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 psychoanalytic 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 "naturalistic theory" and "illumination", and formulating validity in terms of reflexivity and dialectics. In this context, also, action-research's 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 foregoing arguments in order to present six critical propositions, as a set of implications for the renewed practice of action-research.
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**Action-Research and the Problem of Validity**

Versions of "Validity":
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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 - 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 professional 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 generally valid, such that clients' experiences can be conceived as predictable specific instances of prior generalities, i.e. 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 not 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 problematic 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 their 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 challenges a scientific method of inquiry based on the authority of the "outside" observer and the "independent" experimenter, and it claims 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 muddled science); and practitioners are scornful of action-research's claims to feasibility (action-research undermines 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 transcendental 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 Research

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 effectiveness (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 (i.e., historical) matrix. Theme C poses the problematic relation between theoretic and institutionalized 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, i.e. 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 understood 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 intelligibility 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,

* 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 particular 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 glosses (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 interesting 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 denied 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-ificality 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.

* 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 irreversibly particularized and endlessly problematic. This is because the theorist requires 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 between 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 complete 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 corollary 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) emphasizes 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 accomplishment 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 development 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-consciousness 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" experience, 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-condition of presence". (ibid., p. xxxvi), ie. objects are present to consciousness not 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 prolongation 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. 13 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", i.e. 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 (i.e. "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, oscillating 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 and yet unified (i.e., 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 self-consciousness, 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 temporal 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", i.e. 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, and 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 after the main text, it attempts to show the possibility 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 (Another 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 question 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 intelligibility 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 objectively 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 \textit{intelligibility} for theorizing as a project: "possibilities" do not remain "an open horizon"; they are culturally located as a set of \textit{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. 1 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 possibilities and limits which action-research itself invokes, and proceeds to assemble and explore the further resources which action-research's self-proclaimed aspirations 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 problematic of consensus 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 privilege 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 situations" (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 authority of a research stance be distinguished (in its relation to action) from the institutionalized power 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 problem? And there is a further question: Nixon's "radical alternative" is exemplified largely by cases where practitioners' research efforts take the form of self-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 answers ("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 conceived as meshed (however loosely) into a social system, whereas research is the process whereby the self-perpetuating 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 relationship 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, development, 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, & acceptable coherent. In what sense could these procedures be identified as not "systematic"? Could it perhaps be suggested that action is systematic in its improvisation of methods in a particular 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 claim 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 control, 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 (i.e., 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 not (as Brian Fay would have us believe) an effectively controlled society, but a
democratically open, progressive society, based on the possibility of effective evaluation, i.e. 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 contradictions 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 restrictiveness 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, i.e. 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 attempting 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 scientific 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 (Open Society and its Enemies) ... 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

* 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 judgements 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 conjectures 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 "refutation". Action-research's hope that Popper could provide a **unified** 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 the 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 rationalities 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 science (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,

* 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 unite 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 following Garfinkel (by asserting clearly the difference 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) **Choice** - 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 **normative judgement**, (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 not 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 norms 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 separation of the two lists, such that the norms for daily life may not 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 norms: the thrust of Garfinkel's work is that norms are related to specific instances by means of procedures for the construction of intelligibility ("the interchangability of standpoints", "ad hoc", "etc.", etc.) which again common-sense takes as an available resource but "science" must treat as its topic. And this is where science must cease to be the authoritative revelation of "the truth" about common-sense. For in the same moment as it topicalizes the interpretive procedures by which common-sense invokes its norms of rationality, science utilizes 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 theoretical 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 all been glossed but some of which could, now, be formulated as 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, anticipates 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 action. 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 unfinished 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 chooses to finish, and in that choice has to rely once more on the pragmatic rationalities which theory shares 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 strategies and meanings were selected, action thereby acquires resources for conducting a practical 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 not
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 questioning (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; i.e. 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 → Action → Observation → 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 → Diagnosis → Planning → Implementation → 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 investigator 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 vaqueness. 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-transcendental theorizing, and a formulation of culture which will provide for the limits of theorizing without falling into determinism; 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 unproblematic 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, sometimes 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, elsewhere (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 theorizing, 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 framework for self-evaluation 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 intends 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 presupposition, 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 new 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), i.e. to deconstruct the temporal dimension of language and experience, so that (to complete an argument that Elliott merely hints at) their elements and relationships 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 relationships 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, i.e. 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 help 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 institutionalized 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 bureaucracy" (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 relationships, i.e. by substituting "participation" for superordination (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 *Legitimation Crisis*, Habermas, 1976). For Elliott action-research is 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 determinist 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: all 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 non-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 institutionalized 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

* "im konkreten gesellschaftlichen Situationen ...
welche Kluft zwischen Anspruch und 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 determined 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 destinies 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 unreflexively, as

* "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 Schicksal 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' understanding can only be emancipated from subjection to institutionalized 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 carrying 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 consensus 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 interaction, 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 an 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 "collaboration" 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", i.e. 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
tells, who prescribes principles and defines clarity, who possesses (and decides when to provide) "a rationale". Rather than the analytic 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 social 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 fragility 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 introduction 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 development" 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 relationship 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 nature 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 intersections 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 transcend 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 The Interpretation of Dreams, 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" (Jokes and their Relation to the Unconscious, Freud, 1976b, pp. 254-6)*

* From an analytical point of view, one would wish to see jokes as sudden reflexive twists, revealing both limitations of their own conditions and 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 vocabulary of images, derived from "folklore, popular myths, legends, linguistic idioms, proverbial wisdom, and current jokes" (1976a, p. 468); and there is a syntactical process 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 post facto checking 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 The Act of Creation (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 and 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 respond, but to transform its context from an experiential given into a range of symbolic possibilities. The unconscious, in this view, adds to the logical constituents of mind, posited by Kant, both desire and metaphor, and thereby formulates for creative consciousness both motives and pathways.

Jung formulates this view of the unconscious as a resource quite directly:

"The same psychic material which is the stuff of psychosis is the fund of unconscious images, which fatally confuse the mental patient but ... is also the matrix of a mythopoeic imagination which has vanished from our rational age".

(Jung, 1967, p. 213 - my emphases)

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 opposities 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 collect archetypes as a "fund" or dictionary of "real" meanings prescribed with the authority of a universal unconscious. But Jung's emphasis is both 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-understanding, 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 relationship 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-representations 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 language as a further constitutive structure of subjectivity, but language neither as a determining system of concepts and rules, nor as an available mechanism for unconstrained 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, e.g. 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, i.e. 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 ...

"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-dimensionality" 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. "Differance 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 dialectic (rather than in the a-temporal moment, of individual perception or intuition - see Heidegger's critique of Kant in Being and Time (Heidegger, 1962, p. 410). Derrida's "Différence/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 writing. (See Derrida, 1976, p. 9). Through the

* 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" (Derrida, 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, i.e. 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).

* "ma tendance théoricien" (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 determined by the state, whereas in Althusser's writing the state, precisely, is his object. And yet he cannot not be a subject, because all 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 "logocentric" 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 *Eléments d'autocritique*, 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):

* "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. 193) 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

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

** "La dialectique très particulière qui est à l'oeuvre dans la pratique de la philosophie".
contested 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 its 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 representation 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 critique (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 prescription 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 within 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 intellectual 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 exciting story of their struggle. Myth justifies
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 possibility but also the fragility of self-knowledge on the part

* 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 political 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 industrialized 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 industrial 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 "mythopoetic 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 "mythopoetic 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 direct 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 of 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 theorizing, 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 ways 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), i.e. inquiry is forced to become inherently reflexive. Hence:

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

But in this passage we have a statement of the basic ambiguity at the level which Freud did not address: the accomplishment is the patient's, but the terms of that accomplishment are "suggested" by one in the role of "physician"; the accomplishment is thus the acceptance 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 scientific 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 his 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"
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 not 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 commitment 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 at risk; 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 resistance of patients (Freud's version of reflexivity) but therapists' lack of resistance to their own sense of need. Jung thus introduces the notion of a "counter-transference", 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

* 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-of-
action, but a meta-theory - a theory, that is, of the
relationship between theory and 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, i.e. within an analytically reflexive dialectic between action and theory, then action-research's epistemology will be engulfed by positivism, and action-research's investigative 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 symmetrical 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 intersubjectivity" can there be a discourse about

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 enlightenment, 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 unreflexive 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 resistance 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 relationship 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 intersubjectivity 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 otherwise, 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 lead
(in an open-ended sense) to an improved understanding
of the situation; on the other hand such changes
are presented as "strategic", i.e. as planned with a
clearly envisaged end in view, so that improvement in
both understanding and in practice will be induced,
as though the range of outcomes were predictable.
Hence the direct question becomes: will any 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 not 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 practitioners, 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 vs. Action-research

<table>
<thead>
<tr>
<th>The Institution</th>
<th>Action-research</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hierarchical; vertical relationships</td>
<td>Horizontal relationships</td>
</tr>
<tr>
<td>Divided; compartmentalized (subject-based)</td>
<td>Unified; collaborative across subject boundaries</td>
</tr>
<tr>
<td>Bureaucratic; &quot;Top-down&quot; management style</td>
<td>Democratic; &quot;Bottom-up&quot; management style</td>
</tr>
<tr>
<td>Closed</td>
<td>Open</td>
</tr>
<tr>
<td>Formal (teacher-pupil relationships)</td>
<td>Informal</td>
</tr>
</tbody>
</table>

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 Gesellschaftswissenschaft 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 practice, between research and professional work within 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, i.e. 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 Humanities Curriculum Project, 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. 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 suggestive 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 activity 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 classroom 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 enterprise; 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 questionable 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 observe "higher" and "lower" orders of conceptualization represents an unreflexive prescriptiveness 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 situation 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 recommendations 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 relevant as rational features of a situation which children will inevitably be formulating for themselves. Classroom practices are constituted in a mutual difference between teacher and taught which action-research aspires to address as a problematic inter-
subjectivity between "reflective Subjects": if educational practice is conceived as the setting of a task and as a requirement of the answer, this Difference will be glossed by an authoritarian imposition, presented nevertheless as that consensus 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".  

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 (i.e. "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 institutionalized 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 within 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 challenge to its institutional context, the general dimension on which professional improvement is sought, but it fails to formulate action-research's
possibility except as a challenge, i.e. 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 science 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 bureaucratic form: functional specificity, systematic disinterest, and the universalistic rules.* The enormous

* 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.
mythic 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 hero, who reveals the objective error of other 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) and 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, i.e. the autonomy which comes from possessing a moral as well as a technical jurisdiction. Since professional practitioners are also members of more or less bureaucratized 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 successfully 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 "bureaucracy" 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 undermines action-research's aspiration to be a form of investigation which can unite 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 reciprocally 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 both emancipated (from any constraint other than its own constitutive features ("Reason")) and 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".


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 following among the partners of communication".


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) of is that the increasing dependence 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 unavoidably suppose an ideal speech situation that, on the strength of its formal properties, allows consensus 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 normative 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 lapses 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)**

* "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 concerning 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 bureaucracticized roles, for instance - and thus it is to an analytical account of professionalism and its relation to bureaucracy that the argument now turns.

Professional and Bureaucratic Practices: Dialectical Possibilities

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 two "opposites". Firstly, professional work is not "trade": 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 servant of the client's interests; their interaction is

* 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 unreflective 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, i.e. 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 intelligibility) 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 exhaust 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 historically 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 not "mechanized": rather, they always require a specific confidence in their capacity for improvising 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 individualization 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 jurisdictions 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 interpretation.

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 interpretation 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 interaction, not a hierarchy of prescriptions. How ironic, then, that Peter Holly, in his diagrammatic representation 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 all institutions were simultaneously 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 action strategy, rather than an alternative, non-positivist research 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, 1975b, 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 Journal of Curriculum Studies (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 methodical 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", i.e. 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. 108-9) 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 investigative 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 correspondential conception of knowledge. If "validity" resides in the "complexity" of the factors influencing a situation, i.e. 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 undermines their own project of "description". Elliott himself (see the quotation on pp.18-9, 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 dialectical understanding (see chapter one, p.28) which would make any project for the exhaustive 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 intersubjective 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 understand" (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. 97). However, whereas this intersubjective and generalizing property of the symbol is for Stake a methodical resource, 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 superficially 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 characters
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 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 intersubjectivity, 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", i.e. 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 analyzing 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, i.e. the "conditions of its possibility", (see Chapter One, p. 9). "Bias" is thus neither glossed nor abolished but rather confronted, through an analytically

* 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, i.e. 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 knowledge; 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
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 adopt 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 for 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 all perspectives; it is a form of question which attempts to speak for not against its interlocutor (a formulation conventionally espoused within "counselling" for example). It creates a theoretic space by means of a general 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 valid 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 trigonometry? 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 should 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 kinds 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,

* See also Whitehead and Foster (1984) p. 43.
the situation creates a hierarchy of power and status between these roles, hence, some 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 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 between them. In other words, the analysis had to gain acceptance as "objective", evoking the main areas of tension in the situation 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 creating an account of a situation which would be seen by a variety of others 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 imposed 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 "contradiction", 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 convoluted; that the individual's conceptualization is systematically ambivalent or dislocated; that 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. 10 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

* "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 investigators 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", i.e. 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.

* 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 reflexive 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 sign, leaving the larger claims of the logically constructed concept 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 future 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 select 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 know 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 aesthetic 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 (i.e. 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, i.e., 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 practitioners 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 principled recognition). The general thesis that the essence of a modernist aesthetic is its reflexivity is the theme of Gabriel Josipovici: The World and the Book: "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 in general (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 tradition 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, embraced) a role which casts them simultaneously as hero and as victim, "scientists" persist in attempting to evade any destiny whatever, by seeking a role of principled invisibility through an ideology 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 themselves, 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 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 previously 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, metaphors, 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.10f-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 underlying pattern of metaphor and dialectic, i.e. 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 reflexive description can only seek validity through a structure which embodies a principled recognition of the problematics of its own possibility (see references to Barthes, on p. 256 above).

2) The scope 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 structure of each of the problematic relationships noted in 2, is dialectical, as follows:
   a) Symbolic resources 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 between these elements 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 between the description and such resources 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 irony rather than of resolution and negotiated consensus. 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.155 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 - without 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 continuation (ie. amendment and critique), since description will have a dialectical relation with its audience.

* In mundane examples of such reportage, this "abstaining" stance is, of course, merely a carefully presented illusion: it is the textual structure which is being considered at this point in my argument.
b) The audience 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 specific audience, one which shares a particular standpoint or set of relevancies, and may achieve a persuasive validity for that limited audience, while other audiences - with whom the description in question does not 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 varied audience, i.e. a description which structures a dialectical relation between a wide variety of its own heterogeneous elements, and thereby achieves persuasiveness for audiences which begin their reading of the description from a position of provisional identification with only a limited range of those elements. Hence the persuasive task of description can be seen as establishing, through its own processes, that the heterogeneity of phenomena does indeed represent
a dialectical (i.e. 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.210 and p.239 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.

* In principle, much of the argument of this section could be applied to descriptions embodied as painting, film, ballet, music, marble, or papier-maché, 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 not engaged in between people who are both 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 particular, of description's always ambiguous ambition to be 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 "recognition" of its dialectical ambiguities and limits? In general terms we may remember once more Gadamer's axiom that "the structure of the hermeneutical 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
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 unreflective 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 all 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 consensus 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 within the text itself of a "critical" commentary, 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 open, "plural" structures, (Barthes, 1977, p. 159) intelligible only in the light of the reflexivity of language and the constitutive dialectic between writer and reader.

* 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. 255 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. 253 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.
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 social scientist as an employee of one sub-section or other of an institutional 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 someone'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 technical but not its epistemological 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 reflect 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 interpretation. 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, i.e. 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 transforming 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, i.e. 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 commitments 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 consensus 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 previous 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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