THE REPERTORY GRID : A Critical Appraisal

Volume 2

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13 Excursus

To sail beyond the sunset...

It is not grid method as such, which is important, it is the exploration of personal constructs.

D. Bannister and J.M.M. Mair
1968: 217

13.1 INTRODUCTION

The critique that I have slowly built up regarding the repertory grid (and, to a lesser extent, Personal Construct Theory) strongly suggests the need for a reappraisal of the way in which research within the Kellian tradition is both conceptualised and undertaken. If the more radical aspects of the critique are construed as valid, then the need for reappraisal becomes urgent.

A full reappraisal lies beyond the scope of the present work, but I hope in this chapter to give a clear indication of some of the main issues involved. It has to be said at this point that the argument which follows is limited to those aspects of the human sciences which are bounded by methodological individualism, since this captures in practical terms the recovery of individuals as individuals[1] through the medium of personal constructs. The question of generalisation is touched on at the end of the chapter, but the broader reaches of sociology lie outside its terms of reference (which is not to imply that aspects of my argument would be irrelevant: here I am merely recognising the differences in levels of conceptualisation and analysis).

A characteristic of the human sciences that distinguishes them from the physical sciences is their comparative looseness of theoretical articulation. This looseness derives from the construing of the subjects of research, which introduces a second-level set of variables into the proceedings. Predictions in the human sciences are thus at the mercy of many more uncontrolled variables than in the case with the physical sciences.[2] My exploration of these issues in Section 13.2 leads me towards concluding first that the centre of focus of the human sciences is the understanding of events and of the way in which first-and second-level variables interact and, second, that paradigms based upon the

physical sciences cannot address this type of problem.

Despite all Kelly's physicalist rhetoric about prediction and control, 'The Psychology of Personal Constructs' bears substantial witness to an alternative perspective, that of the historian. To read Kelly whilst wearing the spectacles of the historian is to throw into sharp relief his own role as an interpreter and explainer of the psychological problems with which he is confronted. There is surprisingly little of 'Kelly the Scientist' (despite his claimed theoretical stance), and rather a lot of 'Kelly the historian' in his writing. I pick out as an example - there are many others in his two volumes - Chapter 7, entitled 'The analysis of self-characterization', which is a sustained exercise in hermeneutics. Even his view of testing is dominated by the need to reveal as much as possible of his clients' construing and psychological resources. 'Reveal' here implies not only the elicitation of constructs but also the interpretive activity of making connections, and Kelly largely leaves the existential matters of prediction and control to his clients.

This chapter, then, is based upon a switch of metaphor from the scientist to the historian, and reflects the crucial importance of understanding in both explanation and theory building. This shift in perspective does not imply a claim that people are any more accurate as historians than as scientists, for many of the objections to the ' commonsense scientist' outlined in Chapter 2 can be carried over to the 'commonsense historian'. Rather, it underpins my discussion of the conduct of research in the human sciences and the world view under which this might be subsumed.

Following a brief consideration of the weaknesses of paradigms from the

physical sciences (as far as human activities are concerned), I consider the nature of hermeneutics. Some space has to be given to the semantics of this term in view of the number of changes in its meaning over time. This leads to a discussion (starting from a phenomenological perspective, but ultimately going beyond it) of a critical issue for methodological individualism - that of how a researcher comes to understand what a respondent is conveying.

Having dealt with these theoretical matters, I return to my main theme of practical methodology and its superordinate question: how, given all the problems of distortion associated with personal accounts of experience, can research in the Kellian tradition be made maximally systematic and rigorous without formalisation destroying what a respondent might wish to say?[3] In this longest section of the chapter I attempt to establish how research conceived from a hermeneutic perspective can be operationalised in a manner that remains consistent with the philosophical assumptions of the underlying theory.

13.2 THE POVERTY OF PHYSICALISM

If a block of ice is taken from a refrigerator and placed in a warm room, one would predict that it would melt. The prediction, for a scientist, would be informed by a theoretical framework embodying thermodynamic and kinetic considerations coupled with the weight of past experience. The prediction may be expressed in general terms:

> Assuming a set of theoretical statements T, and given initial conditions W, an entity X will act in an observable manner Z.

Accepting for the moment the structural equivalence between prediction and explanation [4], the change W; X→Z can only be explained with reference to T, even though the generalisation of experience alone may

be sufficient for prediction. In these circumstances, the relationship between W and Z is independent of which particular instance of X is taken.

Not every observable event in the physical sciences is as clear-cut [5]. The decay of radioactive atoms and the formation of alternative products in organic chemical reactions can only be expressed in probabilistic terms: W, X and Z can be specified with fair precision, but T may be insufficient to provide an adequate explanation of why one atom may decay before another or why a particular proportionation of products is observed. Data here is underdetermined by theory: further predictions are possible on the grounds of experience but not in terms of theory. The situation at root is a sophisticated parallel of the Babylonians' predicting of tides.

There is a long tradition, extending back beyond the beginnings of behaviourism, of attempts to constitute psychology as an experimental science. Experimentation in psychology seeks to reduce the complexity of the test environment to manageable proportions in order to eliminate extraneous influences on the theoretical relationship being investigated. The Skinnerian experiments on operant conditioning, for instance, investigated the relationship between reward and behaviour. The subjects - here pigeons - are treated as unthinking entities whose behaviour is a function of the conditions controlled by the experimenter. The results are persuasive: conditioning works. Pigeons learn behaviours. Theory begins its transmutation into law[6].

However, when animals constitute the set of entities denoted by X, a variation in performance creeps into the results by virtue of differences

in capacity to perform the task. Learning curves are not identical even though they might exhibit the same structural characteristics. The relationship between W and Z is to some extent influenced by the particular entity X used in the experiment. This leads one to consider the extension of experimentation to human subjects, which is more germane to the present purpose.

Whilst there is now a wide range of sophisticated experimental designs available to the researcher, their basic form is to subject one group to an experimental treatment whilst another acts as a control[7]. The outcomes are measured, and the researcher assesses the extent to which the hypotheses have been supported, the findings typically being presented in terms of means and deviations from the means. The relationship between W and Z is rendered problematic to an extent dependent upon the 'individual difference' deviations observed.

It is not too large a conceptual jump to see the subjects of such experiments in the role of Skinner's pigeons, in that often no attention is paid to any constructions they might have regarding the experimental situation. As Giddens (1976)observes, in a positivistic theoretical framework self-consciousness is a nuisance - he might have said'error variance' - to be minimised.

But the problem of self-consciousness resolutely refuses to go away[9] and, for roughly as long as experimentation has been used in psychology, there has been a persistent critique of the inadequacies of pseudo-scientific experimentalism. Dewey (1916 b), for example, cautioned against a false objectivity which ignored the role of 'personal habits and interests'[10], and echoes of his warning can be found in the tradition of criticism of experimental methodology[11]. The averaging of results

for the purposes of generalisation has been challenged on both conceptual and empirical grounds, the early empirical findings of Sidman (1952) and Bakan (1954) on learning curves [12] being paralleled more recently in repertory grid work by Beail (1983)[13], Stringer (1972) and Keen and Bell (1981) - though it has to be pointed out that in the last two of these papers the difficulties are not acknowledged despite the manifest variations in the data reported[14].

Although an enormous amount has been glossed over and sweeping simplifications have been made in this criticism of experimental method in psychology there are already sufficient grounds for considering an alternative approach. A similar critique could have been mounted in respect of a positivistic sociology in which broad trends can be identified without the acknowledgement of the individuals' personal constructions of their realities [15], but this would involve a detour of little ultimate benefit to my purpose and is hence not undertaken here.

What, then, happens to the generalised predictive statement used above when people are the subjects of research in the human sciences, and what are the implications of the necessary changes in formulation? The amended predictive statement may be expressed in general terms as

> Assuming a set of theoretical statements T, and given initial conditions W, a person X (construing W with reference to his or her construct system Y) will act in an observable manner Z.

In some circumstances the involvement of the person's construct system Y (taken here as predominantly cognitive) will be of little importance - aspects of physiological psychology, for instance, may be comfortably accommodated within "normal" scientific practice. However, in many

circumstances Y is highly likely to mediate the relationship between W and Z: W;X(Y) → Z cannot be predicted with the same confidence as the melting of a block of ice. Further, the theoretical statements T lack precision since they depend for their existence on experiences with people whose construct systems will have introduced a variance not possible with the thoughtless physical objects of science. The uncertainty associated with T and Y create problems for explanation. Z might be observed despite errors in the assumptions regarding Y and/or T, and when Z fails to occur it is not clear whether the 'fault' lies in T, in Y or in both.

The looseness of T in the domain of the human sciences allows the possibility of inclusion of statements which imply contradictory outcomes. There is ample evidence from the Industrial Revolution onwards that people migrate to where employment prospects are better, but against this can be set the cohesiveness of communities and a reluctance to move from one's roots. Predictions regarding the mobility of, say, a Welsh miner whose pit has been closed would not be easy on 'detached' sociological grounds, but might be less problematic given some knowledge of his world-view. Similarly, until one knows something of the nature of Y one is unable to explain Z in terms of T (one can speculate upon the reasons for Z without knowing about Y, but the complexity of T is likely to overdetermine Z and thus limit the confidence with which the speculation could be advanced). In 'The confidential clerk' T.S. Eliot,

through the voice of Colby Simpkins, captures the difficulties of prediction where there is a multiplex of determinants, many of which are unknown.

> 'I meant, there's no end to understanding a person.
> All one can do is to understand them better,
> To keep up with them; so that as the other changes
> You can understand the change as soon as it happens
> Though you couldn't have predicted it'. 16

In her clinical studies of depression Rowe (1978) came to realise that predictions of individuals was impossibly complex and that the most she could do was 'to recognize the change and so behave in what [she] hoped was an appropriate way'[17] Put another way, post hoc analysis and interpretation in subjectively probabilistic terms were the necessary preconditions for Rowe's later actions.

The discussion so far has ignored possible variations in W and X. People differ in their skills and capacities for action (social or otherwise), and this will influence what is observed during the research. In the interests of generalisation these differences are often either ignored or are incorporated into a research design employing some form of covariance analysis. Whilst the latter is preferable, it may not be satisfactory at the conceptual level to use some kind of general skill index as an independent variable in a range of contexts which may be superficially similar but in which the skills actually demanded of individuals are markedly different. As a crude illustration of the point one might find two teachers who were differentially able to explain a concept in a microteaching laboratory, but on testing their ability in nominally similar classrooms social skill could swamp the difference in 'index of explanatory capacity' and be the major determinant of a dependent variable based upon pupil learning[18].

The example brings in the question of initial conditions W. Unless these are tightly controlle , variations will be potential contaminants of research findings. Much work in the human sciences which is conducted in natural settings has assumed a homogeneity of conditions[19] (for instance, in fourth year O-level classes in science[20]) yet where the reality of the circumstances, such as the nature of the pupils, would render the assumption problematic.

Taking these various points together, the potential for uncertainty and variation in T,W,X and Y makes the following of a natural science methodology by the human sciences a questionable undertaking. To make such a statement is not to say anything new, for there have been many in the human sciences who have called for research approaches which recognise the human dimension in their designs[22]- though rather fewer have made specific proposals regarding methodology.

If one takes the view that human action is significantly influenced by the environment in which it occurs, then research in natural settings is a necessity, and this (given the argument sketched above) would minimise the employment of experimentation in the Campbell and Stanley (1963) sense. S. Wilson (1977) is interested in ethnographic approaches to research which involve participant observation, but naturalistic research is not constrained to the adoption of this style of operation. Research in natural settings is demanding since it involves, for each instance observed, the fullest description of W,X,Y and Z consonant with the aims of the research if some form of structural analysis is to underpin the extraction of generalisations.

From a phenomenological perspective Giorgi (1965,1966) criticises psychology's use of the natural sciences' experimental paradigm and, in

the second of these papers, searches towards a parallel but not imitative paradigm for the human sciences. The paradigm he sketches has its weaknesses (in particular, its failure to distinguish between the phenomenological perspectives of the researcher and the respondent), but the contrast he makes with the natural sciences' paradigm is suggestive of a direction in which research with people might proceed (Figure 13.1)

Realm of the natural sciences	Realm of the human sciences
Experimentation	Experimentation and other forms of research activity
Quantity	Quality
Measurement	Meaning
Analysis	Explicitation
Determined reactions	Intentional responses
Identical repetition	Essential phenomenon known only through varied manifestations
Independent observer	Participant observer

Figure 13.1 Polar contrasts relating to research paradigms in the realms of the natural and human sciences.(From Giorgi, 1966:47)

Giorgi's contrast is something of an over-polarisation since aspects of the natural science paradigm can be subsumed under that of the human sciences, but it serves as a stimulus to avoid the subsumption of human processes under the measurement reductionism of an experimental psychology or, in their broader context, under that of a scientistic sociology. How, for instance, could love and depression be investigated under a natural sciences paradigm[23]?

Although Giorgi does not make it explicit, his arguments point towards a hermeneutic psychology containing a strong emphasis on personal construing - and they can also be addressed to the human sciences in general. Despite having a number of reservations about how Giorgi's human sciences paradigm might operate, it provides a useful starting point for the reconnaissance attempted in this chapter.

The approach to research which is sketched in the following pages rejects the primacy of the covering law and its implication of a closed system of concepts[24]. Instead, it recognises that the researcher is never in a position to justify an appeal to a closed system and that human activity is more appropriately studied in terms of open systems (von Bertalanffy, 1973*). From such a standpoint human activity appears as a set of unique - and sometimes related - events, each of which is influenced by a particular cluster of antecedent conditions; and inter-subjectivity is not to be bought at the cost of neutralising individual differences (as would be the case in a positivistic science[25]). This is not to reject completely the idea of covering laws but to see them as, at most, contributing to the explication of the nexus of influences impinging upon any human act[26]. Nor is rejected the possibility of detecting regularities and inducing relevant hypotheses or explanatory principles: the position taken here merely accepts that it is necessary to acknowledge their limitations and not to claim too much in the interests of generalisation.

In rejecting positivism as its guiding philosophy, a hermeneutic human science sets itself against a well-established tradition of positivistic thinking discernible in such disciplines as structural anthropology, linguistics, and in much of sociological, psychological and educational

research. In contrast to these, it stands broadly within a Kantian tradition of thought running from Dilthey (in so far as he views history as an interpretive discipline) and Simmel (1980*) to Collingwood (1961*) and Dray (1957). The Kantian metaphysical tradition is also present in the emphasis given to personal construing [27], (Kelly, 1955), the phenomenology of the life-world (Schütz, 1967*) and - to a lesser extent - the tenets of the existential analysis movement (May et al, 1958). Finally, and of importance for both theory and method, the unification possible through an appeal to hermeneutics draws upon Dilthey's insights regarding the human sciences, progressively developed in ontology by Heidegger (1962*), in aesthetics and linguistics by Gadamer (1975*), in literary criticism by Hirsch (1967), and in the more general context of the human sciences by Ricoeur (1981).

Pointing out how human sciences inevitably look backwards, C. Taylor (1971) succinctly indicates how their openness of texture distinguishes them from the natural sciences, and his words constitute a point of departure for the theoretical position which I seek to develop.

> 'There are thus good grounds both in epistemological arguments and in their greater fruitfulness for opting for hermeneutical sciences of man. But we cannot hide from ourselves how greatly this option breaks with certain commonly held notions about our scientific tradition. We cannot measure such sciences against the requirements of a science of verification: we cannot judge them by their predictive capacity. We have to accept that they are founded on intuitions which all do not share, and what is worse that these intuitions are closely bound up with our fundamental options. These sciences cannot be "wertfrei"; they are moral sciences in a more radical sense than the eighteenth century understood. Finally, their successful prosecution requires a high degree of self-knowledge, a freedom from illusion, in the sense of error which is rooted and expressed in one's way of life; for our capacity to understand is rooted in our own self-definitions, hence in what we are.' 28

13.3 PERSONAL CONSTRUCT THEORY AND INTERPRETATION

In the preceding chapters a considerable amount of Kellian bathwater has been tipped away, and I must be careful not to be over-vigorous with the tub. Kelly's view of people as pragmatic explorers and active interpreters of their world is consistent with a human science grounded in hermeneutics. It is from this base that I shall seek to sketch some ways in which the cognitive aspects of personal construct psychology might be developed, giving particular emphasis to methodological implications.

Like many observers of, and theorisers about, the universe I shall not be tempted into speculating upon origins (in this case, of construct systems), though J.O. Katz (1982 a) has been bold enough to propose an 'Origin Postulate' which asserts that people begin life with an innate set of primitive constructs. I do not propose to take a stand on the 'nature/nurture' determination of constructs, but shall take a basic construct system as a 'given' and begin from there.

As people grow, their personal universes expand. No doubt the rate of expansion varies between individuals, perhaps becoming negative with senility in some cases. Whatever the size of their personal universes people try to interpret and explain them, often conducting 'experiments' (in the loose Kellian sense). In Piagetian terms, learning begins with a sensori-motor phase and develops through 'concrete operations' to the stage of 'formal operations', or abstract thinking. This does not imply that thought processes become progressively more 'right' with age: individuals' constructions can seem remarkably askew, as Piaget's conservation experiments have demonstrated, and as M. Watts and his co-workers (who are using a Kellian framework) are currently showing to be the case with school children's construing of scientific concepts[29].

The point being made here is that the constructions evidenced by these children are the 'best' that they can produce at the time of testing. They are the best interpretations they can give of the situations confronting them in the light of the totality of their experience - or, perhaps more accurately, that part of the totality which they can retrieve from memory. It seems reasonable to suggest that a fundamentally interpretive process similarly operates within adults although its cognitive level may often be considerably higher than in children. (I am taking the view here that interpretation is anterior to purposive behaviour [30].)

One can press a little further and suggest that such an operation is underpinned by an iterative process, a dialectic, between part and whole of experience until - for the time being at least - sense is made of it. There is a distinct similarity here to Dewey's (1916 a) account of reflective experience [31] although it may well lack the rigorously scientific elements of thought. A new experience opens up a new dialectic until it is assimilated into the existing construct system, or until the construct system develops to accommodate it (cf.Figure 3.1).

Or, in different terminology, construing can be considered as a hermeneutic process. Gauld and Shotter (1977) describe a hermeneutic approach to human behaviour which depends upon the elucidation of the intended meanings of actions and therefore upon personal construing [32], but their interest in machine theory takes them in a direction that diverges from mine.

My interests lead in the direction of exploring further a 'personal construct hermeneutics' which retains the reflexivity of Kellian theorising

in that it can be applied to researcher and respondent alike (though perhaps at different levels of sophistication). Further, much of the connection implicitly made by Kelly between epistemology and ontology remains as a foundation. The perspective developed in the succeeding pages does not require some of the assumptive structure of Kelly's theory, and those aspects which were criticised in Chapter 3 are not retained.

Up to this point the term 'hermeneutics' has been used rather lightly, and in view of its semantic evolution it is necessary to establish in the next section the way in which I shall be using it.

13.4 THE EVOLUTION OF HERMENEUTICS

Hermeneutics originally came into prominence in the seventeenth century with a rise in demand for the interpretation of biblical texts, and it referred then to the theory and methods underpinning exegesis (commentary). Palmer (1969) observes that the meaning of the term has shifted, roughly chronologically, in the following sequence:

(i) the theory underpinning biblical exegesis;

(ii) general philological methodology;

(iii) the science of all linguistic understanding (Schleiermacher);

(iv) the methodological foundation of the Geisteswissenschaften, or human studies, (Dilthey);

(v) the phenomenology of existence and of existential understanding (Heidegger), later extended into the realm of linguistics (Gadamer); and

(vi) the systems of interpretation used in the search for meaning behind myths and symbols (Ricoeur)[33].

Of these meanings the last two have the greatest relevance to the thrust of this chapter, though the crucial influence of Dilthey's thought must be acknowledged.

Dilthey sought to separate scientific and historical understanding, seeing the latter as fundamentally distinct from the quantifying, objective practice of the former[34]. He saw the understanding of human affairs as calling into play a personal knowledge of what it is to be human and, given his earlier attempts to ground this perspective in psychology (not a historical discipline) his efforts were compromised by both latent positivism and psychologism. The importance of Dilthey's writing, as far as this chapter is concerned, is the sharp change of emphasis which he gave to hermeneutics - from a somewhat detached philology to an engagement in the broad spectrum of human affairs.

Heidegger locates hermeneutics within the field of ontology, giving primacy to the problem of Being (which he sees as the forgotten question in Western philosophical thought). He construes understanding holistically, and as a necessary condition of Being which transcends and subsumes Dilthey's dichotomy. For Heidegger, understanding always operates within a framework of relationships that have already been established and interpreted[35]; there is a distinct similarity here with the Kellian notion of a construct system. Although at the beginning of'The Psychology of Personal Constructs' Kelly asserts the objective reality of the world (in its broad sense), the emphasis throughout his writing is directed towards the world as construed by individuals, thus placing his ontology close to that of Heidegger who conceived of 'world' in terms of something integral with the self, all- encompassing yet transparent and elusive.

The parallel between the two can be pursued a little further in that both adopt a future-oriented stance. Anticipation is explicitly built into Kelly's Fundamental Postulate, and the Corollaries link this to

the individual's frame of reference - the construct system as it exists at the moment of anticipation. For Heidegger, understanding has a projective character which is necessarily founded on the bases of the person's current situation.

However, there are dangers in pressing the parallel too far, for at the level of articulation of theory it would seem unlikely that Heidegger would be happy with the Kellian emphasis on prediction and control. Palmer (1969), drawing on Heidegger's later writings, captures the distinction:

> 'The will to power that is grounded in subjectism knows no ultimate value, only the thirst for more power. In the present day this expresses itself in the frenzy for technological mastery. Yet the impact of technological thinking is more subtle and persuasive than this, for gradually we have come to consider thinking itself in terms of mastery. Thought becomes technological, shaped to the requirements of concepts and ideas that will give control over objects and experience. Thinking is no longer a matter of open responsiveness to the world but of restless efforts to master it.....
>
> Hermeneutics, as the theory of what understanding and interpretation are, is directly affected by these considerations, for when the interpretive problem is approached within the context of technological thinking, interpretation provides the means for the conceptual mastery of the object'.36

In other words, 'technological' thinking converts hermeneutics into a set of rules or techniques through which the world may be understood (Peirce might have argued that, construed in this way, hermeneutics drew upon deduction and induction but not abduction). Heidegger's claim for hermeneutics is broader than the 'technological' in that it seeks to interpret by going behind the manifest, exploring hidden meanings and disclosing what lies in the gaps; in this there is an anticipation of Ricoeur's position, which I shall shortly discuss. A hermeneutics such as Heidegger's seeks to confront and ultimately transcend (the difficulties here are acknowledged) a neo-Kantian, a priori set of

templets through which the world may be understood. This would seem to go beyond the Kellian systematisation of the notion of construing - in particular, as it is expressed in the Dichotomy and Organization Corollaries - which appears distinctly 'technological'in the sense used above, and against which some arguments were advanced in Chapter 3.

Is it possible, then, to fuse the horizons of Kelly and Heidegger in order to propose a 'personal construct hermeneutics'? Such a task lies beyond the scope of the present work, and would require at minimum a meticulous reading of Heidegger's very complex writings before a well-argued answer could be offered. The present - albeit very limited and selective - review suggests no more than that it is an idea worth exploring, starting from the ontological parallels noted above and drawing both upon the Deweyan-Kellian perspective of people as active, intentional and exploratory beings and upon the Heideggerian construction of hermeneutics: a fusion of pragmatism and hermeneutics which will be difficult to accomplish and which can expect to be confronted with scepticism from established philosophical traditions.[37]

Such might be a redefined ontology, but would it be an adequate basis for research (which, after all, is the centre of interest of this work)? Kelly emphasises the reflexivity of his theory - its capacity to subsume both the therapist's and the client's construing: from the point of view of research it remains to be examined whether, in this context, the notion of reflexivity is tenable in either the original formulation of construct theory or the developments suggested above. In principle, both perspectives would appear to have the capacity for reflexivity, but the asymmetry of the typical transaction between researcher and respondent would seem to make it a more doubtful proposition at the level of practice.

Giddens (1976) makes the further point that the social-scientific observer suspends belief that things are as they appear, and that this is a fundamentally different stance from that of the lay actor who, as practical social theorist operating in the light of socially grounded presupposition, supposes the world to be as it appears to be.[38]

An ontologically-grounded theory runs the risk of being charged with psychologism. In construct theory terms the researcher's task is to subsume the respondent's construct system (cf the Sociality Corollary) to an extent sufficient for a coherent and defensible understanding of intention and behaviour to be constructed [39] (whether this is for the purposes of explanation or prediction is immaterial at this point). Kelly is strangely reticent regarding the nature of subsumption (despite making an extensive reference to dimensions of diagnosis, which are a set of constructs through which the therapist can subsume the client's system[40]), and the reader's fears of a psychologistic, intuitive and empathic approach are not entirely dispelled. To be fair, Kelly does make a passing reference to the need for the clinician to be sensitive to the 'many little cues by which people reveal their anxiety in everyday life' [41], hinting at the value of a normative framework relating to rule-governed behaviour.

This hint, coupled with the much more elaborate role/rule thesis of Harré and Secord (1972), suggests a way in which a researcher operating from the framework of personal construct hermeneutics might come to terms with the ontology of a respondent subsumed within the same theoretical perspective. Whilst the theory - like Kelly's own - is reflexive, from the point of view of the researcher, the construing of construction

system of another tendsnot to be reflexive (which is not to claim that the 'respondent' does not construe the construction system of the researcher, but is merely intended to acknowledge the asymmetry of power in the relationship).

This brings me to the position which Ricoeur (1981) takes regarding hermeneutics, and which is a synthesis derived from a dialectical interrogation of Gadamer's (1975*) historically - conscious hermeneutics and Habermas's (1972*) critique of ideology.[42] Ricoeur's discussion is lengthy and complex, and it will have to suffice here to cite his main conclusion. This is that modern hermeneutics is fundamentally divalent and possessed of a contradiction: on one hand it seeks the deeper significance underlying the manifest text (or text-analogue), whilst on the other it is actively 'suspicious' of the manifest as a representation of reality. Drawing on the examples of Marx, Nietzsche and Freud as great demystifyers, he sees this second aspect of hermeneutics as the demystification of symbols masquerading as a false reality.

Whilst the Heidegger-Kelly fusion may prove adequate as an ontology and as a basis for much clinical practice, the demands of research are more severe in that they require that conceptualisation, methodology and analysis be made available to, and withstand the pressure of, public scrutiny.

The view is taken here that hermeneutics is the pivot on which the human sciences turn, whether the evidence being appraised comes from controlled experimental settings or from research in more natural surroundings.[43] Interpretive research requires that, as far as possible, acts and actions be considered in relation to

(i) the researcher's own theoretical position;

(ii) other possible theoretical positions;

(iii) other acts and actions;

(iv) other evidence relevant to the context;

and (v) the construing of those people at the focus of the research.

It is to the last of these, personal construing, that this chapter gives the greatest emphasis.

13.5 ACTION AS TEXT? A DIGRESSION ON INTERPRETATION AND EXPLANATION

Before the idea of a personal construct hermeneutics is taken any further some clarification of terms must be attempted, for description, understanding, interpretation and explanation all play parts in a hermeneutic schema yet, apart from the first, their meanings vary according to the source consulted.

Description presents few semantic problems. However, although a researcher may be given a description of an event, it is important not to overlook the fact that this is not only a reflection of the way that the individual has construed the event but also of the way in which he or she has chosen to report it.

The other three terms often appear as 'givens' in texts without any attempt being made to delineate the scope of their meanings and, since they interpenetrate, there is fertile ground for confusion. Dilthey was fairly explicit in the distinction he drew between explanation (erklärung) in the natural sciences and understanding (verstehen) in the human sciences. Interpretation was construed as a subset of the latter and was restricted to durable artefacts of human endeavour. Ricoeur (1981)

points out a number of difficulties with Dilthey's view and proposes instead a dialectic between explanation and interpretation, understanding being a necessary (yet undefined by him) condition for both[44].

Ricoeur's acknowledgement of Hirsch's (1967) notions of 'guessing' and 'validation' in literary criticism suggests a way forward.[45] Ricoeur loosely equates theinitial guess regarding the genre of a text to understanding and the validation of the guess to explanation, despite spending little time in discussing the nature of the former. Hirsch (1976) draws attention to Heidegger's priority of pre-understanding, referring to it as the 'primacy of the schema'[46]. This suggests that a person seeking an understanding of the words (or, more generally, the actions) of another is engaging in some form of pattern-matching which may or may not be present at the level of consciousness. With 'concrete' objects it is possible to see, following Rosch's work,that observed features might be mapped on to 'prototypes' sedimented in memory, a positive match being obtained when sufficient fragments of the entity align with those stored in memory. Where intentional activity is concerned,the postulated mechanism would be more complex, involving the mapping of both observed behaviour and (assumed) intention on to sedimented schemata[47].

The Hirschian 'guess', as it emerges into consciousness, is taken as understanding. This is not to suggest that understanding is static, for all understandings are open to invalidation (in Hirsch's sense) and to replacement by further understandings as further cognitive pattern - matching becomes possible.

Interpretation, following Ricoeur, interrogates understanding in two distinct ways in that it explores both the acceptance and rejection

of originally understood meaning. The aim here is to open up a range of possible meanings in order that they may be 'validated' in probabilistic terms. On this view interpretation is an active process in which chains of implication based upon the original guess are systematically explored from one or more theoretical positions, bearing in mind the context of the event being studied. The process can be characterised in terms of the construction and testing of hypotheses according to the canons of procedural evidence.

The aim of this hermeneutic process is the construction of one or more possible explanations of an event, whose chains of reasoning may be laid out and weighed as to plausibility using a quasi-legal approach in which each item of evidence is assessed in terms of its coherence with the whole. Explanations, then, are the formalised outcomes of the interpretive process - but they are not necessarily the termini of reasoning for, as von Wright (1971) observes, they may lead to interpretation at a higher level[48].

The cyclic and complementary nature of the relationship between understanding, interpretation and explanation [49] is shown schematically in Figure 13.2 (and is strikingly similar in form to the relationship between cognitive and purposive behaviour shown in Figure 3.1.). As with all diagrams of this sort, it is an oversimplification in that it separates processes which interpenetrate and which may (in respect of different facets of the event being considered) run out of phase with each other.

Following for the moment Ricoeur's metaphor of action-as-text, it has to be stated that no 'reading' is ever definitively true: truth here

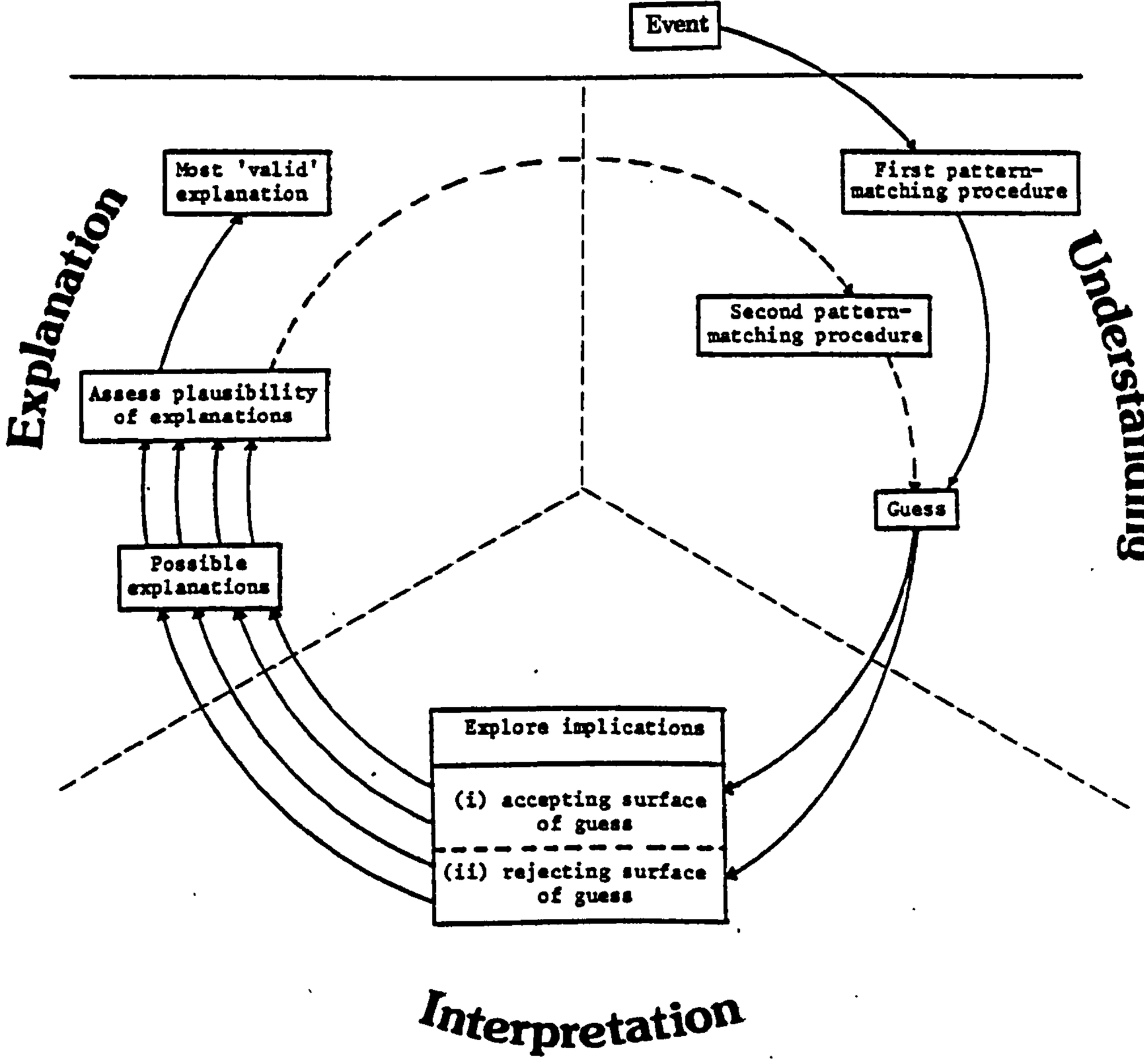

Figure 13.2 A schematic representation of the relationship between understanding, interpretation and explanation.

is related to the extent that the reading can be justified by argument - a qualitative rather than a quantitative matter. There is an indefinite set of readings with different qualitative probabilities and, in Hirsch's terms, the most probable is the most valid.

Hirsch offers four general criteria against which the probability of a reading may be established. The first three are

(i) legitimacy (the reading must be permissible within the public norms of language - Saussure's langue - of the text's composition;

(ii) correspondence (it must account for each linguistic component in the text); and

(iii) generic appropriateness.

These three criteria usually permit several readings, and the fourth is introduced to discriminate the most valid from among the competing possibilities:

(iv) coherence and plausibility[50].

Hirsch makes clear that this last criterion implies a reconstruction of the author's outlook (that is, it is grounded in historicism).

It is at this point that Ricoeur and Hirsch part company. Ricoeur detaches the author from the text as a necessary condition for the dialectic between <u>verstehen</u> and <u>erklären</u>. Gadamer takes a similar view, pointing out that 'everything that is handed down.....is estranged from its original meaning'[51] and that a reconstructionist view of hermeneutics 'is as foolish as all restitution and restoration of past life'[52]. Ricoeur and Gadamer are right to point out that when a reader attends to a text (or other work of art), the originating intentions are dead and gone: a text is what it is.

However, the researcher in the human sciences - and particularly in the methodological subset under discussion in this chapter - has up to three possible advantages over the literary critic:

(i) accounts of experience and action are often 'live', containing vocal inflections and gestures which aid the grasping of meaning (and some of which may be preserved in a recording);

(ii) interaction is dialogic, allowing both clarification and further probing; and

(iii) it may be possible to return to the informant on one or more subsequent occasions in order to cross-check or elaborate matters arising from a preliminary analysis.

When any of these three obtain, the researcher has the opportunity to probe

the noematic qualities of the respondent's thinking - although this is necessarily reconstructive and open to objections of post hoc rationalisation. In other words, it is possible to approach the intentional components of action (or the author's determining will, for which Hirsch's, 1967; 1976, writings on literary criticism make a sustained - and contentious - plea).

The analysis I have presented suggests that, from a personal construct hermeneutic perspective, the metaphor of action-as-text is less appropriate than that rejected by Ricoeur; action-as-speech. In addition to the interpretive possibilities stemming from the objectification of action, the Kellian researcher has the benefit of the dialogic relationship, much of which can itself be objectified for the purpose of rigorous inquiry. Given these advantages over the literary critic, it may be possible to discern the embedded figure of intended meaning within the multiplicity of significances of the relevant action.[53]

13.6 FIRST AND SECOND ORDER CONSTRUING

The uncontrolled variety of influences upon any human activity makes prediction difficult: taken in conjunction with the theoretical limitations noted in chapter 3, Kellian prediction finds it difficult to clear itself of the charge of being an eclectic, ad hoc and sometimes naive procedure. A combination of C. Taylor's (1971) view of the human sciences as fundamentally retrospective and explanatory with Kelly's (1970b) emphasis upon the construing of events (which, for my purposes, will be treated as human behaviour) leads me to read Kelly's writings from the perspective of the historian rather than from that of the scientist. To take this view is not to challenge the claim that people do make predictions in their lives; it merely reiterates the assumption

that the interpretation of events is anterior to, and does not necessarily lead to, prediction.[54]

From this perspective, the understanding and interpretation of action is the critical step for the researcher, entailing an engagement with the construct systems of the actors involved (wherever these are allowed as a significant variable). To clarify the position, the researcher seeks the meanings of an event in at least two ways: first, as mediated through the construct system of the actor him- or herself and, second, through the behaviour of the actor during his or her presentation of the account of the event in question. (This interview is itself an event, but standing at a meta-level to the event at the focus of the inquiry.) Further, where the researcher is present at the focal event, there is the opportunity for interpretation which is unmediated by the construct system of the actor. The position can be represented schematically as in Figure 13.3

In Figure 13.3 there are, from the researcher's point of view, both phenomenological (in Schütz's sense) and phenomenographical strands of evidence, 'phenomenography' being the term that Marton (1981) uses for the second-order construing of how others construe events. Both researcher and actor interpret the particular situations confronting them, thus establishing what Giddens (1976) terms the 'double hermeneutic'[55] of a subject-subject relationship which stands in contrast to the 'single hermeneutic' of the subject-object relationship characteristic of the natural sciences.[56]

There are risks in second-order construing: as Schütz recognised, there is a danger that the researcher might substitute his or her own

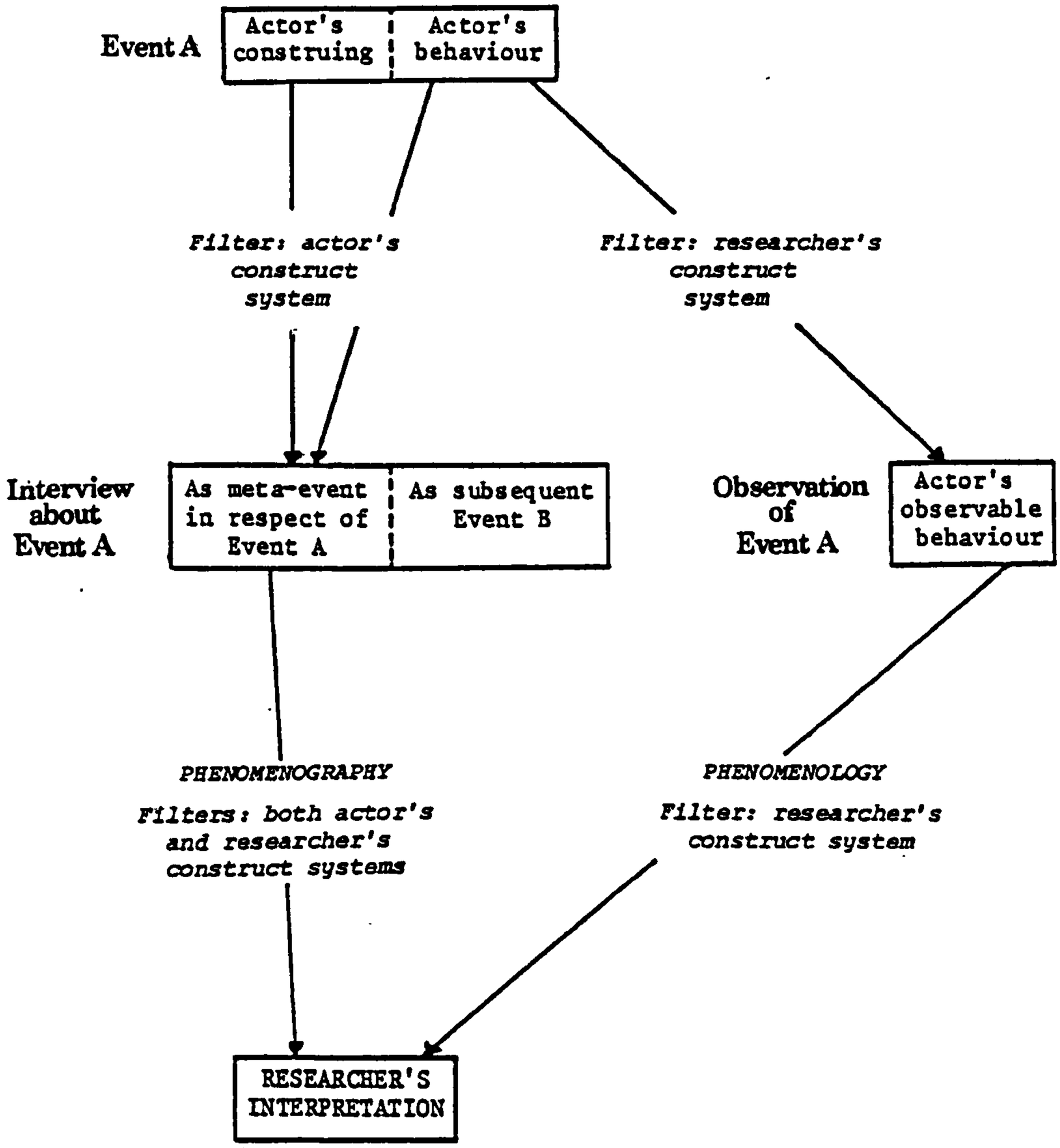

Figure 13.3 First and second-order construing of an event by a researcher.

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interpretive framework for that being used by the actor and, by failing to check, remain unaware of the mistake[57].

Given that inconsistencies may appear both within the 'boxed' components of Figure 13.3 and between the different evidential strands, interpretation is potentially a complex affair which necessitates bringing the canons of procedural evidence to bear upon the lines of evidence in order to probe for the position of the 'best' reading of the event (in

Hirsch's, 1967, sense). Once hermeneutics are introduced into the situation the phenomenological and phenomenographical are, in addition to being accepted at face value, put under 'suspicion': in other words they are granted no privilege of immunity from being unmasked. A hermeneutic human science therefore necessarily goes beyond the bounds of phenomenology and phenomenography.

13.7 THE PROBLEM OF UNDERSTANDING

If one were to broaden Weber's characterisation of sociology, one might depict the human sciences as being based upon rigorous attempts to achieve interpretive understanding of human behaviour, and their purpose as being to arrive at maximally probable explanations of antecedents and consequents. 'Behaviour' is used in a broad sense here, and includes both the intentional and the unintentional. Weber put forward the idea of a purely rational course of action (an 'ideal type') which is clearly understandable and unambiguous, and from which actual action could be seen as deviating to a greater or lesser extent. Rosch's notion of 'prototypes' exhibits a conceptual similarity (although at a much simpler level): presumably there is, for each individual, an 'ideal type' of dog (in Weber's, rather than the common,sense) from which animals with different degrees of 'doggiess' deviate. In other words, the individual constructs a normative frame against which actions and conceptualisations are judged - or, perhaps more accurately, a set of frames, each member of which is particularly apposite to a facet of human behaviour (such as psychological functioning, social interaction and macroeconomics).

Weber saw understanding as involving the interpretive grasp of meaning at three levels:

(i) the actually intended meaning for concrete individual action;

(ii) the average of - or approximation to - the actually intended meaning contained within sociological mass phenomena; and

(iii) the meaning appropriate to a scientifically formulated pure type - the 'ideal type' - of a common phenomenon (illustrated by the concepts or 'laws' of pure economic theory[58]).

As far as the present work is concerned, more emphasis is given to the first category than to the second whilst the third falls outside its terms of reference.

Schütz (1967*) criticised Weber for not distinguishing between detachment from, and participative involvement in, social processes [59]. However, his objection is weakened by the fact that in either circumstance meanings are objectified in behaviour. The difference between the two situations would seem to lie in the greater number of communicative channels opened to the researcher in the case of participation in a social encounter. Understanding is grounded in observable behaviour[60], (though not all of this behaviour will be accessible to consciousness) rather than in terms of some kind of psychologistic empathy. Given Kelly's interest in construing the construct system of the other, the matter needs to be pursued a little further.

The understanding of another's experience runs the risk of being dismissed as mere psychologism, a charge labelled against Dilthey in respect of his concept of Nacherleben, or re-experiencing. Whilst some accounts of Dilthey's philosophy are liable to give the reader the impression that re-experiencing implies psychological isomorphism,[61] Makkreel (1975) makes it clear that by Nacherleben Dilthey meant 'a

mode of re-experiencing which is to be understood as a re-creation..... of an expressed meaning, rather than a psychologistically conceived re-production.....'.[62] Collingwood (1961*) gives this a more practical slant when he describes the historian's picturing of a subject as 'a web of imaginative construction stretched between certain fixed points provided by the statements of his authorities'[63], assuming that the word 'imaginative' is not given the pejorative connotation of fancifulness often accorded [64] and is instead construed as 'rational interpolation of the possibilities that exist between modes of evidence'[65].

Construing the construct system of another is, in Kelly's theory, a necessary condition for a social psychology, but his writing is not very clear on what this involves. One cannot construe directly the construct system of another; rather, one must make inferences about the other's system from observed behaviour. There are hints of behavioural reference at various points in Kelly's writing, but he does not explore in any rigorous manner the formal relationship between behaviour and construct system from the perspective of an external interpreter.

It is here that Husserl's phenomenological distinction between the noetic and noematic properties of mental acts may prove helpful. The former are the psychological qualities characterising a mental act as being of a certain kind (for instance, a belief or an intention), whilst the latter are the logical and conceptual qualities attaching to a mental act by virtue of its particular content or objective import. Neither can be re-enacted by an observer, but it is possible to gain some indication of their nature (or type) through the construing of the other's behaviour in the light of one's own framework of experience [66]. The relationships involved are represented schematically in Figure 13.4.

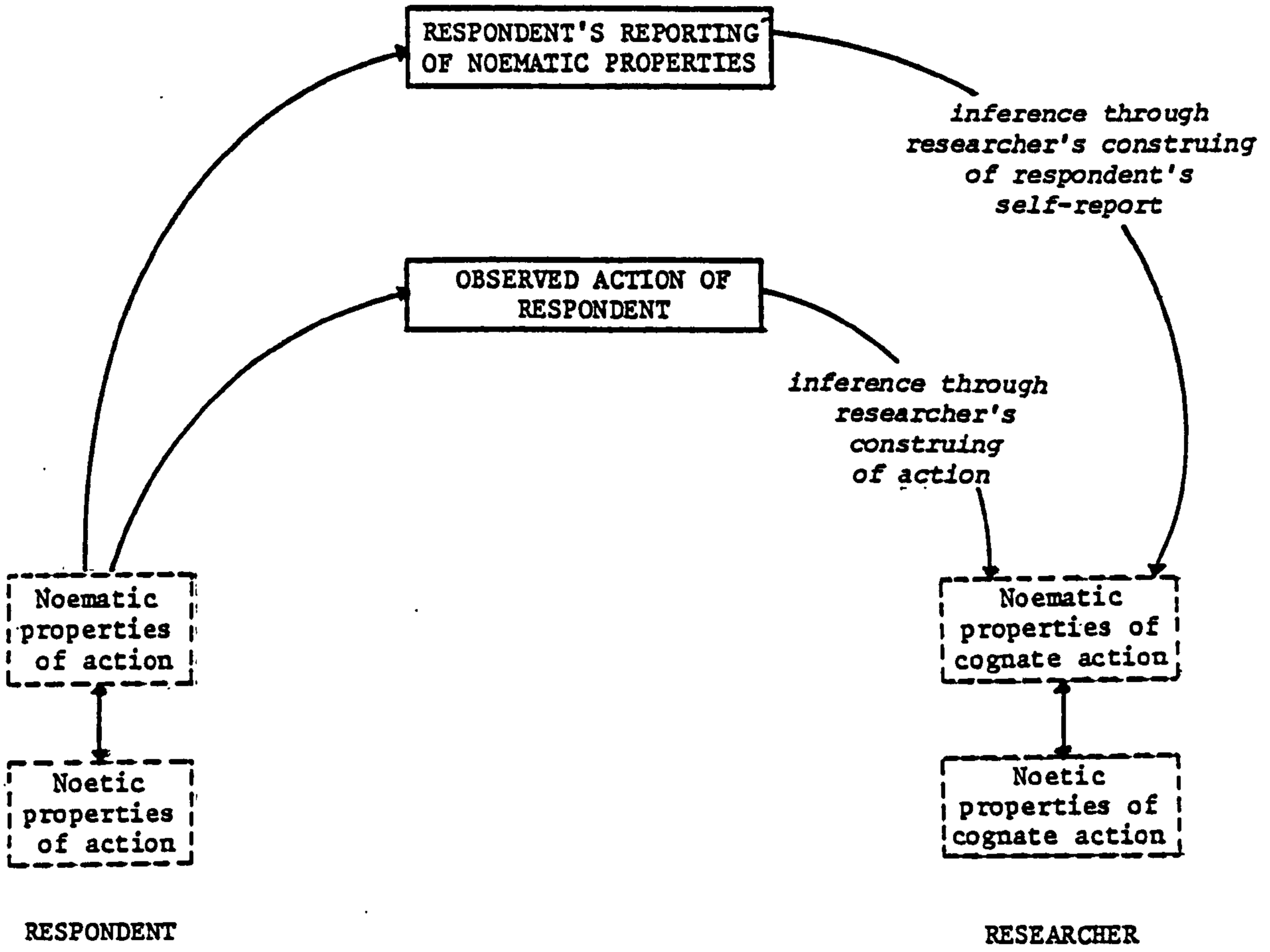


Figure 13.4 A behaviourally-referenced approach to understanding.

In Figure 13.4 the respondent's actions and reportage are shown as providing clues to the noematic, which in turn allow inferences to be made about the noetic [67]. The noematic properties of mental acts have structural implications which can be grasped (not re-enacted) without entailing a psychologistic re-creation of the noetic. Thus I can identify the motives behind the murder committed by a jealous lover without re-creating the murderer's state of mind.But I can only do this because I exist within a culture in which motives for such actions are widely exposed and because I have experienced the emotion of jealousy. There would be no possibility of my interpreting the murder in terms of jealousy if I came from a culture in which jealousy did not exist.

Schütz (1967*), in discussing how an observer of an act seeks to interpret

the actor's motives, points out that the observer has to be satisfied with three indirect approaches:

(i) searching the memory for similar actions of his or her own, and effectively projecting the relevant 'in order to' or 'because' motives on to the other (in other words , putting self in the other's place),

(ii) lacking (i), resorting to knowledge of the respondent's customary behaviour and deducing the underlying motives; and

(iii) lacking (i) and (ii), trying to infer the motives by asking whether the act in question would further them and - while observing - interpreting the act in terms of its effects (assuming them to be intended).[68]

Here Schütz bridges the gap between the phenomena being observed and the noematic substrate (though his categories of inference omit reference to normative aspects of the relation between thought and action): in practice, circumstances may require that the interpretation be based on a combination of categories. It is noticeable that Schütz does not consider here the possibility of inquiring into the respondent's own construing of the observed act.

In the sense described here, the problem of psychologism refuses to go away: it is not a matter of psychological isomorphism but a matter of similarity in socially grounded conceptualisation that makes interpretation possible through what is tantamount to an act of translation.

From the foregoing it follows that a crucial determinant of understanding is the researcher's lived experience (<u>erlebnis</u>). In Kellian terms, lived

experience is generative of a construct system which has some capacity to subsume the construct systems of others. Dilthey's use of verstehen (understanding), which is necessarily based on lived experience, comes close to Kelly's position: it is not, as Nagel (1961) suggests, some form of emphatic identification which is intuitive and nonrational Nor is verstehen stereotypically grounded as Abel (1974*) suggests, since the use of stereotypes implies a positivistic covering law conceptualisation.[70] Makkreel (1975) points out that Dilthey's approach would be to search the contextual ground of an observed action in an attempt to find a basis (or rule-structure) for the action to have taken place. This reference-back to the context shows that verstehen is 'not an immediate act, but an indirect and reflective process'.[71] In other words, one draws upon what one knows through direct and vicarious experience in order to interpret the actions with which one is faced.[72]

If the researcher's lived experience is important in the understanding of another, it also imposes its limitations. Bleicher (1982) points out that where the hermeneutic tradition (as applied to literature) has included attempts to get at an author's intentions, the giving of full justice to what has been created requires that the author and interpreter should be of similar intellectual and moral standing [73]. The 'like minds' argument is seductive - and dangerous.

While such a position is immediately open to attack on the grounds of psychologism, Wax (1974*) further points out the narrowness of the 'taken-for-grantedness' (or transparency) regarding much sociological research in America; research which is done within the researchers' native country and which is largely concerned with problems specified by the authorities of a middle-class society [74]. As regards the problematic

of understanding, Betti notes that a number of obstacles have to be overcome, including

(i) self-righteousness on the part of the interpreter;

(ii) conscious or unconscious resentment of alternative ideas and value-positions; and

(iii) an unwillingness to engage with the 'world' of the other.[75]

These and other obstacles imply a need for the researcher to stand back from too close an identification with the respondent's position and to adopt a self-critical stance towards his or her own world view. The (existential -) phenomenological horizons of the protagonists can never be completely fused (another way of stating Kelly's Individuality Corollary), but may perhaps be described as interpenetrating. This - and other - partial fusions allow the researcher some access to other world views, this facilitating a dialectic process of 'coming to know' the particular constructionsof the respondent.

Whilst some understanding of what it is to be in situation W is necessary for research into how individuals in general construe W-type situations, advances in theory are likely only where such constructions can be set within a matrix of theoretical assumptions T' which is broader and/or more detailed than the set of assumptions T upon which the particular research project has been constructed. One might, following Peirce, refer to theoretical advance as 'abductive generalisation' in contrast to the more statistical 'inductive-actuarial generalisation' produced as a result of limiting interpretation to the realm of T. This question of generalisation is taken up in section 13.9.4.

The upshot of the argument is to recognise a creative tension between identification and distanciation. Too close an experiential relationship

between researcher and respondent, and the researcher will lose the critical detachment necessary to set the research in a broader framework; too much distance, and there will be insufficient congruence at the noematic level for a penetrating interpretation.[76] Further, too much distance, and the researcher may obtain an inadequate response: the experience of Labov's, 1972*, 'large, friendly white interviewer' in trying to elicit responses from a defensive black boy serves as a cautionary tale in this respect.[77]

It may be possible to correct for 'over-identification' by adopting a team approach to research which involves other researchers with different perspectives as constructive critics of the primary researcher-respondent relationship. However, little seems possible to remedy the problem of too great a distanciation between the parties concerned - save perhaps to replace the researcher.

13.8 RESEARCHING WITHIN A PERSONAL CONSTRUCT HERMENEUTIC FRAMEWORK

In this long section I attempt to indicate how the theoretical perspectives developed in the preceding section may be operationalised in a manner consistent with the philosophical standpoint taken. There are two main areas of concern: communication and interpretation. A personal construct approach necessarily sets considerable store upon respondents' accounts of experience, and a systematic approach to the collection of accounts is described. Testimony alone is inadequate for rigorous research, and aspects of cross-validation are discussed. Interpretation, too, cannot be treated simplistically (as will have been evident from the earlier discussion of a 'suspicious' hermeneutics), and some attention is given to the interpretation of what can never be more than an incomplete record of an event.

13.8.1 The Problem of Communication

It is relatively easy to offer an abstract disquisition on the virtues of the respondent's construction of reality, but there are a number of problems to be overcome before a researcher can claim with any confidence to have elicited a sufficiently representative sample of constructs for worthwhile conclusions to be drawn. The respondent may commit, both deliberately and unwittingly, sins of omission and commission, leaving the researcher with the complex task of reconstructing the respondent's 'reality'. It is easy to take testimony at face-value but, as Giddens (1976) remarks, the researcher in the human sciences should suspend the belief that things are as they appear and not be influenced by the pragmatic demands of the 'natural attitude'.[78] In other words, hermeneutics in the hands of the researcher is likely to be a far more forensic matter than when it is being used by respondents in (and of) their normal activities. It is therefore important to indicate - if only at a general level - some of the ways in which this 'forensic hermeneutics' might be applied to research in the human sciences.

Research within a personal construct hermeneutic framework centres upon the construing of those whose thoughts and actions are the subjects of inquiry. That stated, there follows the massive problem of communication. Tycho Brahe and Johann Kepler may both 'be stood' in front of the same sunrise (in Hanson's, 1958, thought-experiment[79]), yet how would an observer recognise the fundamental difference in their construing (based on geocentricity and heliocentricity, respectively)? Would the observer even be aware of the possibility of difference? How, also, would the reader of a report of the experiment be convinced that the interpretation and explanation of the differences were worthy of consideration? Questions such as these are widely generalisable within the human sciences in which

a multiplicity of theoretical positions (both explicit and implicit) abounds, and they are of critical importance to research involving hermeneutics.

The first reaction might be to conclude that the problems of a hermeneutic approach are insuperable and that it would be safer to retreat to a position from which behaviours could be observed, measured and counted, with all the attendant virtues of 'objectivity' and potential consensus - and with the vice of totally evading the question of understanding the respondents' perspectives.

A second reaction might be to recognise that people do communicate meanings (with varying degrees of success) by linguistic and other devices, and that communication is achieved despite considerable vagueness in conceptualisation.[80] People are able to process incomplete information and go beyond the information given, a simple analogy being the interpretive coherence given to the dots making up a newspaper photograph. Communication, however, is not always successful: two people can ostensibly be talking about the same event only for one suddenly to be brought up short by a clue that the presumed contextual grounding is incorrect - "Oh, <u>that's</u> what you mean, is it?".[81] Suddenly the guessing and validation enter a new cycle; the subjective probabilities are twisted into a new configuration, giving a heightened coherence to the totality of meanings exchanged.

A researcher cannot be certain of a respondent's intended meaning since intention and inference can never be directly compared. Further, the respondent has available more meanings than can possibly be accommodated within the channel of discourse[82], though some may be communicated

non-linguistically and may indicate a contradiction between the manifest and the latent (aspects of which may be unavailable to the respondent's consciousness). But, as Hirsch (1967) asserts, the fact that certainty in understanding is impossible does not entail the impossibility of understanding.[83]

In sketching some methodological implications of the theoretical standpoint of this chapter, the optimistic view is taken that

(i) a considerable measure of understanding is possible regarding the construing of others, but that care must be taken to guard against - or at least minimise - misunderstandings. Further,

(ii) generalisation of findings is possible provided that this is undertaken at a level of abstraction above the original data.

13.8.2 Accounts as evidence in the Human Sciences

In recent years there has been a recrudescence of interest in the use of accounts of experience as raw data for research in the human sciences, though the emphasis has shifted from introspectionism to something much closer to Schütz's phenomenology of the life-world. A methodology based on accounts is potentially a better 'fit' with personal construct hermeneutics than any methodology in which the researcher's presumptions regarding the investigation are given a dominant position, as is the case with controlled experiments or surveys employing questionnaires, attitude scales or checklists in which the response options are limited to predetermined categories. A methodology based on accounts is fundamentally historical in that it stresses the importance of events, personal reconstructions of experience and other contextually-relevant evidence.

It is easy to construe an accounts-based methodology as relying rather uncritically on testimony. Collingwood (1961*) points out that testimony alone can never be scientific knowledge unless it can be vindicated by an appeal to the grounds on which it is based[84]. Though Collingwood admitted that one might at times be forced to attempt testimony for lack of anything better[85], he wrote scathingly of what he called 'scissors-and-paste history' comprising an aggregation of testimonies adjusted for credibility or trustworthiness according to the view of the interpreter concerned.[86] Collingwood's remarks serve as a reminder that historical method involves far more than the collection and aggregation of testimony if it is to contribute significantly to the advancement of understanding in the human sciences: the discussion in the remainder of this chapter seeks to indicate how, in general, accounts-based research might be constituted as rigorous inquiry.

The research methodology to be described here is based not upon the canons of scientific experimentation but upon those of history, in which information is validated according to the rules of 'procedural evidence'[87]. Events are not construed unambiguously, as anyone who has been present at court proceedings (relating, say, to a motor accident) is well aware. Legal proceedings exemplify the notion of procedural evidence very clearly, each item of information being tested against the totality of the picture being built up. At root, they constitute a hermeneutic process based on the dialectics of part-part and part-whole which, in addition to 'spiralling in' towards a more complete interpretation of the available evidence, may spiral outwards to seek further evidence within a wider field of knowing (or expanded whole) and hence a more comprehensive base from which to draw conclusions.[88]

It is not intended to imply here that conclusions drawn from procedural evidence are final truths (even in science, certainty is a bold claim indeed), for they can be 'true' only in so far as the conceptual framework and the evidence warrant - as in law, 'beyond reasonable doubt'. And, again as in law, the verdict is open to challenge in terms both of theoretical assumption (that is, alternative interpretations of the legal a priori) and of the evidence (whose components may, on appeal, be accorded different saliences)[89]. In the human sciences the court of appeal is in permanent session - and the supreme arbiter's chair is always left unfilled.

13.8.3 Accounts: some general issues regarding methodology.

Before I discuss the question of the interpretation of the evidence it is necessary to consider the gathering of accounts. In this section some general issues are discussed, leaving a more specific focus on technique to succeeding sections.[90]

Given the context of the investigation, the account which is obtained is influenced by four interrelated factors:

(i) the researcher;

(ii) the respondent;

(iii) the method(s) chosen for the elicitation of the account; and

(iv) the recording of the account for further study.

The problem to be investigated is usually chosen by the researcher, who will approach it from a theoretical position which may or may not be explicated at the outset. Since at the first level the researcher is data collector, interpreter and judge it is crucial to an investigation based on procedural evidence that he or she makes explicit what the theoretical presuppositions are, for these will specify the limits

within which description and interpretation may hold, and indicate the conceptual boundaries within which the internal consistency of the research report should be appraised.[91]

As already mentioned, in most research designs the researcher enters into an asymmetric power-relationship with the respondent which is open to serious biasing effects due to factors such as 'social desirability'. Whilst the potential for bias is readily apparent when the researcher-respondent relationship is confounded with role superordinacy/subordinacy, a more insidious bias may enter into the proceedings from subtle gestures, intonation, particular choice of linguistic form, and suchlike) emanating from the researcher during the interaction. Lest this be dismissed as trivial, the case of 'Clever Hans' gives pause for thought.[92]

The respondent will be construing the situation, usually from a more limited understanding than the researcher (in an extreme case this could go so far as to constitute guessing at the researcher's real intentions) and hence there is an inevitable confounding of response with intermediate processes. There is good reason for the researcher to try to find out at the end of the interaction how the respondent has construed the task and what his or her attitude-set was towards it.[93]

Accounts may be selective, due to partial recollection or to the desire to create a particular impression. They may also be distorted. Apart from the possibility of deliberate lying, people may not remember what they were subjectively experiencing and may interpolate plausible (but incorrect) information between nodes of accurately-remembered evidence.[95] One only needs to look at reports of critical incidents such as bank robberies and car accidents to see the potential for interpolation

regarding events to which (because of their sudden precipitation) participants are not cognitively oriented.

It can be argued that the richest accounts are obtained when a rapport is established between researcher and respondent. But rapport carries its dangers: the enjoyment of the social side of the interaction (as evidenced by phatic communication, body-language and suchlike) may influence the respondent towards the researcher's preferred outcome, and the researcher against penetrative and critical questioning. If there exists a dislike or disagreement between the two parties involved, other problems arise where the researcher's need to keep the channels of discourse open is belied by a series of cues acting in the opposite direction. The whole area of social interaction is affected by many variables which, in practical terms, are uncontrollable: class and ethnic differences, language style and articulateness, gender, setting, explicit and inferred power relationships, and so forth.[96] Perhaps the best the researcher can do is to be aware of such potential 'contaminants' and, as with experiments in physics which often demand estimates of the errors involved, to make an assessment of their possible effects.

Accounts do not necessarily have to be acquired through interview since the respondent could be asked to write down what he or she wishes to say, or even talk into a tape-recorder. However, both of these procedures have their own inhibiting features, and it is likely that 'accounts methodology' will concentrate on some form of interview ranging from loosely-structured conversation to a more direct 'question-and-answer' interview schedule. One approach within the interview genre is outlined later in this chapter.

As far as recording the account is concerned, no record can capture all of the transactions (verbal and non-verbal) during an interview, and the practicalities of most research suggest that the acquisition of accounts will rely on two methods - notes taken at the time, and audio-recording. Having at various times used both, I greatly prefer the latter since it both frees me as interviewer to concentrate upon what is being said (and not give cues by scribbling on a note-pad) and also maximises the possibility of following up potentially fruitful lines of inquiry. An audio-recording covers the whole of the verbal exchange and can be replayed to check on what was said and to review the interpretation put upon it at the time of the interview. In my view the latter is a necessary step for, if one understands a statement with ease (or believes that one does), insufficient attention may be given to the words used, and if understanding is difficult one may focus on the words and their syntax at the expense of the meaning implication.[97] Audio-recording has a further advantage in that it enables others to offer detached perspectives on any interviewer bias and on the interpretations subsequently made: after all, any researcher is limited by the particular construct system within which he or she frames an event. The suggestion is sometimes made that audiorecording inhibits respondents,[98] but my experience has been that this does not happen provided it is made clear that its use is as a form of note-taking and that nothing will be divulged in such a way that the respondents could be identified.

Audiorecordings are not without their disadvantages, for there is the need to 'process' them after the event in order to convert the account into a more manageable form. Using an account 'off the tape' is tedious when one has to spool the tape backwards and forwards to find the appropriate passages, this standing in marked contrast to the ease with

which one can flip through the pages of a document.

13.8.4 The role of the researcher

The distinction drawn in Section 13.6 between phenomenographical and phenomenological stances in respect of evidence implies two different levels of involvement of the researcher and two levels of approach to the evidence.

A. Researcher remote from the event

Phenomenologically speaking - and here I follow Schütz into the life-world, rather than Husserl - the separation of the researcher from the event (for instance, not being present at the lesson in question) is a considerable methodological restriction. The researcher is limited to an account of the event conveyed through the selective interpretive and presentational filters of the respondent. In these circumstances it is desirable to collect the fullest possible account since this will maximise the opportunity for assessing its internal consistency, and hence for detecting contradictions. Where the account is collected through interview - perhaps the most likely method - there is the opportunity to probe particular aspects of what has been said.

In a single interview it is unlikely that all of the possible leads can be followed up - indeed, some may only suggest themselves during subsequent reflection - and a strong case can be made for conducting at least one further interview with the aim of clarifying, and perhaps extending, whatever has already emerged. And where witnesses are present at the event it may be possible to obtain their constructions of it and set them against those of the protagonist.

B. Researcher as observer

When the researcher is present as an observer at the event being studied, the interpretive possibilities are increased. The researcher is able to construe the events directly and also to obtain the constructions of the protagonist and any witnesses. Direct and indirect constructions can therefore be set against each other in order to give an additional dimension of depth to interpretation and explanation.

The researcher's presence is not without risk, since it may distort the event - as might some mechanical means of recording it. Where the researcher is an active participant in the event (say, attending an evening class in pottery) there is the danger that involvement in practical activities will distract from observation and the broader construing of events as they appear.

13.8.5 Three methods of eliciting an account

I have chosen to focus on research methods which bring the researcher and respondent face to face in order to explore the latter's construing of an event (or events: I shall use 'event' in a broad sense in the following discussion).[99]

It would appear that there are three methods through which the researcher might, in conversation, seek to understand the respondent's construing of an event:

(i) by asking the individual to 'talk through' an event as it takes place;

(ii) by stimulating recall, typically through the use of a recording device; and

(iii) by asking the individual to recollect an event from memory, with or without guidance.

Each of these approaches is reconstructionist in that it requires the person to present cognitions in language which at best can only partially convey the conscious aspects of lived experience, for there is immediately a gap between experience and utterance. Further, since (i), (ii) and (iii) above are presented in order of increasing distance from the event, there is likely to be a corresponding ordinal distancing from the relevant cognitions. These - inevitable - distancings pose considerable problems for the researcher, for whom the rigorous use of the rules of procedural evidence can at best produce partial solutions (not that these are necessarily inferior to those suggested by other, less responsive, methods of inquiry).

A. '<u>Talking through</u>'

'Talking through' an event as it happens is likely to be used relatively infrequently because it is often impractical. People cannot subtitle a commentary on their behaviour like a Woody Allen film; nor can they easily deliver an operatic aside without disturbing the social interaction in which they are engaged, and hence the research.

Where the investigation focuses on the solitary individual's construing, such as in problem-solving experiments, 'talking through' may be an appropriate strategy for the researcher to gain an understanding of, and subsequently explain, the way in which the person tackles the task. An example of this is provided by Rokeach (1960) who investigated the ways in which people tackled the 'Denny Doodlebug' problem but, unfortunately for the perspective

adopted here, Rokeach concentrated his attention on the statistical comparisons of groups at the expense of the construing involved.[100]

Wason's (1960) study of students' elucidation of an underlying rule is superior from the point of view of accounts methodology in that he asked his respondents to write down their thoughts as they tried to solve the problem. This enabled him to distinguish between the employment of confirmatory or refutational approaches to the testing of hypotheses. There is an extensive literature extant on problem-solving (some of which is cited in earlier chapters), but the majority of studies appear to involve the observation of the problem-solvers' activities without making use of the possibilities opened out by a 'talking through' approach.[101] As far as interpretation is concerned, this more 'detached' observational methodology requires the researcher to make larger inferential leaps regarding the construing that has taken place.

B. Stimulated recall

Sometimes a researcher will be in a position similar to that of a football commentator who, after the match is over, gets a goal-scorer to describe how he contributed to the supreme moment being replayed through a videorecorder. This method of stimulated recall has the virtue of making a link between construing and action but, unless that link is blatantly implausible, there is no way of distinguishing between post hoc rationalisation and (the re-construction of) the actual construing which took place during that particular peak experience.

The ready availability of audio- and videorecording equipment

during the last fifteen or so years has facilitated the teaching of a range of social skills, although researchers have generally not used these media from a construct theory perspective: micro-teaching, for instance, has often been used behaviouristically to reinforce specific teaching skills rather than to explore, and help to encourage the development of, the personal construing of the teachers involved.[102]

Where stimulated recall has been employed in research based upon construct theory, videorecording has allowed the researcher to define repertory grid elements consisting of excerpts of an individual's teaching performance. The grid procedure has proved time-consuming and limited both Bell (1980) and Rolph and Rolph (1982) to studies of a single teacher's construing.

C. <u>Recollection from memory</u>

Unfortunately for the researcher, it is often the case that 'talking through' or stimulated recall are impossible or infeasible and recourse has to be made to the respondent's recollection of an event without offering any 'external' cross-referencing to what took place. Under such circumstances (which are easily reduced to the collection of uncorroborated testimony) the latitude for sloppy subjectivity in research is considerable, and this is not necessarily narrowed through the use of 'systematic' idiographic techniques such as the repertory grid - a point that has been made, with varying degrees of explicitness and force, throughout the chapters dealing with grid methodology.

Where the researcher is present at the event, there are two

advantages. First, the researcher's experience of the event allows a 'running check', as it were, upon the account being provided by the respondent. Of more direct relevance is the second point - that the researcher is able to ask questions about the event from the standpoint of having experienced it. These questions serve as prompts to the respondent to elaborate the account, but care has to be taken to ensure that both the manner and timing of such questions do not inject the researcher's constructions into the account. It would seem wisest to begin by allowing the respondent to recollect without interruption,[103] only prompting once the initial flow has dried up: this allows a distinction to be made between the two modes of elicitation which may be of importance when the account is subsequently appraised.

It is not easy for the researcher operating from a basis such as that of personal construct hermeneutics to overcome the problems of bias, distortion, and generalisability that threaten to overwhelm the validity and acceptability of the research. It has to be said that the precedents are not encouraging. Much of the research conducted within the framework of Kellian theory has been conceptually naive, falsely claimed to be objective, and partial; conclusions have often been unwarrantably generalised. Precedents notwithstanding, an attempt must be made to equip personal construct hermeneutics with a methodological framework which is consistent with its orientation towards the philosophy of history rather than the philosophy of science, and which will give research findings a reasonable prospect of attaining credibility. It is with this task that the remainder of this chapter will be primarily concerned.

13.8.6 A systematic approach to the elicitation of accounts

At the end of Chapter 8 I offered a heterodox critique of the repertory grid from the standpoint of the uniqueness and primacy of the event. In the present chapter I have sought to shift the theoretical bedrock of Personal Construct Theory sideways from science to history in order to provide a coherent philosophical foundation for an 'events-oriented' methodology which stands in contrast to the quasi-physicalism of the repertory grid. It is now possible to weave together these two strands of argument and to show how they might be used to develop a systematic approach to the elicitation of accounts. Whilst I have in mind research in natural settings, there is no reason why the approach which I outline cannot be applied to other research milieux.

I have argued strongly at various points in this text that both repertory grids and implications grids are very crude methods with which to probe respondents' lived experience. To make such a statement is not to imply that everything contained within grid methodology is to be rejected out of hand: there are aspects of both types of grid which are worthy of retention in the attempt to help an individual to reconstruct his or her experience.

As far as I am able to ascertain, the literature has little to offer on the systematising of the elicitation of accounts in such a way that the interviewer does not predetermine to a considerable extent the categories of response. Eden's work with managers and their problems comes the closest to capturing the essence of the approach to be described below. Parsons et al (1983), in a case study of an individual teacher, blend a repertory grid with both observation and discussion of classroom practice in order to explore the teacher's construing of how pupils learn.

There are, however, a number of problems with their approach, notably the philosophical disjunction between the positivism of the grid and the anti-positivism of their interpretation, and the use of a grid focusing upon general learning to provide a base-line for an investigation centred upon the specific discipline of mathematics.

In what follows I draw mainly upon Eden's insights and my own experience and - of necessity - extrapolate speculatively from this base. In order to convey the essence of the approach I am proposing, I ground the description in a particular type of context rather than treat it abstractly, believing that extension to other circumstances is in principle relatively straightforward.

The hypothetical research problem is the investigation of teachers' construing of teaching methods, with particular emphasis on their effectiveness. For the purposes of my argument I shall concentrate on one lesson given by a teacher to a class of pupils, and shall assume that the researcher is observing the lesson but that no mechanical recording devices are available - in other words, there are limitations on the 'sharing' of the event by the teacher and the researcher. I shall further assume that the researcher records a chronology of the lesson's structure and of those happenings construed to be of significance in its development.[104]

It would not be untypical of educational research if the teacher's cognate account of chronology and critical incidents was collected at the end of the lesson, perhaps being tape-recorded. Lessons in a secondary school often last for about 40 minutes, and one lesson contains such a wealth of intentions and actions that it is unrealistic

to expect the teacher to produce in a 'free' account a comprehensive record of all the construing that took place during that time.[105] A 'free' account is, like a conversation, prone to meander and leave potentially fruitful leads unexplored. And prompting conducted by the researcher from recollection or notes may suffuse a hitherto unremarked incident with an importance with which the respondent would not have credited it, and hence introduce a distortion into the account.

A more systematic approach is desirable if the respondent's account is to be as full as possible, as undistorted as possible, and aligned with the happenings of the lesson. This is where some aspects of grid methodology - but not grids themselves - re-enter the arena. Instead of giving a 'free' account, the teacher can be asked to identify the salient aspects of the lesson as elements (Phase I). These elements could be expected to include the teacher him- or herself, and the salient pupils, situational features and incidents (including the teaching methods as practised), all of these being identified <u>by the teacher</u>. An explicit contrast is made here with Nash (1973) who chose to identify the significant events in the classroom himself: in a personal construct framework primacy must be given to the construing of the respondent.

In Phase II the respondent would be asked why the various elements were selected as salient, thus providing a set of initial constructs. It could be expected that some indications of relationships between the elements would emerge during this phase: for instance, in a science lesson 'lack of sufficient equipment' might be linked with 'pupils A, B and C fooling about' via the practical impossibility of running experimental work with sufficiently few pupils per group to ensure that all were fully occupied. During this phase - as during subsequent phases - there would

be the opportunity for the respondent to add any further elements springing to mind.

In Phase III the teacher would be asked to identify the key relationships (i.e. constructs) between the elements elicited. The outcome would be a mesh of elements linked by constructs, bearing upon and influenced by, the teaching methods used. It is very unlikely that the mesh would be complete at this stage, and the researcher will be faced with deciding whether to spend time in tidying up loose ends, or in following up the implicative relationships that seem to bear most closely on the problem being investigated.

At this point it is probable that any constructs elicited would be in unipolar form. The researcher can ask the teacher what the opposite pole of any construct might be, in the interests both of giving greater definition to the emergent pole and of offering the possibility of ascertaining the respondent's value-position in respect of the construct.[106]

The 'construct mesh' of Phase III is elaborated in Phase IV through the use of Hinkle's (1965) laddering technique to probe the superordinate and subordinate implications of the constructs already elicited. However, there is an important contrast to be made with Hinkle's method in that he chose to ladder constructs in the abstract, detached from events, whereas here the reconstructions of specific experiences are laddered. The distinction being made draws upon Eden et al's (1979,1982) technique of exploring the implicative relationships of constructs related to particular problems identified by managers in various contexts.

The presentation of the interaction between researcher and respondent

in terms of four phases implies a distinct procedural sequence, but the practicalities of the situations are likely to merge the phases to some extent. It is to be noted that Phase IV goes beyond what would be expected in a 'naturalistic' account, in that it probes implicative relationships. The purpose of this extra step is that it offers the researcher an enhanced opportunity of seeing the constructs as a relational whole and of submitting them to a hermeneutic process which (following Ricoeur) is both seeking deeper significances and suspicious of what is present.

So far, attention has only been given to the respondent's reconstructions: it will be recalled that the researcher noted a number of features of the lesson, the assumption being implicit that he or she had the experience to judge what might be worth writing down - a point to which I shall return in Section 13.9.1.

Once Phases I to IV of the elicitation have been completed, the researcher may introduce any item which he or she believes to be salient and which the respondent has not identified. This does not confer a licence to press particular happenings on to an unwilling respondent - rather, such material would be introduced with some diffidence. This could be illustrated by saying "I noticed that it was almost always the boys who volunteered answers to your questions on energy conversion. Do you think that that may have had an influence on the way the lesson went?" The initiative could be followed up should the response suggest it, using appropriate techniques from the phases already outlined.

The elicited information can then be drawn out as a cognitive map showing a number of relationships between the elements and constructs provided

by the respondent. This is likely to be a complex diagram from which the researcher may need to extract the key features. There is a virtue in discussing the diagram (in its original and/or reduced form) with the respondent in that to do so allows evidence and/or interpretation to be challenged. I make the point from my experience of discussing polar co-ordinate plots of grid data[107] with my respondents: occasionally a juxtaposition of features was quite strongly disputed, leading to further fruitful discussion. There is also to be borne in mind a respect for the 'ownership' of the data being used.

The presence of the researcher at the event offers the prospect of cross-validating the teacher's account. Further cross-validational evidence may be available from 'significant others' - in classroom research, these might be pupils present at the lesson. My own experience of teaching warns against confusing salience with obviousness. I will remember one blue-eyed boy who behaved well in my lessons yet who turned out to be the instigator of a number of threats to classroom control: such is the innocence of the beginning teacher!

Cross validation can be assisted by the appraisal of 'silent' evidence, in which would be included details of the setting, lesson plans and - more detached from the event - departmental statements and curriculum schemes.

The more fully the lesson (or, more generally, the event) is recorded, the greater is the amount of cross-validational evidence available, as is shown in Figure 13.5.

Level of observation of lesson	Cross-validational evidence
0. Not observed	A. Internal consistency of the account B. Evidence from other persons present at the event C. 'Silent' evidence regarding the context D. Evidence from the respondent about previous cognate occasions E. Cross-checking of the account with the teacher.
1. Observed. Notes taken but no electro-magnetic recording.	A to E above, plus F. Observer's records of the event.
2. Observed. Notes taken. Electromagnetic recording made.	A to F above, plus G. The recording

Figure 13.5: Possibilities of cross-validating an account of an event under three levels of observation.

At this point the researcher has collected the evidence and needs to cross-check it for consistencies and inconsistencies. This having been done, the next task is to interpret what has been collected.

13.8.7 Interpretation

In Section 13.5 an argument was presented for the metaphor of 'action as speech' in that it foregrounded the intentional dimension of human action.[108] To follow this metaphor allows the researcher a closer

approach to the respondent's intention than is legitimated by the metaphor of text, the degree of closeness being determined by the researcher's presence at, or absence from, the event under study. The distinction between the metaphors is,however, not clear-cut. The recording of the account makes possible analysis at levels ranging from the semiotic to the hermeneutic, and allows statements regarding intentions to be cross-validated against the body of the account (and any observations which have been made). Whilst the cognitions present at the time of the event are irretrievable, the meshwork of evidence allows inferences to be made regarding them and their systemic coherence to be tested. No more than this may be claimed for fear of tripping into the psychologistic pit. Despite this caveat the respondent's account is the primary source of evidence regarding intentions.

Palmer (1969) observes that 'it takes a great listener to hear what is actually said, a greater one to hear what was not said but what comes to light in the speaking'.[109] There are two inferences to be drawn here: first, that the researcher should be aware of the contextual potentials of the research (a contextually naive researcher may miss matters of considerable significance); and, second, that interpretation requires both prongs of Ricoeur's hermeneutic fork. I shall deal with the latter first, and leave a consideration of the researcher's contextual awareness till Section 13.9.1.

In interpreting an account it is necessary to scrutinise it at a number of levels and to test its consistency in terms of both internal and external criteria. Internal consistency is construed here in terms of an inverse function of the contradictions present, whilst external

consistency refers to the congruence of the account both with other accounts of the same event and with the researcher's observations. Taking the point of Kelly's Fragmentation Corollary, it is improbable that an account of any length will be completely self-consistent: or, in the light of the Individuality Corollary, that it will be fully consistent with external evidence. But inconsistency should not be mistaken for invalidity since it may reveal fundamental dilemmas or contradictions within the respondent's cognitions: these would be of direct relevance to research in the verstehen tradition. It is at this point that the systematic elicitation and elaboration of an account have considerable potential.

A hermeneutic approach to the evidence requires the researcher to look very closely at the respondent's choice of language and the ways in which the meaning-complexes interlock. The use of 'laddering' during elicitation allows an interpretation to be made of the super-ordinate and subordinate cognitions elicited regarding the event. Cognitions and actions can be interpreted within the contextual frame-work: idealised intentions may not stand up to the pressures of a 'tough' class. 'Action-as-speech' has to be construed within its social horizon, and the - probably implicit- rule structure exposed.

Interpretation carries its dangers - for instance, that the researcher will read into inconsistencies meanings to which the respondent would not subscribe. From the latter's point of view there may be syntheses available within which apparent inconsistencies can be subsumed - but this implies the need to return to the respondent to investigate whether such might be the case: in the Kellian tradition, Allport (1962) remarks that there is too often a failure to consult the

person's self-knowledge, the richest source of data.[110]

So far, interpretation has taken the account at face value, seeking connections among the manifest. This may well be inadequate since, as Jane Austen wrote in 'Emma':

> 'Seldom, very seldom does the complete truth belong to any human disclosure; seldom can it happen that something is not a little disguised, or a little mistaken.....' 111

'Seldom' is probably not strong enough, as psychoanalysis and the structuralist school of literary criticism would aver. Further, Dunn (1978) casts heavy doubts upon the 'sparse and somewhat randomly selected records of states of consciousness which ever enter a public domain and the far sparser set of such records which remain at all durably within one', and suggests that retrospective statements are a 'nastily mangled palimpsest'[112].

Whilst Dunn's criticism is forceful and has to be acknowledged, it has to be borne in mind that all verbal statements are retrospective in relation to thought processes but may nevertheless be the best available pointers towards them.[113] Further, it is surprising what can be done with artefacts possessing several layers of meaning, whether these are differentially erased or concealed. The practical researcher has a range of forensic techniques for the detection of erasures and overwriting: such, in a cognitive sense, are the skills of the psychoanalyst or literary critic. And, like these two, the researcher with a personal construct hermeneutic standpoint will seek to reveal what is 'un-said'.[114]

The 'un-said' here is circumscribed by the context of the research. In

my use of grids with the probationary science teachers, the 'un-said' could be interpreted inter alia in terms of constructs relating to discipline and control (and which were not elicited by the grid technique yet plausibly could have been expected to emerge). The researcher is entitled to ponder upon-and perhaps probe - why such constructs have failed to appear or have been 'forgotten'. In the circumstances mentioned above the explanation might simply lie in the respondents' construing of the research in terms of aims and objectives rather than in terms of the managerial aspects of science teaching. On the other hand, it may be that these omissions reflected an unwillingness (and here one might speculate on whether this was a matter of consciousness or unconsciousness) to bring into the open aspects of teaching in which the teachers may have been self-aware of being weak. If, for instance, a lesson has been observed it is likely that evidence suggestive of an interpretation will be available - though there is no guarantee that the parties will agree, as Rolph and Rolph (1982) found.

Inconsistencies, mentioned a few paragraphs ago, can have a darker significance in that they may reflect contradictions between surface and depth meanings, between espoused theory and theory-in-use. Here, as with the 'un-said', the second prong of Ricoeur's hermeneutic fork can be used to prise away the mystifying veneer from the existential substrate - which is not to suggest that the latter would be recognised by the respondent if he or she were confronted with it (the influence of social determinism comes to mind in this respect).

The concept of consistency based on 'external' criteria has to be handled with care given that personal construct hermeneutics is grounded in the individual's 'existentiale'.[115] Taking the Kellian view that the

world really exists (even though each individual can only come to understand it within the framework of a personal construct system) the researcher has to identify those objective aspects of events which can act as anchoring points or nodes of evidence in the account. Not that parties to the events being studied will construe them in the same way even if they share a broad world view: what an observer might construe as a loss of control in the classroom might be construed by the teacher in terms of a rightful freedom for pupils.

Difficulties arise when there are inconsistencies between the account and the observations of the researcher. Clearly the construing of either (or both) may be at fault and, if negotiation to resolve the differences proves unavailing, it will be necessary to turn to other sources of cross-validation (see Figure 13.5) even though to do so may go beyond the original intention.

The validity of accounts and interpretations is always open to question. Consistency might be indicative of a post hoc rationalisation rather than of recall. Inconsistency may merely reflect inadequate recall or, at the other extreme , may indicate a failure to sustain a tissue of lies across a lengthy account. There is no simple relationship between consistency and validity. The researcher has to satisfy him- or herself that the account is, as far as is possible, a reasonable repre-sentation of the person's existential position at the time of the event, and that the interpretation is justifiable in terms of the evidence.

As Tajfel and Forgas (1981) observe, information in the social environ-ment, particularly that concerning people, is ambiguous and lacking in clear criteria for validity.[116] Consequently both disconfirmation of

interpretations and confirmation beyond reasonable doubt become processes requiring a substantial amount of evidence. Where this is not available, a multiplicity of possible interpretations may coexist, though these are likely to be differentially plausible.

Words andbehaviour are the objectifications from which hermeneutic understanding is necessarily constructed,[117] and these provide the justification for attempting to ground interpretation in both the researcher's experience and analytical skills. Since the researcher can never gain access to the mental processes of the respondent, interpretation can only be couched in terms of hypotheses which may turn out to be false.[118] Some hypotheses may be more plausible than others: Hirsch's (1967) notion of 'validation' takes as the best interpretation that which is most likely to be true on the evidence available (the criterion being qualitative probability),[119] and he draws a sharp distinction between validation and verification. Hirsch's position is consistent with Ayer's (1975) observation that the process of proof should come to an end at a point which satisfies the researcher[120] (by which is meant coherence with the interpreter's current belief system). Ayer is careful to point out that this is not an absolute end of an inquiry andthat any question can be reopened should sufficient reason arise - and, in reporting research, it would seen essential that interpretations be presented in conjunction with the maximum amount of evidence which is feasible in order that readers of the report may consider alternative possibilities.

This last point is important in that there is always the possibility of misunderstanding: communications can so easily be misinterpreted because of incongruence in contextual assumption between researcher

and respondent. Posy Simmonds captures the essence of the point in her cartoon 'A many splendoured thing' (Figure 13.6) - though not every misunderstanding is blessed with such a happy ending.

Figure 13.6: 'A many splendoured thing' by Posy Simmonds. (Reproduced with the permission of the author and The Guardian).

In the cartoon George apparently settles for the first plausible interpretation that suggests itself to him,[121] In hermeneutics the semiotic and the semantic are combined in a continuing cycle of guessing and validation until one or more interpretations can be adjusted to meet the criterion of consistency with the evidence. At this point the chains of reasoning can be laid out as formal explanations in order that they may be appraised by others. Rigorous research demands no less.

13.9 SOME BROADER IMPLICATIONS FOR RESEARCH

A methodology based upon personal construct hermeneutics implies (at least) the following:

(i) that the researcher requires sufficient experience of the topic of the research to enable him or her to study it in depth;

(ii) a research context which maximises the possibility of a dialectical approach to interpretation;

(iii) that the research is likely to demand a more extended period of contact between researcher and respondent than is typically the case; and

(iv) that the generalisation of research findings will require abstraction at a level higher than that of the data itself.

13.9.1 Experience of the researcher

The range of work on ideal types, prototypes, genres, schemata and scripts, when coupled with a Diltheyan *verstehen* perspective,[123] implies that the researcher needs to have a repertoire of theory and experience pertinent to the research topic if he or she is to penetrate it in depth. In research into classroom processes, for example, it will be necessary to have an understanding of both educational factors (teaching strategies, pupil characteristics and responses) and relevant contextual factors (such as school ethos, parental expectations and the nature of the catchment area) in order to set the research data - be it observation of action or account - against perhaps unarticulated possibilities.[124]

Kitwood (1976), in the context of a critique of research methods in education, makes the point that teachers have a repertoire of tacit

knowledge which may not be available to "research professionals", and which as a body of commonsense knowledge, systematised, tested and extended might form the authentic raw material for educational inquiry.[125] Unfortunately for Kitwood's suggestion, it appears that he has overlooked the point (partially made explicit earlier in the same article[126]) that teachers on the whole are not research-oriented. There would hence appear to be practical difficulties with the concept of "teacher-as-researcher". The general approach to methodology outlined in the previous section offers a way out of the problem caused by the separation of a teacher's experiential understanding of the classroom and a researcher's theoretical and technical expertise - but it does require thelatter to have, in addition, a measure of broadly cognate classroom experience capable of being integrated into the framework of the research.[127]

Implicit in this "specification" for the researcher is the capacity to be alert to the subtleties of meaning embedded in events (such as the things that give the game away in the classroom [128]). Eisner's (1979) notion of 'educational connoisseurship' catches something of the point,[129] and he goes on to draw the parallel with the connoisseur of wine who has to have both drunk a large quantity of wine (spread out over a long period, it must be said) and retained in memory many fine discriminations.[130] Given the analogy, it is not surprising that some of his (presumably relatively youthful) doctoral students were weak at interpreting and evaluating the teaching episodes which they observed and overindulged themselves in fanciful metaphor.[131]

A mature understanding of a situation has as a prerequisite maturity in experience of both theory and practice -a combination not possessed by all intending researchers in the human sciences.

13.9.2 Dialectics of interpretation

Whilst wide and differentiated experience of theory and practice is potentially of considerable advantage, no researcher can be omni-competent in these respects. Each person is to some extent blinkered by his or her own theoretical position, and there will be inevitable lacunae in the theory and practice of research.

A hermeneutic approach to research requires that the available evidence be probed in such a way as to explore a variety of possible interpretations. Since this variety will be maximised when the number of informing theories and associated insights is maximised, it leads to the suggestion that researchers might work more profitably as a team, pooling ideas and being critically supportive. This would require them to put their intended methods, and their evidence and interpretations, before each other and to expose them to alternative theoretical positions with a view to refining them before committing themselves to a public stand on, and defence of, the outcomes of the research.

Dialectic teamwork in research can, and does, take place in some situations but, where research is conducted on a part-time basis (and perhaps with limited opportunity for contact with a supervisor or other students), such an approach is difficult to sustain. Since many part-time researchers have an accumulation of experience in the context of their research which many newly graduated students beginning in research do not, the difficulties associated with the establishment of the dialogues necessary to hermeneutics are doubly unfortunate.

13.9.3 Contact between researcher and respondent.

From time to time critiques appear in the literature regarding the

superficiality and triviality of much research in the social sciences and the continuing reliance on approaches imported from the domain of science. As an example, Carlson (1971) reviewed the articles published in 1968 in the Journal of Personality and the Journal of Personality and Social Psychology, and reported that 78 per cent were 'experimental', 20 per cent were 'correlational', whilst the remaining two per cent were related to observation in naturalistic settings. The time-scale of the various researches followed a similar distribution, being overwhelmingly 'single-session' studies (78 per cent): only 15 per cent spread over a period of up to one month, and the remaining seven per cent taking longer than one month. Carlson remarks trenchantly on the validity of research into personality when the dominant approach is limited to a time of the order of one hour, raising such questions as: how can the researcher expect to know more than a few facts about any respondent; what can be inferred about the stability of personality; and what can be generalised from a sample of predominantly 'late adolescents'?[132]

Eisner (1979) offers much the same critique of educational experiments (in this case basing it upon a review of papers appearing in the American Educational Research Journal over a two year period)[133]. Noting that modal time for experimentation is one hour, he suggests that it is likely that such research will produce little of significance for educational practice.

Both Carlson and Eisner, from their respective spheres of interest, argue for more emphasis to be given to longitudinal studies in which careful observation in naturalistic settings would play a large part.[134] Whilst the researcher would have to have a purpose and rationale for

conducting the study this (often ethnographic) approach would allow, and indeed would demand, that unanticipated events be incorporated and that the study would not be blinkered by the researcher's ideological and methodological preconceptions.[135] It would appear that both Carlson's and Eisner's positions are not inconsistent with personal construct hermeneutics: Eisner, in fact, nods approvingly in the direction of Dilthey, Simmel and Schütz[136], but does not elaborate the relation between these writers' theoretical positions and his practical approach to 'educational connoisseurship'.

Carlson and Eisner, however, stress observation of events rather than the construing of individuals. Both of these approaches to research require the investment of considerable time and more contact between researcher and respondent than is typical in the human sciences. There are differences in the way in which this time is likely to be used during research based on construct theory. If one were exploring the construing of a teacher in the classroom this might necessitate observation and/or recording of a lesson, a post-lesson structured interview with the teacher, possibly interviews with significant others, possibly analysis of cross-validating documentary evidence (such as lesson plans), a careful appraisal of all the notes, recordings and evidence, and perhaps a further interview with the teacher concerned in order to test the validity of the interpretations reached. A single lesson could produce a vast mass of evidence and interpretation, but the corollary is that a research programme based on this type of approach would have to be limited to a relatively small number of cases. Such is the penalty to be paid for attending to the contextual evidence and, in particular, to the construing of the respondents themselves.

Although a procedure of this type assumes an initial frame of construct theory, it is more concerned with the further building of theory on an 'infill' basis. There is, therefore, an implicit intention towards generalisation through the means of theorisation. This mode of operation stands in marked contrast to the easier option in research - so often taken - which is to administer tests, attitude scales, questionnaires and suchlike to a large sample, thereby collecting a wealth of information in a form amenable to statistical processing.[137] Here, where theory is being tested, generalisation is of a fundamentally different kind, and the circumstances are such that questions of meaning, intention and social significance [138] are easily overlooked.

13.9.4 Methodological individualism and generalisation

Research based on Personal Construct Theory or a derivative implies a methodologically individualistic approach, and hence sets a limit to thescale of the enterprise. The following discussion accepts that methodological individualism is unable to respond adequately to the broad, nomothetic demands of the human sciences. However, its comparative inability to cope with large samples is offset by its potential contribution to theory-building through detailed analysis of individual cases.

Earlier in this chapter I made a connection between the Kellian perspective and a methodology grounded in the philosophy of history. Stenhouse (1978) sees case studies as the instrument of an analytical history, and argues that they contribute to the refinement of judgment rather than of prediction:

> 'In the face of an unfolding experience which is largely unpredictable history attempts to equip us to understand the unpredicted by being able to fit it very rapidly into a systematically ordered and interpreted grasp of experience so far'. 139

Day-to-day experience provides commonsense theory. Case study research attempts to draw on systematic enquiry in order to build theory on more secure foundations. Case studies ('the examination of instances in action'[140]) do not take place in isolation, for they are grounded in theoretical assumptions. The instances sampled reflect the theoretical patterning underlying the investigation, but will almost always be consistent with more than one theoretical substrate (the issue of overdetermination, prevalent in the human sciences, was noted in Sections 2.5 and 13.2).

Kazdin (1980) summarises the advantages and disadvantages of case study work.[141] The latter include the well-known difficulties of selection - of the case(s) to be studied, in the respondent's utterances, and in the researcher's construing and reporting. The suggestions made in Sections 13.8.6 and 13.8.7 go some way towards meeting these objections, but cannot completely overcome them. Similarly, the problems with postdictive accounts are partly countered by cross-validational strategies including both observation and the comparison of the focal person's account with the accounts of others. Further, case studies are claimed to be weak in terms of their capacity to lead to generalisation and prediction.[142]

However, to attack case studies for their predictive weaknesses is to assail straw men, for prediction is not their purpose. Nor are case studies necessarily chosen on the basis of sampling statistics with direct generalisation in view. Frequently undertaken in natural settings, they allow the entry of variables that a controlled experiment might eliminate and, as Kazdin points out in respect of psychology, have a unique contribution to make in suggesting variables that may

account for behaviour.[143] This argument suggests they have a particular potential for expanding theoretical frameworks and for suggesting interconnections.[144] In other words, they offer the prospect of contributing to a superordinate theory within which subordinate theoretical statements may be differentially weighed. Such would be my interpretation of Stenhouse's 'refinement of judgment', noted earlier.

The study of individual instances is not divorced completely from generalisation, however.[145] Location of cases within a set of theoretical assumptions implies their membership of a generic type (though the research might ultimately indicate that the assumed typification was incorrect) whilst at the same time they are treated as unique. Case studies stand at the balance point of opposing tensions in research activity - the needs to particularise and to generalise. This tension is clearly present in the theoretical/practical stance of this chapter, for the methodological individualism which is central to the collection of evidence is set against the nomothetic concept of genre which provides the framework for interpretation and explanation.

Care has to be taken with the concept of genre, however. It can be used at the idiosyncratic level by the respondent who construes an instance as being one of a particular type, or by the researcher who construes the respondent's construing in a similar generic way. In either case there are no clear limits to what may be included within a particular genre and thus the comprehensiveness of the concept is indeterminate. At the broadest level it is possible to construe genre as synonymous with Weltanschauung. A detailed discussion of the nature of genre is not attempted here, and for the present purposes genre will be taken to mean a superordinate framework capable of subsuming the

data under discussion, and its use will be confined here to the researcher's interpretive activities.

The advantage of genre is that it is a fuzzy concept [146] capable of subsuming a range of events not too distant from a central, and perhaps hypothetical, ideal type or prototype: one might draw a parallel with statistical terminology and speak of an instance lying, say, within one standard deviation of a mean. In such an approach an event can only be subsumed within a genre to a degree of probability warranted by the evidence, yet this may be sufficient justification for action in the life-world. There is no attempt here to underpin interpretation with a tight set-inclusion system typical of formal logic: rather, genre recognises a measure of indeterminacy in the event-genre relationship not dissimilar from that embedded in Atkin's (1981) notion of cover sets and Laudan's (1977) idea of research traditions(though the latter is at a level of abstraction beyond the intentions of the current discussion).

The fuzziness of genre imposes costs. Parsons, in criticising Weber's ideal type, draws attention to its abstractness and generality andto the indeterminacy of its relation to action.[147] Further, the assumption of an ideal type could restrict the possibilities of variation among the components of the ideal type if they were construed as having to fall within a particular rubric.[148] Parsons's criticism has some force in that an event may, in Kellian terms, be pre-emptively construed as nothing but an instance within one particular genre when it could be construed in more propositionalterms. But Parsons's position implicitly seeks cut and dried causality in circumstances in which the sheer complexity of events renders this extremely difficult

to determine. It has to be faced that it is impossible to locate any item of evidence unambiguously within a particular genre since it is open to a range of superordinate constructions (in other words, it is always overdetermined by theory). In accepting the overdetermination of data, hermeneutics attempts dialectically to narrow down the possible range of interpretations and to suggest a 'best reading' according to the canons of procedural evidence rather than those of statistics (which nevertheless have an acknowledged subsidiary role in many circumstances).

Genre in the social sciences is regarded here as a dynamic, rather than a static concept. Each piece of evidence is interpreted in relation to the generic framework assumed by the researcher, and thus may alter it or require the researcher to switch to a different framework (the parallel with the situation characterised in Figure 3.1 is quite close). As an example of the evolution of genre-albeit at a rather general level - one might cite the transformation of behaviourism from its Watsonian origins to its current stance of neobehaviourism in which intervening processes are allowed between stimulus and response.

The identification and evolution of genres, then, are important. The processes are abductive [149] in that they require the researcher to go beyond the data to try to identify underlying rules, principles and theories spiralling outward from the initial assumptions. There is a sharp contrast to be drawn with the actuarial-inductive generalisations of the life insurance industry and of those methodological individualists such as du Mas (1955) who construe nomothesis in terms of the summation of idiographies.[150] Arguing for a methodologically individualistic approach to research, Harré (1978) points out that a 'social collective' is not a statistical aggregation of individuals but a supra-individual

ensemble whose general principles can only be elucidated (abductively) on the basis of detailed particularistic studies of typical members.[151]

However, some detailed particularistic studies may not yield the type of generalisation which I am seeking to depict. My point is quite well illustrated by a comparison of studies by van Kaam (1959) and Rowe (1978). Van Kaam asked 365 students to give descriptions of the experience of 'really feeling understood' and subjected these to a content analysis according to a predetermined schema which included the reduction of 'concrete, vague and overlapping expressions' to 'more exactly descriptive terms in order that the constituents of the experience could be classified'.[152] Passing over some difficulties in van Kaam's conceptualisation of the study, the generalisation here is little different from that summation of idiographies challenged in the preceeding paragraph: the focus is on the common at the expense of the superordinate dimensions capable of subsuming the idiosyncratic. Van Kaam's philosophical position is revealed when the states that his approach is mainly concerned with the laws that govern human experience.[153]

Rowe on the other hand, whose work has the advantage for the present purposes of having been conducted in the light of construct theory, used a very different approach in her study of depression. She presents nine very detailed case studies of depressives in such a way as to show how depression is related to a group of propositions which 'enclose' a person and cut him or her off from interactions with others.[154] Although Rowe does not make the explicit claim, her generalisation regarding depression is a new genre to which the Kellian constructs of threat and anxiety make a prominent contribution.[155] In extending Kellian theory Rowe's genre is at a level of abstraction above the

evidence she collected and yet is sufficiently in touch with people's experiencing to be useful in both clinical practice and research. Her formulation is capable of coping with a wide range of specific instances of depression (none of which need to evidence all the constructs of 'enclosure') in a more complete way than van Kaam's prematurely normative rubric.

Abductive generalisation in the sense I attribute to Rowe requires a rigorous hermeneutic approach to the evidence, pursuing its interconnections and contradictions to the limits of feasibility. This is a far cry from what Stake (1978) terms 'naturalistic generalization' which is (circularly) 'arrived at by recognizing the similarities of objects and issues in and out of context and by sensing the natural covariations of happenings':[156] such might reflect the thought-processes of a Kellian personal scientist or the initial abductions of a more rigorous scientist, but they would need a much sounder base before they could aspire to the superordinate relationship with evidence indicated in the preceding paragraph.

It must be emphasised that generalisations of this type cannot be expected to subsume perfectly the subordinate evidence because of the sheer multidimensionality of influences on human action. No individual case is likely to present all the facets of the genre yet the person may be construed as suffering from depression, for example. If such superordinate generalisations are not to be dismissed as glib and naturalistic (in Stake's sense), then it is incumbent on the researcher to supply a considerable amount of detail in order that the reader may follow the development of the theoretical argument being advanced.

Where the evidence being discussed is largely - and possibly wholly -

qualitative in nature the linearity of report-writing may make it difficult to illustrate the inherent network of relationships. In a typical experimental report the researcher has available the convenience of statistical summaries, and in standard grid use there is the opportunity to summarise the grid content in a number of diagrammatic ways such as through polar co-ordinate plots. With qualitative research it should be possible to summarise the argument being presented in terms of a mapping of implicative relationships in a manner similar to the output of Eden's COPE program.[157] Such mappings would be needed at two levels; first, at the level of the individuals' construings and, second, at the level of the argument constructed by the researcher from the sets of idiographic data. Such a device offers the prospect of a solution to the minimax problem regarding evidence and may increase the findings' chances of entering the public arena. This issue is crucial, for without communication research is an empty venture.

14 Envoi

We shall not cease from exploration
And the end of all our exploring
Will be to arrive where we started
And know the place for the first time.

T.S. Eliot
Little Gidding

14. ENVOI

Looking back over the preceding chapters, I am aware of how much the halting dialectic between theory and method has been smoothed in the linearity of sequencing. In undertaking this study it has been necessary to alternate my stance between placing my weight on the leg of theory whilst testing the ground with the foot of methodology, and vice versa. To describe matters thus is to offer an untidy description of walking, but it perhaps captures the saccadic nature of progress.

Although there are many books on theory and method in the human sciences, these tend to focus on one aspect to the virtual exclusion of the other. Further, methodological compendia rarely indicate the limits within which a particular method might hold and the extent to which method and theory might cohere. Although the present study originated as, and has been concerned to a large extent with, a critique of method, a superordinate conceptualisation has slowly emerged:[1] it is therefore more appropriately characterised as an investigation into the theory of social methodology,[2] despite my intention to produce a text that would be of direct value to the practice of research.

This study has drawn attention to a number of pathways which personal construct theoreticians and methodologists might find it fruitful to explore: for instance, language, memory, meaning, intention and action spring quickly to mind. In touching on these and other issues, it has become all too apparent that the reach of this study has been greater than my grasp, the hope greater than the achievement. I am thus no less penetrated with a sense of the unsatisfactoriness of the present condition of my knowledge[3] than I was at the outset.

Yet some modest aims have been fulfilled. The repertory grid has been subjected to a severe and sustained scrutiny. Although there are points at which the appraisal could have been extended, sufficient evidence has been collected to call the grid seriously into question - whether it be used as the sole instrument of research, or as one among a number of other instruments. The greatest weakness, on the analysis presented here, is the grid's restricted potential for communication: its positivistic reductionism denies the construer a full chromatic scale of meaning and leaves the researcher the daunting task of reconstructing semantic and intentional richness from a rectilinear abstraction - like a Mondrian canvas, devoid of other than primary colours.

To challenge methodology is a soft option, given the theoretical pluralism and conflict in the human sciences. To expose weaknesses tells little more than is known already, and does not offer much by way of a direct and positive contribution to research. There are, however, advantages of an indirect kind, for the exposure of methodological weaknesses encourages the discarding of outworn orthodoxies and clears the ground for new developments.

The critic, in my view, has an obligation to indicate a way out of the impasse created by the criticisms that have been advanced. The preceding chapter is a response to that self-imposed imperative: it is less an attempt to rebuild shattered stones than an architect's design study for a more firmly founded, but perhaps less imposing, methodological edifice. The empirical test of this design-sketch has yet to take place, and it is not improbable that further theoretical work will prove to be a necessary precondition.

Others might produce different and superior solutions to the problems

identified in this study. My belief is that such solutions are more likely to be based on qualitative rather than quantitative data. In consequence, analysis and interpretation can be expected to require a quasi-legal approach to the handling of evidence. Disciplined inquiry[4], perhaps involving something akin to grounded theorising[5], may well prove to be more valuable to the human sciences than some of the positivistic methods of investigation currently in wide use.

To conclude, I would argue that my criticisms of the repertory grid constitute not a drawing-down of blinds, but an opening up of possibilities to many Kellians who have hitherto construed the grid as the primary instrument through which personal construing may be captured. In this study I have been able to offer, as an alternative,

'...only hints and guesses,
Hints followed by guesses...'

which require, if not prayer, further 'observance, discipline, thought and action'.[6] Eliot's words frame an appropriate charter for research in the human sciences.

In this end is my beginning.

Notes

NOTES TO CHAPTER 1

1. The translation of Geisteswissenschaften which I prefer in that it implies (for me) a systematic, rigorous approach to the acquisition of knowledge which, in the light of the etymology of 'science', can go beyond methodology grounded in observation and experiment. Others might prefer 'human studies'.

2. See Radnitzky (1968, I: 15-16).

3. My interest in the early years of teaching developed steadily during nine years varied science teaching in comprehensive schools, four years in teacher education and (in 1978) two years in staff development in higher education.

4. Each chapter is divided into sections and, where appropriate, subsections. Section 12.2 implies Section 2 of Chapter 12.

5. I have chosen to write of theoretical statements in the present tense, taking the view that these continue to 'live' in the literature even after the death of their advocates. Research findings, in contrast, relate to past events and are hence described in the past tense.

   This work is written from a committed personal construct standpoint (though one not entirely congruent with Kelly's), and this accounts for an extensive use of the first person singular.

   A number of references are made to translations, printings and reprintings which post-date the first publication. Where I have been able to identify these the reference is asterisked. Schutz (1967*) thus refers to 'The phenomenology of the social world' which was first published in German in 1932, an indication of origin being included in the References.

6. To make this statement is not to disparage the natural and applied sciences.

7. The situation may well be rather different in clinical psychotherapy per se: my concern here is with the repertory grid as a technique of research and not with the legitimacy of its place in the therapist's case of instruments.

8. In which the barrel-vaulted ceiling is painted to give the impression of tall columns and much greater height, but this illusion only holds from one particular vantage-point. From any other point the curvature of the "columns" is obvious.

NOTES TO CHAPTER 2

1. Novack (1975: 15)

2. Kelly (1955: 154)

3. Peirce: Collected Papers ed. Hartshorne and Weiss (1934), volume 5 paragraph 423. Referenced hereafter in the form CP 5.423.

4. James (1907: 57-8) wrote that 'ideas ... become true just in so far as they help us to get into satisfactory relation with other parts of our experience', thus allowing truth to be interpreted as that which suits us, and hence leaving himself open to be charged with formulating a hedonistic criterion of truth. Elsewhere in the same volume James describes truth in terms of agreement with reality (p.201) and usefulness (p.204). The problem seems to lie in the meaning of 'satisfactory' which, in James's writing, remains ambiguous even after his later attempt to clarify his position. (See, for instance, James, 1909: 190, 207).

5. See Ayer (1968: 39).

6. Popper (1972b: 78) writes of certainty 'enough for practical purposes'. If people construe (believe, define) a situation as 'real', then it is 'real' in its consequences, irrespective of the way in which the construction is grounded. Or as Peirce (CP 5.568) put it, inverting Berkeley's dictum, 'The percept is the reality'.

7. Quoted in Popper (1972b: 60)

8. Kelly (1955: 6)

9. Kelly (1978: 228)

10. Festinger (1957)

11. Dewey (1916a: 176). Dewey (1933: 107) also presents five phases of reflective thought which I find to be a less satisfactory sequence. Where this formulation is superior is in its recognition that hypotheses may be tested by imaginative action as an alternative to empiricism.

12. Scheffler (1974: 237) criticises Dewey for allowing thought to subside once the main hypothesis has been corroborated or refuted. Kolb's (1971: 28) less tightly specified experiential learning model , which has affinities with Kelly's theorising, would appear to avoid this problem.

13. Kelly (1955: 48). Von Bertalanffy, (1973*: 280-1) takes a similar view. Shaw and Bransford (1979: 3-4) note the tendency to forget the active, investigatory qualities possessed by humans and animals. Peirce, CP 7.50f, describes science as a mode of life (see Peirce, 1958*).

14. Dewey (1930: 85) writes of 'ordinary procedures' in terms of experiments for the sake of knowing, and leading to commonsense knowledge. Kelly (1970a) summed this up in the title 'Behaviour is an experiment'.

15. Kelly (1955: 8-9). Kelly's position, hovering between realism and rationalism, presents a philosophical problem in that the reality which construct systems are supposed to approach is only accessible via the construct systems themselves.

16. As Kelly (1955: 8-9) is aware, the 'match' need not necessarily be a good one; there are various ways of construing the world, some of which may be better than others (1955: 15-16). The goodness-

of-fit depends in part upon the construer's frame of reference. Kuhn (1970: 50-1) gives a good example when he describes the difference between physicists and chemists regarding their construing of the helium atom: physicists opted to construe it as an atom because it gave no molecular spectrum, whereas chemists argued that it behaved like a molecule with respect to the kinetic theory of gases.

17. Kelly (1955: 9).

18. Peirce (CP 5.467) describes pragmatism in terms of the ascertaining the meanings of "intellectual concepts" pertinent to 'objective fact' but not feelings. That this is a social theory is indicated in CP 5.402, note 2. Buchler (1978*) makes the point that Peirce introduced the concept of meaning into empiricist methodology (p.xi) and that Peirce's thinking is fundamentally opposed to methodological individualism (p. xiv).

19. Kelly (1955: 16).

20. It will be argued, passim, that in his writing (and, a fortiori in the repertory grid) Kelly did not fully extricate himself from a positivist position, a point made by Rogers (1956: 357) when he observed, reviewing Kelly's Psychology of Personal Constructs, that his theory 'superimposes normative thinking upon the phenomenological data'.

21. The existentialist position is that people are their own choices: Kelly seems very close. It will be argued, later in this chapter, that this underestimates the power of society to determine the individual's choice.

22. Kelly (1955: 11) indicates his awareness of the danger of extending the range of convenience of theories. Feyerabend (1978*:43-4) points out that, when a theory is extended beyond its original boundaries, it can turn into a rigid ideology.

23. Kelly (1955: 36-7).

24. Dewey (1916a: 163).

25. Rychlak (1973: 502).

26. Watts and Pope (1982: 4) point out that Kelly's 'scientist' appears to arrive on the scene viviparously.

27. J.O. Katz (1982b) has proposed, as an addition to Kelly's theoretical structure, 'The Origin Postulate' which makes the assumption of some innate, phylogenetically-rooted constructs capable of elaboration through postnatal experience. This prompts me to recall that young chicks will cower when a bird-silhouette is moved above them in such a direction that the silhouette has a hawk-like motion, yet are unconcerned when the direction of motion is reversed, i.e. the silhouette loses its 'hawkishness'.

28. This is a more complex issue than is represented here in that many responses to stimuli are unobservable yet may act as further stimuli leading to observable responses: such would be a neobehaviourist view of response to the environment's effects.

29. See, for example, Holland (1970; 1977). The importance of social and historical forces was recognised a century ago by Dilthey (Hodges 1974*, 170). Stefan's (1977) account of his investigation of change in core constructs is revealing here, in that he found group processes among high-school students blocking the role-play which he was trying to get them to undertake in order to change their core constructs.

30. Hawkes (1977: 14), drawing on Vico, puts the matter bluntly:

> '... customs and rites act as a forceful brainwashing mechanism whereby human beings are habituated to and made to acquiesce in a man-made world they nevertheless perceive as artless and "natural".'

Belsey (1980, Chapter 1) takes a similar view. There is an irony in Kelly's use of 'transparent' here, for transparency suggests that the templets may be unnoticed by the wearer and, therefore, may not be subjected to scrutiny - and possibly change. It will be argued shortly that the templets may, on occasion, act as blinkers.

31. See T. Mischel (1964: 184f). However, constructs can only become rules in contexts defined by elements. Mischel's critique of Kelly's position is inadequate in a number of respects, particularly in respect of his failure to recognise the significance of the event (i.e. interaction between something happening and someone construing it). Harré and Secord (1972), (and Harré subsequently, in a number of papers) make a sustained case for rule-governed behaviour.

32. Attneave (1959: 30).

33. Taylor and Fiske (1978: 250-1). They further suggest (p.252) that people frequently respond, with little thought, to the most salient stimuli in the environment, whether they be trivial or not. Further, novelty tends to elicit spontaneous attention (p.257).

34. Bruner's (1957) view of categorisation is suggestive of this type of bias in human functioning. He describes a three-stage process: primitive categorisation, cue search, and confirmation (p.130-131) and observes that first impressions are difficult to disconfirm (p.142). Hirsch's (1967) view regarding the identification of 'genre', which is discussed in Chapter 13, seems very similar. Shaw (1980a: 147) - perhaps inadvertently - describes a confirmatory strategy when she writes that the personal scientist 'is collecting evidence to support his theories'.

35. See Kelly (1955: 14-45, 500, 940-941).

36. Nagel (1961: 99)

37. Popper (1972a: 91-2).

38. Kelly (1955: 23-4); my emphasis.

39. Kelly (1955: 24).

40. See page 23 above.

41. For a discussion of Weltanschauung in scientific theorising, see Suppe (1977: 125-221 and 633-649).

42. Kelly (1955: 46).

43. Kelly (1955: 24).

44. Nagel (1961: 99).

45. Kelly (1955: 24).

46. Kelly (1970b: 1-2).

47. Kelly (1955: 13).

48. Cf. , inter-alia, Vannoy (1965).

49. Contrast, for instance, Bannister's (1960, 1962) notion of 'intensity' with Makhlouf-Norris et al's (1970) classification of 'segmented', 'articulated', and 'monolithic' structures.

50. D.E. Bolton (1978: 195).

51. See Kelly (1955: 28-30).

52. Kelly (1955: 29).

53. Kelly (1955: 46).

54. Bannister and Mair (1968: 36).

55. Kelly (1955: 452).

56. Kelly (1955: 810-811).

57. Kelly (1955: 810).

58. Mackay (1975: 129).

59. On this point Kelly (1955: 10) writes: 'No one has yet proved himself wise enough to propound a universal system of constructs. We can safely assume that it will be a long time before a satisfactorily unified system will be proposed'.

60. For instance, see Kelly (1955: 8-9), Hanson (1958: passim), Popper (1972a: 107), Laudan (1977: 15) and Harris (1979: Chapters 1 and 2, passim).

61. Feyerabend (1978*) would perhaps claim this loosely-connected pluralism as an advantage to theory-building (cf. especially p.47-53).

62. Kelly (1955: Chapters 9 and 10).

63. Reflecting the unintentional and the intentional, respectively.

64. Kelly (1955: 510).

65. Kelly seems here to have 'told it truer than he knew', in that the

Lakatosian position (which would probably have appealed to him) post-dates him by more than a decade.

66. Laudan (1977: 76-78).

67. Laudan (1977: 78f).

68. See the discussion in Laudan (1977: 48f)

69. Popper (1970: 56) is explicit on the issue of truth,

70. Empirical investigation may never progress so far as to reveal the absolute truth.

71. Kelly (1955: 85f) grounds his view of consistency in anticipation, whereas here consistency is to be construed in terms of explanation.

72. Contrast Kelly (1955: 12,15) with (1955: 13). Warren (1964: 11) takes the view that the theory is wholly coherence-oriented.

73. Laudan (1977: 84); author's emphasis.

74. Cf. Kelly (1955: 24, 46).

75. Laudan (1977: 85); the author emphasises the whole quotation.

76. Atkin (1981: 22f)

77. This can be seen, for instance, in the distinction he draws between 'normative' and 'phenomenological' thinking (Kelly 1955: 455), in which the former is linked with behaviour and the latter with personal constructs.

78. Cf Kuhn (1970).

79. Kelly (1955: 5f).

80. Aspects of biology, geology and geomorphology also come to mind in this respect.

81. Adams-Webber (1979: 213-214); my emphasis.

82. Kelly (1955: 940-941).

83. 'The appraisal of experiences and activities' (Kelly 1955: Chapters 13,14) demonstrates the point, the emphasis being on description, interpretation and explanation. Kelly only makes a direct link with the historian in his discussion of the use of the client's autobiography (p. 989), in contrast to Meehl (1954: 65) who explicitly refers to the clinician's need to reconstruct history 'from fragments chiefly on the dependent variable side'.

84. Kelly (1955: 810-811).

NOTES TO CHAPTER 3

1. Bannister (1977: 26-7) laments the construal of Personal Construct Theory as a cognitive theory (cf Bruner, 1956; Mackay, 1975; Peck and Whitlow, 1975; Rogers, 1956), but the vast majority of research emphasises the cognitive aspects of construing. The use of repertory grids forces conation and affection through the filter of cognition.

2. Sorting tasks, such as those used by McGaughran (1950) and Scott (1962, 1963a) do not bring language overtly into the evidence of construing. That is not to claim that language is not involved in the process of construing - cf, for instance, Dearborn (1910).

3. Kelly (1955: 46). The discussion of the Fundamental Postulate is based upon Kelly's elaboration (p. 46-60) in which the unattributed quotations may be found.

4. The ambiguity of 'processes' is highlighted by Kelly's elaboration of 'ways' in the Fundamental Postulate, whose meaning seems to become increasingly elusive the more one thinks about it. Of 'ways' Kelly (1955: 49) writes

> 'The channels are established as means to ends. They are laid down by the devices which a person invents in order to achieve a purpose. A person's processes, psychologically speaking, slip into the grooves which are cut out by the mechanisms he adopts for realizing his objectives.'

'Channels', 'devices' and 'mechanisms' are all means to ends, yet their conceptual level is indeterminate. If one conceives of 'processes' at different levels of abstraction, distinguishing - say - cognition from purposive behaviour, one can begin to resolve the ambiguity. In the same manner, Kelly's elaboration of other key terms in the Fundamental Postulate can be shown to be inadequate. Despite Kelly's claim for the flexibility of the 'network of pathways' the metaphor 'channelized' seems unnecessarily mechanistic, and is perhaps more appropriate to binary switching in computers or the functioning of individual nerves than to the complexity of the brain's neural interrelationships. Schütz (1967*: 61) draws on Bergson (1910*) to indicate that the making of a choice should not be considered in terms of spatial metaphors. One could envisage, as alternatives, a field potential metaphor along the lines suggested by Lewin (1952), or a more 'fluid' metaphor such as the delocalisation of electrons in complex organic melocules. These might prove more fruitful contrasts to channelization than Kelly's 'straw mannish' contrast with 'fluttering about in a vast emptiness'.

It is interesting to note that, in an earlier formulation of the Fundamental Postulate used by Landfield (1951) and Lyle (1953), the notion of 'channelizing' did not appear:

> 'A person's psychological processes evolve towards what he construes to be an optimal anticipation of events.'

Whilst this presents some difficulties, the notion of evolution manages to avoid the mechanistic connotation of the latter formulation.

5. The diagram is an adaptation of Kolb's (1971: 28) experiential learning model which seems to emphasise accommodation at the expense of assimilation. Piagetian theory leads me to suggest a distinction between accommodation and assimilation (whilst recognising that these will overlap in practice), and Rosch's (1975 et seq.) papers on'prototypes' suggests that a 'pattern-matching' operation might determine whether assimilation or accommodation were the predominant cognitive strategy to be employed. The diagram, which is consistent with Tesser's (1978) conceptualisation of the development of cognitive schemata, is an over-simplification of what is likely to be occurring during a learning cycle, since the 'search' procedures are themselves dependent upon the existing cognitive structure. Given the evidence presented earlier regarding people's scientist-like behaviour, it may well be that the mapping process is 'adjudged' satisfactory on the most superficial and inadequate of criteria - hence the 'inertia in construing', discussed earlier. The diagram is also over-simplified in that I have limited it to my own interests in respect of the present work (cognition and purposive behaviour) and have not sought to apply it to 'processes' in general.

6. See Kelly (1955: 64f) and the discussion of the Choice Corollary in this chapter (see p. 87f below).

7. Dewey (1916a: 117-129, especially 123-124). See also the discussion in Scheffler (1974: 229-239).

8. Schutz (1967*: 59-61).

9. Kelly (1955: 72f, and especially 170f). The reaction of the individual to adventitious events is not discussed explicitly until page 170, although his discussion of the Experience Corollary (p.72f) can be interpreted with these in mind. One is left to wonder whether his opposition to behaviourism led Kelly to marginalise the reactive aspect of experience by locating his discussion of it so distantly from his discussion of the Fundamental Postulate.

10. Pepper (1942: 253).

11. It is important to distinguish my cognitions of the event from my subsequent anticipatory behaviour which is governed by the cognitions. If this differentiation is not made, the situation is reduced to that stated in Kelly's Fundamental Postulate, 'processes' being construed in terms of behaviour.

12. Taylor and Fiske's (1978) suggestions have already been mentioned in this respect (see page 28 above).

13. Kelly (1970b: 3).

14. The flexibility of Kelly's position is echoed by Duhem (1954*: 16) who writes 'No metaphysics gives instruction exact enough or detailed enough to make it possible to derive all the elements of a physical theory from it'.

15. As, for instance, by D.E. Bolton (1978: 194).

16. Kelly (1955: 50): Kelly's elaboration is given on p.50-55. The Corollary is rather clumsily expressed. For an event to be anticipated, it cannot yet have impinged upon the person and hence no replications exist to be detected and construed. In his explication of 'replications' Kelly makes it clear that he means that people detect recurrent patterns in their day-to-day experience and use these as the basis for predicting future events.

17. Kelly (1955: 50-51).

18. Implying a prescriptive framework for interpretation: see Bannister and Mair (1968: 26) and Kelly (1970b: 13).

19. Kelly (1955: 126f)

20. Kelly (1955: 129). Or, more briefly, 'Constructs are hypotheses' (p.940). Kelly would have been on safer ground had he referred to <u>constructions</u> as rules or hypotheses (see the discussion in the following paragraph and note 30).

21. Kelly (1955: 129).

22. T. Mischel (1964: 182).

23. T. Mischel (1964: 184f, especially 185).

24. T. Mischel (1964: 182-183)

25. Harré and Secord (1972: 269). J. Wilson (1969: 38) makes a similar point about the ambiguity of terms.

26. Kelly (1955: 13).

27. Hirsch (1967: 78f)

28. Bateson (1973*: 245).

29. Bruner, Goodnow and Austin (1956: 246).

30. Warren (1964: 12) makes this distinction. In referring to proper names as constructs, Kelly (1955: 114) does not. One might extend Warren's point to the underlying verbs 'to construe' and 'to construct', and suggest the following:

    (a) 'to construe' is to place an interpretation on events, using previously laid-down constructs as yardsticks, thus producing 'constructions' (which are essentially assimilative), whereas

    (b) 'to construct' is to build up a 'construction system' (essentially accommodative, in that it implies an increase in the number of dimensions available as yardsticks).

    The notion of rules, discussed earlier, suggests a further terminological coinage: perhaps a set of rules should be termed a 'constriction system'.

31. Kelly (1955: 59). Kelly's elaboration is to be found on p.59-64.

32. Kelly (1955: 68). The Corollary is elaborated on p. 68-72.

33. Schütz and Luckmann (1973: 172) also describe a 'relevance conditioned' contrast theory of meaning.

34. In that judgments of similarity and difference are likely to be influenced by, and/or determine, the implicit rating scale metrics.

35. The negation/opposition category of bipolarity hovers a little uneasily between negation and opposition, and it depends on one's view regarding the effect of lexical marking (see section 8.3.5). If what appears as a non-contiguous opposition is shown to be subsumed beneath a superordinate concept, then it comes close to falling into the 'opposition' category.

36. Or function: I am using 'property' very broadly here.

37. For example, 'tall-short' subsumed under the concept of height.

38. What appears to be a non-contiguous opposition appears in Kelly's (1955) own writing: see the first row of Figure 3 opposite p.271. A selection of personality constructs drawn from my own group-administration of grids (which method presents problems - see Section 8.2.1) includes 'happy-serious', 'unreliable-capable', 'sulky-placid', 'nosey-reserved', and 'forthright-obtuse'. It may be the case that the respondents did have some superordinate in mind, but evidence was not collected on the point.

39. Kelly (1955: 60).

40. Kelly (1955: 114).

41. Ogden (1967*: 92-93). The point is illustrated in Ogden's discussion of the polarities between ruler and ruled (p.68-75), man and brute (p.72-75) and love and hate (p.87).

42. Though not exclusively. For instance, A. Ryle's (1975) 'double-dyad grid' has elements which are relationships and constructs which are verbs.

43. In his original presentation of the repertory grid Kelly (1955: 271-272) comes close to retaining the concept (as opposed to the construct) in his acceptance of negations as constructs (see Figure 3, opposite p.271) and in the light of his assumption, for analytical convenience, that any element not assigned to the emergent pole can be subsumed under the implicit pole.

44. The example is from Leech (1974: 99). There is, however, a danger in pressing the attack on bipolarity too hard: Grant (1963*), in my view, defeats his own intentions in this respect.

45. See Kelly (1955: 763) for an indication of the value of the construct 'sick-free'. Slater (1977: 43) and Eden, Jones and Sims (1979: 47, 107 and passim) argue for this point.

46. Holland (1977:161). Austin (1971*: 92) also warns of the danger of assuming that a word must have an opposite, or (only) one opposite.

47. Miller and Johnson-Laird (1976: 260); see also Lyons (1977: 287-290). J.J. Katz (1964), Bever and Rosenbaum (1971*) and Hinkle (1970) also make claims for explicit multiple oppositions. Straus (1966: 166) refers to the implicit multiple oppositions in the otherwise near-tautologous statement 'The weather is fair'. Watts (1978*: 32) offers a similar point of view to Straus, seeing the explicit statement as the figure against a ground of implicit possibilities.

48. Kelly (1955: 48).

49. Kelly (1955: 63).

50. Though whether the brain actually operates in either of these ways is another question.

51. E.E. Smith (1978: 7) suggests that there is no reason to believe that dictionary-type information is stored separately from encyclopaedia-type information. For instance, whilst 'bachelor' and 'spinster' are defined as unmarried, each has accessible implications - the former implying 'young', the latter implying 'past marriageable age'. In their later model Collins and Quillian (1972) give more emphasis to the storage of encyclopaedia-type information.

52. Loftus and Loftus (1974) suggest that search strategies may vary according to the degree to which the categories have been learned. Loftus (1977) indicates, further, that search strategy may vary according to the nature of the task: finding an attribute may be a different process from finding a name. It may be that the formal demands of repertory grid methodology are a daunting task for respondents who are relatively unacquainted with the subject-matter of a grid-based inquiry.

53. These two, and other, models of semantic memory are reviewed in E.E. Smith (1978) and Bransford (1979). Battig and Belleza (1979: 325) suggest that the complexity and instability of organisation in memory implies a more complex, flexible and non-deterministic account than is offered by the information processing assumptions currently dominant in cognitive psychology. Pribram (1977: 84f) suggests that information is stored along holographic principles allowing both simultaneous access to items and parallel processing.

54. Some features of the 'slow worm dilemma' can be laid out in a grid-like table.

FEATURE / CONSTRUCT 1	0	LIZARD	SNAKE	SLOW WORM
Legs	- No legs	1	0	0
Eyelids	- No eyelids	1	0	1
Ear openings	- No ear openings	1	0	1
Fleshy tongue	- Thin tongue	1	0	1
Sheds skin in bits	- Sheds skin whole	1	0	0
Can break off tail	- Cannot break off tail	1	0	1
'Lizardish' appearance	- 'Snaky' appearance	1	0	0

Quine (1969: 128-129) gives the example of the marsupial mouse which is, in general appearance, more similar to ordinary mice than to the kangaroo - yet on genetic criteria the reverse is the case. See also Kuhn (1970: 50-51), a brief outline being given in note 16 to Chapter 2.

55. Martin and Caramazza (1980: 321f). Mancuso and Eimer (1982) refer to overlapping sets of probabilistically assigned features, picking up the current interest in the possibilities of fuzzy set theory (see section 11.4).

56. Rosch's series of papers (some of which are under her former name, E. Heider) includes those cited in the bibliography.

57. For the distinction between induction and abduction see Peirce (CP 5.167-174 and 180f).

58. As would be the case with 'family resemblances' (Wittgenstein, 1953: 32e). Bambrough (1968*: 189) gives a schematic illustration of the problem. Stereotypes may be constructed in this way, as a result of the need to cope with an overload of information, much of which may be ambiguous: for a discussion of stereotyping see Stewart, Powell and Chetwynd (1979). Cauthen, Robinson and Krauss (1971: 118) see stereotypes as clusters of connotative and denotative words in dimensional space, with circumstances altering the stereotypes. For instance, the American construal of the Japanese altered in emphasis from 'intelligent' to 'sly' as a result of World War II.

59. Shepp (1978: 164).

60. Garner (1978: 101).

61. Mancuso and Adams-Webber (1982: 19) see prototypes as self-standards in the 'anticipation-surprise-motivation cycle'. M.T, Taylor (1976: 185) makes the point that concepts (such as 'bus') depend upon a unique co-presence of defining attributes, though the context of judgment will highlight certain attributes at the expense of others. In other words, positive attributes are necessary to the concept's meaning: oppositional contrast provides refining and further contextual support for positive instances. In many contexts of construing, however, it is likely that element complexity requires a 'fuzzy' interpretation of Taylor's position.

62. Given the evidence for the inadequacy of the naive scientist (see section 2.4), it would seem likely that much similarity data is open to bias.

63. Attributed to O.G. Selfridge by J.R. Anderson (1980: 43). F. Heider (1958: 48) discusses the resolution of ambiguous stimuli.

64. The issue of similarity/difference is complex, as Gregson (1975) and Tversky and Gati (1978) show. The two terms are not clearly inversely related (Torgerson, 1965: 390; Gregson 1975: 86; Hollingworth 1913: 273). As the stimulus being judged becomes more complex, judgments of similarity become increasingly sensitive to matters such as attitude and decision-making strategy (which are not intrinsic properties of the stimulus): see, for instance, Forgas (1979: 270). Even where the number of parameters is small (as in

Eisler and Knoppel's, 1970, study of geometric figures) the person may flick from one to another during a sequence of judgments. In his study of handwriting similarity, Hollingworth found his respondents reporting such flickering of attention - though his task was rather more complex.

65. See Chapter 13, page 512.

66. Bannister and Fransella (1980: 21).

67. See Kelly (1964). A reading of the early pages of Vaihinger's volume gives a useful background to Kelly's theoretical position.

68. Kelly (1955) appears not to distinguish clearly between dichotomy and bipolarity (compare, for instance, p.59f with p.106). Bipolarity does not necessarily imply dichotomy since the latter is a special case of the former (see Bonarius 1965: 27). On the topic of contrast, Ogden (1967*: 92-93) observes that when (abstract) adjectives are treated in terms appropriate to sensations, a variety of vague contrasts may emerge.

69. Kelly (1955: 271).

70. Lemaire (1977*: 117) and Lindsay and Norman (1977: 490) make this point.

71. Kelly (1955: 61).

72. Kelly (1955: 56). The discussion below relates to p.56-59.

73. Fransella (1972: 74).

74. Kelly (1955: 58). Kelly's view is consonant with the suggestions of Schroder, Driver and Streufert (1967) regarding integrative complexity: the parallel is discussed in Section 4.6.

75. For example, Piaget (1950: 119-155, esp. p.150-153); Werner (1957: 126f); E.J. Gibson (1969: 75f). Quine (1960: 4) holds a similar view with respect to the development of formal theory. The change from 'black-white' dichotomous thinking to greater differentiation is exemplified in de Beauvoir (1963*: 14f, contrasted with later pages), Applebee (1976) and Gould (1981: 163). Hayakawa (1965: 230f) points to what might be termed the 'political pathology' of the two-valued (dichotomous) orientation.

76. Honess (1976) concludes that cognitive complexity lacks an adequate theoretical base. Empirical evidence for this view is offered, inter alia, by Vannoy (1965), Kuusinen and Nystedt (1975) and Seaman and Koenig (1974). Epting (1972) and Goldstein and Blackman (1978: 129) indicate that cognitive complexity may not be stable across subject areas. It is perhaps worth making the point that, if cognitive complexity is so elusive, its use as an independent variable in research is unlikely to result in significant differences in dependent variables (cf. Leventhal and Singer, 1964).

77. See ten Kate (1981: 169 and 172).

78. See Section 4.7.

79. Kelly (1955: 58).

80. Kelly (1955: 55). His discussion is on p.55-56.

81. Kelly (1955: 72). See p.72-77 for Kelly's elaboration.

82. Bakan (1956: 658).

83. Kelly (1955: 56). See also p.93.

84. Kelly (1955: 13) refers to the ability of people to assess their construing at a higher level of abstraction. McCoy (1975: 141) points out the circularity of in/validating constructs on the grounds of construing experience. The circularity is not vicious in that it is a 'spiral staircase'. The similarity with Dilthey's 'classificatory circle' (Makkreel, 1975: 340-342) is worth noting.

85. Kelly (1955: 83). Kelly discusses the Corollary on p.83-90.

86. Kelly (1955: 77). See p.77-82 for Kelly's elaboration.

87. Landfield (1977: 145) remarks that, whilst the Fragmentation Corollary implies some complexity of structure, its essence is not captured in the wide-meshed net of 'cognitive complexity'. Holland (1977: 250) interprets the problem of 'double bind' as implying contradictory injunctions at different levels, which in some cases makes unbearable consequences inevitable, given the range of responses apparently available. The bind can only be broken when the person can communicate about the communication process itself.

88. Bannister and Mair (1968: 22). The argument is repeated in Bannister and Fransella (1980: 26-27).

89. Weick (1968: 512).

90. Kelly (1970b: 20). Kelly modified his original formulation (1955: 90) to make 'psychologically' qualify 'similar' rather than 'processes'.

91. Kelly (1955: 95). The Corollary is discussed on p.95-103.

92. Holland (1970: 128).

93. Kelly (1955: 97-98) does not help the reader in that his own construing of 'role' hovers between a psychological process akin to <u>verstehen</u> and a pattern of behaviour derivable from understanding. Construing the other's construct system is at the heart of the 'interpersonal perception method' of Laing, Phillipson and Lee (1966), and is exemplified in Pask (1975: 388) and Childs and Hedges (1980). The latter authors, in studying the difficulties experienced by a married couple, curiously eliminated from repertory grids any construct which was not agreed by <u>both</u> partners as important.

94. Holland (1981: 26-27) stresses the importance of ideologies and power in social processes. He points to Kelly's beginnings as a 'role theorist' and to the subsequent definition of role with respect to the 'relatively sheltered context' of psychotherapy where it is important to establish a moratorium from social pressures.

95. Bannister and Fransella (1980: 28).

96. Kelly (1970b: 15). His original formulation (1955: 64) refers to the 'extension and definition' of a person's system.

97. Tyler (1981: 31).

98. Sartre (1948*: 29) remarks that to choose is to affirm the value of the chosen: it is impossible to choose the worse. The tautology is preferable to Kelly's Corollary. Kelly (1955: 733) makes a statement very similar to Sartre's when he discusses human behaviour.

99. Kelly (1955: 171). See also Bannister (1975: 132), who refers to the obsessional as 'endlessly successful in repeating his non-elaborating cycle ...', and von Bertalanffy (1973*: 225) who refers to the atrophying of potentiality in those parts of organisms that are not exercised. Habit-formation would also appear to be fundamentally non-elaborative.

100. Bannister and Fransella (1980: 23).

101. Holland (1970: 130).

102. Bannister and Fransella (1980: 23).

103. For instance, Marcuse (1964); Schütz and Luckmann (1973); Tajfel (1977).

NOTES TO CHAPTER 4

1. Kelly (1955: 219f). The Repertory Test is not to be confused with the repertory grid.

2. Kelly (1955: 242f).

3. Kelly (1955: 267f).

4. Thomas (1978: 49).

5. Salmon (1978: 42).

6. D.E. Bolton (1978: 193-4).

7. Watts and Pope (1982: 14).

8. Kelly (1955: 219) is, therefore, probably wrong to characterise the Vygotsky block-sorting test as a concept-formation procedure: rather, it would seem to be a procedure involving concept selection and integration.

9. E.g. Bartlett (1932); Cofer (1973).

10. Neisser (1967: 289).

11. Olson (1981: 273), who also investigated the constructs of science teachers, remarks that his respondents often said that the interviews had helped them probe their own thoughts, some of which they were

unaware of. Thompson (1975: 19), working with nursery school teachers, also notes that they found the procedure interesting and challenging. I also obtained a number of similar reactions in my work. Whether this implies reproduction of stored constructs, reconstruction, or 'completely new' construction is unclear.

12. It is also indeterminate whether the elements flick between these levels of construction or reconstruction during the process of completion of the grid.

13. This point is due to Mary Tyler (1982, personal communication) who drew my attention to the problems of trying to construe people known some time ago (and with whom contact had not been maintained) in the same grid as a number of current acquaintances. There is a further point: words change over time their meanings for the individual. Murdoch (1970: 29) notes that 'courage', for example, may mean very different things at age 20 than at age 40. One might say the same of 'love'.

14. Kelly (1955: 272).

15. Kelly (1955: 301-2).

16. Kelly (1955: 307).

17. Ziman's (1978: 14) tart comment seems pertinent here: it is 'all too easy to derive endless strings of interesting-looking but untrue or irrelevant formulae instead of checking the validity of the initial premises'. It is equally dangerous to assume that failure to find a correlation implies no implicative relationship: Henry (1981: 356), for instance, claims that in 'segmented' structures independent judgments are made which have no implications for each other.

18. See, for instance, Harri-Augstein (1978: 93).

19. Ziff (1972: 72).

20. See Eden, Jones and Sims (1979: 77-87).

21. Rosenthal (1969: 181). This observation is set against a lengthy discussion of 'contaminating' experimenter effects. Laing, Phillipson and Lee (1966: 8) make the important point that a conversation between two people A and B is a dialogue of the form $a_1 - b_1 - a_2 - b_2 - a_3 - b_3$ ..., but that A might only 'see' $b_1 - b_2 - b_3$ ... in intrapersonal terms and overlook his or her own influence on what B has said.

22. Bannister and Fransella 1980 (179-181). The point is made less explicitly by Kelly (cf 1955: 5, 96-97).

23. Weick (1966: 231).

24. Labov (1972*) noted the marked difference in black children's willingness to communicate once a change was made from a white to a black interviewer, coupled with changes in the format of the discussion.

25. Reid (1976: 234).

26. Lipschitz (1972: 33).

27. E.g. Makhlouf-Norris, Jones and Norris (1970: 271). Smith and Leach (1972) confuse hierarchy of cognitive structure with the hierarchical dendrograms of cluster analysis.

28. Kelly (1955: 302). By 'perceptual' Kelly would appear to mean 'conceptual' or 'constructional'.

29. The problem would seem to be greater with small grids where the samp ling of elements and constructs is likely to be 'thin'.

30. Goldstein and Blackman (1978: 173) observe that almost all research into integrative complexity has been undertaken in the area of interpersonal relations.

31. There are some similarities with Carnap's (1967*: 98) construction system. Carnap places basic relations (in Kellian terms, constructs) rather than objects (elements) at the beginning of the system. It might be argued that to debate about the epistemic primacy of elements and constructs is to indulge in pseudoproblem-solving in that neither is meaningful without the other: they stand locked in dialectical embrace.

32. Kreitler and Kreitler (1976: 71).

33. Landfield (1982: 209) recognises the problem. He presents an example of a teacher's construing in which 'good-bad' is unrelated to other bipolar constructs to which it would normally be expected to be related (e.g. 'organized-disorganized'), and shows that, whilst 'good' implied 'organized' etc., there were no reverse implications. The situation Landfield describes may be rather more complex: 'good' may well imply a number of positively evaluated poles; one may be 'good' despite some negative evaluations; one could be positively evaluated on a number of dimensions and yet be construed as a 'bad' teacher for reasons which may or may not be apparent from the grid data. It is just possible that one could be negatively evaluated on a number of dimensions and yet be construed as a 'good' teacher. My own experience in teacher education would lead me to recognise the possibility, inter alia, of the following clusters (given the construct dimensions and evaluative loadings provided by Landfield):

    'Good' (in the traditional authoritarian sense) = organised (+)/ distant (-)/ unhumorous (-)/unimaginative (-);

    'Bad' = disorganised (-)/warm (+)/humorous (+)/imaginative (+).

34. See section 6.5.4 and Adams-Webber (1979: 56-58).

35. An extended summary of Hinkle's thesis is given in Bannister and Mair (1968: 78-96). A briefer treatment is given in Fransella and Bannister (1977: 42-49).

36. Kelly (1955: 222).

37. H.H. Kelley et al (1955: 149, 156) see the method of pairwise comparisons as superior to the method of equal-appearing intervals in that it elicits fine discrimination where the latter would 'bunch' items and thus fail to discriminate.

38. Bannister and Mair (1968: 78-96, especially 87,90). This example is chosen because of the relative inaccessibility of Hinkle's thesis.

39. Hinkle (1965: 59).

40. Armstrong and Eden (1979: 21) noticed that their respondents to grid elicitation would refer back to activities which generated outcomes rather than focus on the latter themselves (as was wanted). This difficulty in remaining task-specific leads one to wonder whether the same problem might present itself more generally in laddering procedures.

41. Hinkle (1965: 47).

42. See Kelly (1955: 13); Diesing (1972: 161); Eden, Jones and Sims (1979: 148).

43. Bannister and Mair (1968: 136).

44. Chetwynd (n.d.: 1-2).

45. Bannister and Mair (1968: 138).

46. Kelly (1955: 47).

47. Kelly (1955: 41-42) indicates that this does not preclude nomothetic use.

48. Allport (1962: 419).

49. Coulter (1974*: 151) takes the view that Kelly's grid procedures are based on a positivistic demand for 'hard' data from what are, in effect, descriptions of a continually changing experience.

50. Kelly (1970b: 3).

51. And very few mentions of the Role Construct Repertory Test. Kelly's psychotherapeutic methods seem close to those of the existential analysts (cf. May, Angel and Ellenberger, 1958).

52. My own work with the science teachers (Appendix 1.), as originally conceived, is open to this criticism of conceptual naivete.

NOTES TO CHAPTER 5

1. The distinction between elements and constructs is a matter of convenience rather than of principle. I follow tradition in this respect, treating the constructs in the repertory grid as the dimensions in respect of which the elements are sorted.

2. Kelly (1955: 219).

3. See Section 6.5.1.

4. See the example in Kelly (1955: 270ff).

5. Kelly (1955: 215).

6. This is a dual problem: the grounding of the construing is uncertain, and (as Ziff, 1972: 31, remarks) the prevailing circumstances predispose the hearer to resolve ambiguities in particular ways. Anderson et al (1977) present empirical evidence to this effect.

7. Mair (1967: 269). Fransella and Bannister (1977: 116-7) make a similar point and suggest that this might account for some confusing results.

8. As Hargreaves (1977), having worked in schools himself, points out. It may be simply an oversight on Nash's part that he did not consider the possibility that teachers might construe their pupils 'outside the classroom'. Alternatively, he may have considered the distinction irrelevant. Canter (1977: 31) gives an example of two people construing New York in different terms - one as around 42nd Street, the other as the area of Greenwich Village.

9. Eggleston, Galton and Jones (1976) took a similar view of context when, in their study of styles of science teaching, they limited their work to a narrowly defined group of pupils in the penultimate year of O - level courses.

10. Bannister and Mair (1968: 203).

11. Pope (1977: 13). Smith (1978), analogously, asked for items 'important in industrial relations' with reference to a grid on management training.

12. The elements listed by Students No.8 and 19 are more accessible in Pope and Keen (1981: 120).

13. Pope discarded from consideration any construct on which an element was rated 'not applicable'. The number of constructs actually used in the research is likely to have been markedly smaller than the number elicited, since it is difficult for all of a group of heterogeneous elements to be subsumed under any but the broadest of constructs. There may well have been problems in providing constructs for some of the triads: one can imagine the difficulty of providing a construct when faced with the triad LEAs/Arts/Boredom, for example.

14. See (accessibly) Pope and Keen (1981: 123).

15. Other respondents, who were teachers (but not of physics), were asked to construe physics teachers <u>as if</u> they were undergraduate students of physics: Keen (1979: 111).

16. <u>T</u>eaching <u>A</u>ppraisal by <u>R</u>epertory <u>G</u>rid <u>E</u>licitation <u>T</u>echniques. This approach to teacher self-appraisal is described in Hopwood and Keen (1978) and Pope and Keen (1981: 94-97).

17. Two examples, each of which deals with the case of a single teacher, are given by Bell (1980) and Rolph and Rolph (1982).

18. A similar criticism can be made of Perrott et al (1976), who asked their respondents to construe a series of personally-relevant teachers (see p.347f).

19. This was the intention in the work conducted by Olson (1980a) and myself (Appendix 1).

20. Kevill, Shaw and Goodacre (1982: 47).

21. Shaw and Thomas (1978: 141).

22. Kelly (1955: 230).

23. Kelly (1955: 307).

24. Pope and Keen (1981: 42); Easterby-Smith (1981: 10).

25. Easterby-Smith (1981: 10).

26. Easterby-Smith (1981: 10).

27. If 'irrelevant' elements remain in the grid throughout, they may have an effect on the constructs produced and on the ratings given to other elements.

28. E.g. Thompson (1977).

29. Slater (1977: 128). Karst and Groutt (1977: 71) explicitly acknowledge the problem.

30. Keen (1979: 117, 119); Hopwood and Keen (1978: 192).

31. It was noted earlier (Section 5.3) that Kelly's list of role titles might not always prove adequate.

32. Easterby-Smith (1981: 11).

33. Olson (1981: 264).

34. Kelly (1969: 137) suggested that respondents might become partners in the design of the research, a theme echoed by some contributors in Reason and Rowan (1981). Not every researcher would probably wish to go as far.

35. As in the Bannister-Fransella grid test of schizophrenic thought disorder (Bannister and Fransella, 1966).

36. Luchins's (1942) work on the '<u>Einstellung</u> effect' comes to mind here.

37. Reid (1976: 241).

38. Pope (1977: 82).

39. See Section 5.2.

40. Shaw and Gaines (1979: 5-6).

41. Stringer (1979: 94) chose to ask one set of students to construe a set of role titles whilst another construed people chosen to fit the role titles. The validity of the former seems questionable. D.H. Hargreaves (1980: 225) observes that different responses are likely when general, as opposed to specific, cases are considered.

42. Easterby-Smith (1981: 11).

43. And between individual constructs.

44. Whether these are 'meaningful' in any deep sense is doubtful: it would appear that they cannot be considered to be salient.

45. It is difficult to see how the researcher can discern whether an appropriate sample of constructs has been obtained.

46. Kelly (1955: 1058-9).

47. Collett (1979: 246).

48. See Mair's (1967: 269) suggestion regarding the failure to obtain a sharp bipolarity. Weick (1968: 516) makes a similar point.

49. Not that stability is to be elevated into a superordinate virtue (see Section 10.2).

50. Kelly (1955: 48).

51. By examining change in the grid columns (treated separately). See Section 10.6.

52. A similar point can be made in respect of constructs.

53. In some instances the semantic differential scales may have been inapplicable to the concepts being rated.

54. Bannister and Mair (1968: 170f).

55. See Appendix 10.

56. Kelly (1955: 210).

NOTES TO CHAPTER 6

1. Kelly (1955: 229-231 and 270-272).

2. I.e. with no vacant cells.

3. Landfield (1971) offers a solution to the problem which extends dichotomous allocation of elements.

4. For an explication of 'emergent' and 'implicit' poles, see Kelly (1955: 269).

5. The term is borrowed from geology.

6. I.e. constructs which are not semantic opposites: see Resnick and Landfield (1961).

7. See, for example, Oetting (1967); Carter et al (1968).

8. M.T. Taylor (1976) observes that the term 'construct' underscores the inventive and creative aspects of categorisation and pointedly asks whether the grid retrieves everyday thinking.

9. See page 21 above.

10. I draW here on the distinction between espoused theory and theory-in-use (Argyris and Schön, 1974).

11. The question of language is treated in greater detail in Chapter 11.

12. See Ravenette (1977: 262-3) for a cognate example.

13. Kelly (1955: 231).

14. Kelly (1955: 272).

15. Kelly (1955: 233) offers a procedure for investigating whether two constructs are functionally equivalent, but its cumbersome nature does not seem to have commended it to researchers.

16. See, for example, Jane's grid in Shaw (1980a: 79).

17. There is a similarity with a correlation grounded upon an unarticulated superordinate relationship.

18. The parallel here is with the overdetermination, by the latent, of a dream's manifest content (cf Freud 1954*: Chapter 6, p.283-284 and passim).

19. The grid format, often with small spaces for construct labels, may act as a constraint here.

20. Although this assumption is not related to construct elicitation, it is convenient to consider it here.

21. Kelly (1955: 273).

22. For example; Bender (1969), Reid (1976), McKnight (1977) and Keen (1979).

23. Bonarius(1971: 42-3).

24. For example; Fransella (1972), Shubsachs(1975) and Yorke (1978). Both Nash (1973: 106) and Easterby-Smith (1981: 10) note that the negative valence of full opposition may be resisted where people are construing acquaintances.

25. Easterby-Smith (1981: 12).

26. Kelly (1955: 230-1).

27. The example given between 270 and 271 in Kelly (1955) does not support this assumption to any great extent.

28. See Bannister and Mair (1968: 158) and Adams-Webber (1979: 21).

29. Bonarius (1965: 2).

30. See, for example, Fuller (1969: 210), Bradley and Eggleston (1978: 94) and Ikeobi (1978: 43).

31. Wood and Napthali (1975: 152).

32. Shaw and Gaines (1979: 6).

33. But Fransella and Bannister's (1977) manual is silent on the issue.

34. See Kelly (1955: 231-2). This is not to suggest that this is an upper limit to the number of constructs which the respondent might produce in other circumstances (see Section 11.3). Landfield (1977: 134) notes that Kelly did not intend that the constructs elicited via a repertory grid should be treated as the equivalent of all of the constructs within the system. Kelly (1955) gives little attention to this issue.

35. Bannister and Mair (1968: 49-50).

36. Pope and Keen (1981: 44).

37. Which is used in the general sense of Bieri's (1955) original formulation.

38. Bell and Keen (1981: 214). There seems no particular reason why the element intraclass correlation should <u>decline</u> to an asymptotic value (<u>contra</u> the assumption on p.211), since this coefficient will be dependent on the particular order in which the constructs were elicited. The data presented by Bell and Keen shows no consistent pattern of fluctuation consonant with their assumption.

39. See Appendix 3.

40. Collett (1979: 232).

41. Keen (1979: 126).

42. Federer (1955); Cochran and Cox (1957).

43. Schroder, Driver and Streufert (1967: 169) wrongly assume that the number of constructs is limited to the number of triadic sorts. I have found some triads to produce more than a single construct. Pope and Keen (1981: 45) see triadic elicitation as merely an aid, to be discarded when the respondent is able to produce constructs freely.

44. Stringer (1976: 194) suggests that a relatively large first component is indicative of a lack of discrimination.

45. Thompson (1977: 17) suggests that triadic elicitation could force the respondent to use trivial constructs.

46. Pope and Keen (1981: 47) describe the completion of a grid as an unusual task for many individuals.

47. See Kelly (1955: 224f) and Bannister and Mair (1968: 50f).

48. The distinction is clearly set out in Atkin (1981: 22-28).

49. Reid (1976: 443). The example given on p.702 suggests that he might have meant <u>poles</u> here, implying contrast rather than similarity.

50. Nash (1976: 94f).

51. Kelly (1955: 58).

52. Bannister and Mair (1968: 84-5).

53. Hinkle's discussion of implicative relationships is more accessible in Bannister and Mair (1968: 80) and Fransella and Bannister (1977: 49).

54. Landfield (1971: 134).

55. See Fransella and Bannister (1977: 50).

56. See Section 4.7.

57. Fransella and Bannister (1977: 18).

58. Gray (1980) used a preliminary telephone interview to elicit constructs which were later incorporated into a grid.

59. Fransella and Bannister (1977: 118).

60. Bannister and Fransella (1980: 79).

61. See Kelly (1955: 319f).

62. Ravenette (1980: 47).

63. Bonarius (1971: 27).

64. Kreitler and Kreitler (1976: 33f).

65. Bannister (1973: 2). Glossop et al (1975: 147-148) rejected the assumption of intersubjectivity and asked their sample of 13-14 year-olds for the meanings they were assigning to the constructs elicited from them.

66. Bonarius (1965: 26).

67. Adams-Webber (1970: 352).

68. Mitsos (1961) found that the nine most personally meaningful dimensions from a 21-scale semantic differential attracted greater rating extremity.

69. See Figure 6.1 (Section 6.4) and Appendix 3.

70. Fransella and Bannister 1977: (108-109).

71. Defined in terms of the tendency towards asymmetry in locating elements on constructs: see Slater (1977: 58).

72. Slater (1977: 93-94 and n.d. : 6.).

73. Although in a study of cognitive complexity Tripodi and Bieri (1963) had earlier found provided constructs to have greater stability than elicited constructs, suggesting that the former might be superior. One can expect that the relationship between stability, 'superiority'

and mode of generation of constructs to be complex and influenced by the context and psychological nature of the rating task.

74. See Section 6.5.1.

75. Fransella and Bannister (1977: 106).

76. Heather (1979) adopted the same principle.

77. Osgood, Suci and Tannenbaum (1957: 187).

78. Osgood, May and Miron (1975: 353).

79. Mann, Phillips and Thompson (1979: 221).

80. This pilot study is reported in Appendix 16. Rosenberg (1977: 204) gives a similar example of unexpected clustering in which a respondent associated 'stubborn' with 'independent', 'successful', 'determined' and 'industrious'. The idiosyncracy here seems, however, to be Rosenberg's. Further, Warren (1966) found that 'unusual' tended to be associated with negatively-valenced adjectives for working class students in further education, whereas no similar association existed for middle-class boys in a minor public school (p.260-261).

81. Olson (1979b: 27).

82. Kelly (1955: 156).

83. See Kelly (1955: 157).

84. Rowe (1978: 33).

85. Kelly (1955: 156).

86. See, for example, Stewart, Powell and Chetwynd (1979); Cauthen, Robinson and Krauss (1971).

87. The example is from Fransella and Bannister (1977: 100).

88. This is not a contradiction of the point made on page 156 (note 17 refers): I am arguing that the cause of high statistical associations between constructs is very often likely to be obscure, and that it is dangerous to jump to conclusions.

89. Easterby-Smith (1981: 14). There are, no doubt, some occasions when such constructs are of interest in their own right.

90. I am led to wonder what the effect would have been of eliminating a similar number of abstract constructs from each grid.

91. Kelly identifies other types of constructs which will not be considered here: Bannister and Mair (1968: 220-221) give a convenient summary.

92. Fransella (1981: 172).

93. There are further possibilities, such as 'the element as the respondent would wish it to be'.

94. Thayer (1969*: 89).

95. The 'ought' issue is itself complex. Hollingworth (1949: 18) claimed to have detected ten varieties of 'ought' in his empirical study, but a detailed analysis of the implications for grid method is outside the scope of the present work.

96. Schools Council Integrated Science Project.

NOTES TO CHAPTER 7

1. Kelly (1955: 59f).

2. For example, Kelly (1955: 312f); Beail and Beail (1982).

3. Humphreys (1973: 9).

4. See Kelly (1955: 141f) and (1969: 103f).

5. Osgood, Suci and Tannenbaum (1957: 25). Osgood, May and Miron (1975: 38) claim that 'lexicographers' assure that true verbal opposites do cancel each other out - though this seems open to debate.

6. Osgood, Suci and Tannenbaum (1957: 96).

7. Osgood, Suci and Tannenbaum (1957: 29). Despite its illogicality, the practice lingers on: see, e.g., Collett (1979: 230) and Banikotes and Neimeyer (1981) on its application to repertory grid work.

8. Osgood, May and Miron (1975: 22, 101).

9. For example, Kelly (1955: 60, 106).

10. See Kelly (1955: 106-108). Kelly reports Lyle's factor analysis inadequately since he fails to give the percentage of the variance explained by each factor. Lyle's (1953: 106) analysis is reproduced in Appendix 5.

11. Bonarius (1965: 27) overinterprets Lyle's findings, claiming that errors in respect of 'cheerful' paired up with those in respect of 'sad', but not with any of the other target words.

12. See Appendix 5.

13. Wishner (1960: 110).

14. Mordkoff's experiments are open to the criticism of circularity.

15. There is, however, a weakness in the procedure. Resnick and Landfield offered their respondents a pool of 40 constructs whose realms of meaning may have limited the respondents' choice. The use of, say, 100 constructs would have afforded a more stringent test.

16. Bannister and Mair (1968: 60-61) describe the split-half grid.

17. Hamilton and Deese (1971) provide some - albeit limited - evidence

that a concept's valence shifts when it is presented on its own, rather than with a constrasting concept: hence, there are some grounds for the point suggested here.

18. Kelly (1955: 271).

19. Shaw (1980a: 157).

20. The argument here is taken from Yorke (1978: 66-7).

21. For instance, Grant (1963* 264); Bruner, Shapiro and Tagiuri (1958: 279, note 2). Armstrong and Eden (1979:21), working with professional valuers within a Local Authority, found on a number of occasions that their respondents were unable to provide a contrast pole when asked to construe a series of outcomes of professional activities. However, the outcomes were elicited by a form of laddering and may have been at a variety of levels of abstraction, which might have added to the difficulty of providing bipolar constructs.

    Some researchers deliberately opt for unipolar constructs in which the negation or opposition is left implicit: see, for example, Fransella (1972) - also reported in Fransella and Bannister (1977: 31) - and Boxer (1981).

22. Rating scales could be used, but the underlying point is unchanged.

23. Harrow's (1972) taxonomy of the psychomotor domain implicitly makes this point throughout.

24. Gaines and Shaw (1981: 167f).

25. 'Fuzzy grids' are discussed in Section 11.4.

26. See Gaines and Shaw (1981: 158f).

27. Constructs of this type may be used as simple dichotomies.

28. The 'steps' are likely to collapse skills into broad categories.

29. In the manner of a Guttman scale.

30. Eliot (1962b: 194).

31. Duck (1973: 56).

32. In selecting this grid to exemplify their entailment analysis, Gaines and Shaw (1981: 150f) seem unaware of the problem.

33. Ogden (1967*: 92-93).

34. Moscovici (1981: 200).

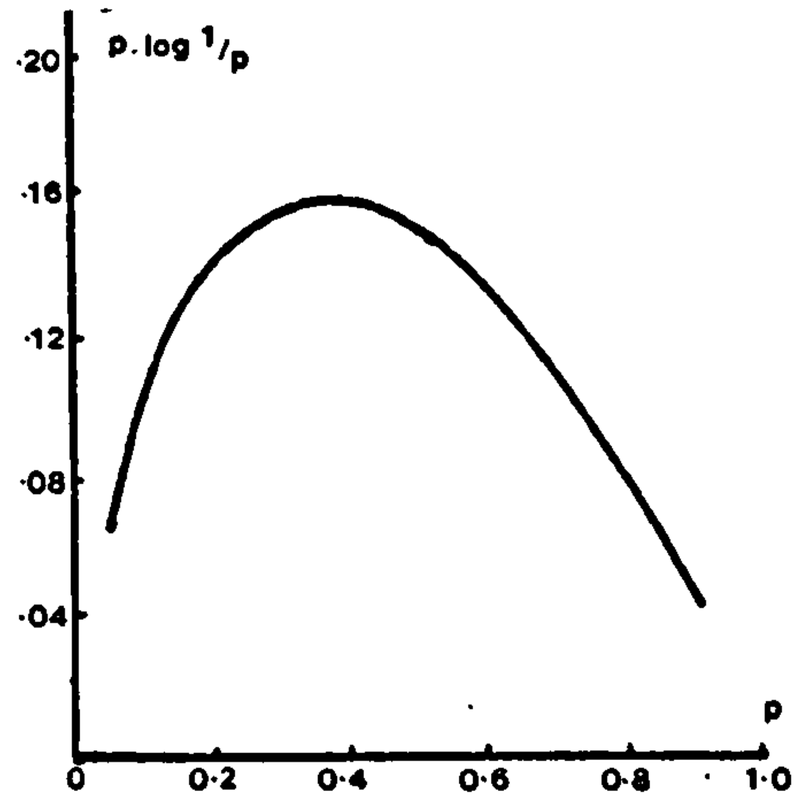
NOTES TO CHAPTER 8

1. Kelly (1955: 271).

2. Applebee (1975: 476) found, when working with children, lopsidedness so severe as to produce no variability on a construct.

3. See Adams-Webber (1979: 160f) for a summary of this work. Kanouse and Hanson (1972: 49-50) suggest that negative information carries more weight than positive information because of its comparative infrequency or 'surprisingness'. Adams-Webber (1977) and Adams-Webber and Davidson (1979) specifically explore the idea of 'strikingness'.

4. 'Strikingness', given by $p.\log {}^{1}/_{p}$, remains near its maximum value over a wide range of values of p, the relative frequency of occurrence. See the graph below, and Adams-Webber (1979: 160-1) for a discussion of the theory.



5. Constructs may not be 'logical' oppositions, and the valence of particular scale positions may not be monotonic with the rating metric (see the discussion in Sections 8.3.8 and, particularly, 9.7.1).

6. Bannister and Mair (1968: 59-60). The use of matching scores has been criticised by Phillips (1973: 51-53), but Phillips's suggestion of the phi coefficient has in its turn been criticised by Beail (1980).

7. Bannister and Mair (1968: 60-61).

8. Bannister and Mair (1968: 61).

9. There does remain the question of how the construct systems thus revealed relate to action.

10. Fransella and Bannister (1977: 30-39) give an account of this method.

11. Honess (1978) used a similar technique.

12. Ravenette (1975: 81).

13. Humphreys (1973: 21).

14. Ravenette (1975: 82) points to the advantages of this method for work with children from about the age of eight and upwards.

15. Clift et al (1978: 103). H.H. Kelley et al (1955: 156) make the same point in respect of discrimination.

16. Three of the elements are specified by the teacher; the remaining eleven are short clips of videorecordings of teaching. Individual

rows may be left blank if a particular triad fails to elicit a new construct.

17. The examples are taken from page 14 of an early version of the paper published as Hopwood and Keen (1978). Printing and proofreading errors account for these and cognate absences from the published paper (Keen, 1983a).

18. Most of these points appeared first in Yorke (1978: 68-69).

19. Lifschitz (1974) used group administration of grids in her research, but I have no evidence as to whether these difficulties might have intruded. Chatwynd (n.d.: 6) sees the completion of a grid as a simple process which can easily be done in the absence of an administrator.

20. Orley (1976: 224).

21. Slater (1977: 36).

22. I recall that the same point has been made in respect of lists of election candidates on ballot papers.

23. For instance, Osgood, Suci and Tannenbaum (1957: 150) note a tendency for extreme ratings to the disproportionately favoured over the mid-point. They remark on the consistency with which the intervals of their metric appear to have been distorted (p.149).

24. Boucher and Osgood (1969). Fuller details of this analysis are given in Appendix 7.

25. See Appendix 8.

26. Olson (1980a: 56).

27. Keen (1979: 134).

28. Lipschitz (1972: 15).

29. Or heterogeneous elements. Pope (1977) avoided the problem by eliminating constructs on which the complete set of elements could not be located.

30. See Eiser and Ströbe (1972: 7-8).

31. See Section 6.2.2.

32. The point is reminiscent of Duhem's (1954*: 183) insight into the interlocking nature of theories in Physics.

33. Humphreys (1973: 10).

34. Sherif and Hovland (1961: 35-36).

35. Leventhal (1957: 180).

36. Lemon and Warren (1974: 123).

37. Cf. Kelly (1955: 225); Bender (1974: 329).

38. Adams-Webber (1979: 166f).

39. Hunt (1941: 400).

40. Isaacson and Landfield (1965: 163).

41. Kelly (1955: figure 1 opposite p.270). Other protocols involve 'self' to a greater (p.225) or lesser (p.221) extent.

42. That the figure is a maximum derives from the possibility that the respondents could construe the statements as moderately like themselves.

43. Rosch and Lloyd (1978: 74). Mancuso and Eimer (1982: 147) seem to take a similar view in their suggestion that 'prototypes' may be located at the extremities of bipolar rating scales.

44. The ratios of allocation to 'X' and 'not X' poles were, respectively: Eiser and Mower White 62:38; Adams-Webber 63:37.

45. It is paradoxical and confusing that 'unmarked' is a marked term.

46. Rating was a subsequent part of the same experiment.

47. Osgood, Suci and Tannenbaum (1957: 79).

48. Thus the scaling may not be linear.

49. For a clear discussion of the issues involved, see Lyons (1977: 305-311).

50. Kelly (1955: 271).

51. Cf. Yorke (1978: 64).

52. Duffey (1972: 23) and Norris (1978: 92) give examples of scales, anchored at each interval, that would be very difficult to treat as linear continua. 'Profiling' in education seems subject to the same difficulty.

53. Gaines and Shaw (1981: 171f).

54. Keen (1979: 114) and (1983b: 11).

55. Shaw (1980a: 156-157).

56. Galtung (1969: 98-102).

57. Gaines and Shaw (1981: 156). Kelly (1969: 311) himself identifies five different types of mid-point.

58. See Section 8.3.6.

59. Hollingworth (1913: 284-285).

60. Switching of attention to different parts of the stimulus is also reported by Eisler and Knoppel (1970) in respect of geometrical figures, and by Green and Carmone (1971) in respect of bakery products.

61. Mair (1967: 269). See also the discussion regarding the construing of 'sin', page 223f.

62. Duck (1973: 56).

63. See, for instance, Smith and Nicholls (1973).

64. See Section 5.5.

65. Sperber (1977 : 70), who asked child patients to construe patients and friends, suggests that even if only lay language is used it will take on a specialist meaning when applied to patients within a diagnostic context. More generally, Miller (1978: 310) makes the point that evaluative adjectives tend to change in meaning depending on the noun being qualified.

66. Collett (1979: 228).

67. Tajfel (1959) discusses the theory further. Eiser and Ströbe (1972: 65f) offer an appraisal.

68. Eiser and Ströbe (1972: 56-57).

69. W. Mischel (1973: 263).

70. Bantock (1961:279f) makes the point that little attempt has been made to investigate the respondent's experience of responding to a questionnaire.

71. Gregson (1975: 147).

72. Rychlak (1968), whilst acknowledging the limitations of introspection, nevertheless points to its heuristic value in theory-generation: see especially page 213.

73. A similar response was given, in a pilot study, by ten students on an M.Sc. course who provided their own construct dimensions. In the study reported in the main text time was very limited, necessitating the use of supplied constructs.

74. In the Schützian sense.

75. See Kelly (1955: Chapter 1 and 71-77). Kelly's position is profoundly Cartesian: see page 6-7 for his mechanistic view of the universe.

76. If cognitive operations are performed using less precise categories, it would seem necessary to include a 'not applicable' option in addition to the presence/absence criterion.

77. From this perspective, N.H. Anderson's (1974) investigations of 'cognitive algebra' appear trivial and grossly inadequate, despite his recognition of the psychological complexity of making judgments (p.89).

78. See Section 3.3.2.

NOTES TO CHAPTER 9

1. Cicourel (1973: 82) observes that the researcher's method of analysis, however unstructured, imposes conditions for the finding of order within the data.

2. Kelly (1955: 280f).

3. The treatment for columns is analogous.

4. The significance of the matching score is determined with reference to the binomial expansion. For criticism of this method, see Slater (1969: 1293) and Phillips (1973: 51-52).

5. Cronbach (1956: 177).

6. Fransella and Bannister (1977: 73f); Watts and Pope (1982: 15); Adams-Webber (1979: 33).

7. Shaw (1980b:121) remarks revealingly that the FOCUS program is 'carefully validated mathematically'.

8. Lewin (1952: 1), for example, warns of the dangers of producing 'but an empty formalism' if the statistical techniques used are incongruent with the quality of the data.

9. See Fransella and Bannister (1977: 33f). Slater (1969:1290)criticises this.

10. Landfield (1971: 166f).

11. Fransella (1981: 172).

12. See Thompson (1975: 22-24).

13. There are a number of warnings in the literature on analytical techniques. For examples see Shepard (1972: 11) on multidimensional scaling, Sokal (1977: 7) on cluster analysis and Rathod (1981: 117) more generally on the analysis of repertory grids. Clemson (1980: 174) makes the point that factor analysis assumes underlying relationships between characteristics whilst cluster analysis treats them as discrete. The position of grid data is uncertain in this respect.

14. Sneath and Sokal (1973: 209).

15. Sneath and Sokal (1973: 247).

16. Sneath and Sokal (1973: 209).

17. Shaw (1980a: 37f).

18. See Bennett (1976) in the light of Aitkin and Bennett (1980) and Aitkin, Bennett and Hesketh (1981).

19. Hope (1969: 1077).

20. Cattell (1957: 88).

21. Everitt (1974: 66-67).

22. Slater (n.d.: 10).

23. Fiske (1971: 324). Sneath and Sokal (1973: 247) argue that rotated, oblique solutions make scientific sense when factors are 'undoubtedly correlated'. Rump (1974: 286) found that components which were orthogonal in an INGRID analysis showed a curvilinear relationship when the analytical routine was changed.

24. For example, Cattell (1957: 403); Osgood, Suci and Tannenbaum (1957: 90-91); Brennan (1972: 45, 63-65) and Everitt (1974: 53-54).

25. Gregson (1975: 52).

26. Kelly (1969: 313) is aware of the functional similarity in parametric analysis of two sets of ratings differing by a constant value.

27. Brennan (1972: 45).

28. Keen (1979: 136).

29. Slater (1977: 90).

30. Slater (1977: 39).

31. Slater (1977: 90). Slater also remarks that altering ratings is unlikely to improve matters (1977: 38).

32. Keen (1979: 140). Applebee (1975: 475) made the deliberate choice not to normalise.

33. Slater (1977: 103).

34. This 13 x 10 grid produced ten components whose percentages of the variance were 42.98; 25.12; 16.17; 8.97; 2.65; 1.63; 1.06; 0.87; 0.52; 0.03. Only the last component was deemed insignificant by the Bartlett test. Other grids have given similar, but not so extreme, results.

35. Slater (1977: 46).

36. Shaw (1980a: 32-33). Murphy (1978: 172) suggests that where the management client's 'ownership' of the data is minimised, this can discourage subsequent action.

37. See Thomas and Shaw (n.d.) for a fuller account.

38. Shaw (1980a: 159-160).

39. The 'mean character difference' (Sneath and Sokal, 1973: 123) has been used in Figure 9.1 rather than the city block metric because it is adjusted for the number of observations involved. An analogous 'mean Euclidean difference' has been used in respect of the last two columns. The respective formulae are

$$\text{m.c.d.} = \frac{1}{n} \sum_{i}^{n} \left| X_{ij} - X_{ik} \right| \qquad \text{and} \qquad \text{m.E.d} = \frac{1}{n} \sqrt{\sum_{i}^{n} (X_{ij} - X_{ik})^2}$$

40. Rathod (1981: 121) is incorrect in his claim that the city block and Euclidean metrics are monotonic, but correct in stating that Slater's

measures of association r and $\cos^{-1} r$ are not monotonic with distance measures. The diagram given on p.122 gives a falsely-simplified picture because of its restriction to two dimensions. Everitt (1978: 45) notes that increasing the power coefficient of the Minkowski metric increases the contrast between large and small distances.

41. See Youngman (1976).

42. Garner (1974: 101).

43. Burgoyne (1981: 78).

44. Shaw (1980a: 75-77) has developed a program (MINUS) which does this. Since the city block metric ignores the sign of any difference it is difficult to see what meaning may be given to the subsequent FOCUSing. Negative signs disappear with the Euclidean distance, but I am only suggesting this as an indicator of change and am not suggesting the clustering of the changes in the manner of Shaw.

45. Thomas and Shaw (n.d.: 15).

46. Blashfield (1976: 384) criticises the single linkage method for its proneness to chaining, indicating that its performance declined with increasing number of entities to be classified. It is uncertain whether this objection holds in respect of the smallish numbers of elements and constructs in a grid. Single linkage analysis appears to perform particularly well with well-separated, spherical groups of entities (Everitt, 1974: 78f; Blashfield 1976: 384-5) - but this condition would seem generally untypical of repertory grid data.

47. See Shaw (1980a: 47).

48. Shaw and Thomas (1978: 142), my emphasis.

49. Youngman (1979: 88-90).

50. This program was written for elements, and cannot cater for the possible 'reflection' of constructs. It would seem possible to develop the program to cope with the complications introduced by bipolarity.

51. See, for instance, Fleiss and Zubin (1969: 241f); Everitt (1974: 43f).

52. See, for instance, Riley and Palmer (1976: 161).

53. Smith and Stewart (1977: 212).

54. Hopwood and Keen (1978: 194). Unfortunately the example of analysis relates to a different grid from that which is presented.

55. See Hopwood and Keen (1978: 189). The authors similarly do not justify their claim that a respondent whose analysis shows more than four significant bars 'has the ability to appraise a teaching act ... in an open and unbiased manner' (1978:192).

56. And between confidence and effective teaching!

57. Hope (1966: 116). Kitwood (1976: 77) similarly points out that factors and clusters are often treated with great seriousness, as if they had an independent life.

58. Kelly (1955: 277).

59. Kelly (1955: 307).

60. For instance, Levy (1956); Hopwood and Keen (1978).

61. For instance, Bannister (1965: 977); Makhlouf-Norris, Jones and Norris (1970: 269). Regarding the latter, it is doubtful whether a correlation coefficient of + 0.44 is an adequate criterion of construct association. Henry (1981: 356), in exploring the construct systems of Irish teachers, set this criterion of association as low as 0.4 (Pearson r) for seven out of the eight teachers sampled. It is extremely doubtful that any claim for the validity of structure-type can be sustained on such a flimsy criterion.

    Thompson (1975: 28,33) seems to assume that a structure which does not significantly link all of the constructs lacks integration. However, the restricted sampling possible in her - and any other - grid methodology militates against this (not that there seems to be any a priori reason to expect such linkages). For this reason it is doubtful whether Makhlouf-Norris et al's (1970) distinction between monolithic, segmented and articulated structures is sufficiently well grounded to be used as a reference typology.

62. Makhlouf-Norris, Jones and Norris (1970: 271)

63. Sneath and Sokal (1973: 7).

64. Smith and Leach (1972: 564). Ravenette (1968: 5) and Hayden (1982: 178-179) make the same mistake.

65. Adams-Webber (1979: 55-56) suggests that the failure of their measure to correlate with the Bieri measure is its main advantage.

66. See Slater (1977: 139f); Shaw (1980a: 75f).

67. See note 18 to Chapter 5. The broader issue of 'match' between the researcher's intentions and methodology was discussed in Section 5.2.

68. Perrott et al (1976: 31). Langrish and Smith (1979: 7f) are in a similar bind. Given the technical criticism being made, it is hardly surprising that their sample of women managers placed 'A colleague' at 'an indistinct point at the centre of the grid' (1979: 14, 16).

69. Although the criticism of ADELA (page 346) must be borne in mind.

70. Beail (1983) observes that he averaged 30 grids and conducted a principal components analysis which established a close relationship between the self and the stereotype. However a subsequent idiographic analysis revealed that this relationship only held in five of the 30 cases and was not statistically significant. Pask (1975:9) notes the loss of crucial information about individual performance in most investigations of learning.

71. Du Mas (1955: 72).

72. Stewart, Powell and Chetwynd (1979: 117) indicate, for example, that both fat and thin female body-types are negatively evaluated.

73. Perrott et al (1976: 35). Eiser (1980: 89) notes the possibility of E+/E-/E+ constructs, but his example suggests that this might apply mainly to non-contiguous oppositions.

74. Burgoyne (1981: 71-72) notes that the ideal point may vary with circumstances.

75. Kelly (1955: 58).

76. Honikman (1966: 177).

77. The distinction is made by Argyris and Schön (1974).

78. The classic study of La Piere (1934) is a good example of the distinction between espoused theory and theory-in-use.

79. Sneath and Sokal (1973: 109).

80. The term is borrowed from du Mas (1955).

81. Giorgi (1966: 40).

82. Eden (1978: 664).

83. Unless they are 'physical' constructs.

NOTES TO CHAPTER 10

1. Bannister and Mair (1968: 156); Fransella and Bannister (1977: 82-83).

2. Guilford and Fruchter (1973: 379).

3. Snow (1974: 270).

4. Slater (1977: 128).

5. Bannister and Mair (1968: 180).

6. Guilford and Fruchter (1973: 403).

7. It seems unlikely that satisfactory 'parallel form' sets of constructs can ever be constructed. As far as elements are concerned, it cannot be taken for granted that a list of role titles will produce adequate 'parallel form' lists of elements.

8. Kelly (1955: 48).

9. Hampshire (1959:146). F. Heider (1958: 80) writes of dispositional properties as the invariances that make possible a more or less stable, predictable and controllable world.

10. Bannister and Mair (1968: 156).

11. The point is made by Caplan et al (1975: 225).

12. Mair (1970: 175).

13. Slater (1972: 48-49). Fransella and Bannister (1977: 79) make the same point.

14. Butler and Stokes (1969: 315).

15. The method of presenting this consistency score has evolved with time: the current approach is clearly described in Fransella and Bannister (1977: 78-81).

16. See Bannister and Mair (1968: 170-174) for the results and discussion.

17. This may only be a reflection of the particular interests of Bannister and his colleagues.

18. See Reid(1976: 378-382).

19. In later work Bieri standardised his grid measure of cognitive complexity by supplying constructs: see Bieri et al (1966: 191).

20. In theory, both elements and constructs could be presented in 'parallel form' terms - though it is unlikely that many would wish to assert the validity of any stability index subsequently calculated.

21. Bannister and Mair (1968: 166). This statement sits a little uncomfortably beside their treatment of grid responses as a population (p.180).

22. See page 370. The same basic experimental format was used as for investigating test-retest stability.

23. There is, further, the 'quality' of the elicited constructs ('peculiar' or 'logical'), on which no evidence is offered.

24. For example, in Guilford and Fruchter (1973).

25. Reid (1976: 701). The work of McGuire (1960) on syllogisms supports the proposition that completing a test has an influence upon subsequent performance.

26. See Pope (1977: 224f).

27. Shaw (1980a: 160-161), in contrast, is aware of this problem.

28. My calculations are based on Pope's original grid data, since there are some minor discrepancies between the original grid data and the total difference scores shown by Pope. The corrected stability is calculated as (1 - corrected CHAT score).

29. See Siegel (1956: 75f).

30. Duck and Spencer (1972) noted that the use of psychological constructs (as opposed to 'role' and 'other' constructs) increased with time. They suggested that this might have resulted from exposure to university training - but greater familiarity with the technique and, perhaps, with the researcher's intentions might also have been contributory factors. They make no claim for stability in respect of any of the particular grid administrations.

31. See Appendix 10 for a fuller account.

32. The collection of these data is described in Appendix 6. The stability indexes are given in the final column of the table in Appendix 12.

33. Both of these indexes are described in Slater (1972).

34. In all of these indexes there is an assumption that the construct acts as a consistent linear scale, but the coefficient of convergence further assumes that this consistency exists even when the set of elements may differ between the two grids being compared. The discussion in Section 8.4 suggests that these assumptions are open to question.

35. Slater (1972: 46).

36. It has to be pointed out that the indexes differ regarding range. Bannister's consistency score and the coefficient of convergence can range from -1 to +1, whereas the 'per cell'index ranges from 0 to + 1. The range for the general degree of correlation is not given in Slater (1972), and is difficult to infer from the limited details provided by Slater.

37. The smaller the test-retest grids are, the greater is the potential damage to stability from construct reversal.

38. See Kelly (1955: 1058).

39. See also Frith and Lillie (1972); Collett (1979).

40. See Pope (1977: 266).

41. Tippett and Silber (1965: 329).

42. See page 370.

43. See Bannister and Mair 1968 (170-171; 174-175) for the relevant evidence and discussion.

44. Miller and Bieri (1963: 324).

45. See Ryle (1975: 123-5). The data refer to a control-group of (normal) people who completed 'standard' 16 x 16 grids. Similar results were obtained with small groups of patients and students in academic difficulties. Dyad grids with 12 social workers and 19 patients evidenced a stronger connection between extremity and stability on Ryle's criterion.

46. See Bannister and Mair (1968: 171-172). Fransella and Bannister (1977: 86) repeat the findings from the reanalysis of Bannister's work.

47. Contrast Bannister and Mair (1968: 172) with Mair and Boyd (1967: 225). Neither of the two accounts of computational method is sufficiently clear for a definitive comparison to be made here.

48. I am taking high variance as indicative of extremity of rating, although the correspondence between the two is not necessarily perfect.

49. See the discussion in Section 8.3.8.

50. See Section 10.8.1.

51. A fuller presentation of the data is given in Appendix 8.

52. This point was discussed in Section 8.2.1.

NOTES TO CHAPTER 11

1. See Section 3.3.1.

2. For instance, Kelly (1955: 56, 93, 95, 687f).

3. Kelly (1955: 689) recognises that it is easier to see the other's cultural determinants than one's own.

4. For instance, Kelly (1955: 8-9, 12, 50).

5. It was noted in Sections 2.3 and 3.2, that Kelly gives insufficient attention to the determining influence of events upon construing. See his account of 'experience' (1955: 170f, especially 174-5; 687f).

6. Maslow (1973*)

7. Kelly (1955: 688).

8. See, for instance, Whorf (1956).

9. In his later theorising Chomsky (1971) makes more of the connection between semantics and structure, though it is clear from the examples he presents that the interpretation of meaning goes well beyond the information given by the syntactical structure.

10. Brown and Lenneberg (1954: 456).

11. Cole and Scribner (1974: 41-42).

12. J.R. Anderson (1980: 386).

13. Gadamer (1975*: 401).

14. Lacan (1977*: 65).

15. The argument is not new. Steiner (1975: 74) tracks it back to a paper written by Leibniz in 1697.

16. A number of affidavits to the power of language are to be found in Postman and Weingartner (1971: 121-130).

    The importance of language, and of its interpretation, are neatly summarised by Hastorf et al (1970: 59) who write (of person perception, but more generally applicable):

> 'Perhaps all we really have is a set of linguistic labels, as well as a set of conventions for applying them to people. If that is true .... then the study of how our linguistic labels are organized and how that organization determines and is determined by the conventions of application is absolutely appropriate and fundamental.'

As noted earlier (note 9 above), this study requires far more than structural analyses.

17. Peters (1974: 392).

18. Cf. Berger and Luckmann (1967: 173). Procter and Parry (1978: 160) point to the role of the family as 'significant others' in the early development of construct systems.

19. Dearborn (1910: 59).

20. Dearborn (1910: 61).

21. Giddens (1976: 19-20).

22. The metaphor is given by Steiner (1975:81) after von Humboldt.

23. Entwistle (1976: 2).

24. Marcuse (1964: 93).

25. Schütz (1962*: 58-59).

26. Wilden (1980: 303).

27. Kelly (1955: 50-52), it will be remembered, gives construct theory a broader remit than the cognitive perspective taken here would suggest.

28. See Section 4.4.

29. See Peirce's 'Logic as semiotic: the theory of signs', reprinted in Buchler (ed), 1978*: 98-119, especially p.102.

30. See Brown (1958). Bannister and Mair (1968: 129) discuss Brown's criticism (of the semantic differential) from the point of view of construct theory. Glucksmann (1974: 8) exemplifies the connotative attachments to a denotative signifier, and the uncertainty and complexity these produce for linguistic analysis.

31. M. Adler (1927: 85).

32. Richards (1942: 73). Dilthey, in discussing signification and meaning, points out the indeterminacy, within a certain range of possibilities, of a word in isolation. (Hodges 1974*: 143).

33. Campbell (1969: 44).

34. Brenner and Bungard (1981) survey the literature concerning psychological experimentation, and much of their account is relevant to 'naturalistic' research. Labov (1972*), discussing the linguistic capabilities of alienated black children, neatly illustrates a number of the problems.

35. Bergson (1910*: 164). Schütz (1967*: 126) makes much the same point in his distinction between objective and subjective meaning.

36. Ricoeur (1978*: 67) quotes Benveniste as pointing out that the fundamental unit of language is the sign, and of discourse is the sentence, He criticises Saussure's view of language for missing the semantic at the expense of the semiotic (p.69), and points out that metaphor - and here one might extend the argument to figurative language in general - depends upon the syntagmatics of sentence structure rather than its paradigmatic components (p.76).

37. Ricoeur (1978*: 66).

38. See especially Garfinkel (1967: Chapter 2).

39. Searle (1969: 16). The importance for meaning of contextual assumptions is emphasised in Searle (1979: 117).

40. Russell (1979*: 60) is incorrect in his assumption that synonyms have identical sets of associations.

41. The point was made in Chapter 8, page 277-8, that lexical marking and valence may on occasion be gender-specific. This could have considerable effect on connotative meaning.

42. A Chomskian deep structure analysis would be unable to resolve the ambiguity.

43. The point is made with some force in Fowler and Kress (1979) who examine the relationship between language and social process.

44. Austin (1971*: 96-97), for instance, points to the importance of the exact phrase and its place in the sentence.

45. The phrase is used by Bannister and Mair (1968: 158).

46. Oswalt's sample is likely to have produced a response at the upper end of the range of possibilities.

47. Zadeh (1976: 250). The scales of the semantic differential use linguistic modifiers of this sort.

48. Gaines (1976: 649-650). The point is reiterated in Gaines and Shaw (1981: 168-169).

49. See Herman (1982).

50. Mosier (1941: 134) noted that 'good' was rated better than 'better', perhaps as a result of 'better' being construed with respect to some unfavourable standard, and merely being indicative of improvement. The examples given by Sapir (1944: particularly 114-115) are also indicative of the problem of obtaining a correspondence between semantics and statistics.

51. Lakoff (1972: 197-198).

52. See Section 10.8.2.

53. Cf. Gadamer (1976*: 83); Ricoeur (1978*: 66f).

54. Wilden (1980: 11); author's emphases.

55. Ricoeur (1978*: 70).

56. Wilden (1980: 10-11). Culler (1975: 75-95 and 256-7) also criticises the assumption that linguistics ought to be able to account for meaning of all kinds by working upwards from the meanings of small bits of language.

57. Eden suggested the metaphor to me.

58. See, for instance, the case of 'Helen Greaves' in Eden, Jones and Sims (1979: 112f).

59. De Mauro (1967: 40).

60. Kelly (1955: 88).

61. The metaphor of the text indicates something of the interpretative breadth I have in mind, but difficulties arise if it is pressed too far (as is indicated in Chapter 13).

NOTES TO CHAPTER 12

1. Cronbach and Meehl (1955: 281-282).

2. For example, Fransella and Bannister (1977: 92).

3. Fransella and Bannister (1977: 94).

4. Fransella and Bannister (1977: 94).

5. Fransella and Bannister (1977: 102); authors' emphasis. J.K. Galbraith (1961*: 158-160) once made reference to 'no-business meetings' which were analogously justified on the grounds that an exchange of ideas could always be construed as useful.

6. Fransella and Bannister (1977: 59) are surely wrong to state 'We need not rely on normative data for an understanding of the construct patterning revealed'. It is our normative frameworks, our generalised expdriences, that enable us to understand an event to be of a particular type despite - always - its non-identity with any other.

7. Fransella and Bannister (1977: 93); authors' emphasis.

8. Fransella and Bannister (1977: 59).

9. This is not to claim that the problems that I have identified will necessarily be co-present.

10. Adams-Webber (1979: 200). The conceptual gap between the measures used in this study, when coupled with the measures' inherent looseness, suggests that the comment is over-generous.

11. I gain this through my observation and interpretation of behaviour (construed broadly).

12. Roche (1973: 93).

13. Pepper (1942: 142).

14. Pepper (1942: 142).

15. Pepper (1942: 143).

16. Pepper (1942: 146).

17. Fransella and Bannister (1977: 4).

18. Fransella and Bannister (1977: 4).

NOTES TO CHAPTER 13

1. The point is emphasised by de Waele and Harré (1979: 190-193).

2. Hesse (1978: 7) indicates this difficulty of theorising in the social sciences.

3. Polanyi (1967: 25) indicates the risks of formalisation of methodology. My argument has been that the repertory grid suffers badly from this weakness.

4. Hempel (1949*: 462).

5. 'Observable' is used here in a broad sense. Many events in the physical sciences are inferred (for instance, atomic and sub-atomic interactions) rather than directly observed.

6. For a discussion of laws in science, see Achinstein (1971: Chapters 1-3).

7. Problems of sampling (and subsequent generalisation) are often ignored: see Snow (1974) for a discussion of this issue.

8. Giddens (1976: 19).

9. It is immaterial here whether one construes 'self' in terms of determinism or free will.

10. Dewey (1916b: 326-327).

11. For instance; Harvey, Hunt and Schroder (1961: 5); Bakan (1967: 41-42); Orne (1962, 1973) and Wallen and Travers (1963).

12. See also Baloff and Becker (1967).

13. Beail (1983) reanalysed nomothetic 'consensus grid' data at the individual level, showing that the latter could be seriously misrepresented by aggregation of responses.

14. Inter-individual variance may exceed inter-group variance: see Shapiro and Tagiuri (1959: 129) for an example.

15. Here there are similarities with observational science. Bleicher (1982) offers a critique of positivistic sociology.

16. Eliot (1962a: 247). The aptness of the quotation was recognised by Rowe (1978: 262), for whose insight I am grateful.

17. Rowe (1978: 262).

18. This presents further difficulties because pupils are also human. Stefan (1977: 292-293) indicates how social pressures can override the researcher's intention.

19. Or, at least, that differences in settings do not contribute significantly to the variance of the findings.

20. Eggleston, Galton and Jones (1976).

21. Whether this be experimental or observational.

22. Reason and Rowan (1981) is a recent example.

23. Giorgi (1965: 230-231).

24. Eldridge (1970: 16) indicates Weber's awareness of the point. Falk (1956: 62) argues the Cartesian thesis that a set of general laws is potentially sufficient to account for the uniqueness of personality, but as Kelly (1955: 10) remarks, it is likely to be a considerable time before a universal construct system is established.

25. Habermas (1972*:142) makes this point in his critique of science.

26. See Popper (1957: 143-145) for this argument.

27. Which retains its pragmatist connections with exploration and purposiveness.

28. C. Taylor (1971: 51).

29. See, for instance, Watts, Gilbert and Pope (1982); Watts and Zylbersztajn (1981).

30. I do not intend to discuss accidental behaviour in this account. Suffice it to say that Kelly does not always distinguish clearly between accidental and purposive behaviour.

31. See Dewey (1916a: 176) as reproduced on page 22 above.

32. It is surprising to find no mention of Kelly in this book in view of Shotter's (1970) earlier chapter on construct theory.

33. See Palmer (1969: 33-45) for a fuller account.

34. Makkreel (1982: 2) suggests that the distinction in Dilthey's thinking is not so sharp.

35. Heidegger (1962*: 225f).

36. Palmer (1969: 145).

37. Brown and Lyman (1978: 2) propose a similar fusion of perspectives - and also recognise the difficulties.

38. Giddens (1976: 35).

39. A number of difficult philosophical questions are begged here. The rest of this chapter offers partial attempts at answers at both conceptual and practical levels. The assumption of an interlocutor's re-enactment of another's psychological processes (see Dray 1957:119 for an example of this assertion) is sharply dismissed by G. Ryle (1973*: 50-59).

40. See Kelly (1955: 452-485).

41. Kelly (1955: 596).

42. See Ricoeur (1981: 63-100, especially p.87f).

43. The emphasis is upon explanation, but does not exclude prediction.

44. See Ricoeur (1981: 145-164 and 197-221).

45. Ricoeur (1981: 210-215). See also Hirsch (1967:170).

46. Hirsch (1976: 32).

47. Or 'scripts' (Schank and Abelson, 1977).

48. Von Wright (1971: 134).

49. Apel (1979: 10-13) emphasises the complementariness involved.

50. Hirsch (1967: 236-237).

51. Gadamer (1975*: 147).

52. Gadamer (1975*: 149).

53. See Linge's introduction to Gadamer (1976*), page xxiii-xxiv. Giddens (1979: 44) makes the point that the consequences of action escape the originator's intentions: the corollary is the danger of superficial inference regarding intentions.

54. See my discussion of the Fundamental Postulate in Section 3.2.

55. Giddens (1976: 146).

56. And of a scientistic social science.

57. Schütz (1967*: 205-206).

58. See Weber (1947*: 96).

59. Schütz (1967*: 151f).

60. Not restricting 'observable' tothe visual sense, in that it subsumes, for instance, a telephone conversation.

61. For instance, Hodges (1974*: 120, 136-7); Palmer (1969: 104, 123). Makkreel (1975: 252n) lays the blame for some of this upon Rickman's (1961: 79) translation of Nacherleben as empathy.

62. Makkreel (1975: 361), my emphasis. A similar point is made on page 328-330.

63. Collingwood (1961*: 242).

64. For example Kockelmans (1975: 85).

65. In his attack on Hempel's positivism Dray (1957: 119) is incautious in his requirement that the historian identify with the protagonist, self-project into the situation and re-experience the other's life-world. Meehl (1954: 65) more soberly remarks that the clinician must reconstruct the client's history from fragments. Lemaire (1977*: 136) indicates that the aim of psychoanalysis is to recover the 'unconscious text' which complements the nodal points.

    Rational interpolation is exemplified in Leakey's (1981: 83f) account of the implications of stone-knapping. The traditional view was that flakes were chipped from a stone in order to shape it for use as a tool, but a research assistant's experience (coming close to Nacherleben) served as a heuristic to suggest that the flake may have been the primary product of the technique. Here the re-experiencing facilitated alternative constructions of the event being considered: one can envisage its extension to other circumstances.

66. Von Wright (1971: 6) makes a connection between understanding and intentionality, and Schütz (1967*) writes of actions as 'signitive' of lived experience (p. 100-101) and discusses the structural parallelism between minds (see p. 106, 115). Simmel (1980*: 99, 103) expresses a similar view. Natsoulas (1967: 262) also mentions mental parallelism and suggests that communication depends upon the respondent's reporting of experience and the retrieval of cognate experiences from the researcher's memory. Kelly's (1955: 90-94) discussion of the Sociality Corollary can be read in a similar way.

67. The noetic and the noematic are logically distinct (see Oakes's introduction, page 64 and 93 note 23, to Simmel, 1980*). The clues discerned by the researcher may conflict, thus requiring a more extended analysis of matters such as the 'in order to' and 'because' motives (Schütz, 1967*: 86-96), or of grounds and dispositions (Pettit 1975: 261).

68. Schütz (1967*: 174-175).

69. Nagel (1961: 484). Kelly (1969: 140) seems close to psychologism when he writes of a psychologist experiencing, to the fullest possible extent, what his subjects experience. Lewis (1956*: 73) makes the more extreme psychologistic claim of mental identicalness regarding something when two people understand each other.

70. See Wax's (1974*: 74) criticism of Abel's construing of Verstehen in terms of an operation.

71. Makkreel (1975: 253n).

72. Using both episodic and semantic memory (Tulving, 1972) or, more generally, the 'second record' (Hexter, 1972).

73. Bleicher (1982: 64).

74. Wax (1974*: 76).

75. The list is drawn from a quotation in Bleicher (1982: 65).

76. Weber (1947*: 91) makes a similar point.

77. Labov (1972*: 200).

78. Giddens (1976: 35). Kelly's (1955: 322) 'credulous approach' is not as credulous as the label suggests, and one can sense the twin points of Ricoeur's hermeneutics in his account.

79. Hanson (1958: 5f).

80. Peirce (CP 6.498) remarks that 'No concept, not even those of mathematics, is absolutely precise; and some of the most important for everyday use are extremely vague'. Gardiner (1961*:7) takes a similar view. One has to concur. Von Neumann (1958: 77-78) points out that signal intensity in the nervous system is frequency modulated, which allows the brain to process exceedingly complicated information on a rather low level of precision. The question of conceptual fuzziness was touched on in Section 11.4.

81. Vygotsky (1962*: 134) makes a similar point.

82. As does the researcher's interpretive system.

83. Hirsch (1967: 17).

84. Collingwood (1961*: 257) - who seems to be using 'scientific' in a rather broad sense.

85. Ziman (1978: 56), in the context of science, takes a similar view.

86. Collingwood (1961*: 258).

87. See Rychlak (1968: 74f) for a fuller account.

88. Dilthey drew the distinction between the hermeneutic circle of the human studies and the classificatory circle of the natural sciences' thrust towards generalisation (see Makkreel, 1975: 340-342). I am unconvinced by the distinction, seeing both circles as applying to each sphere of interest - albeit with different levels of emphasis.

89. Further evidence may also be adduced.

90. I shall concentrate this discussion on the interaction between a researcher and a single respondent, but much will apply, <u>mutatis mutandis</u>, to other research situations.

91. Giorgi (1976: 312).

92. Rosenthal (1969: 197-199).

93. Orne (1973: 161) and Bannister (1965: 979), amongst others, make this point but insist on the scientific experiment as the research paradigm. Orne asserts that there should be no place in the published account for <u>post hoc</u> interpretation of respondents'

constructions - a point of view which I would strongly contest.

94. Ellenberger (1958: 99).

95. Nisbett and Wilson (1977: 233f) discuss some of the issues involved.

96. Ziff (1972: 127) suggests that phonetic, phonemic, morphologic, syntactic, semantic, discourse and perceptual factors may impinge on the interaction.

97. See Ogden and Richards (1949: 210f).

98. For instance, Brown and Sime (1981: 181).

99. There seems no reason in principle why generalisations of experience cannot be treated in the same manner, but I am limiting my discussion here to one or more identifiable instances.

100. See Rokeach (1960: 171-195).

101. Hateley (1979) asked a small number of students to 'talk through' their responding to multiple-choice items in order to investigate their reasoning processes.

102. See, for instance, Allen and Ryan (1969). McDonald (1972) states the behaviourist position with considerable clarity and force.

103. Other than content-free structuring devices.

104. In other words, interaction analysis of the type proposed by Flanders (1970) is not being considered here.

105. Three points must be made here. First, the channel of discourse cannot accommodate all of the respondent's construing (cf Broadbent 1973: 179). Second, following Polanyi (1967: 4), 'we know more than we can tell', which can be converted into Bruner's (1962: 94) premiss that the principal problem of human memory is not storage but retrieval. The problem can be extended to information systems in general (Jones, Winn and Bramer, 1978; Swift, 1979). Third, the past is susceptible to change as it is remembered from successively new presents, each with its own focus of interest (Giorgi 1976: 304).

106. See, for instance, Landfield (1982: 201).

107. As produced from Slater's INGRID analysis.

108. The argument is also consistent with my emphasis on speech act theory (see Section 11.3).

109. Palmer (1969: 234).

110. Allport (1962: 414).

111. Austen (1964*: 381). Broadbent (1973: 179) emphasises the unintentional character of omissions whereas Steiner (1975: 220) writes that people 'select and elide' in respect of what is communicated.

112. Dunn (1978: 154).

113. Support is given to this view from a range of standpoints by Binswanger (1958*: 201-202); Spiegelberg (1960: 104-105); Spence (1968: 350) and Lacan (1977*: 50).

It may well be the case that fish from the depths burst in the lower pressure at the surface of the sea (to draw on Bauman's, 1978: 127, analogy), but our capacity to survive in their depths is very limited. Analysis of the fragments of flesh may nevertheless yield the best information regarding their 'being in the world'.

Gibson (1981) indicates how 'curriculum criticism' has been irredeemably compromised by an uncritical use of interpretive activity, and Creelan's (1978) psychoanalytic reading of Talcott Parsons seems to take 'interpretation' to the wildest and most fanciful extremes.

114. Lacan (1977*: 93).

115. The term is taken from Heidegger (1962*: 183).

116. Tajfel and Forgas (1981: 117).

117. See Habermas (1972*:171).

118. Weber makes this point (see Weber in Eldridge 1970: 95), as does Wittgenstein (1953: 212e). The view is consistent with Kelly's theorising.

119. Hirsch (1967: 169).

120. Ayer (1975: 87-88). Ayer has in mind empirical testing, but it is not unreasonable to extend the argument to interpretive activity.

121. This point was put forward in criticism of the Kellian metaphor of the scientist (Section 2.4).

122. Gadamer (1975*: 163).

123. In which the interpretation of others depends upon the interpreter's own knowledge of cognate contexts (Hodges 1974*: 119).

124. A naive empathy is not being suggested here: Pinar (1981: 178) points out that this risks complicity with another's delusions and legitimations. A 'suspicious' hermeneutics grounded in relevant experience would seem to be a necessity for rigorous research.

125. Kitwood (1976: 80).

126. Kitwood (1976: 70).

127. Which is not to imply that, for all research into classroom activities, it is necessary to have had experience in the role of teacher.

128. Eisner (1979: xii).

129. Though it has some undesirable connotations and implications, as R. Gibson (1981) trenchantly makes clear.

130. Eisner (1979: 193).

131. See the accounts given in Eisner (1979: Chapter 11). Pinar (1981: 176) criticises Eisner's students for failing to be explicit regarding their critical criteria.

132. That is, psychology undergraduates.

133. Eisner (1979: 194).

134. Which is not to deny the existence of ethnographic modes of research in the human sciences (Stenhouse, n.d.: 6, for instance, indicates a number within the field of education): rather, the point is being made that these tend to be underrepresented. There are, of course, substantial difficulties barring the path of the ethnographer.

135. The importance of a research team was stressed in Section 13.9.2. Individual ethnographers - as others - are always blinkered to some extent: the establishment of a team does not guarantee that the problem will be reduced to an acceptable level.

136. Eisner (1979: 18).

137. Cf Hudson (1975: 47), who - in contrast to Rose (1982: 9-12) - does not discriminate clearly between theory building and theory testing.

138. The contrast made here is with statistical significance.

139. Stenhouse (1978: 22).

140. Macdonald and Walker (1975: 2).

141. See Kazdin (1980: Chapter 2).

142. Elliott (1978: 16) makes this last point.

143. Case studies may offer counter-instances to what is believed to be generally applicable (Kazdin, 1980: 22).

144. Walker (1983: 155) suggests that case studies may be one step ahead of current theorising.

145. Macdonald and Walker (1975: 2) write of case studies as implying a goal of generalisation .

146. There seems a distinct similarity between the 'looseness' of genre, Wittgenstein's (1953: 32e) 'family resemblances' and Merlean-Ponty's (1964*: 67) statement that essences are inexact by nature and incapable of univocal determination. Runciman's (1983: 150f) 'relaxed' characterisation of theory in social science makes much the same kind of point as my use of genre.

147. Parsons, in his introduction to Weber (1947*), p.13.

148. In other words, an 'ideal type' can be construed as too loose or too tight, depending on one's point of view.

149. In Peirce's sense.

150. Du Mas (1955: 72-73).

151. Harré (1978: 44-5). Harré avoids facing the problem of an a priori identification of typicality.

152. See van Kaam (1959: 68). It is not apparent from van Kaam's account how his method remains faithful to the phenomena.

153. Van Kaam (1959: 71).

154. The propositions are listed in Rowe (1978: 235-236).

155. Kelly (1955) makes only a comparatively slight reference to depression.

156. Stake (1978: 6).

157. See Eden, Jones and Sims (1979; 1982).

NOTES TO CHAPTER 14

1. Hudson (1976*: 18) also remarks on the emergence of an alternative perspective during his research. The constraints were such that, in his case, his insights had to be presented in a separate book. In my case, the relationship is such that the emerging theoretical argument can be linked with the methodological critique.

2. In contrast with the subtitle of Runciman's (1983) peritheoretical excursion: 'The methodology of social theory'.

3. Cf Peirce (CP 5.583) and the title page to Chapter 1.

4. In the sense of Cronbach and Suppes (1969).

5. Following the usage of Glaser and Strauss (1967), but avoiding their tendency to slide towards the inductive fallacy,

6. The quotations are from 'The dry salvages' (Eliot, 1962b: 213).

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Appendices

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APPENDIX 1. An outline of the research involving probationary science teachers.

Problem.

The original intention of this study was to explore the construing of science teachers at the beginning and end of their probationary year, with a view to identifying necessary alterations and additions to an L.E.A.-sponsored induction program. The reasons for the change in direction of this study were given in Sections 1.1 and 1.2 and will not be repeated here: this account is limited to an outline of the work conducted with the science teachers, and the methodological implications of the evidence are followed up in other Appendices.

Initial design

At the start of the academic year 1978-79 I attended the first meeting of the Manchester L.E.A.'s induction course for probationary science teachers (which ran regularly on Wednesday afternoons) in order to ask for the probationers' co-operation, the L.E.A's and head teachers' approval for this research having previously been given. Since the majority of newly-appointed science teachers would be involved in teaching lower school science (and not necessarily upper school work) I decided to limit the study to those teaching first-year science. Given that the majority of schools were using the Nuffield Combined Science scheme, this meant that there would be a fair measure of congruence regarding curriculum experience.

Nine teachers indicated that they were willing to participate in the study, two of whom requested a further clarification before they were prepared to commit themselves. These nine turned out to be the full complement of those with first-year science on their timetables: a

number of others would have been prepared to participate had the limitation regarding level of science teaching not been imposed.

At the same time the opportunity arose to conduct a similar investigation in a smaller nearby authority which did not run a comparable induction course. The advantage of extending the study to this authority lay in the comparison that might be possible between a group of teachers who were released for half a day per week to work through a structured sequence of activities and discussions linked closely to the context of teaching within their L.E.A's schools, and those for whom no such provision existed.

I wrote to each of the newly-appointed science teachers in this second authority, explaining the purpose and scope of the intended study, and obtained the agreement of five to participate: again this proved to be the full complement of those with first-year science on their timetables.

The overall plan of study was as follows:

During October and early November, 1978	(a) Administer first repertory grid to each teacher (b) During the week following each grid administration send out a replication of the 'rating ladders' used in order to assess stability. (c) Analyse each grid using INGRID and prepare a polar co-ordinate plot. (d) Return to each teacher within a fortnight to administer a resistance-to-change grid in order to assess construct importance. Also to discuss the INGRID analysis and to provide the promised feedback.

During June and July, 1979	(e)	Repeat (a) to (d) above, but including in the repertory grid and resistance-to-change grid those constructs identified as highly important in the first resistance-to-change grid.

Operationalisation.

The above programme was completed by all of the participants, with the following exceptions.

(i) One of the Manchester teachers, B.I. (the initials are fictitious) was on a temporary contract ending in December 1978. This contract was not renewed, and B.I. left the study at this point.

(ii) Two other teachers did not complete the 'replication' rating ladders sent through the post in June 1979 on the grounds of pressure of work.

Instrumentation

Fifteen aspects of science teaching were selected as elements for the repertory grid, the choice resulting from discussions with two lecturers in science education. Triads for construct elicitation were determined according to part of a balanced incomplete block design, and care was taken to avoid the presentation of any element in successive triads.

Pilot work with three teachers in Manchester schools during the summer of 1978 influenced the repertory grid administration in the following ways.

(i) Short verbal labels on their own were inadequate to convey the meaning I intended, and a sheet was subsequently provided on which a brief description of each element was given (see next page).

CONTEXT: FIRST YEAR SCIENCE TEACHING

1.	Teacher exposition:	in which the teacher lectures, or otherwise instructs the class
2.	Discussion with class:	in which the teacher engages the whole class in a discussion
3.	Teacher demonstration:	the teacher demonstrates an experiment or shows the use of a piece of apparatus or equipment to the class
4.	Pupils' practical exercises:	where the pupils undertake exercises specified by the teacher
5.	Pupil project work:	where the pupils contribute to the direction of individual or group investigations
6.	Outside visits:	fieldwork, museums etc.
7.	Pupil individual work in school	pupils spending time working on their own in the classroom
8.	Homework:	any work set by the teacher to be done outside school hours
9.	Science club:	an out-of-school-hours activity to which pupils may come if they wish
10.	Showing film or videorecording:	or TV broadcast
11.	Discussion with small groups:	during the course of the lesson
12.	Discussion with individuals:	during the course of the lesson
13.	Using models and analogies:	including simulation work
14.	Achievement testing:	during the year, rather than end-of-year-examination
15.	Display:	of work and other materials in the classroom.

(ii) Rating direct on to a grid 'blank' was found to be difficult, since this did not allow the respondent to see how the elements stood in relation to each other. This problem led to the provision of a 'rating ladder' on which small cards bearing the names of the elements could be located.

(iii) A five-point scale appeared to be insufficiently sensitive to discrimination among 15 elements, and was replaced by a seven-point scale.

(iv) Respondents seemed to become locked into a particular dimension of thinking (for instance, in regard to aims of science teaching) and did not construe the elements from other points of view. Accordingly, a small card was prepared suggesting that respondents might like to construe the elements from the perspectives of aims, methods, outcomes and practicalities of management.

(v) The re-presentation of the elements and elicited constructs in a semantic differential format was found to be vulnerable to 'response set' and it was therefore decided to use rating ladders for the stability check, respondents being given the task of locating the elements upon them. (This, too, proved to present a problem in that respondents occasionally missed out the allocation of an element).

A description of how the revised repertory grid procedure was used with the fourteen probationary science teachers is provided in Section 8.2.1 and will not be repeated here.

The procedure adopted for the administration of the resistance-to-change grid was found to be satisfactory. A description of its use is included in Appendix 2.

Reflections on Method

This text as a whole is a sustained critical reflection upon repertory grid method, and I shall merely present here some comments on the specific grid used with the science teachers.

Although no adverse reaction to the elements was shown by the pilot work, almost all of the fourteen respondents saw 'Outside visits' and 'Science club' as outside the compass of first-year science teaching. Approximately one quarter of the respondents did not use 'Display' in their science teaching (despite the importance of a classroom exhibition during the early stages of Nuffield Combined Science): the lack of a 'base' laboratory for the individual teacher seemed to be a contributory factor here. During the year a few teachers showed a marked movement towards the use of display (one commenting that the initial repertory grid had stimulated her thinking in that direction). The upshot of the inclusion of elements apparently irrelevant to the respondents is likely to have resulted in some of the data contained within the grid contributing to error variance.

Regarding this issue, I think with hindsight that it would have been preferable to have included a common core of basic elements, and to have asked the respondents to add others as appropriate. The cost would have been a difficulty in using balanced sets of triads (though a researcher could prepare in advance a series of protocols for different numbers of elements and select that which is appropriate to the respondent's needs).

Although pilot work had not suggested it, a number of respondents appeared to construe 'Homework' and 'Achievement testing' to be of

a different kind of element from the remainder, in which teacher-pupil interaction was a more obvious substrate. In this sense, then, these two elements were heteregeneous compared with the rest. Triads in which either of these appeared were noticeably more difficult for the respondents to construe, and occasionally 'blocked' the elicitation procedure.

But these triads were not the only ones to give trouble. Others also caused difficulty, the problem appearing to lie in construing three elements (more or less simultaneously) in terms of a common dimension. Dyadic elicitation in which one asks for either a difference,or a similarity (followed by an opposite or a difference), seemed less restrictive and - as is argued in Section 6.5.2 - is still consistent with the Kellian view of similarity and contrast. This recognition led me subsequently to use dyadic elicitation in the investigation reported in Appendix 10. Occasionally the reverse happened, and a triad would elicit more than one construct - in which case all of the constructs were incorporated into the grid.

The use of a rating ladder and my undertaking all the recording of data as the grid was completed proved satisfactory: errors were almost completely eliminated.

There was generally an acceptance of the polar co-ordinate plots based upon INGRID analysis, although occasionally the contiguity of elements was disputed. This level of dispute must be regarded as something of an underestimate since it is likely that respondents tend to believe that an analysis presented by a researcher is somehow 'right' (particularly if it involves the power of a computer). Furthermore, it is difficult for a person to identify what is <u>missing</u> from an analysis:

the manifest makes forceful claim upon the attention.

Towards Broader Issues

By and large, I found relatively little difficulty in using the repertory grid in the manner outlined above and in Section 8.2.1. With one main exception, respondents were able to produce sufficient constructs to give a grid of reasonable size for analysis. The repertory grid could clearly be relied upon to produce an impressive quantity of data. But what about quality?

As the study unfolded I became increasingly disturbed by the impoverishment of the short construct labels and by problems such as the ascription of meaning to a rating of (say) 5 on a construct. In assigning ratings to elements, respondents were trying to tell me something - but my concentration on the completion of the grid matrix made me profoundly hard of hearing regarding what was actually being said. Subsequent attempts to reconstitute meaning from the desiccation of grid data proved fruitless even when the tape-recordings of the elicitation were replayed, for the location of elements upon constructs was almost always done by the respondent in silence. The recordings may have shown me that, on the whole, I was not injecting my own thinking into the respondents' grids (there were a few notable exceptions), but the manner of the grid elicitation was such that little was captured regarding the meanings of ratings.

My dissatisfaction with method led to the reorientation of this study towards a critique of method outlined in Section 1.2. This reorientation draws upon the data contributed by the science teachers, which is not presented here but is contained within other Appendices bearing upon specific aspects of repertory grid method.

APPENDIX 2: The use of Hinkle's resistance-to-change grid in determining the relative importance of constructs.

Problem

One of the difficulties for the researcher using repertory grids is that they provide no indication of the degree of importance of the constructs for the respondents. While some researchers have asked their respondents to indicate (by ranking or rating) the order of importance of their constructs, this becomes progressively more difficult with increasing numbers of constructs and, as is implicit in Bannister and Mair's (1968:174) results regarding ranking, is likely to become increasingly unstable with the size of the task.

Hinkle's resistance-to-change grid offered the prospect of ordering the constructs in such a way that 'error variance' in the task could be minimised. A description of the procedure is given below, one of the science teachers' grids being used as an example. Specific investigations using resistance-to-change grid data are reported in Appendix 3.

Method

The respondents having completed a repertory grid as indicated in Appendix 1, each construct was written on a small card (save for the first "warming-up" construct 'used a lot in my teaching - not used in my teaching'). On my subsequent visit to the school the respondent was asked to indicate the preferred pole of each construct: in a handful of cases constructs were discarded from the procedure because the respondent saw preference as irrelevant. The cards were presented in pairs and the respondent was asked, if he or she were forced to change one of the two indicated preferences, on which of the constructs would he or she change. It was recognised that (following Hinkle) it might be impossible for a respondent to exhibit a differential preference, or that change on one construct

logically entailed change upon the other of the pair.

In the interests of speed of administration, Construct 1 was paired sequentially with all of the others. This construct was then removed, and Construct 2 was made the focus of comparison; the procedure was repeated until all the pairings had been made.

The outcomes of the preferences were recorded by constructing an upper triangle matrix in which a 1 in a row indicated that this construct was more resistant to change than the construct identified at the head of the column with which the row intersected. $\frac{1}{2}$ was entered where the respondent could not make a change on only one construct; a superscript L was used to indicate that this was as a result of logical entailment. The full matrix was constructed, and each row was then totalled to give a score for each construct's resistance to change.

An index of the consistency of this procedure was determined by re-arranging both rows and columns in terms of the order of the constructs' resistance-to-change scores (cf Phillips, 1977).

Results (here exemplified by those from H.T.)

Constructs (underlining indicates the initially preferred pole)

1.	Used a lot in my teaching	-	Not used in my teaching (discarded)
2.	Requires pupil initiative	-	Teacher directed
3.	Involves pupils with objects	-	Involve pupils with thoughts
4.	Emphasis on oral work	-	Emphasis on written work
5.	Aim is subject input	-	Aim is subject recall
6.	Involves the outside world	-	Limited to within the classroom
7.	Pupil centred	-	Teacher centred

8.	Didactic	-	<u>Heuristic</u>
9.	<u>Visual</u>	-	Verbal (oral and written)
10.	<u>High teacher/pupil interaction</u>	-	No teacher/pupil interaction
11.	<u>Easy to control</u>	-	Difficult to control
12.	Individual work	-	<u>Pupils working as a class</u>
13.	Theoretical orientation	-	<u>Practically orientated</u>
14.	<u>Formal teaching style</u>	-	Informal teaching style
15.	<u>Compulsory</u>	-	Voluntary

<u>Illustration of the way in which the resistance-to-change score is calculated</u>.

CONSTRUCT NUMBER	2 3 4 5 6 7 8 9 10 11 12 13 14 15	RESISTANCE TO CHANGE SCORE	RANK
2	- - - 1 ½ 1 1 - ½ 1 1 - -	6	7=
3	1 - - 1 - 1 - - - 1 1 - -	5	9
4	1 1 - 1 1 ½ - - - 1 ½ - -	6	7=
5	1 1 1 1 1 ½ 1 - - - ½ 1 -	8	4=
6	- - - - - - ½ - - ½ 1 - -	2	14
7	½ 1 - - 1 ½ ½ - - 1 ½ 1 1	7	6
8	- - ½ ½ 1 ½ 1 - - - - - -	3½	11=
9	- 1 1 - ½ ½ - - - 1 ½ - -	4½	10
10	1 1 1 1 1 1 1 1 - 1 1 1 1	12	2
11	½ 1 1 1 1 1 1 1 1 1 1 1 1	12½	1
12	- - - 1 ½ - 1 - - - 1 - -	3½	11=
13	- - ½ ½ - ½ 1 ½ - - - - -	3	13
14	1 1 1 - 1 - 1 1 - - 1 1 -	8	4=
15	1 1 1 1 1 - 1 1 - - 1 1 1	10	3

Notes. 1. In this particular example there is no instance of logical entailment.

2. The protocol for identifying preferences produces the upper right triangle. The matrix is completed by inversion/ reflection about the leading diagonal.

Illustration of the calculation of a consistency score for the resistance-to-change procedure.

CONSTRUCT NUMBER	11	10	15	5	14	7	2	4	3	9	8	12	13	6
11		1	1	1	1	1	½	1	1	1	1	1	1	1
10	-		1	1	1	1	1	1	1	1	1	1	1	1
15	-	-		1	1	-	1	1	1	1	1	1	1	1
5	-	-	-		1	1	1	1	1	1	½	-	½	1
14	-	-	-	-		-	1	1	1	1	1	1	1	1
7	-	-	1	-	1		½	-	1	½	½	1	½	1
2	½	-	-	-	-	½		-	-	1	1	1	1	1
4	-	-	-	-	-	1	1		1	-	½	1	½	1
3	-	-	-	-	-	-	1	-		-	1	1	1	1
9	-	-	-	-	-	½	-	1	1		-	1	½	½
8	-	-	-	½	-	½	-	½	-	1		-	-	1
12	-	-	-	1	-	-	-	-	-	-	1		1	½
13	-	-	-	½	-	½	-	½	-	½	1	-		-
6	-	-	-	-	-	-	-	-	-	½	-	½	1	

Note. This matrix is constructed by rearranging the previous matrix in terms of resistance-to-change score.

The consistency of the resistance-to-change procedure is based upon the number of discrepancies from a perfect Guttman scale, and is calculated according to the formula

$$\text{Consistency} = 1 - \frac{\text{Total score in lower left triangle}}{\text{Number of entries in lower left triangle}}$$

In the example quoted, the consistency score is 0.80

Consistency of resistance-to-change scores for the science teachers on the two occasions are given below.

RESPONDENT	R-C CONSISTENCY AUTUMN 1978	R-C CONSISTENCY SUMMER 1979
E.X.	.83	.85
K.C.	.89	.91
M.I.	.92	.92
S.D.	.88	.93
B.I.	.80	-
T.L.D.	.84	.83
T.B.	.85	.92
K.E.D.	.84	.81
G.C.	.82	.82
H.T.	.80	.89
T.S.	.89	.95
D.S.	.97	.89
N.X.E.	.97	.95
N.M.	.89	.76

Discussion

The evidence from these 14 respondents suggests that the rankings of the constructs derived from the resistance-to-change grids are reasonably reliable. Rankings from this procedure can be given rather more credence than those from simpler methods because of the comparison of each construct with all of the others.

However, the inference that the rankings represent an order of personal importance is rather less justified in the circumstances of this study. Take, for example, Construct 15 'Compulsory - Voluntary'. This represents an aspect of teaching over which the respondent had no personal control and yet which was a determinant of the way in which he operated in the classroom. Resistance-to-change for this construct, therefore, may reflect personal powerlessness rather than personal importance. Cognate examples could have have been provided from other 'situations' grids, though it was not always the case that 'Compulsory - Voluntary' (which appeared in a number) was highly resistant to change. The problem may be less significant when personal characteristics comprise the constructs being used,since 'external locus of control' is unlikely to be a feature of such grids.

There is a further problem. Suppose that the respondent exhibits a marginal preference for pole A of construct A-A', but that both A and A' are favoured - a situation that could easily happen with 'peculiar' constructs. Suppose also that the respondent evidences a marked preference for pole B of construct B-B', but that B would not be preferred to either A or A'. Faced with a resistance-to-change comparison, it is likely that the respondent would switch from A to A' (because it would make relatively little difference) in preference to switching from B to B'. The result would be a false preference in the resistance-to-change

grid for B over A, running counter to the 'absolute' order of preference of the four poles.

The presence of 'X-not X' constructs also intrudes a complication, in that the 'not X' pole lacks definition and hence preference for or against it will be difficult to interpret.

A possible solution is to treat the construct poles as separate entities (i.e. as concepts), eliminating any 'not X' constructs from consideration. There would be a price to pay for so doing: the number of paired comparisons would rise from $\frac{n(n-1)}{2}$ to a possible maximum of $\frac{2n(2n-1)}{2}$.

If the researcher had the time to prepare the construct pairings, a balanced incomplete block design containing a sample of the possible pairs might be a way of resolving the dilemma. Alternatively, ranking within triads might be preferable to the laboriousness of many pairwise comparisons.

Conclusion

Although the proportion of intransitivities in the science teachers' resistance-to-change grids was fairly small, and hence the derived rank order of constructs can be assumed to be reasonably reliable, resistance-to-change grids contain features which have yet to be explored fully. It may prove necessary to amend the procedure in order to accommodate the objections discussed above. The resistance-to-change data described above should therefore be treated as a rather crude index of importance in the work to be described in Appendix 3.

APPENDIX 3: Does the early elicitation of a construct imply its salience?

Problem

Tajfel and Wilkes (1964) found that salient constructs tended to be produced early in an elicitation routine (and also were produced with greater frequency, a matter not being considered here). Their finding is widely quoted in the literature but has been subjected to little further empirical test, and the resistance-to-change data described in Appendix 2 allowed the 'salience-earliness hypothesis' to be further investigated at both nomothetic and idiographic levels.

Method

The position in the elicitation sequence of the most resistance-to-change construct in each of the 27 'science teacher' grids was identified and categorised according to whether it appeared in the first, middle, or final third of the constructs elicited. Where there was a tie for first place in order of resistance, both constructs were included. A simple frequency table was constructed using these data.

At the level of the individual grid, the rank correlation was computed between order of elicitation and resistance to change, on the assumption that resistance-to-change was an indicator of salience. If salience, importance and resistance-to-change are strongly connected, then, on the Tajfel and Wilkes argument, positive correlations should be detected within the 27 grids being analysed and (presumably) the correlations would be consistent over time (it will be recalled that 14 of the grids were completed in Autumn 1978 and 13 were completed by those remaining in the sample in summer 1979).

Results

(a) The table below shows the combination of data regarding the location of the most important (resistant-to-change) construct.

RESPONDENT	DATE	POSITION OF MOST RESISTANT CONSTRUCT	NUMBER OF CONSTRUCTS	FIRST, MIDDLE OR FINAL THIRD OF CONSTRUCT
E.X.	Aut. 78	3, 10, 12	12	First, Final, Final
	Sum. 79	8	16	Middle
K.C.	Aut. 78	4	11	First
	Sum. 79	10	15	Middle
M.I.	Aut. 78	7, 10	12	Middle, Final
	Sum. 79	7, 10	14	Middle, Final
S.D.	Aut. 78	20	21	Final
	Sum. 79	11	17	Middle
T.L.D.	Aut. 78	7, 14	14	Middle, Final
	Sum. 79	6	14	Middle
T.B.	Aut. 78	8	13	Middle
	Sum. 79	9	9	Final
K.E.D.	Aut. 78	3	10	First
	Sum. 79	2	9	First
G.C.	Aut. 78	6	11	Middle
	Sum. 79	5, 8	12	Middle, Middle
H.T.	Aut. 78	10	14	Final
	Sum. 79	8	11	Final
T.S.	Aut. 78	3, 9, 11	12	First, Final, Final
	Sum. 79	11	11	Final
D.S.	Aut. 78	9	11	Final
	Sum. 79	6	8	Final
N.X.E.	Aut. 78	10	14	Final
	Sum. 79	12	15	Final
N.M.	Aut. 78	5	15	First
	Sum. 78	2, 4	13	First, First
B.I.	Aut. 78	7	16	Middle

TOTALS	First 8
	Middle 12
	Final 16

Notes. 1. The 'number of constructs' refers to those which were used in the resistance-to-change grid.

2. The categorisation refers to this same number of constructs. Where the number of constructs was not exactly divisible by 3, the middle category was expanded or contracted in order to leave the other two categories containing equal numbers of constructs.

(b) The table below shows the rank correlations between resistance-to-change and order of elicitation of constructs for each of the 27 grids.

RESPONDENT	SPEARMAN'S RHO	
	Autumn 1978	Summer 1979
E.X.	-.20	+.08
K.C.	+.07	+.04
M.I.	-.17	-.38
S.D.	+.36	+.46*
T.L.D.	-.15	+.42
T.B.	+.30	-.20
K.E.D.	+.32	+.39
G.C.	-.04	-.26
H.T.	-.10	-.31
T.S.	+.17	+.24
D.S.	-.01	+.21
N.X.E.	+.21	-.33
N.M.	+.60*	+.03
B.I.	+.05	no data

Notes. 1. The median value of the 27 coefficients is +0.05.

2. An asterisk indicates significance at the .05 level.

3. The Spearman rank correlation between the two columns (excluding data from B.I.) is +0.26

Discussion

The evidence presented here offers no support for Tajfel and Wilkes's claim that salience (here operationally defined in terms of resistance-to-change) and earliness of elicitation are positively related, even allowing for some uncertainty regarding the connection of salience to resistance-to-change. If the hypothesised relationship were strong, it could be expected to show through data which contain a measure of uncertainty.

When the positions of the most resistant-to-change constructs in each grid are combined across the whole sample there is a tendency for these to be biased away from earliness of elicitation (though the quality of the data is such that a formal statistical test, such as chi square, would be inappropriate here.)

When the 27 grids are considered individually, only two show significant positive relationships corresponding to Tajfel and Wilkes's findings, barely more 'han would be expected by chance. There is relatively little consistency in individual respondents across time, and an overall median correlation of only +0.05 clearly implies that, for these data at least, there is no support for the 'salience-earliness hypothesis' at the idiographic level either.

It is, of course, another question whether the 'salience-earliness hypothesis' would be supported in respect of the elicitation of constructs relating to personal characteristics: personality theories may be more readily available for use than theories about aspects of science teaching.

Conclusion

Making the assumption of a fairly strong relationship between salience and resistance-to-change of constructs, there is no evidence from the construing of situations (aspects of science teaching) that the more salient constructs tend to be elicited in the early stages of a grid protocol. This conclusion stands whether the data are treated nomothetically or idiographically.

APPENDIX 4: An empirical investigation of the potential properties of rating scales.

Problem

This investigation stemmed from the implicit assumption in repertory grid analysis that all constructs can be treated as equivalent. My research with science teachers had elicited a range of construct types including simple dichotomies (as evidenced by rating confined to 1 and 7), constructs in which the mid-point appeared indeterminate, and constructs in which a steady gradation of an attribute appeared between one pole and the other. Further, the nature of the mid-point appeared more problematic than had been acknowledged in the literature.

The account presented below is largely confined to methodology and results, since the findings are discussed at some length in Sections 8.3.7 and 8.3.8.

Method

Fifty-one students, most of whom were nearing the end of their Postgraduate Certificate year, took part in this investigation in May-June of 1981. Four separate groups of postgraduates, and one group of students on an in-service B.Ed. course, were involved: details of the groups are given below.

Postgraduates,	Science teaching	9
Postgraduates,	English teaching	18
Postgraduates,	Mathematics teaching	12
Postgraduates,	Art teaching	3
In-Service B.Ed. (Educational handicap)	mixed background	9

The combination of the flexible demands on attendance for the Art students and the closeness to the end of term may account for the disproportionately low number in this group.

Each student was given a stapled set of seven pages ('Profiling I': see addendum), each of which contained a number of questions designed to probe the way in which they might operationalise a particular bipolar construct. The seven constructs, a sample from a set previously elicited from teachers, were selected on the grounds that they would be likely to cover a range of manners of use. The seven constructs were:

Logical	-	Intuitive
Extrovert	-	Introvert
Trustworthy	-	Untrustworthy
Overconfident	-	Lacking in confidence
Inarticulate	-	Articulate
Thinks in concrete terms	-	Thinks in abstract terms
Conscientious	-	Not conscientious.

Each student was asked to give the meanings which he or she would ascribe to the two poles of each construct, and also to the mid-point rating. The student was then asked to identify which among the two poles and mid-point of each construct was the clearest in their mind, and which was the vaguest, it being indicated that more than one position could be identified as clearest or vaguest.

When this task was completed, each student was given the sheets labelled Profiling IIA and IIB (see addendum). The second of these sheets comprised eight diagrams, each accompanied by a written description, relating to different ways in which a bipolar construct might be

operationalised. The student was then asked to look back over the descriptions he or she had given for the two poles and mid-point of each construct and to identify the representation on Profiling IIB which best matched his or her descriptions. If none of these representations were satisfactory, it was open to the student to supply an alternative: in the event only five out of 351 responses were the students' own diagrams.

Results

Although there was a wide variety of idiosyncratic construals of the constructs (some examples are given in Section 8.3.6) there seemed to be little to suggest the presence of marked inter-group differences, and so the results from the 51 respondents have been aggregated.

On the whole, the mid-point tended to be construed as the most vague of the three construct-positions surveyed, though the effect was noticeably lower in respect of the two constructs in which the mid-point was likely to stand out as positively valenced against the negative valence of the poles - 'Extrovert-Introvert' and 'Overconfident-Lacking in confidence'. The aggregated results are given overleaf.

The data from the second part of the investigation (that dealing with choice of construct type) is reported as Figure 8.12 in Section 8.3.7 and therefore is not repeated here.

Conclusion

In the light of the findings and the discussion contained in Sections 8.3.7 and 8.3.8, there is some evidence for the following propositions

LOCATION ON CONSTRUCT		NUMBER OF TIMES CHOSEN AS CLEAREST	VAGUEST
Logical	Pole	39	2
	Mid-point	6	36
Intuitive	Pole	15	18
Extrovert	Pole	28	11
	Mid-point	18	25
Introvert	Pole	27	14
Trustworthy	Pole	40	3
	Mid-point	2	40
Untrustworthy	Pole	27	10
Overconfident	Pole	30	13
	Mid-point	21	20
Lacking in confidence	Pole	33	11
Inarticulate	Pole	31	17
	Mid-point	10	31
Articulate	Pole	39	2
Thinks in concrete terms	Pole	25	11
	Mid-point	6	30
Thinks in abstract terms	Pole	24	14
Conscientious	Pole	40	5
	Mid-point	8	35
Not conscientious	Pole	29	11

(i) although poles of constructs tend to be seen as clearer than their mid-points, the effect is weakest when the mid-point is likely to be positively valenced in comparison with the poles;

(ii) a minority of respondents are likely to construe the mid-point with some clarity;

(iii) the mid-point is not necessarily a point of meaninglessness or total uncertainty; and

(iv) constructs may be operationalised in very different ways, and hence may have differential implications for analysis and interpretation.

This investigation must be regarded as an initial exploration of what is clearly a complex set of issues, and it needs to be supported by studies of how people actually use rating scales, rather than of how they talk about using them.

Acknowledgement

This investigation took shape as a result of a number of conversations with my colleague David Melling, whose contribution to conceptualisation was of considerable significance.

ADDENDUM

NAME/PSEUDONYM.....

DEGREE SUBJECT.....

PROFILING I

Imagine that you are required to rate pupils on an 'assessment profile' made up from a series of five-point scales, and that one of these scales is...

EXTROVERT 1 2 3 4 5 INTROVERT

On this scale a rating of 1 is labelled EXTROVERT and a rating of 5 is labelled INTROVERT

A. If you were to use the label EXTROVERT what would you take it to mean?

B. If you were to use the label INTROVERT what would you take it to mean?

C. What would you mean if you were to give a pupil a rating of 3 on this scale?

D. Look back at the meanings you have written down in response to questions A, B, and C, and indicate

(a) which of the three rating levels 1, 3, and 5 has the CLEAREST meaning in your mind

(b) which of the three rating levels 1, 3, and 5 has the VAGUEST meaning in your mind

(NOTE: it is possible for you to give more than one rating for 'clearest' or 'vaguest'.)

One of the seven pages from the stapled set comprising form Profiling I. The seven constructs used are given on page A4.2.

NAME/ PSEUDONYM.....

DEGREE SUBJECT.....

PROFILING IIA

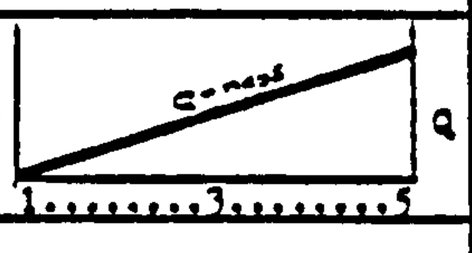
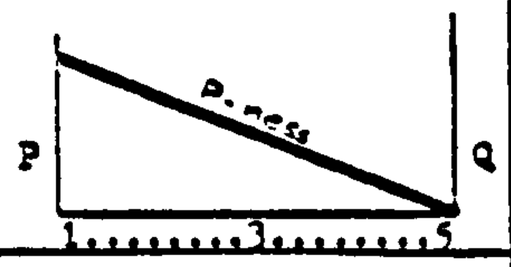
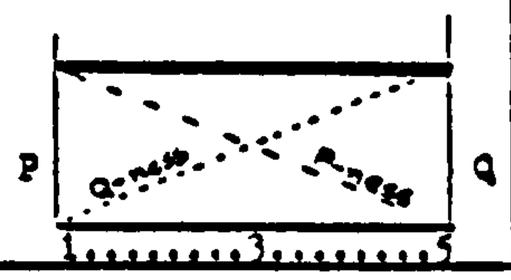
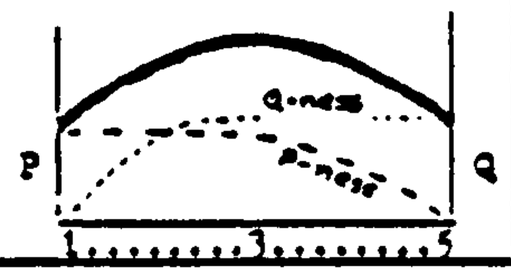
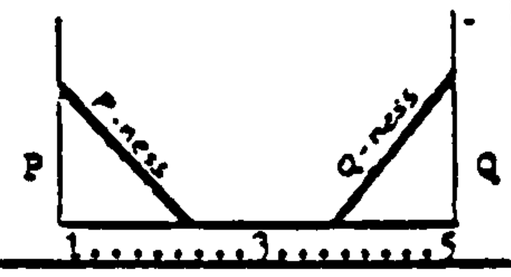
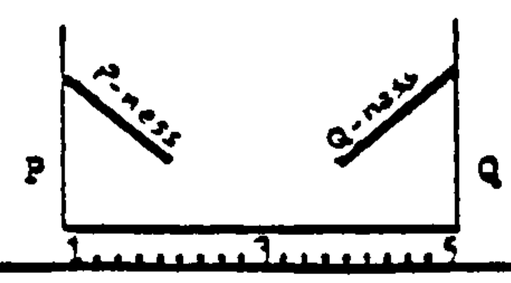
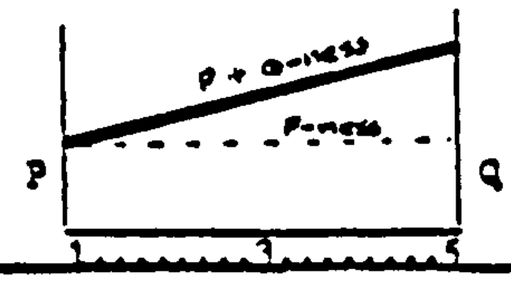
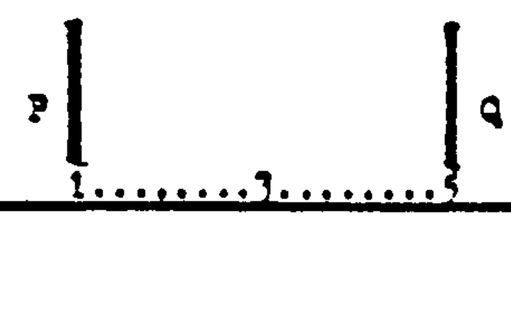
The accompanying sheet, PROFILING IIB, contains a set of representations, in diagrammatic and descriptive form, of some ways in which the various profile dimensions might be conceptualised and used.

Look back over the dimensions you have just been working on and select the representation from sheet PROFILING IIB that best matches each dimension. There is no single correct answer for any of the dimensions: what I am interested in is the way in which you as an individual view each of the dimensions.

You may feel that none of the representations adequately matches one or more of the dimensions listed here - in which event please use the space available to draw and/or describe a more suitable alternative in each case.

DIMENSION	NUMBER OF THE MOST APPROPRIATE REPRESENTATION	ALTERNATIVE REPRESENTATION (if preferred)
LOGICAL..... ...INTUITIVE		
EXTROVERT..... ...INTROVERT		
TRUSTWORTHY... ...UNTRUSTWORTHY		
OVERCONFIDENT... ...LACKING IN CONFIDENCE		
INARTICULATE... ...ARTICULATE		
THINKS IN CONCRETE TERMS..... ....THINKS IN ABSTRACT TERMS		
CONSCIENTIOUS... ..NOT CONSCIENTIOUS		

PROFILING IIB

REPRESENTATION NUMBER	DIAGRAM	DESCRIPTION
1		To be wholly P implies only 'not being Q'. As ratings increase from 1 to 5 they imply increasing amounts of 'Q-ness'.
2		To be wholly Q implies only 'not being P'. As ratings increase from 1 to 5 they imply <u>decreasing</u> amounts of 'P-ness'.
3		Rating 1 implies wholly P, no 'Q-ness'. Rating 5 implies no 'P-ness', wholly Q. Intermediate ratings 2,3,4 (in that order) reflect declining 'P-ness' and simultaneously increasing 'Q-ness', as indicated by the dotted lines.
4		Rating 1 implies wholly P, no 'Q-ness'. Rating 5 implies no 'P-ness', wholly Q. In this case, the mid-point rating 3 implies being wholly P <u>and</u> wholly Q at the same time.
5		Rating 1 implies wholly P, no 'Q-ness'. Rating 2 implies partial 'P-ness', no 'Q-ness'. Rating 3 implies no 'P-ness' <u>and</u> no 'Q-ness'. Rating 4 implies no 'P-ness', partial 'Q-ness'. Rating 5 implies no 'P-ness', wholly Q.
6		Rating 1 implies wholly P, no 'Q-ness'. Rating 2 implies partial 'P-ness', no 'Q-ness'. Rating 3 implies that the mid-point of the scale is so vague as to be meaningless. Rating 4 implies no 'P-ness', partial 'Q-ness'. Rating 5 implies no 'P-ness', wholly Q.
7		Rating 1 implies wholly P, no 'Q-ness'. Ratings 2 to 5 (in that order) imply the addition of increasing 'Q-ness' on top of a basic platform of 'P-ness'. In other words, it is necessary to have attribute P before acquiring any 'Q-ness'.
8		A simple dichotomy. People can be classed as having <u>either</u> attribute P (rating 1) <u>or</u> attribute Q (rating 5), but no intermediate ratings are possible.

Photoreduction of form Profiling IIB. P and Q refer to the ends of whichever dimension is under consideration: thus, if the dimension were HAPPY.....SAD, P would refer to HAPPY and Q to SAD.

APPENDIX 5: Reanalysis of Lyle's (1953) data

Problem

Kelly (1955:108) presents the results of a factor analysis of Lyle's data (see Section 7.2) and offers the interpretation that four of the five factors indicate the dichotomous nature of constructs. However, Kelly does not give the percentage of the variance explained by each factor. Examination of Lyle's (1953:106) original data analysis reveals the following table, which refers to his experimental sample of 53.

	Rotated factor loadings I	II	III	IV	V	R^2
Cheerful	60	-03	01	02	52	.63
Broadminded	00	12	42	05	66	.63
Sincere	06	02	-10	75	12	.59
Refined	-04	55	-07	20	00	.35
Sad	66	22	-03	-04	47	.71
Narrowminded	-02	04	57	03	72	.85
Insincere	-01	-05	12	48	27	.32
Vulgar	03	72	23	-06	43	.76
Σh^2	.80	.89	.58	.84	1.72	4.84 $\Sigma\Sigma h^2$
% variance extracted: (sic)	10	11	.07	11	.22	.61

The dichotomous nature of the constructs is rather less impressing when the variance extracted is taken into account.

Given that Lyle presents his original data in full and that the results for his experimental and control groups were very similar, it seemed

worthwhile to reanalyse the data using a more modern factor analysis in order to see whether the 'dichotomous' relationship still held.

Method

The combined experimental and control group data (N=102) was analysed using the factor analysis program FTAN from the PMMD suite (Youngman, 1976).

Results

The four-factor oblique solution was of excellent factorial simplicity (index = 0.96). The factors were highly intercorrelated, as shown in the lower triangle matrix given below.

FACTOR	1	2	3
2	0.73		
3	0.76	0.93	
4	0.73	0.70	0.71

The high intercorrelations are construed as supporting Kelly's interpretation of a large verbal facility effect (cf. Factor V in Lyle's table).

The factor pattern matrix (conventionally scored) is shown below, the salients being asterisked.

FACTOR	1	2	3	4
Cheerful	0.05	0.57*	-0.08	0.05
Sad	-0.06	0.59*	0.07	-0.04
Broadminded	0.66*	0.01	0.00	-0.03
Narrowminded	0.67*	-0.02	0.01	0.02
Sincere	-0.02	-0.04	-0.01	0.40*
Insincere	0.05	0.06	0.01	0.35*
Refined	-0.11	-0.07	0.46*	0.07
Vulgar	0.09	0.06	0.44*	-0.05
Relative Contribution %	46.5	29.4	14.7	9.4

The oblique solution presented here gives a 'cleaner' dichotomy than Lyle was able to demonstrate, and confirms - if anything, more strongly - the tendency for Lyle's 'error scores' to pair up according to the semantic opposition.

Given the dominance of the factor intercorrelation matrix, and the alternative interpretations put forward in Section 7.2, the claim for dichotomy in construing requires substantially stronger evidence than Lyle's data and analysis provide.

Conclusion

Whilst this reanalysis broadly supports Lyle's earlier analysis, it does not necessarily imply dichotomy in construing.

APPENDIX 6: An investigation into the effect of reversing the order of construct poles.

Problem

FOCUS analysis of repertory grids involves the reversal of the order of construct poles where this can improve the matrix of construct matching scores. There is an unstated assumption that this makes no difference to the construct interrelationships. However, it could be argued that some construct ratings might be influenced by the order of presentation of thepoles. The fairly widespread use of FOCUS analyses suggested that an investigation into the order of presentation of construct poles would be worthwhile.

Method

Sixteen teachers on a part-time in-service B.Ed. course selected ten pupils well known to them from one of their classes and wrote their names on slips of paper. They were then asked to construe thirteen triads previously chosen such that no element appeared in successive triads. Nine elements appeared four times, whilst the tenth appeared on three occasions. The number of constructs produced ranged between seven and sixteen, the modal number being thirteen (consistent with the number of triads presented).

During the following week the constructs were entered on seven-point rating ladders (which also had a 'not applicable' category), and seven days after the construct elicitation, fourteen of the students located each of the ten elements on each rating ladder. The data was subsequently transposed into a standard grid format.

Following this first grid completion, the constructs were divided into

the categories 'logical' and 'peculiar' since it could be hypothesised that the effect of reversal of presentation of a 'peculiar' construct might differ from the reversal of a 'logical' construct. Half of the 'logical' and 'peculiar' constructs (taken as separate categories) were then reversed at random. The reversed and unreversed constructs were written on sets of rating ladders in preparation for the students' return for re-rating after a second period of a week.

Ten of the original sixteen students returned seven days after completing the first grid and rated their original elements on their original constructs, no mention being made of the reversal (and no student giving any evidence of having noticed the changes). After this rating was completed, each student was given a FOCUS printout of his or her original grid and a short seminar was held, during which an outline of construct theory and grid method was given. Some of the students commented on the appositeness of the cluster analysis, and two remarked that they had not realised the similarities between pupils whom they appeared to have construed as dissimilar.

These second sets of data were transposed into the standard grid format, care being taken to re-reverse constructs where appropriate in order to align the dimensions with the first grids. The change for each construct was then computed as the mean Euclidean difference (see Section 9.5). A 2 x 2 analysis of variance was computed using the SPSS ANOVA program (the factors being reversed/unreversed and logical/peculiar, and the dependent variable being construct change.).

Results

The analysis of variance showed no significant main or interaction effects.

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIFICANCE
Peculiar/logical	0.397	1	0.397	1.547	n.s.
Reversed/unreversed	0.513	1	0.513	1.996	n.s.
Interaction	0.040	1	0.040	0.155	n.s.
Residual	31.323	122	0.257		

The use of analysis of variance provides a convenient summary of the results, but is not entirely justified since the 126 constructs are not independent (the modal value being 13 constructs per respondent). A simpler presentation, however, shows that there would be little reason to expect either main effect to be significant.

(i) Peculiar v Logical

	No. of constructs	Mean change	S.D.	Range.
Peculiar	34	0.97	0.45	0.32-2.07
Logical	92	1.09	0.52	0.45-2.85

(ii) Reversed v Unreversed

	No. of constructs	Mean change	S.D.	Range
Reversed	63	1.12	0.54	0.32-2.85
Unreversed	63	1.00	0.47	0.45-2.55

Conclusion

There is nothing in these data to suggest that reversing the order of presentation of construct poles influences rating behaviour. The practice of reversing (or reflecting) constructs for the purposes of cluster analysis

-hitherto assumed to have no effect - is thus given some limited empirical support.

APPENDIX 7: An investigation into the asymmetry of rating.

Problem

The use of a seven-point 'rating ladder' in which 7 is at the top and 1 at the bottom could be expected to be accompanied by a bias towards the higher ratings on the assumption that respondents scan the ladder from the top downwards. Although the following data were not collected with this in mind, the two sets of grids produced by the science teachers and the set of grids produced by the in-service B.Ed students offered an opportunity to explore the issue.

Method

The frequencies of occurrence for each rating category were counted for each grid. These frequencies were totalled for each of the three sets of grids, the totals being tested for deviation from random by chi square (excluding the 'not applicable' category X).

Results

(i) Science teachers, circa November 1978.

RESPONDENT	RATING CATEGORY 7	6	5	4	3	2	1	X	NUMBER OF CONSTRUCTS
H.T.	55	18	22	44	14	36	35	1	15
G.C.	33	21	25	18	24	27	32	-	12
E.X.	38	33	29	30	21	21	23	-	13
B.I.	24	38	48	43	22	39	39	2	17
M.I.	42	28	21	27	15	24	31	7	13
K.E.D.	3	36	24	32	19	29	17	5	11
K.C.	69	28	15	9	7	11	41	-	12
T.B.	96	16	16	17	6	14	90	-	17
D.S.	26	28	26	20	12	17	45	6	12
S.D.	101	46	11	26	16	38	88	4	22
N.M.	32	46	41	52	22	16	26	5	16
T.L.D.	51	31	18	33	23	32	33	4	15
T.S.	82	16	16	25	11	14	55	6	15
N.X.E.	32	47	48	37	24	17	18	2	15
TOTAL	684	432	360	413	236	335	573	42	205
PER CENT	22.3	14.1	11.7	13.5	7.7	10.9	18.4	1.4	

Excluding category X, 'expected' total for each rating category = 433(14.3 per cent)

Chi square = 314 , p << .01

(ii) Science teachers, June-July 1979.

RESPONDENT	RATING CATEGORY 7	6	5	4	3	2	1	X	NUMBER OF CONSTRUCTS
H.T.	24	18	19	33	24	17	24	6	11
G.C.	37	32	28	26	14	19	36	3	13
E.X.	42	41	43	37	29	29	33	1	17
M.I.	62	18	13	22	10	9	54	37	15
K.E.D.	-	19	31	31	22	22	13	12	10
K.C.	69	28	20	33	16	16	57	1	16
T.B.	68	24	5	20	10	9	96	8	16
D.S.	21	24	14	15	17	17	25	2	9
S.D.	96	18	8	32	14	17	78	7	18
N.M.	22	43	46	41	15	18	15	10	14
T.L.D.	32	49	30	30	24	30	26	4	15
T.S.	60	27	19	20	8	11	58	7	14
N.X.E.	18	58	60	47	26	23	8	-	16
TOTAL	551	399	336	387	229	237	523	98	184
PER CENT	20.0	14.5	12.2	14.0	8.3	8.6	18.9	3.5	

Excluding category X, 'expected' total for each rating category = 380

(14.3 per cent)

Chi square = 250 , p << .01

(iii) In-Service B.Ed. students, June 1980

RESPONDENT	RATING CATEGORY 7	6	5	4	3	2	1	X	NUMBER OF CONSTRUCTS
B.M.	13	16	25	22	20	17	15	2	13
D.L.	22	20	13	16	15	14	22	8	13
K.E.	14	9	5	6	8	7	12	9	7
T.T	16	37	32	33	23	14	5	-	16
S.C.	26	21	14	21	19	15	14	-	13
B.E.	-	28	30	24	14	18	5	1	12
C.D.	31	18	15	16	8	9	26	7	13
Q.N.X.	16	27	12	21	13	26	15	-	13
Q.S.	31	17	19	9	12	17	23	2	13
K.C.Q.	30	14	10	20	15	14	26	1	13
TOTAL	199	207	175	188	147	151	163	30	126
PER CENT	15.8	16.4	13.9	14.9	11.7	12.0	12.9	2.4	

Excluding category X, 'expected' total for each rating category = 176 (14.3 per cent).

Chi square = 18.57 , $p < .01$

Discussion

In each of the three sets of grids the overall distribution of ratings was significantly different from random, though some differences are apparent between the sets. The science teachers were nomothetically consistent across time (the idiographic data is discussed in Appendix 8), showing a marked preference for the extreme categories, and an equally marked reluctance to use categories 3 and 2. There is also a bias towards thetop three and away from the lowest three categories (48.1 to 37.0 per cent and 46.7 to 35.8 per cent on the two occasions). One might speculate that the rating of aspects of teaching could favour the ends and middle of the scale on constructs which might not be easy to treat as linear continua. A number of the constructs elicited (for instance 'visual-verbal') were such that they could have come close to being treated as a dichotomy with an additional category subsuming 'both characteristics' or 'don't know'.

The data from the in-service B.Ed. students reveals a broadly similar bias (46.1 to 36.6 per cent for top three and bottom three categories, respectively), but the deviations from 'expected' are rather lower. Further, the frequency of category 1 is below 'expected', in contrast to the data from the science teachers. Since the elements in this set of data were pupils, one might speculate on a 'social desirability' bias as well as bias due to methodological artefact. But social desirability will only be correlated with high rating if the constructs are elicited in such a way that the majority have the positively valenced pole at the top of the ladder. The question then is, do respondents tend to produce positively valenced poles first?

A check on the 126 constructs showed that 64 had been produced with the

positively valenced pole first, in contrast to 41 in which the negatively valenced pole was produced first. The remaining 21 were such that no clear 'evaluative overlay' could be attributed. This suggests that there could have been a combined effect of social desirability and methodological biases (in elicitation and/or use of the 'ladder') operating in the circumstances of this study.

Conclusion

The evidence suggests that a vertical rating ladder may be susceptible to a bias towards the upper end. However, other influences (such as the nature of elements and constructs and the order of production of construct poles) may also bias ratings, and the various effects are difficult to disentangle.

APPENDIX 8: The stability of rating style.

Problem

Considerable attention has been given to the correlates of rating extremity in grids (see the discussion in Section 6.6.2), but little to whether rating style is consistent. The data from the science teachers made it possible to investigate whether rating extremity endured over a period of about seven months (and in respect of a different set of constructs), whereas that from the in-service B.Ed students allowed an investigation of test-retest rating extremity over a period of seven days.

Method

The grids elicited from the science teachers in autumn 1978 and summer 1979 were taken as the basis of the first investigation. Rating extremity was operationally defined in terms of the average variance per construct in each grid: in practice it proved necessary to divide this value by the numbers of elements accepted in the INGRID analysis (the presence of gaps occasionally necessitated the elimination of a grid column) in order to produce comparable figures. The product-moment correlation coefficient was then computed between the autumn 1978 and summer 1979 sets of average variances.

A similar approach was used for the second comparison, but in this case no elements had to be eliminated and hence the average variance per construct could be computed for each grid (a few constructs being eliminated because of gaps). Further, the fact that the B.Ed. students were using identically-labelled grids on each occasion enabled a check to be made on whether rating style was consistent within individuals'

grids, that is, whether the pattern of construct variancesin an individual's first grid correlated highly with that in the second grid.

Results

(i) Science teachers, across grids analysis.

RESPONDENT	AVERAGE VARIANCE PER CONSTRUCT IN EACH GRID (SCALED TO A COMMON BASIS FOR COMPARISON)	
	Autumn 1978	Summer 1979
D.S.	4.44	4.29
N.M.	2.99	2.96
H.T.	4.07	3.12
G.C.	4.32	4.35
T.S.	4.91	5.87
T.B.	6.80	6.14
K.E.D.	2.52	2.11
K.C.	5.15	4.92
E.X.	3.76	3.94
S.D.	5.80	5.98
T.L.D.	4.51	3.81
M.I	4.61	5.32
N.X.E.	2.51	2.09

These results show a considerable consistency in rating extremity, and the two sets of data correlate (Pearson r) +0.92, p < .01.

(ii) In-service B.Ed. students, across grids analysis

RESPONDENT	AVERAGE VARIANCE PER CONSTRUCT IN EACH GRID	
	First grid	Second identical grid after seven days
B.E.*	20.61	17.82
B.L.	41.63	38.67
B.M.	31.92	29.85
Q.N.X.	37.83	43.41
C.D.	46.06	46.56
K.C.Q.	32.16	31.49
T.T	22.18	16.73
Q.S.	45.53	48.28
S.C.	37.01	34.55
K.E.	43.28	54.08

* B.E.'s second grid probably contained one inadvertently reversed construct which has been eliminated from this analysis.

These results likewise show a considerable consistency in rating extremity, and the two sets of data correlate (Pearson r) + 0.95, $p < .01$

(iii) In-Service B.Ed. students, within grids analysis.

RESPONDENT	CORRELATION BETWEEN PATTERNS OF CONSTRUCT VARIANCE WITHIN GRID	
B.E.*	+0.44	n.s.
D.L.	-0.52	n.s.
B.M.	+0.60	$p < .05$
Q.N.X.	+0.68	$p < .05$
C.D.	+0.74	$p < .05$
K.C.Q.	+0.44	n.s.
T.T.	+0.54	$p < .05$
Q.S.	+0.49	n.s.
S.C.	+0.52	n.s.
K.E.	+0.25	n.s
Median	+0.51	

Notes. 1. B.E.'s data again excludes theconstruct presumed to have been reversed.

2. D.L.'s grids were both notable for the small range of per-construct variance (9.63 to 13.05, and 8.74 to 14.63 respectively), hence allowing 'error variance' to have a marked influence.

3. K.E.'s data refers to only four of the seven constructs elicited, three being eliminated because of gaps.

Conclusion

The evidence presented here from two different kinds of replication suggests strongly that, taken over a whole grid, a person's rating style

is stable and enduring. Further, there appears to be a tendency towards intra-grid construct rating consistency.

APPENDIX 9: A test of the extremity/stability hypothesis in respect of individual grids.

Problem

In Appendix 8 evidence was presented to show that an individual was very likely to exhibit a consistent style of rating. The assumption in the literature regarding a connection between extremity and stability of rating could be tested, using the already available data from the science teachers, at two levels - first within individual grids and, second, over the sample as a whole.

Method

The INGRID analysis of the science teachers' and the in-service B.Ed students' grids indicated theproportion of the total grid variance that was contributed by each individual construct. These grids were, where necessary, reduced in size by deleting constructs in which there were gaps: in a small number of cases an element, too, was eliminated in order to retain the maximum amount of data for the INGRID analysis. These deletions were assumed to have little effect upon the investigation reported here.

In exploring the issue of 'per cell' stability I had considered the virtue of, and had calculated, the mean Euclidean differencebetween the test and retest sets of ratings for each construct: this constituted an index of change (and hence was an inverse function of stability). For each of the 25 science teachers' grids for which it was possible and for the 10 B.Ed. students' grids the product-moment correlation coefficient was calculated in respect of the two variables involved - percentage variance (as an index of relative extremity) and the mean Euclidean index of change. A high negative coefficient would be consistent with

the extremity/stability hypothesis.

The inability of the product-moment correlation coefficient to take into account whether an individual's ratings were extreme or moderate (percentage variance being monotonic with actual variance) could, however, lead to a situation in which a near-zero correlation could conceal support for the extremity/stability hypothesis. This could obtain, for instance, where all constructs exhibited high variance and/or the stabilities were high - in which case 'error variance' in either or both measures might obliterate an underlying relationship.

If this were to be the case, it could be expected to be revealed by a nomothetic analysis of the correlation between average construct variance per grid and grid stability. Accordingly, the data (already available from INGRID analyses and prior calculations of 'per cell' stability) were tested in this way.

Results

Science teachers: within grid analysis

RESPONDENT	CORRELATION BETWEEN PERCENTAGE VARIANCE PER CONSTRUCT AND INDEX OF CHANGE Autumn 1978		Summer 1979	
E.X.	-0.04	n.s.	+0.04	n.s.
K.C.	-0.07	n.s.	-0.39	n.s.
M.I.	-0.61	$p < .05$	+0.15	n.s.
S.D.	-0.38	n.s.	+0.35	n.s.
T.L.D.	-0.47	n.s.	+0.24	n.s.
T.B.	+0.13	n.s.	+0.62	$p < .05$
K.E.D.	+0.20	n.s.	-0.62	n.s.
G.C.	+0.09	n.s.	-0.06	n.s.
H.T.	-0.53	$p < .05$	-0.75	$p < .05$
D.S.	+0.30	$p < .05$	+0.10	n.s.
N.X.E.	+0.18	n.s.	-0.12	n.s.
N.M.	-0.07	n.s.	no change scores available	
T.S.	-0.45	n.s.		
B.I.	-0.03	n.s.		
Median	-0.06		+0.04	

B.Ed. students: within grid analysis.

RESPONDENT	CORRELATION BETWEEN PERCENTAGE VARIANCE PER CONSTRUCT AND INDEX OF CHANGE	
D.L.	+0.58	n.s.
B.M.	+0.19	n.s
Q.N.X.	-0.15	n.s.
C.D.	-0.29	n.s.
K.C.Q.	+0.68	$p < .05$
T.T	-0.05	n.s.
Q.S.	-0.64	$p < .05$
S.C.	-0.71	$p < .05$
K.E.	-0.64	n.s.
B.E.	+0.03	n.s.
Median	-0.10	

Note: In the preceding two tables a negative correlation is consistent with the extremity/stability hypothesis.

Science teachers: across grids analysis

RESPONDENT	AUTUMN 1978		SUMMER 1979	
	AVERAGE VARIANCE PER CONSTRUCT	'PER CELL' WHOLE GRID STABILITY	AVERAGE VARIANCE PER CONSTRUCT	'PER CELL' WHOLE GRID STABILITY
E.X.	3.76	0.71	3.94	0.76
K.C.	5.15	0.74	4.29	0.83
M.I.	4.61	0.74	5.32	0.87
S.D.	5.80	0.79	5.98	0.81
T.L.D.	4.51	0.81	3.81	0.77
T.B.	6.80	0.76	6.14	0.69
K.E.D.	2.52	0.74	2.11	0.77
G.C.	4.32	0.66	4.35	0.81
H.T.	4.07	0.79	3.12	0.79
D.S.	4.44	0.79	4.29	0.87
N.X.E.	2.51	0.72	2.09	0.82
N.M.	2.99	0.81	no whole grid stability available	
T.S.	4.91	0.81		
B.I.	3.52	0.71		
CORRELATION	+0.23	n.s.	-0.05	n.s.

Note: 'Average variance per construct' has been divided by the number of elements involved in order to produce a common basis for comparison.

B.Ed. students: across grids analysis.

RESPONDENT	AVERAGE VARIANCE PER CONSTRUCT	'PER CELL' WHOLE GRID STABILITY
D.L.	41.63	0.85
B.M.	31.92	0.86
Q.N.X.	37.83	0.83
C.D.	46.06	0.87
K.C.Q.	32.16	0.76
T.T.	22.18	0.83
Q.S.	45.53	0.91
S.C.	37.01	0.88
K.E.	43.28	0.92
B.E.	20.61	0.89
CORRELATION	+0.33 n.s.	

Notes: 1. B.E.'s grid contained one construct which was probably reversed inadvertently on the second occasion. This construct has been eliminated from the analysis.

2. In both of the above tables a positive correlation is consistent with the extremity/stability hypothesis.

Discussion

The data provide little evidence for the extremity/stability hypothesis either at the within-grid or across-grid levels. Since the data span two different types of grid ('situations' as supplied elements, and pupils as elicited elements), the challenge to the extremity/stability hypothesis is rather more than marginal.

Although a number of individuals showed high negative correlations between percentage variance per construct and index of change, the evidence from the science teachers suggests that these may not be stable. Only respondent H.T. of the eleven science teachers who provided two full sets of data maintained a consistently moderate negative correlation, and a number of the others showed marked differences over time.

Conclusion

The evidence from this investigation gives little support for a positive connection between extremity of rating and stability of rating.

APPENDIX 10: Element fuzziness and stability

Problem

The stabilities of the science teachers' grids, in which the elements were a set of supplied general teaching situations, were found to be noticeably lower than the stabilities of grids produced by in-service B.Ed. students who were rating a set of pupils chosen by themselves. It was not clear whether the discrepancy might be attributed to 'element fuzziness' in respect of the general teaching situations or to the difference between supplying and eliciting elements. The opportunity presented itself to make an investigation of the effect of 'element fuzziness' whilst limiting respondents to elicited elements.

It was hypothesised that grids in which pupils were to be rated would be more stable than grids in which specific teaching situations were the elements, and both of these would be more stable than grids concerned with general aspects of teaching (see Section 5.5).

Method

Twenty-two students on an in-service Diploma course in Counselling were randomly assigned to three groups and were asked to write down eight elements of their own choice drawn from one of the following fields:

(i) pupils (N=8)

(ii) specific teaching situations from their own experience (N=7)

(iii) general aspects of teaching (N=7)

Dyadic elicitation of constructs was chosen on the grounds of speed and simplicity, and because it was necessary to administer the grids to the whole group.

As each construct was elicited, the respondents were asked to write the labels at the top and bottom of a seven-point rating ladder (an extra 'not applicable' category was provided), and to locate the elements on the dimension, Three respondents worked sufficiently quickly to produce a second grid based on a different field of interest from (ii) to (iii) above. Copies of the instruction sheet, the element elicitation sheet and a page from the rating ladder booklet are attached at the end of this account.

The data were transposed into standard grid matrices, and 'retest blanks' of the rating ladders (including the elicited labels) prepared for each respondent.

Seven days after the first administration the respondents re-rated their elements on their rating ladders. Seventeen respondents were present on this second occasion, providing twenty test-retest comparisons. Unfortunately, absence on this second occasion were markedly different between the three groups, group (iii) being reduced to four.

Results

The stabilities for the three groups calculated on the 'per cell' basis (see Section 10.6) were as follows.

TYPE OF ELEMENT	NO. OF GRIDS	STABILITY INDEXES	MEDIAN
(i) Pupils	9	.89 .89 .88 .88 .87 .87 .85[a] .85 .83[b]	.87
(ii) Specific teaching situations	7	.90 .88 .84[c] .83 .80 .76 .74	.83
(iii) General situations	4	.83[b] .82 .80[c] .55[a]	.81

Note: the superscripts a,b and c indicate where three respondents each completed two grids.

Smirnov tests (Conover, 1971:309f) were run between the types of element (taken in pairs), with the following results.

COMPARISON	VALUE OF THE SMIRNOV TEST STATISTIC T,	SIGNIFICANCE LEVEL (ONE-TAILED)
(i) - (ii)	0.603	$p < .05$
(i) - (iii)	0.889	$p < .01$
(ii) - (iii)	0.429	n.s

Conclusion

Whilst acknowledging the dubiousness of the statistical comparisons (particularly in view of the fact that data from three respondents appeared in more than one group), there is nevertheless some evidence in support of the hypothesis that the 'fuzzier' the elements are likely to be, the lower will be the stability of rating. Clearly, a more rigorous investigation is necessary than was possible under conditions of marked differential 'mortality'.

Statistical footnote

My original intention was to undertake the pairwise comparisons by using the Fisher Exact Probability test. However, there is a problem regarding the criterion by which one dichotomises the (combined) data. For example, if groups (i) and (ii) are being compared, it makes a considerable difference whether the 'cut' is made such that the upper group is composed of stabilities above and including the combined median (.86), or above and including the value .85. Siegel (1956:96f) offers no guidance.

The difference is immediately visible from the respective 2 x 2 contingency tables.

TYPE OF ELEMENT	CRITERION FOR 'CUT' ⩾.86	CRITERION FOR 'CUT' <.86
Pupils	6	3
Specific teaching situations	2	5
Significance level (Fisher)	n.s.	

CRITERION FOR 'CUT' ⩾.85	CRITERION FOR 'CUT' <.85
8	1
2	5
p < .01	

In other words, making a minor shift to the position of the 'cut' can make a marked difference to the conclusion one might draw. One would expect this problem to arise not infrequently with small samples where distributions of results are uneven.

In the study reported here, the rank order of the stabilities is of some importance and other nonparametric tests are more sensitive. Siegel (1956:157) states that the Kolmogorov-Smirnov test is more efficient for very small samples than is the Mann-Whitney U test, but does not give a method for unequal sample sizes. However, Conover (1971) provides an appropriate method (under the title 'Smirnov test'), which was used in this investigation.

ELICITING CONSTRUCTS AND RATING ELEMENTS

1.a. Take the first pair of elements indicated on the ELEMENTS sheet. Can you find a way of contrasting between them in respect of a particular attribute? (E.g. one pupil might be "well-mannered", and the other "ill-mannered"). This contrast forms the first dimension, or construct : label each end of the rating ladder according to the 'poles' of the contrast - it does not matter which pole goes at the top, and which at the bottom.

The example of "well-mannered"----------"ill-mannered" is shown on the specimen rating ladder opposite.

1.b. Alternatively, you may prefer to identify a similarity between the two elements. If so, the similarity becomes one pole of the dimension, and you have to supply the opposite pole yourself by thinking of the appropriate contrast.

Using the same example with pupils, the two pupils might both be "well-mannered", and you might choose to contrast this with "ill-mannered", "rude", or some similar adjective or phrase.

2. Now place all eight elements (using the code letters) on the 7-point rating ladder whose poles you have just defined. The placing of each element is entirely up to you. You do not have to put an element in each box on the ladder - some boxes may well remain empty. (See the example opposite)

If you have second thoughts about any placing, feel free to cross out and re-place.

The box labelled X is for any element that cannot sensibly be fitted on to the rating ladder.

Please check that all eight elements appear somewhere on the rating sheet.

3. Repeat the identification of dimension poles, and the rating of elements on dimensions for the second and subsequent pairings listed on the element sheets.

Do not use any dimension more than once : try to identify a new dimension for each new pairing. I hope you will be able to identify at least eight different dimensions, though this is not always possible. If a pair does not 'trigger' a new dimension, simply pass on to a new pair - it is not essential to produce a dimension for each pair.

Use a fresh rating sheet for each dimension.

4. If a dimension suggests itself independently of a pairing, it is perfectly acceptable: the pairs are only there to 'trigger' dimensions in the mind. Equally, a pairing may suggest more than one usable dimension : again, perfectly acceptable.

5. I have given you 12 rating ladders just in case you come up with 12 different dimensions, but you are under no pressure whatever to fill all 12 in!

pole label: *well-mannered*

7	A
6	DF
5	
4	H
3	CEG
2	
1	B

pole label: *ill-mannered*

Not applicable X	

A10.5

Photoreduction of the instruction sheet used in the investigation.

Name or pseudonym .....

ELEMENTS

LEARNING ACTIVITIES

Identify EIGHT different types of learning activity which you arrange for a particular class. (For example, you might, in the course of a Nuffield Combined Science lesson to first year secondary pupils, use a 'discovery practical' to separate ink into its component dyes).

Write down the learning activities, one to each of the boxes below.

CODE LETTER	ELEMENT: LEARNING ACTIVITY
J	
K	
L	
M	
N	
P	
Q	
R	

PAIRING OF ELEMENTS

1.	J with K	7.	L with Q
2.	L with M	8.	J with N
3.	N with P	9.	P with Q
4.	Q with R	10.	N with M
5.	K with P	11.	L with K
6.	M with R	12.	R with J

Name or pseudonym .....

pole label .....

7	
6	
5	
4	
3	
2	
1	

pole label .....

Not applicable	X

A blank page from the rating ladder booklet.

APPENDIX 11: Is there a 'practice effect' with regard to grid stability?

Problem

The possibility that experience of completing grids might influence grid stability arises from inspection of the two sets of 'per cell' stability indexes for the science teachers. No a priori hypothesis was advanced in respect of the data presented below.

Method

The 'per cell' stability indexes for the eleven science teachers who completed two 'test-retest' grids were compared across time (Autumn 1978 to Summer 1979) using the Wilcoxon test (Siegel, 1956:75f).

Results

The two sets of stability indexes were as follows.

RESPONDENT	STABILITY INDEX AUTUMN 1978	STABILITY INDEX SUMMER 1979	CHANGE IN INDEX
E.X.	.71	.76	+.05
K.C.	.74	.83	+.09
M.I.	.74	.87	+.13
S.D.	.79	.81	+.02
T.L.D.	.81	.77	-.04
T.B.	.76	.69	-.07
K.E.D.	.74	.77	+.03
G.C.	.66	.81	+.15
H.T.	.79	.79	nil
D.S.	.79	.87	+.08
N.X.E.	.72	.82	+.10

The Wilcoxon Matched-pairs Signed-ranks Test applied to the changes in index gave a value of T=8, equal to the critical value for $p \leq .05$, two-tailed test.

Discussion

The analysis of the data suggests that ratings on grids may reach a higher level of stability once the respondents have become accustomed to the demands of repertory grid methodology. However, it could also be the case that the science teachers may have tightened their construing of science teaching during their probationary year, thus making their ratings appreciably more certain and hence reliable on the second occasion.

These findings, which are by no means conclusive regarding a practice effect, nevertheless suggest that the ratings given in a respondent's first grid should be given a particularly wary interpretation.

Conclusion

Whilst the finding of a statistically significant increase in stability on the second occasion of testing is suggestive of a practice effect, the alternative explanation put forward indicates that too much reliance cannot be placed upon its existence, and that further research on this problem is needed.

APPENDIX 12: A comparison of grid stability indexes

Problem

The grids collected from the science teachers contained sufficient gaps to undermine the validity of either of Slater's stability indexes. The 'per cell' stability index described in Section 10.6 overcame that problem, but it was still unclear how this would relate to other indexes where comparisons were more feasible. The data from the in-service B.Ed. students, though not collected with this investigation in mind, were nevertheless sufficiently free from blank cells to enable this small-scale exploration to be undertaken.

Method

These grids were completed as indicated in Appendix 6 and were not originally intended to serve the purpose of comparing stability indexes. The relatively low percentage of gaps, together with no evidence of any - even slight - effect of reversing some of the constructs, made it possible to use this data for this study. The key parameters were as follows.

Respondents	:	ten teachers on an in-service B.Ed course
Elements	:	ten elicited pupils
Constructs	:	elicited by the method of triads
Re-test	:	after seven days.

Results

See the table overleaf.

Respondent	Number of constructs	Number of usable constructs	STABILITY INDEX (REDUCED GRID) Bannister consistency	Slater Coefficient of convergence.	Slater general degree of correlation	'Per cell' index	'Per cell' index (whole grid)
BM	13	12	.80	.891	.878	.86	.86
DL	13	9	.86	.917	.864	.85	.85
KE	7	4	.94	.987	.962	.92	.92
TT	16	16	.87	.813	.783	.83	.83
SC	13	13	.78	.793	.854	.88	.88
BE	12	11	.50	.490	.712	.84	.84
CD	13	10	.91	.921	.873	.87	.85
QNX	13	13	.91	.906	.832	.83	.83
QS	13	12	.94	.937	.937	.91	.91
KCQ	13	12	.68	.702	.616	.76	.75
BE*	12	10	.96	.962	.889	.89	.84

Notes. (a) The 'number of usable constructs' is the number of constructs in which no 'gap' appeared.

(b) All stability indexes, save that in the final column, are based upon 'reduced' grids where appropriate.

(c) Respondent BE's grid contained one construct which was probably reversed in error. Elimination of this construct from consideration gave the data recorded in row BE*.

The correlation matrix relating the four stability indexes for the original ten reduced grids is given as Figure 10.3 in Section 10.7. Removal of the (probably) reversed construct in BE's grid altered the correlation matrix to that shown below.

Index	Bannister consistency	'Per cell' index	Coeff. of convergence
'Per cell' index	0.73		
Coefficient of convergence	0.90	0.80	
General degree of correlation	0.78	0.96	0.90

It is noticeable that the correlations of the Bannister consistency score and the coefficient of convergence with the other two indexes are raised to a substantial extent.

Discussion

The implications of these results were discussed in Section 10.7, and are not reiterated here.

Conclusion

The 'per cell' index is shown to be a robust index of grid stability, and its capacity to cope with missing data renders it superior to Slater's general degree of correlation as computed in his DELTA program. Both the Bannister consistency score and Slater's coefficient of convergence (from his COIN program) are shown to be markedly affected by the reversal of ratings on a construct (and, by extension, part of a construct).

APPENDIX 13: An illustration of Youngman's solution to the problem of analysing 'gappy' grids

Problem

Neither INGRID nor FOCUS (nor, for that matter, almost all of the other analytical routines available) can cope adequately with data missing from grid cells. If an appropriate distance matrix could be computed, this could be input into a nonmetric multidimensional scaling program in order to produce a spatial representation of the elements.

Method

Youngman devised a program which computed the mean Euclidean distance between each pair of elements, omitting from the calculation any pair of construct ratings which contained at least one 'gap'. The matrix was output as the lower left triangle, each distance being accompanied by the number of construct ratings taken into account. The matrix was then used as input into the nonmetric multidimensional scaling program NMMS (originally MINISSA-1B) to depict the spatial relationship of the elements.

Results

An example of theoutput from Youngman's program is given below.

DISTANCE MATRICES

15 ELEMENTS AND 14 CONSTRUCTS FOR N.M. GRID 2A ONLY

RATINGS FOR ELEMENT	10	6.	1.	2.	7.	5.	2.	2.	1.	1.	4.	6.	6.	5.	3.
RATINGS FOR ELEMENT	20	6.	5.	6.	6.	6.	5.	4.	4.	5.	5.	6.	5.	5.	5.
RATINGS FOR ELEMENT	30	6.	4.	5.	5.	4.	4.	5.	1.	2.	4.	6.	6.	4.	5.
RATINGS FOR ELEMENT	40	7.	4.	6.	6.	4.	4.	7.	7.	7.	6.	2.	7.	0.	7.
RATINGS FOR ELEMENT	50	2.	7.	6.	5.	4.	6.	4.	7.	7.	7.	3.	4.	4.	6.
RATINGS FOR ELEMENT	60	1.	0.	7.	2.	0.	7.	0.	0.	4.	3.	2.	1.	5.	6.
RATINGS FOR ELEMENT	70	6.	6.	3.	4.	3.	5.	6.	7.	7.	5.	5.	5.	4.	0.
RATINGS FOR ELEMENT	80	6.	4.	2.	4.	1.	2.	2.	4.	4.	7.	3.	3.	3.	2.
RATINGS FOR ELEMENT	90	1.	6.	7.	5.	5.	7.	6.	6.	6.	4.	5.	4.	4.	7.
RATINGS FOR ELEMENT	100	3.	0.	6.	5.	5.	6.	5.	4.	1.	2.	4.	5.	4.	6.
RATINGS FOR ELEMENT	110	5.	5.	5.	6.	6.	5.	4.	6.	6.	5.	6.	4.	5.	4.
RATINGS FOR ELEMENT	120	5.	4.	5.	6.	6.	5.	4.	6.	6.	5.	6.	4.	5.	4.
RATINGS FOR ELEMENT	130	5.	0.	5.	5.	7.	2.	1.	4.	2.	5.	2.	6.	3.	5.
RATINGS FOR ELEMENT	140	3.	0.	1.	3.	1.	0.	1.	4.	2.	7.	0.	2.	3.	1.
RATINGS FOR ELEMENT	150	1.	6.	6.	4.	3.	7.	5.	5.	5.	3.	4.	5.	6.	6.

DISTANCES BETWEEN ELEMENTS

ELEMENTS	10	20	30	40	50	60	70	80	90	100	110	120	130	140
20	24													
N	14.													
30	17	15												
N	14.	14.												
40	37	21	27											
N	13.	13.	13.											
50	38	20	29	21										
N	14.	14.	14.	13.										
60	40	29	31	34	22									
N	10.	10.	10.	9.	10.									
70	33	18	24	16	18	31								
N	13.	13.	13.	12.	13.	9.								
80	24	25	24	30	28	35	22							
N	14.	14.	14.	13.	14.	10.	13.							
90	37	19	26	24	13	18	20	34						
N	14.	14.	14.	13.	14.	10.	13.	14.						
100	25	18	16	28	24	21	26	31	18					
N	13.	13.	13.	12.	13.	10.	12.	13.	13.					
110	26	8	20	22	17	28	15	24	18	21				
N	14.	14.	14.	13.	14.	10.	13.	14.	14.	13.				
120	25	8	20	21	18	28	15	24	18	21	3			
N	14.	14.	14.	13.	14.	10.	13.	14.	14.	13.	14.			
130	19	20	22	30	27	31	29	23	31	23	22	22		
N	13.	13.	13.	12.	13.	10.	12.	13.	13.	13.	13.	13.		
140	28	33	31	43	32	34	30	13	38	33	31	31	28	
N	11.	11.	11.	10.	11.	8.	10.	11.	11.	11.	11.	11.	11.	
150	34	20	23	25	16	16	21	31	11	16	19	20	28	33
N	14.	14.	14.	13.	14.	10.	13.	14.	14.	13.	14.	14.	13.	11.

Discussion

The potential usefulness of Youngman's program is readily apparent. The 15 x 14 grid contains only 11 gaps (5 per cent), yet these would be sufficient to eliminate six elements or eight constructs, (or, if elements 60 and 140 were eliminated, three constructs) - a minimum elimination of nearly one third of the grid. In contrast, the number of pairwise comparisons in this distance matrix is reduced by only ten per cent from the maximum possible - evidence that Youngman's program is making much greater use of the data. The NMMS output showing element relationships is relatively meaningless in isolation, and hence is not shown here.

It might be possible to devise a similar program for constructs, though this would have to be rather more complex because of their bipolar nature. The reflection of appropriate constructs in order to minimise the inter-construct distances (in an analogous manner to the FOCUS routine's maximising of matching scores) would seem to be necessary to an appropriate solution, and some method of depicting the bipolarity of the constructs would need to be devised.

Conclusion

If element interrelationships are important to the researcher, Youngman's distance algorithm offers the prospect of maximising the use of the data and can be input into a nonmetric multidimensional scaling program in order to obtain a visual display.

APPENDIX 14: A comparison of the effect of different metrics upon element clustering.

Problem

In Section 9.4.2 it was argued that the Euclidean distance metric is superior to the city block metric in the analysis of grid data on the grounds that it better captures the larger discriminations made by the respondent. Given that the theoretical argument was sound, it remained to be seen whether the choice of metric affected the clustering of elements.

Method

The data collected in the study of 'element fuzziness' (Appendix 10) was used in this investigation. The original 26 grids had been FOCUSed in order to provide feedback to the respondents, and thus the FOCUS element similarity matrices were readily available. Six of these grids were selected in order to give a sample with a range of dispersions of element similarity scores. The same grids were also analysed by the cluster analysis program CARM (Youngman, 1976) using both the squared Euclidean distance and product moment correlation (similarity) coefficients to obtain element relationship matrices. INGRID element distances are monotonic with squared Euclidean distances, and though available, were not used since they would have given identical multi-dimensional scaling plots.

The argument in Section 9.4.2 was concerned with the comparison between the city block metric used in FOCUS and the mean Euclidean difference (closely related to the squared Euclidean distance), and the rank correlation coefficients (Spearman's rho) between the FOCUS and CARM (Euclidean) element relationship matrices were computed for each of

the six selected grids.

Four of these grids were chosen for further study in order to explore the comparative effects on element relationships of the three coefficients used in the analysis (lack of time preventing a fuller comparison). The element relationship matrices were input into the nonmetric multidimensional scaling program NMMS; a two dimensional output being selected in order to minimise stress values in accordance with Kruskal and Wish's (1978:50) recommendation to maximise the ratio of the number of elements to be dimensionality of the solution. The ratio used, 8:2, was the closest approach possible to the recommended ratio of greater than 4:1. The output enabled the element interrelationships to be compared visually.

Results

For the six grids originally selected, the correlations between the FOCUS similarity matrices and the CARM Euclidean distance matrices (N=28 comparisons in each case) were

-.99, -.99, -.98, -.98, -.96, and -.95 (p values all $\ll$ .01)

the negative signs indicating the comparison of a similarity with a distance measure.

The comparisons for the reduced sample of four grids were as follows.

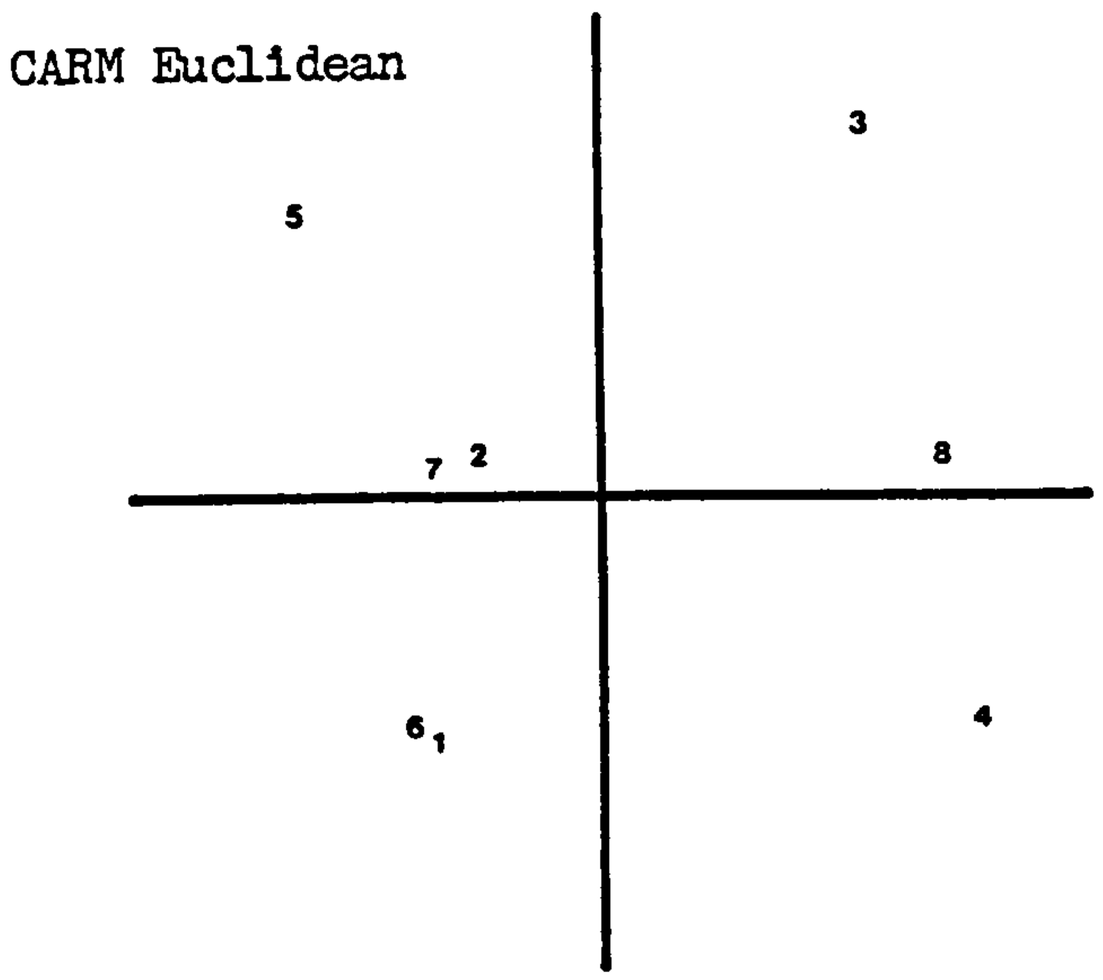
MEASURES COMPARED	RANK CORRELATION (SPEARMAN RHO) BETWEEN ELEMENT RELATIONSHIP MATRICES FOR FOUR RESPONDENTS: H.E	C.F.C.	E.U.	C.X.
FOCUS (Sim)/CARM Euclidean (dist)	-.96	-.99	-.98	-.98
FOCUS (Sim)/Product moment (Sim)	+.78	+.59	+.94	+.89
CARM Euclidean (dist)/Product moment (Sim)	-.76	-.62	-.96	-.90

Discussion

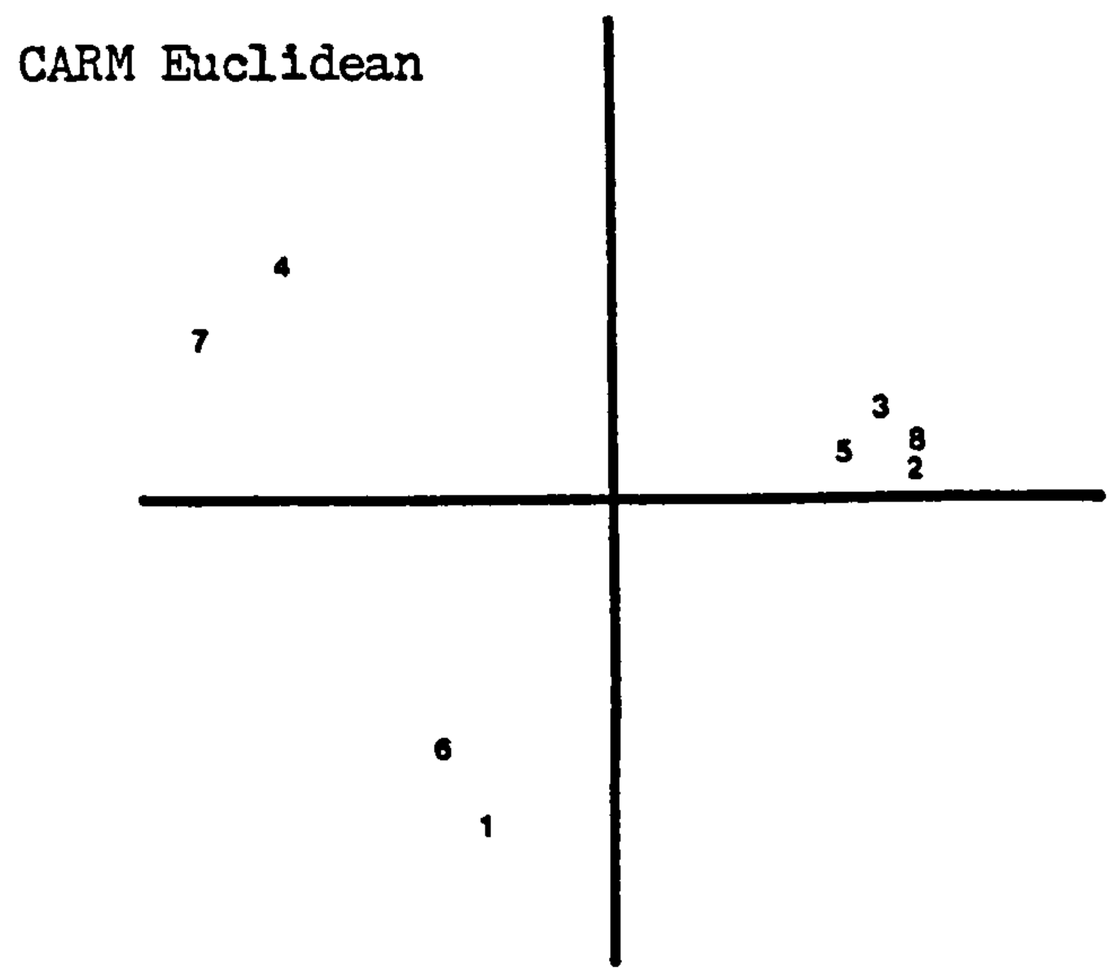
The striking feature of the results is the very high relationship between the effects of the FOCUS similarity and CARM Euclidean distance measures. The rank orders of element relationship in the two circumstances are very nearly monotonically related, and hence the NMMS-generated clusters are very similar (compare the first two representations from C.X.'s and C.F.C's grids overleaf). This finding, surprising at first sight (given the argument presented in Section 9.4.2), appears to stem from the fact that in grids using seven-point scales it is rare for two elements to differ highly in respect of one construct whilst remaining close on the remainder. The element relationship matrices are thus relatively little affected by which of these two measures in chosen, and are 'locked' by the extreme values of element relationship: if elements have similar patterns of ratings, they will be calculated to be close irrespective of the measure; if the patterns differ widely, then they will be shown to be distant. If the clustering dendrograms based on the element relationship matrices are examined, they often exhibit some discrepancies but - as one might expect in the light of this discussion - the grosser distinctions between clusters are stable (see also Appendix 15).

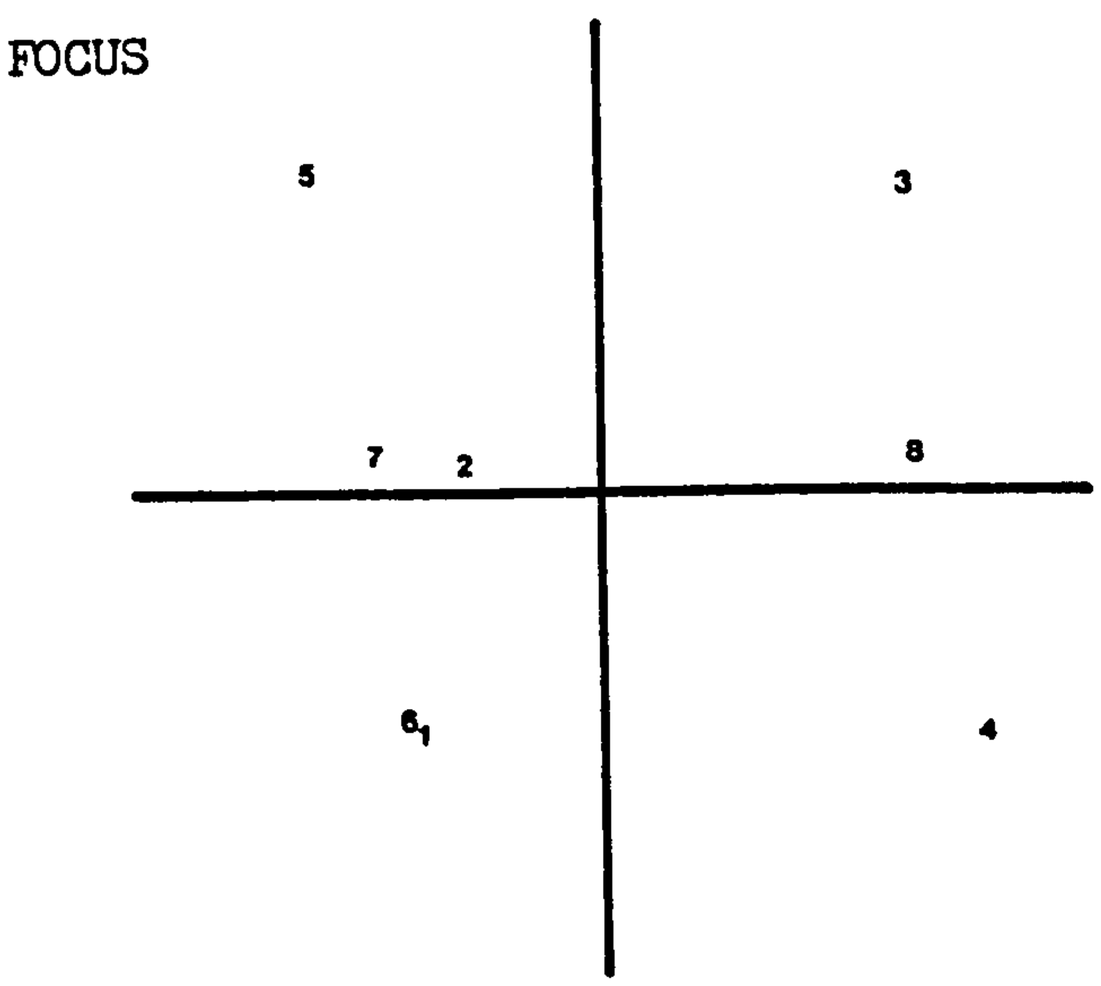
The comparisons of the effects of the FOCUS and CARM Euclidean measures with those of the product-moment coefficient are instructive. In the case of C.X.'s grid there are close relationships between the effects of all three measures, yet in the case of C.F.C.'s grid the NMMS plot for the product-moment coefficient shows the element relationship pattern to have undergone a considerable transformation, the relationship between C.F.C's elements 1 and 6 (discussed below) providing a convenient index. The discrepancies can be put down to the inadequacies of the

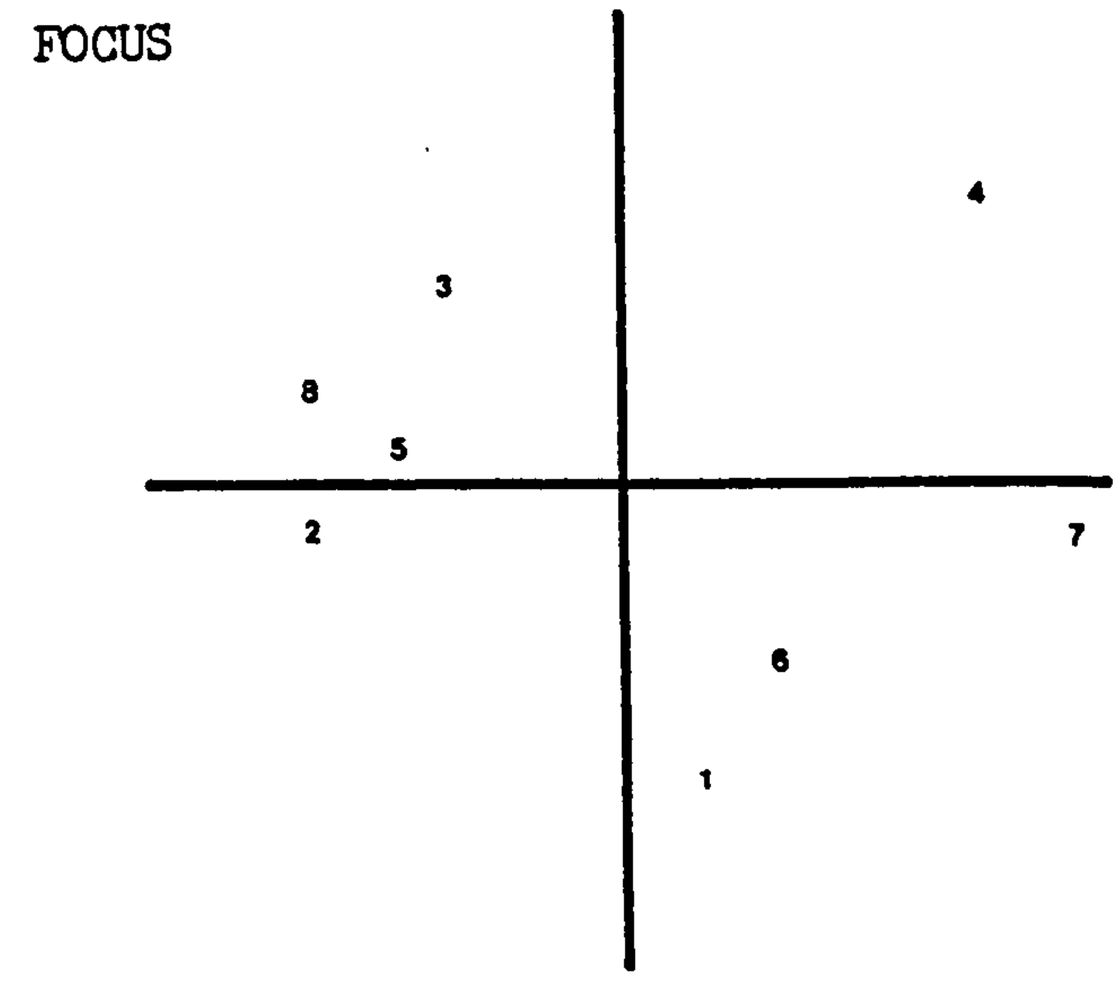
C.F.C.

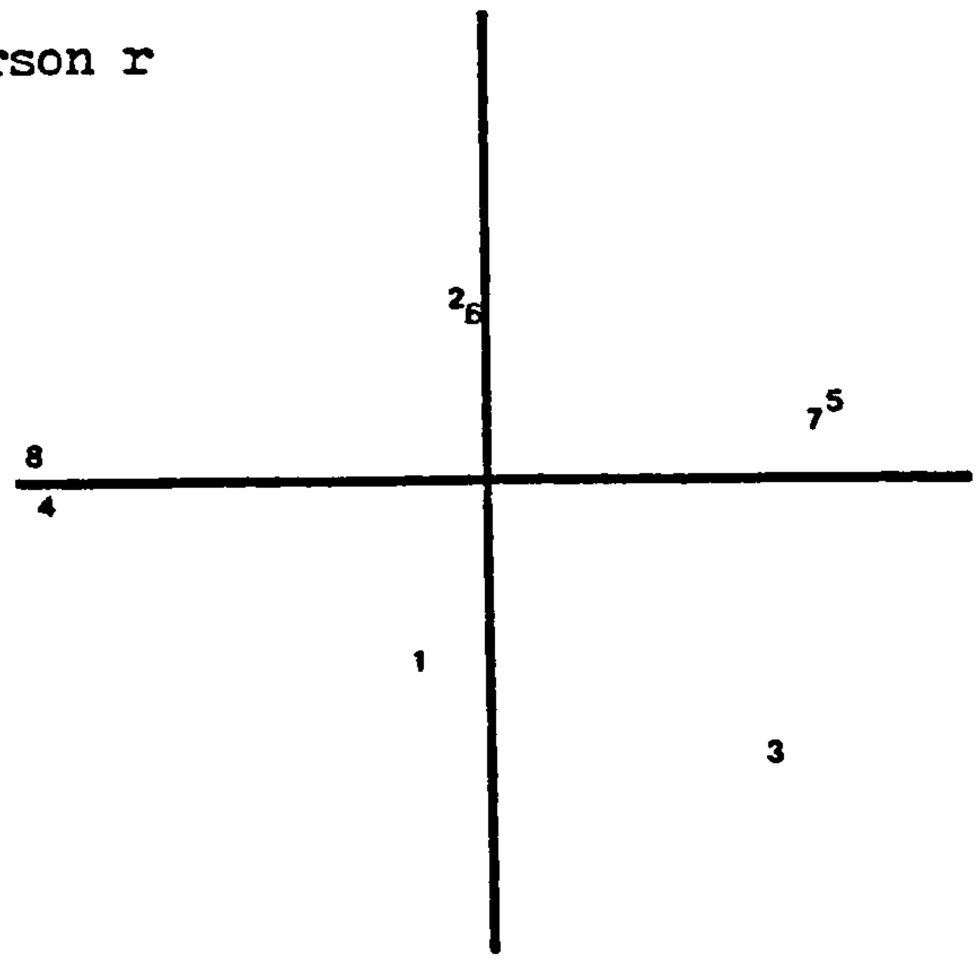


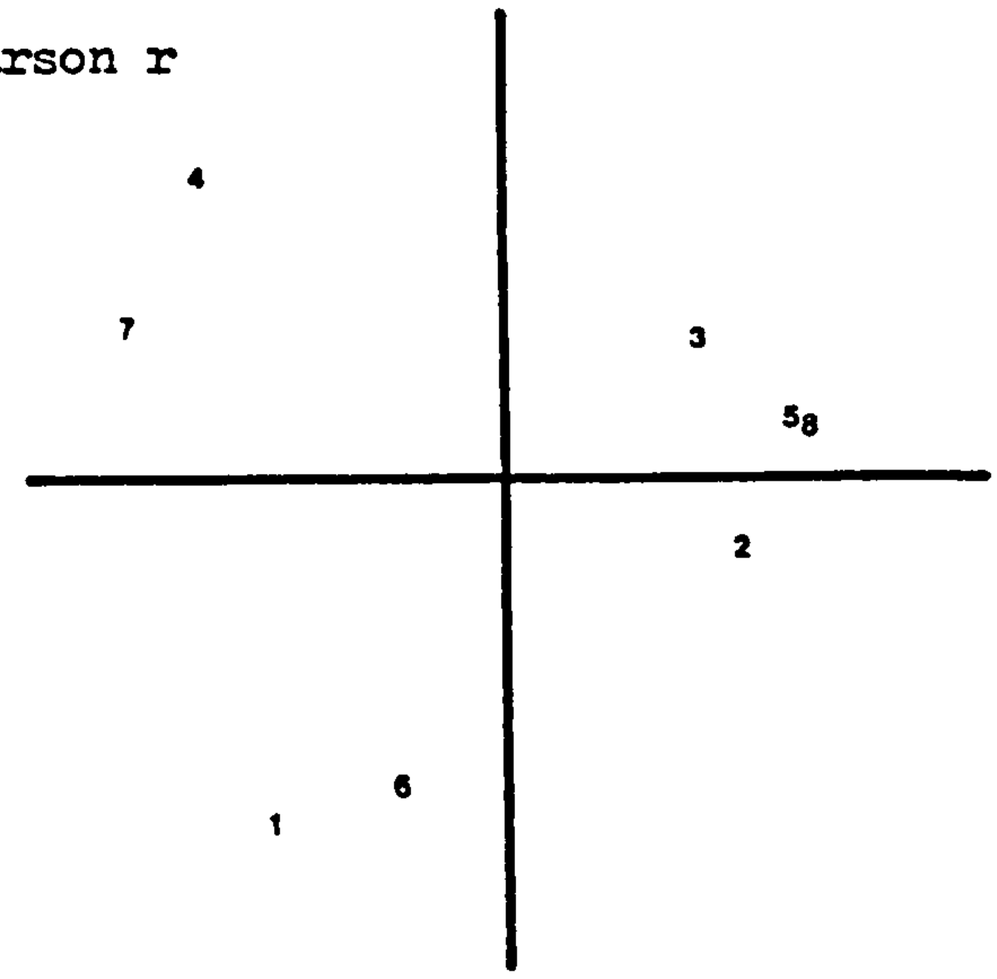
C.X.











NMMS plots for C.F.C.'s grid (left) and C.X.'s grid (right), showing the effects of varying the element relationship coefficient.

product-moment coefficient (see the discussion in Section 9.4.1).

Examination of C.F.C's original grid shows that four of the elements receive a lopsided distribution of ratings, those for elements 1 and 6 being shown below, from which it is readily apparent that C.F.C.

ELEMENT 1	6	6	4	6	7	6	6	7	6	6	7	6
ELEMENT 6	5	6	6	5	7	5	6	7	7	4	5	7

construes the elements in a similar way. The product-moment coefficient between the two sets of ratings, however, is only +.13 despite their closeness in terms of either of the other two metrics (the FOCUS similarity measure is actually a transformation of the city block distance measure). Reflecting constructs 1, 2, 5, 10 and 11 (as performed in the FOCUS analysis of this grid) enhances the computational 'moment' and gives a correlation of +.88 whilst the other two measures remain unchanged. Similar examples could have been given in respect of the other elements with lopsided sets of ratings. Brennan (1972:50) discusses the issues involved.

The problem appears to have arisen in this acute form as a result of the way in which C.F.C. produced constructs: all of those elicited seem to have had the left-hand pole as evaluatively positive (assuming C.F.C's liking for noisy extroverts), and the right hand pole negative. Pupils 1 and 6 seem to have been generally positively regarded, and pupils 3 and (to a lesser extent) 8 construed in negative terms. Although eight of the nine constructs produced by C.X. were evaluatively oriented in a similar manner, the ratings for the ninth appear to have provided a 'spread' to the sets of element ratings sufficient for the problem not to have arisen. The distinction between the two cases is evident both in the table of correlations and in the contrasts within the sets

of NMMS plots given above.

Before leaving the topic of element relationships it is worth noting that INGRID analyses output, inter alia, an element relationship matrix expressed in terms of both angles and their cosines - in effect, a correlation matrix which is based upon principal components analysis and takes into account the grid data as a whole. This matrix avoids the idiosyncracies associated with pairing lopsided sets of element ratings and, although based upon a correlational analysis, would appear to be closer to those based upon FOCUS similarity or CARM Euclidean distance than that derived from the product-moment coefficient in cases where the product-moment is demonstrably inadequate. Comparisons for the four grids are given below.

INGRID ANGULAR DISTANCE MATRIX COMPARED WITH	RANK CORRELATION (SPEARMAN RHO) BETWEEN ELEMENT RELATIONSHIP MATRICES FOR FOUR RESPONDENTS			
	H.E	C.F.C.	E.U.	C.X.
FOCUS similarity	-.93	-.78	-.89	-.88
CARM Euclidean distance	+.89	+.81	+.92	+.89
Product-moment (similarity)	-.80	-.67	-.88	-.85

Conclusion

This limited investigation suggests that there is little difference in practical outcome whether element relationships based upon the city block or Euclidean metrics are used as inputs into a multidimensional scaling algorithm despite the greater conceptual attractiveness of the latter. The product-moment coefficient, in contrast, is markedly

affected by the distribution of ratings given to elements: it would therefore seem inadvisable to use a matrix based upon this measure.

APPENDIX 15: A detailed comparison of the clustering of elements in a single grid.

Problem

In Sections 9.4.1,. .2 and .3 some of the advantages and disadvantages of INGRID, FOCUS and 'Cluster and profile' analysis were discussed. The point was made in respect of FOCUS (though it is more widely relevant) that clustering can be influenced by the particular algorithm chosen. It therefore seemedworthwhile to undertake a limited exploration, based on a single grid, to see what differential effects might obtain regarding the clustering of elements.

Method

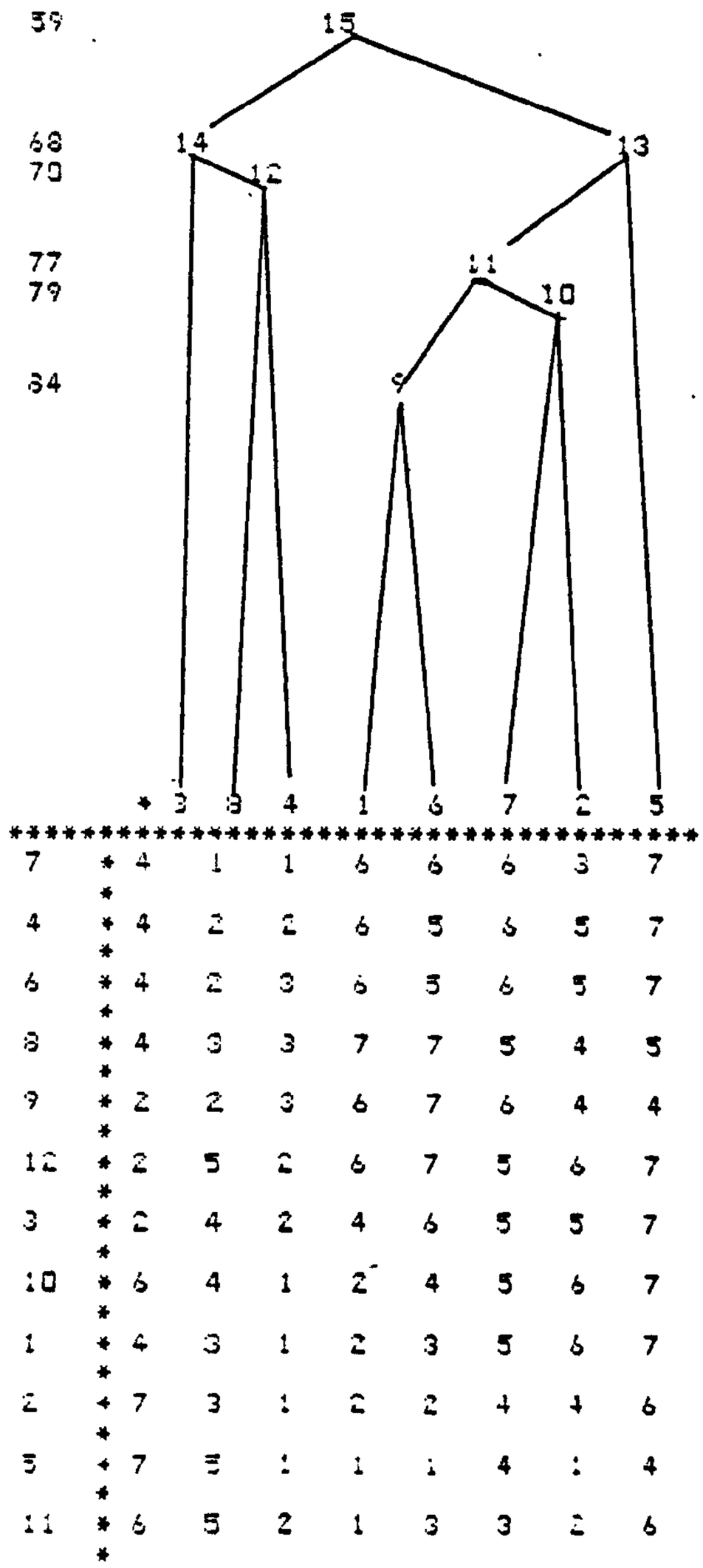
C.F.C's first grid from the 'Element fuzziness' investigation was chosen on the grounds that it contained no gaps and was of a reasonable size (8 x 12). This grid was analysed by FOCUS, INGRID, and CARM (using the error sum coefficient previously found to give good cluster discrimination). The clustering of the elements was examined in each case since this was at the centre of interest of my work: a comparable analysis could have been undertaken in respect of constructs, but was not attempted.

Results

The original grid, the FOCUSed grid, the polar co-ordinate plot from the INGRID analysis and its associated element relationship matrix, and the CARM dendrogram are given on the following pages.

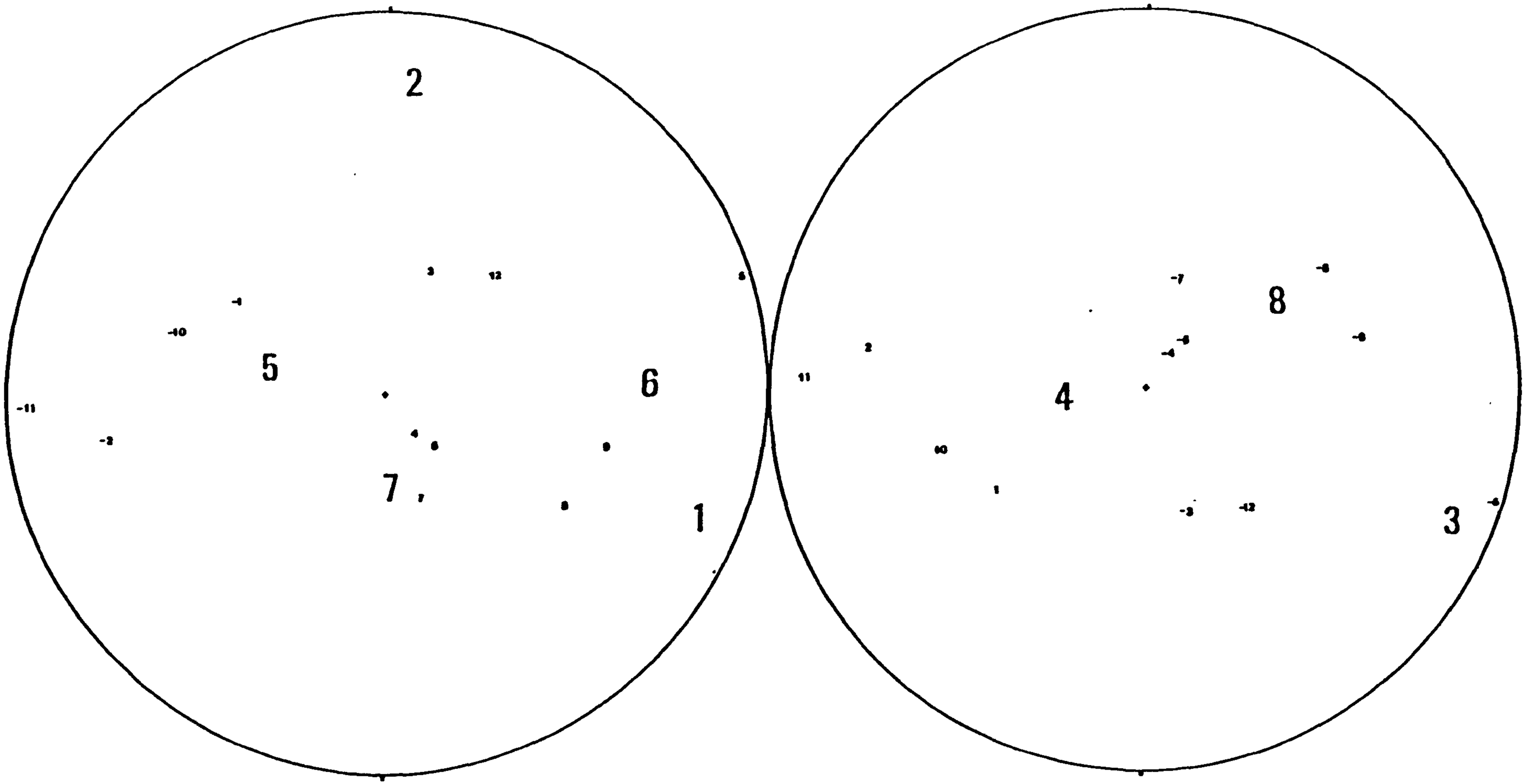
CONSTRUCT			ELEMENT (PUPIL)							
			1	2	3	4	5	6	7	8
1.	Noisy	- Quiet	6	2	4	7	1	5	3	5
2.	Extrovert	- Introvert	6	4	1	7	2	6	4	5
3.	Tidy	- Untidy	4	5	2	2	7	6	5	4
4.	Well-mannered	- Ill-mannered	6	5	4	2	7	5	6	2
5.	Lively	- Staid	7	7	1	7	4	7	4	3
6.	Mature	- Immature	6	5	4	3	7	5	6	2
7.	Hard-working	- Lazy	6	3	4	1	7	6	6	1
8.	Helpful	- Unhelpful	7	4	4	3	5	7	5	3
9.	Happy	- Sad	6	4	2	3	4	7	6	2
10.	Carefree	- Worried	6	2	2	7	1	4	3	4
11.	Active	- Inactive	7	6	2	6	2	5	5	3
12.	Attender	- Non-attender	6	6	2	2	7	7	5	5

C.F.C.'s first grid.



FOCUS analysis of C.F.C.'s grid.

A15.3



Polar co-ordinate plot of element and construct relationships computed by INGRID from C.F.C.'s grid data.

INTER-ELEMENT RELATIONS
EXPRESSED AS COSINES

ELEMENT 1 WITH ELEMENT

2 -.134 3 -.724 4 .109 5 -.258 6 .755 7 .223 8 -.628

ELEMENT 2 WITH ELEMENT

3 -.350 4 -.185 5 .237 6 .814 7 .047 8 -.185

ELEMENT 3 WITH ELEMENT

4 -.061 5 .124 6 -.815 7 -.097 8 .427

ELEMENT 4 WITH ELEMENT

5 -.938 6 -.246 7 -.192 8 .518

ELEMENT 5 WITH ELEMENT

6 .385 7 .647 8 -.442

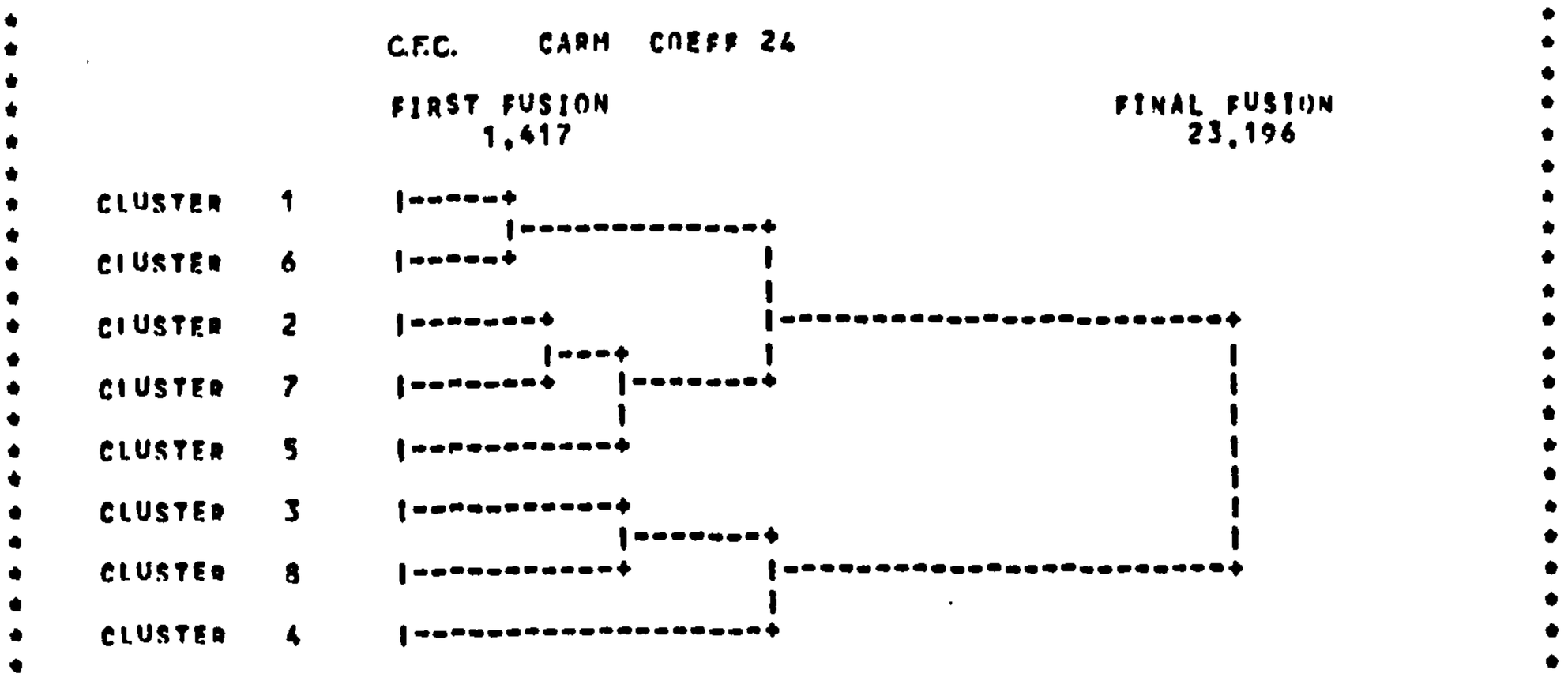
ELEMENT 6 WITH ELEMENT

7 .337 8 -.595

ELEMENT 7 WITH ELEMENT

8 -.759

Element relationship matrix from the INGRID analysis of C.F.C.'s grid.



CARM cluster analysis of the elements from C.F.C.'s grid.

If one works 'up' the FOCUS and CARM element dendrograms and examines the successive fusions from the respective six cluster solutions, it is noticeable that the six and two cluster solutions are identical but that the intermediate solutions diverge. The comparison is shown in the table below.

NUMBER OF CLUSTERS	CLUSTER FOCUS	MEMBERSHIP CARM
6	(3)(8)(4)(1,6)(7,2)(5)	(1,6)(2,7)(5)(3)(8)(4)
5	(3)(8)(4)(1,6,7,2)(5)	(1,6)(2,7)(5)(3,8) (4)
4	(3)(8,4)(1,6,7,2) (5)	(1,6)(2,7,5)(3,8) (4)
2	(3,8,4)(1,6,7,2,5)	(1,6,2,7,5)(3,8,4)

It might be felt that the distinction is trivial, but from the point of view of the presentation of results it could be of some consequence. With this particular set of data it would not be unreasonable to look for a four cluster solution (the error plot of the CARM analysis suggests the possibility), in which event there is a marked difference between the two analyses. There are no firm guidelines as to which algorithm is to be preferred, though an argument in favour of clustering based on squared deviations (as used in CARM but not FOCUS)was put forward in Section 9.4.2.

This argument, however, does not overcome the difficulty of deciding when to claim that elements are close. A person who uses a restricted range of ratings (say, 2 to 6 on a scale ranging from 1 to 7) is likely to produce an element relationship matrix showing a large number of high similarities (low distances): a number of such instances are scattered throughout the grids I have collected. Others, who use the full scale range, produce matrices with a much wider variation in element relationship. If, as the evidence in Appendix 8 strongly suggests, personal rating style is an enduring characteristic which varies between individuals, the determining of clusters presents the researcher with a rather intractable problem.

There is a further difficulty with dendrogrammatic representation in that it may not reflect 'nearest neighbours' very well. In the FOCUS analysis, for example, Element 5 is shown as closer to Element 7 than to Element 2 (similarity 75 as opposed to 68), yet is forced out of position by Element 7's slightly greater closeness to Element 6 than to Element 5 (77 as opposed to 75). Similar, and sometimes more marked, examples could have been provided from other grids and from CARM analyses. The CARM dendrogram shown, for instance, suggests that Elements 3 and 8 are closer than Elements 8 and 4, yet the distance matrix reveals identical distances (4.75 in each case). The clustering might have been represented as (4,8)(3) instead of (3,8)(4) - and an effect of this sort could influence interpretation if it is treated only slightly casually.

Inspection of the INGRID polar co-ordinate plot suggests the closeness of elements 1 and 6, and 5 and 7, the remainder being rather isolated. However, it must be borne in mind that this plot squeezes a hyperspatial representation into only three dimensions, and hence there is inevitably some relational distortion - in some grids I have analysed, this has been quite marked for a few elements and/or constructs. It is necessary to return to the element relationshipsoutput by the analysis to cross-check the polar plot. In this particular example the polar plot is a reasonable representation of the element relationship matrix.

INGRID does not produce dendrograms like the other two methods discussed, but it is possible to use the element relationship matrix to obtain a rough series of fusions based upon the positive coefficients. Taking successively declining positive associations gives the following 'clustering sequence'.

CRITERION OF ELEMENT ASSOCIATION	NUMBER OF CLUSTERS	INGRID 'CLUSTER MEMBERSHIP'
> +0.5	5	(2)(5,7)(6,1)(4,8)(3)
> +0.4	4	(2)(5,7)(6,1)(4,8,3)
> +0.3	3	(2)(5,7,6,1)(4,8,3)
>+0.2	2	(2,5,7,6,1)(4,8,3)

Although the final pair of clusters is identical to those produced by other methods, the intermediate stages are different yet again.

It is at this point that the use of multidimensional scaling may well be helpful. It was argued in Chapter 9 that clustering based on a distance metric is likely to be superior to a procedure such as INGRID which has at its heart the product-moment correlation coefficient. Even given this weakness, the INGRID element relationship matrix appears to rank-correlate highly with both FOCUS and CARM-generated matrices (see the evidence in Appendix 14). Nonmetric multidimensional scaling (which is based on ordinal matrix relationships) is a very robust technique (Kruskal 1977:310) and, to judge from the limited evidence provided by the NMMS plots in Appendix 14, retains element groupings so long as the relationship matrices are highly correlated. More evidence than this study has provided is necessary before a lower boundary to the correlations between element matrices can be suggested with any confidence, but a figure of the order of 0.85 might prove to be appropriate.

The use of multidimensional scaling would free the ordering of the elements from the strictly linear arrangement of the FOCUS and CARM printouts, thus giving greater weight to 'near neighbourliness'. A

two-dimensional output is appropriate for eight elements (cf Kruskal and Wish, 1978:50), though the dimensionality might with advantage be increased where the number of elements exceeds twelve.

Conclusion

This limited study, whose results find echoes in many other grids I have analysed, suggests that each of the three analytical methods discussed gives rise to problems of interpretation. The evidence suggests that the most reliable clustering of elements may be obtained when an element relationship matrix based upon a distance metric is input into a nonmetric multidimensional scaling program - though this will not eliminate entirely the researcher's judgment regarding what constitutes a cluster.

APPENDIX 16: A pilot investigation into implicative relationships between constructs.

Problem

If (following Kelly and the discussion presented in Section 4.6) a construct system is viewed as hierarchical, there are likely to be in any grid constructs drawn from different levels in the hierarchy. Hinkle's implications grid method was designed to probe for different levels. His technique (of repeatedly taking a single construct and asking a respondent to indicate on which among the remaining constructs would he or she be likely to change a polar preference, assuming a forced change of preferred pole on the focal construct) is however open to question because of the complexity of the task, and pairwise comparisons (such as those used in the resistance-to-change grid) might be expected to lead to more reliable results.

Another approach to the problem is through the methods designed to explore implicit personality theory (Hays, 1958; Bruner et al 1958), in which respondents are typically asked questions such as 'A person is p. How likely is it that he is also q?' and are provided with a scale on which to record their estimates of 'q-ness'. Hays is aware that implications may well be unidirectional, but it is less clear whether Bruner et al share this insight.

Assuming an epistemology such as that of Bloom et al (1956), it would be possible to produce a number of constructs which, following the six Bloom hierarchical levels, should be nested in a series of one-way implicative relationships. Application, for example, could be expected to entail the remembering of facts, but no converse entailment should be expected. The investigation described below was designed to explore the problem of discerning implicative relationships between constructs.

Method

A class of lecturers from further education was asked to write down individual lists of characteristics they employed when assessing students. The unipolar constructs used in this study were selected from the lists using three criteria:

(i) a reasonable degree of commonality across the sample; together with either

(iia) a discernible relationship with Bloomian levels; or

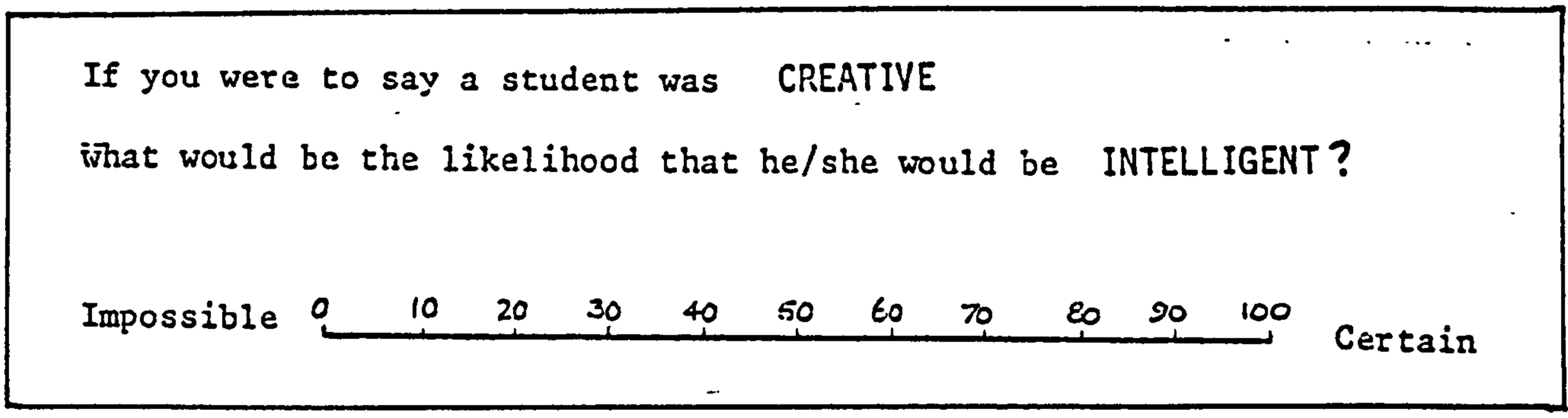
(iib) a potential orthogonality with 'cognitive domain' constructs.

The constructs selected were:

1. Good at remembering facts;
2. Good at understanding the subject;
3. Good at applying knowledge to new situations;
4. Good at thinking logically;
5. Perceptive;
6. Unimaginative;
7. Conscientious;
8. Creative;
9. Intelligent;
10. Able to give clear explanations.

'Unimaginative' was used as the negative of the elicited 'imaginative' on the grounds that it would be disruptive to response set or the halo effect. A negative relation with 'creative' would give some indication that the respondents were taking care with an implicit personality theory instrument built around the ten constructs.

Each construct was paired in both ways with all of the others as indicated below in order that the implicative relationships in both directions might be explored.



The pairings were arranged, seven to a page, in such a way that the distances between the successive appearances of the two constructs in a pair were maximised.

Twenty-nine students from the same class completed the 90 pairwise comparisons a fortnight after the constructs had been elicited. The order of presentation of the 13 pages of the instrument was rotated in order that respondents sitting at the same table would not be working on the same items simultaneously.

Results

The averaged estimates of the 90 relationships are given below.

ABBREVIATED CONSTRUCT IF ↓ THEN →	Remembering facts	Understanding	Applying knowledge	Thinking logically	Perceptive	Unimaginative	Conscientious	Creative	Intelligent	Clear explanations
1. Remembering facts	-	58	52	56	53	45	51	42	59	58
2. Understanding	64	-	62	61	63	37	49	50	74	65
3. Applying knowledge	59	68	-	66	68	33	48	68	81	68
4. Thinking logically	61	69	67	-	62	39	42	51	66	70
5. Perceptive	52	68	68	60	-	36	50	69	64	61
6. Unimaginative	49	39	28	42	31	-	41	25	37	41
7. Conscientious	51	49	44	48	38	46	-	43	49	45
8. Creative	41	49	69	48	71	21	41	-	62	52
9. Intelligent	62	70	70	72	69	39	52	55	-	66
10. Clear explanations	66	74	56	75	63	40	47	50	67	-

Discussion

The instrument was found to present a number of weaknesses (discussed below), but does give some indication at a nomothetic level of implicative directionality. For example, the relationships between the first three constructs are consistent with Bloomian epistemology although the effect is relatively small. Directionality of implication is rather more marked with

Applying knowledge implying intelligence (more than the reverse, 81:70)
and clarity of explanation (68:56); and

Clarity of explanation implying understanding (74:65).

The 'check' pairing of 'creative' and 'unimaginative' suggests that the respondents were using the instrument with some care: average implication scores of 21 and 25 indicate a strong negative relationship.

Assuming that an average of 50 represents a maximum of implicative indeterminacy, it is interesting to note that creative people tend not to be seen as

good at remembering facts,

good at understanding the subject,

good at thinking logically,

conscientious, and

able to give clear explanations,

yet they tend to be seen as perceptive and intelligent appliers of knowledge. There is, in these figures, a hint of a 'two cultures' stereotyping.

One would expect 'conscientious' to be orthogonal to the other constructs (as is suggested in the results) on the grounds that it contains no necessary implications for them. However, when the data from individuals was examined, it became apparent that five respondents saw conscientiousness as implying a clear negation of between four and eight of the eight other positively-valenced constructs. Loosely, these respondents seem to have construed 'conscientious' in terms of a lack of imaginative capacity and low intellectual ability - as 'stolid ploddingness', perhaps.

Given the inadequacies of the instrument these findings should not be accorded great weight, though my suspicion is that they would have been broadly replicated with an improved approach. Reflection upon practice suggested a number of possible improvements.

(i) The wording of the stems does not discriminate clearly between implication, (what might be termed) 'semantic overlap', and trait co-occurrence. This failure can be suspected particularly when the relationships are high in both directions. A similar difficulty is probable in respect of studies reported in the literature. Rewording of the stem might reduce the problem, but is unlikely to eliminate it.

(ii) The use of a scale in which zero represents an 'impossible' implicative relationship allows respondents to use the zero (erroneously) to mean 'no clear association'. A scale centred on zero, and in which negative numbers imply degrees of implicative unlikeliness, would provide psychological markers for the respondent and hence improve the instrument.

(iii) Two respondents appeared to overlook the negative prefix of 'unimaginative', and it would be preferable to replace this by 'lacking in imagination' in order to draw this label more sharply to respondents' attention. Further, the rather global 'understand' could be replaced by 'comprehend' (with its more limited Bloomian meaning) for samples to whom this distinction could be expected to be meaningful.

Conclusion

The exploration of construct relationships by methods such as the above is more problematic than the literature leads one to believe. Even though the instrument used in this investigation exhibited a number of defects, it has been useful in the conceptualising of implicative relationships - and has indicated quite sharply the dangers of assuming that normative meanings apply in individual cases.

Postscript

The instrument was revised to include the suggested improvements, and a different format was chosen in the interests of attractiveness to respondents, and simplicity. It was recognised that the revised version was more open to response set and halo effects, and an attempt was made to counter these by interspersing the 'cognitive domain' constructs with the others. A sample page is given at the end of this Appendix.

The revised instrument was administered to ten lecturers in further education who were attending an evening City and Guilds 730 course, care being taken to rotate the order of its pages between different respondents.

Scrutiny of the results at both idiographic and nomothetic levels suggested that the amendments to the format improved the instrument. The averaged results are given in the table overleaf.

ABBREVIATED CONSTRUCT IF ↓ THEN →	Remembering facts	Comprehending	Applying knowledge	Thinking logically	Perceptive	Lacking imagination	Conscientious	Creative	Intelligent	Clear explanations
Remembering facts	-	.9	.3	.7	-.3	.2	.2	-.3	.5	.3
Comprehending	1.0	-	.5	.9	.4	-.1	.3	0.0	1.2	.7
Applying knowledge	1.1	1.2	-	1.2	1.2	-1.4	.3	.7	1.1	.4
Thinking logically	.7	1.2	1.1	-	.1	-.1	.2	-.2	1.0	1.1
Perceptive	.1	1.1	.8	.4	-	-.6	.3	.4	.8	.2
Lacking imagination	.1	-.3	-1.2	.2	-1.2	-	.4	-1.4	-.2	-.4
Conscientious	.1	.1	.2	.4	.2	.1	-	-.2	0.0	.1
Creative	.2	.7	1.1	.4	1.3	-1.6	0.0	-	.7	.1
Intelligent	.8	1.0	.9	.7	.6	-.9	.1	.6	-	.6
Clear explanations	1.5	1.5	.6	1.4	.9	-.2	.1	0.0	.8	-

Note: These results relate to a range of -2 to +2 (the maximum negative and positive implicative relationship scores, respectively).

Comparing these results with those in the previous table, undirectionality of implication has been manifested to a much greater extent. If the arbitrary criterion of a difference of 0.5 between the two figures relevant to a pair of constructs is taken as indicating a unidirectional component to their relationship, then unidirectionality is a feature of the following pairs.

Applying knowledge implies remembering facts (rather than the reverse, 1.1:0.3) and comprehending (1.2:0.5)

Ability to explain implies remembering facts (1.5:0.3)
comprehending (1.5:0.7) and
perceptiveness (0.9:0.2)

Creativity implies comprehending (0.7:0.0) and
perceptiveness (1.3:0.4)

Perceptiveness implies comprehending (1.1:0.4)

Intelligence implies the unlikeliness of lack of imagination (-0.9:-0.2)

Lack of imagination implies the unlikeliness of perceptiveness (-1.2:-0.6)

As with the earlier instrument, there was a strong negative relationship between 'creative' and 'lacking in imagination', suggesting that the respondents had treated this instrument with some care. No idiosyncratic connotations of 'conscientious' were apparent on this occasion, but it is unclear whether this may be attributable to the small sample or to the refinement of the instrument.

This later set of results suggest that the refined instrument could form the basis of investigations into implicit personality theory,

provided that a small number of unipolar constructs is employed. It is, however, an instrument aiming to probe the nomothetic substrate of an individual's construing rather than his or her construing of an identified individual (as would be the case with the 'events-based' methodology described in Chapter 13.).

Imagine that a new student is about to join one of your classes. All you have been told is that the student is

INTELLIGENT

Basing your opinion <u>on this information alone</u>

what is the probability that the student is

	Impossible	Unlikely	Cannot say	Likely	Certain
Good at remembering facts	-2	-1	0	+1	+2
Conscientious	-2	-1	0	+1	+2
Able to give clear explanations ..	-2	-1	0	+1	+2
Creative	-2	-1	0	+1	+2
Good at applying knowledge to new situations	-2	-1	0	+1	+2
Lacking in imagination	-2	-1	0	+1	+2
Good at thinking logically	-2	-1	0	+1	+2
Perceptive	-2	-1	0	+1	+2
Good at comprehending subject matter	-2	-1	0	+1	+2