once
more Gadamer's axiom that "the structure of the hermeneuti-
cal experience ... is the dialectic of question and
answer" (Gadamer, 1975, p. 340), quoted on p. 249 above).
But it is more helpful at this stage in the argument to
"reverse" - or rather to extend - Gadamer's statement,
and to consider a dialectic of answer and question, in

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which description's answers to its presupposed questions
are presented in conjunction with the questions it in
turn raises. This would then enable us to formulate
"description" as embodying a similar "questioning dialectic"
to that relating action and theory, presented at the end
of Chapter Two and on p. 246 above. And the format
that this might take in the context of a descriptive
account is in fact suggested by a comment of Lawrence
Stenhouse, the doyen of writers on educational action.
research - a comment which in fact makes curiously
little sense in its original context (a proposal for
Popperian "scientific method") but which seems to have a
very precise relevance for the present argument.

Stenhouse says that, "The dialectic between
proposition and critique ... is personified in the
relationship between artist and critic". (Stenhouse,
1975, p. 124). Now although much "art criticism"
displays a numbing combination of blandly unreflexive
evaluation and crude technicism, there is a certain ideal
for critical writing on works of art which could indeed
be taken as a formulation of the moment of analytical
recognition in the complex dialectic of description.
In this sense, the "critic" (at best) writes a commentary
which accompanies the structure of a work in order to make
explicit the implicit pattern of its complex internal
relationships; in order to do so, it will reveal
ironies, point to dialectical relations between elements,
show ramifications, analyze the detail in terms of the
whole, and insist upon the irreducible complexity of the

whole as (at least) the sum of <u>all</u> its details -

hermeneutics, after all, originated as a method for

textual criticism. In this way, if "description" is

to be, as has been argued, a dialectical structure of

irony and metaphor (rather than - positivistically -

a unified structure of concensus and literal reference),

then perhaps the descriptive text can be differentiated

from, say, the novel or the documentary (as an analytical

from a rhetorical text) by the inclusion <u>within</u> the

text itself of a "critical" <u>commentary</u>. in the sense

outlined here, one which addresses directly its own

problematic and how its processes address that problematic.*

Here, then, is a final dimension for the "validity"

of descriptions, namely the adequacy of its own explicit

recognition of its reflexive and dialectical structure.

There is a link between this suggestion and the comments

of Peter McHugh et al. on the collaborative process

of their own text, in which "response papers seek to

enter into relationship with the original by transforming

its present but unexplicated features" (McHugh et al.,

1974, p. 5). The point is, that texts are <u>open</u>,

"plural" structures, (Barthes, 1977, p. 159) intelligible

only in the light of the reflexivity of language and

the constitutive dialectic between writer and reader.

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* I am indebted for this argument to my colleague
  David Ball and to members of the Essex Institute
  M Ed course in Educational Research.

Textual openness and plurality have been fully explored
as principles for the understanding of literature
(see p. 2SS above), which may explain the affinity for
aesthetic forms expressed by a number of action-research
writers, who thus perhaps sense the general argument of
this Chapter: that such openness is a necessary
requirement for action-research, since action-research is
predicated upon the assumption that a descriptive
account will not be a finality but a moment in a
continuing process.

The "validity" of description, in this context,
then, is not a matter of being "correct", but of
adequately representing "the conditions for its
possibility" (see Chapter One). Or, following Levi-
Strauss's argument - see p. 2S3 above - "validity"
concerns the necessary rather than the contingent
features of description. The contingencies of
"correctness", on the other hand, will not be entirely
unintelligible when the dialectics of action-research
moves to the moment of action, when - as was argued
at the end of Chapter Two - the question becomes: which
of the possibilities made explicit through the open
text of "description" would be a feasible practical
strategy now? But, in general, the notion of
"validity", as applied to the complex processes of
action-research, may not be sought in terms of a
"correspondence" between two simple entities - "account"
and "reality" - but rather in the appropriately complex

principles of reflexivity and dialectics, which (it
has been argued in this chapter) can guide the internal,
textual structuring of action-research's accounts, as
well as - at the same time and without incoherence -
the other moments of action-research's process.

CHAPTER SIX

CONCLUSION

My aim in this study has been to reconstruct the intelligibility of action-research by disentangling its inherent possibilities from its heterogeneous claims. Some of these claims are simply borrowed from positivist social science - claims to possess an authoritative methodology for the production of accurate descriptions or of "grounded theory", for example - and others seem to be defensive counter-claims, made against positivism's rejection of action-research's adequacy - claims to flexibility, creative idiosyncracy, immediate practical relevance, democratic process, and aesthetic form, for example. The contradiction between these two sets of claims can be traced to the contradictions in the relationship between orthodox social science and the social world which is its topic and its resource. The Good of action-research is that it glimpses the need to reformulate this relationship between science and world, knowledge and action, theory and practice; the Lack of action-research is its failure to carry through this reformulation. At various points in this study - especially in the final section of each chapter - aspects of this reformulation have been presented. In this concluding chapter I wish only to draw together an overall statement concerning the potential contribution of action-research (in the reflexive and dialectical formulation I have put forward) to social inquiry as a

general project.   It is a conclusion only in a dialectical
sense: it draws together the phase of study into a
moment of reflection which anticipates, now, an explorative
and ultimately transformative continuation, through
attempts at exemplification in particular contexts.
"Conclusion", otherwise, would threaten to overwhelm
one of the central themes of the study with the unaddressed
irony of its implication of finality, and hence of
theory's claims to prescribe methods for practice.
It is to the danger of that irony that the following
section is addressed.

Action-research, Factuality, Meta-theory

At the centre of action-research stands its hyphen:
it proposes an axiomatic and inescapable relation between
action (which must treat knowledge as adequate) and
research (which must treat knowledge as problematic).
Yet the clumsiness of the phrase "action-research",
as a mere juxtaposition - with or without a hyphen -
expresses the irremediable problem of the relationship.
Unlike other expressions (such as "applied science"
or "theoretical practice")which have their syntactical
point of rest in one term or other, "action-research"
merely vibrates with its own irony, its unresolved
difference, and hence its interminable internal question:
the dialectic of action-research-action-research-action ...
can begin anywhere, and once begun, it is without ending,
since it is without prescription, and thus without a
principle for completion.

It is through this unending quality of its dialectic that action-research provides for the irremediably problematic combination of irony and responsibility which characterizes theorists' relation to their social context, a relation which — in contrast — orthodox social science would wish to formulate either as non-ironic (leaving the socia scientist as an employee of one sub-section or other of an institutiona order which provides both topics and purposes) or as non-responsible (leaving social scientists proclaiming both their free-floating abstrac from all social interests and their potential availability to any such interest.)

Now, action-research's principled commitment to both irony and responsibility enables it to grasp the problematic status of "theory" in relation to the "action" which constitutes theory's social world. In a crucial sense, there is no action which is not informed by theory, and this applies in a broadly similar fashion to the following series: a racist street brawl, police arrests of some but not all protagonists, a government inquiry into urban law and order, a survey of attitudes carried out by the Commission for Racial Equality, and a study of the ideological bases of government inquiries. In each case "theory" takes the form of factual knowledge generalized as a justificatory principle for a range of envisaged action. In this way, knowledge is continuously being socially constructed within the technical / moral debates (concerning capitalism, industrialization, urbanization, etc., etc.) which constitute everyday culture as a pattern of discursive agendas. These agendas only exist because their

relevance is relatively urgent, and thus the theory
with which they are informed tends to be presented
in a rhetorically persuasive, maximally plausible form -
namely as "facts". However, the irony of factuality
is that, as a practical rhetorical format, it is utilized
by all competing interest groups, with the result that,
although all claims to factuality are made in the name
of an objective validity that aims to silence opposition,
all such claims are immediately contested as distorted
selections. Hence the typical format for social
science knowledge has become the "highly-significant-
statistical-finding", presented in a prestigious journal
with all the mythic trappings of mathematical absolutism,
only to be dismissed in the next issue as a random
illusion created by some-one's crass technical blunder.

The general form of the problem, of course, is
that "theoretical knowledge", here, is differentiated
from "common-sense" only by the capital- and/or
labour-intensiveness of its resourcing, by its <u>technical</u>
but <u>not</u> its <u>epistemological</u> sophistication. Hence,
positivist social science can only relate "theory"
to "practice" by constructing a factual claim for the
authority of a particular interpretation - and thus of
the practical policies which require this interpretation -
even though this factual authority is always liable to
be challenged, so that "theoretical" debates either merely
<u>reflect</u> debates within practice itself, or they are
concerned with methodological technique. Clearly,
the sequence of cognitive claim and challenge within

these technical / moral debates constitutes one of the
basic processes of politics, culture, and the daily
life of institutions and professions; but this is to
characterize such debates as, precisely, topics of
the deepest sociological interest, whose nature it
must be sociology's task to address. And here the
merit of a reflexive action-research begins to emerge.
For it is clear that the political clash of rival
factualities and their attendant theoretical generalities,
will not be addressed (but will, rather, be taken as
given, sustained, and prolonged) by a social science
which invokes a methodological warrant for its own
factualities, its own theories-as-generalizations.
Instead, what a reflexive action-research would offer
to its action context is not "theory" in this sense at
all (since there is, as it were, enough theory there
already) but rather what might loosely be called
"Meta-theory".

"Meta-theory" conventionally means, in some sense,
"the theory of theory", but this is potentially highly
misleading, and we must therefore carefully ask: in
what sense? Firstly, if (as has been argued above)
"theory" conventionally takes the form of persuasively
organized factual grounds for interpretation (and
thereby loses its differentiation from the practical
rhetorics of everyday life), "meta-theory" can be taken
as: "persuasively organized factual grounds for the
persuasive organization of factual grounds" for inter-
pretation. In other words, meta-theory would become

the prescriptive elaboration of absolute techniques
for the achievement of "objectivity", that combination of
logic, statistics, and reductive behaviourism which
is to be found (variously disguised) in innumerable
text-books on "methodology". Meta-theory in this
version makes a merely technical contribution to the
presentation of interpretive theory, just as the latter
is hardly distinguishable from a "jargon" for the stylistic
re-articulation of social actors' common-sense. At
both levels, then, the emphasis on techniques for the
creation of persuasive factuality prevents a "critical"
or independent formulation of theory's task in relation
to social purposes and categories. Secondly, the
conventional sense of "the theory of theory" suggests
a double move away from practice, whereas the specific
contribution of both the principled recognition of
reflexivity and of even conventional action-research
is to reassert the mutual dependence of theory and
practice. Action-research as meta-theory proposes,
then, as a first step, to subject the factualities and
organizing conceptions of specific bodies of professional
expertise to the critical recognition of their located-ness
within the practices whose intelligibility they serve.
Action-research thus proposes a move "beyond" a form
of theory which prescribes and justifies a basis for
action, not to be a more rarified theory, a more abstract
prescription or justification, but to a reflexive
dialectic between theory and action, which sustains
their mutuality while transforming both.

It is in this way that a reflexive action-research is essentially and inevitably committed both to the theoretical critique of action's taken-for-granted rationalities and to the continued possibilities of action in its mundane context; it can be content neither with providing a mere technical service (of refining the factual basis of expertise) which leaves action uninterrupted, nor with providing a mere theoretical ideal or model which (by threatening to interrupt action's mundane process for ever) must necessarily, in the end, be ignored by practitioners. In other words, reflexive action-research is proposed here as a way of reconciling that central dilemma of social inquiry: its inherent tension between theory and practice, between "critique" and "relevance". In the following (final) section this general contention is illustrated by reference to the general principles by which its epistemology is embodied in its process.

Principles for a Reflexive Action-Research

A)   Action-research is grounded in the topics of professional expertise, but also has grounds for transforming them. (Action-research will not simply organize and report members' topics and categories; neither will it encapsulate them as merely illustrative of prior theoretical systems). Action-research will begin by recording the cognitive resources which underpin the invocation of professional expertise in the specific context - its particular range of plausibly warranted

"factual" bases, and its particular set of concepts

which provide for the intelligibility of those "facts"

in relation to the requirements of action in that

context, ie. concrete versions of normality, predictability,

event, motive, chance, relevance, etc. Action-research

will then make explicit the essentially reflexive

basis of this expertise, its grounds in contextually

specific judgements, rather than in general laws.

By drawing attention to the process and structure of

these judgements, action-research transforms the assumed

resources of expertise into topics, and transforms

received topics into questions. By drawing attention

to the contextual basis for claims to generality,

expertise's "necessity" is transformed into contingency,

and "irrelevancies" are transformed into thinkable

possibilities.

B) Action-research is grounded in the phenomena of

practical experience, but also has grounds for trans-

forming them. (Action-research will not simply treat

members' meanings as criteria for its own adequacy;

neither will it treat members' meanings as merely

epiphenomena produced by supposedly objective societal

processes). Action-research will begin by recording

members' experiential accounts of the context - as sets

of relatively unified phenomena organized into relatively

fixed relations of similarity and difference.

Action-research will then explore the dialectical basis

of these phenomena, exploring the differences which

have been collected as "similar", the similarities

which have been set apart as "different", and the

historical (and "futurological") dimensions within which current categories of similarity and difference may be seen as both contingent and transient. By drawing attention to the developing contradictions within the categories of experience, implicit necessities (as labels of the present) will be transformed into explicit possibilities (as metaphors for thinkable futures).

C) Action-research's resources are personal, but its transformative outcomes have valid grounds. (Action-research will not simply claim to discover objective empirical generalizations; neither will its outcomes merely be expressions of personal opinion). Action-researchers are constitutive elements of their contexts-in-question. When they begin to subject contexts to a principled reflexive and dialectical critique (see A and B above), they are required by those same principles to initiate and/or accept a reflexive and dialectical critique of their own resources which have provided for the original critique. Clearly this is to embrace a form of potential "infinite regress" which would strike terror into the hearts of logical positivists; but action-research, in contrast, must accept and require the recognition of this risk as the ontological and epistemological structure of its (reflexive and dialectical) intersubjectivity. Two consequences are important. Firstly, by means of the inter-personal process of action-research, subjective

commitments to interpretation ("points-of-view") will be deconstructed by a review of possible resources for such commitments, ie. the pragmatic and rhetorical unity of subjectivity will be required to recollect its fundamental potential for disunity, and hence its resources for alternative commitments. Secondly, and consequently, subsequent commitemnts to interpretation (re-made in the light of such recollections) will recognize the limits of their specific personal and contextual resources as part of a provisional, reviewable, interpersonal, and contextual strategy. Reflexive action-research does not seek to replace personal resources with "im-personal" techniques or "universal" theories, but rather to push to their here-and-now limits the inherent resources of interpersonal contextualized understanding.

D)   Action-researchers recognize that they will suffer the transformations of the processes they initiate. Action-researchers' interactions with members will not simply provide prescriptions for action; neither will they merely result in interpretive insights which members can take or leave). Action-researchers will require from themselves, as well as from others, a recognition of the reflexive and contextual limits of their interpretive judgements, and the dialectical contradictions of their activities as action-researchers (see C above). Only insofar as they enact their requirements upon their own activities will they be able to persuade practitioners that these requirements

are indeed requirements for all, including practitioners,
rather than special requirements typically inflicted
upon others by, say, "theorists", and thus carrying no
persuasive import (but only a rejectable instruction)
for those who are "practitioners-but-NOT-theorists".
Hence, whatever understandings action-researchers bring
to a context (in terms of methods, interpretive theories,
and anticipations of the processes of action-research
itself) will - they know - be transformed by their
enactment first in this and then in that context. In
this way, reflexive action-research will not be a
version of "applied science" (as though procedures
for social inquiry had been created "somewhere else",
so that action ("here", could simply learn from science)
but a formulation of social inquiry's own capacity to
develop ("everywhere") as a dimension of social
inquiry's constitutive relation with its social world.

E)   Action-research transforms the relationship
between the disparate elements in an action context, but
it does not attempt to construct them into a unity.
(Action-research will not simply attempt to negotiate
a concensus in order to supercede contradictions;
neither will it merely record contradictions as they
present themselves).   Given that action-research's
frames of reference will be challenged by its own
process, and are thus part of an unending development
(see D above), the format for reporting action-research's
outcomes will not be an integrated descriptive account,
presenting a reader with a specific state-of-affairs, but,

rather, for example, a sort of "collage-text" which artfully sets in play a dialectical (and hence always potentially ironic) pattern of relationships between alternating elements, authored by various members of the action context, including explicitly reflexive accounts (see A above), dialectical analyses (see B above), reviews of possibilities and of grounds for commitments (see C above), and diaries of action. A key element in this "plural text" will be a series of contributions which attempt to make explicit the structure of the relationships enacted in the sequence as a whole. Such an "open" text will express both the contradictions of its origin and its non-prescriptive availability to its varied audience for their varied and unpredictable purposes and responses (including responsive action). Action-research will thus be able to turn to its own advantage the inescapable and fundamental tensions in which it is constituted (between theory and action, between the valid and the concrete) by learning from and drawing upon those rich traditions of ironic and/or reflexive symbolization (narrative, drama, myth, rhetoric, counterpoint, and aesthetic criticism) which are so much more securely grounded in the structures of consciousness than the recent but politically prestigious (and hence understandably seductive) procedures of positivist social science.

F)   Action-research has grounds for the critique of action, but these grounds require also action-research's commitment to the (transformative) continuation of action.

(Action-researchers will not simply be observers, who can arrive with a repertoire of skills, re-describe the action scene, and depart unscathed and unimplicated; neither will they be participant members, who learn merely by taking part in the implementation of practical change). Action-research's moment of critique (see A and B above) assembles and expands a range of previously "repressed" possibilities (see C above). But these possibilities are derived from action's own cognitive resources, and will not be left merely as possibilities "in theory" (as realizable in, say, an ideal world but not here-and-now) since action-research's dialectic requires that the possibilities created by critique be confronted by the requirements of action, always given that action's limits will have been transformed by the exploration of its possibilities. It is this double confrontation (the question posed to practice by theory, and the question posed theory by practice) which ensures that no-one will escape the transformations of the action-research process (see D above), a process whose dialectic disqualifies claims either to be an observer who can leave to others any responsibility for the continuation of action, or to be a practitioner who can leave to others any responsibility for originating the transformation of action. Action-research formulates action as inescapably responsible to the grounds for its transformability by theorizing, and theory as grounded in its responsibility for action's transformed necessity. "Action-research" thus

expresses a double responsibility as well as a double irony.

The prevous principles (A - E) formulate the nature of action-research's inherent resources and processes; this final principle refers most directly to action-research's fundamental capacity for structuring (however delicately, ironically, provisionally, and non-prescriptively) that crucial interplay between theory and practice, critique and responsibility, ideal and actual, Reason and politics, which constitutes the central problematic of social inquiry.

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