

**Bangor University**

## **DOCTOR OF PHILOSOPHY**

**Towards a science of human action: an investigation into the applicability of the "self-identification form" of Kelly's Rep. Test to selected areas of occupational, educational and medical description.**

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*Award date:*  
1982

*Awarding institution:*  
University College of North Wales, Bangor

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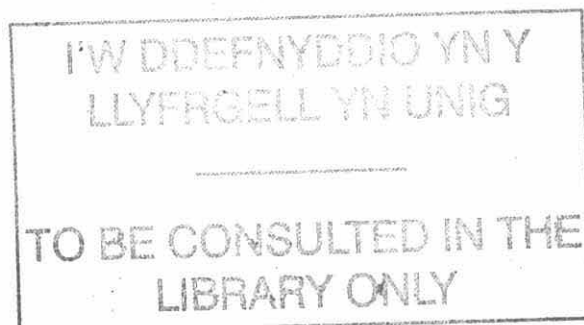
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TOWARDS A SCIENCE OF HUMAN ACTION:

An investigation into the applicability of the  
'self-identification form' of Kelly's Rep. Test  
to selected areas of occupational, educational  
and medical description.

Edward Geoffrey Knasel

Submitted for the degree of Philosophiae Doctor of the  
University of Wales, July 1982



ADRAN SEICOLEG  
COLEG PRIFYSGOL GOGLEDD CYMRU





"We speak often, with respect, of scientific research. But, after all, what is research but the same old urge of the monkey to handle some new or curious thing? It may well be on a higher plane, but it is the same monkey curiosity which has made our simian relatives the pests of the jungle."

Alfred Sherwood Romer "THE VERTEBRATE STORY" (1959) p. 317

"I engage with the Snark - every night after dark -

In a dreamy delirious fight:

I serve it with greens in those shadowy scenes,

And I use it for striking a light."

Lewis Carroll "THE HUNTING OF THE SNARK"

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ACKNOWLEDGEMENTS

An unusually large number of people have given their help in the completion of this thesis.

Neil Cheshire acted as my principal supervisor, and his help and wisdom have been truly invaluable. His guidance has always been useful without ever being intrusive, and his support has always been warm and 'unconditional'. He certainly 'pulled me through' at an early stage when I seriously doubted that I would ever have any research to report.

Isobel Hargreaves shared a very small office with me during the second half of my study period. She certainly influenced the work reported here, and I owe her a much greater debt of gratitude than she will ever realise.

Robin Davies of Ysbyty Dewi Sant, Bangor introduced me to a research problem which I found totally fascinating and absorbing, and which I would never have encountered otherwise. His help was crucial to the completion of this research.

Of course, many of my friends have helped me in various ways during the years since I started this work. Of these, I should like to single out Duncan Mackinder - who has always been willing to discuss my latest enthusiasm or disenchantment for any obscure part of my research.



I must also thank Professor T.R. Miles and Alun Waddon for their help with the study reported here in Chapter Three. I do not know whether they will appreciate the way in which I have finally presented this research - but I have certainly appreciated their help and support.

My colleagues at the National Institute for Careers Education and Counselling have given me invaluable help over the last three-and-a-half years. They have also allowed this thesis to intrude upon the work which I was being paid for! I should particularly like to mention Jenny Kidd, Tony Watts and Don Super who each, in their different ways, have been responsible for making this thesis more adequate than it would otherwise have been.

Very special thanks are due to my secretary Sandra Greenhall. She works far too hard - it is appreciated!

It is conventional in such acknowledgements for the author to thank his parents. This will be no exception. Over half of this thesis was written whilst I was staying with them during October-November 1981. I really do not think that I would ever have got it finished if I had needed to make my own coffee, cook my own meals and - most important - pour my own beer!

Finally, I owe a lot to Neil Young for his albums 'After the Goldrush', 'Zuma' and 'American Stars and Bars'. It helps to know someone else can feel the same way - and say so.

CAMBRIDGE  
MAY 1982

## SUMMARY

Edward Geoffrey Knasel: Towards a Science of Human Action: An investigation into the applicability of the self-identification form of Kelly's Rep. Test to selected areas of occupational, educational and medical description.

This thesis explores the suitability of the Self-Identification Form of Kelly's Rep. Test in research taking seriously the notion of 'human agency'.

The concept of 'an effective science of human action' is introduced. Traditional laboratory-based approaches to psychology are criticised as being based on restrictive views of science which have resulted in a failure to take account of common knowledge and a neglect of 'exploratory' research. The Self-Identification Form of Kelly's Rep. Test is introduced as one technique which may avoid these pit-falls.

Chapter Two details a study of the relationships between self-perception and occupational stereotypes, choices and preferences amongst 'students' pursuing four occupationally relevant courses. It was found that self/occupational congruence could 'predict' chosen occupations, but that a clear relationship between the degree of such congruence and preferences between non-chosen occupations only emerged for one of the groups.

Chapter Three explores the hypothesis that pupils who have had a degree of 'failure' at school would adopt a response strategy involving a minimum number of novel replies. A variant of the Rep. Test, which left open the option of repeating constructs, was administered to 20 'dyslexics' and to 20 pupils who were relatively successful at school. Confirming the hypothesis, dyslexics were significantly more likely to repeat constructs.

Chapter Four investigates the phenomenon of Recurrent Abdominal Pain in Childhood. A 'battery' of tests including a Self-Identification variant of the Rep. Test was administered to 20 children suffering recurrent pain and to 20 recovering from Appendicitis. Significant differences in self/other perception were found, supporting the view that the condition is 'psychogenic'.

The three studies are considered in relation to criteria for 'a science of human action'. They are held to meet these criteria, supporting the suitability of the Self-Identification Form as a flexible tool for use in such research.

Chapter One: Introduction - 'The Flight from the Laboratory'

1.1. Towards an 'Effective Psychology'?

"..... an effective psychology would eventually develop a central conception of human behavior which not only would be fundamentally 'right' in the sense of enabling us to understand\* behavior, whatever that might mean, but would generate powerful techniques having important applications in every field of human affairs."

Skinner (1958) The Flight from the Laboratory

Skinner laid down this challenging manifesto for an 'effective science of psychology' at the beginning of a paper which represents an attempt to coax young psychologists back into the experimental laboratory, which he saw as the only venue at which such aspirations could be realised. This thesis is based around precisely the same views of the ultimate objectives of psychological inquiry; but explores a very different route towards these common ends.

Specifically, this thesis is an attempt to gain some impression of the utility of a particular technique for investigating how individual's describe themselves in comparison with other people in three areas of 'applied' psychology. At no point were the people who assisted the investigator in this research invited into a laboratory. Further, whilst participants were certainly assigned to particular 'groups' because of certain characteristics which they had in common - such as having recently had their appendix removed; having chosen to study accountancy at university, or having experienced some

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\* Italicised in original

particular difficulties in learning to read or to spell - and were compared to other groups having other characteristics in common; no attempt was made to 'systematically manipulate' their experience in such a way that 'dependent and independent variables' could be identified by the investigator. In short, they were not 'experimented upon'. Similarly, the topics which were under investigation; namely, a student's choice of a career; a school child's difficulties with spelling and reading, or a child's recurring and unexplained belly-aches, were issues which were of significance not only to the investigator and the scientific community to whom he hoped to communicate his findings, but they also meant something to the participants themselves. Each of these characteristics of the research which is reported in the following chapters could be seen as straying in some important respect from the broad 'yellow-brick road' recommended by Skinner, and also by many other experimental psychologists who would not necessarily accept all or even many aspects of his 'radical behaviourism'. At the risk of seeming needlessly polemical, it is argued that this kind of non-experimental 'person-focussed' research may well represent every bit as viable a route towards an 'effective psychology' as that pursued by the experimentalist, and may in fact turn out to be a more dependable and perhaps less meandering highway.

#### 1.1.1. The Experimentalist's View

Skinner's paper, The Flight from the Laboratory represents the views of a very eminent, albeit somewhat idiosyncratic and iconoclastic, experimentalist who seeks to attract psychologists towards laboratory-based research and away from what he regards as competing approaches which, in his view, for many are more superficially attractive but are less likely to yield a sound and effective psychological

paradigm. He lists four such competing attractions, which he refers to as sources of 'pleasurable dalliance'. In his view these represent competing 'levers' which psychologists may turn to in preference to the 'experimental lever', and he goes on to identify the reinforcing contingencies which may explain such profligate behaviour. Skinner's four 'easy roads' are: 'the flight to mathematical models'; 'the flight to real people'; 'the flight to inner man', and 'the flight to laymanship'.

Skinner's remarks concerning mathematical models are of no relevance to this thesis, and in any case he seems unaware of the fact that many of the mathematical models employed in psychology are actually derived from laboratory-based experimentation (see e.g. Neisser 1968). His remaining three targets are, however, of considerable relevance to the research reported here.

Skinner explains the appeal of the flight to 'real people' - a phrase which he does not define or explain - in two ways. Firstly, he suggests that laboratories are frequently boring places and cannot compete with contact with 'real' or 'whole' people as a source of diverse interest or reinforcement:

"The experimental subject in the laboratory is only part of a man, and frequently an uninteresting part while the whole individual is a fascinating source of reinforcement."

He illustrates this by pointing to the supposed efficiency of introducing a colourful case-history to revive flagging interest in an otherwise boring lecture. He implies, however, that such interesting by-ways more properly belong to the domain of literature and have no real place in science. Secondly, he suggests that psychologists might turn to 'real people' out of an altruistic desire to alleviate

their problems. He argues, however, that their time would be better spent in laboratory experimentation - which holds out the promise of more wide-ranging and long-term solutions to such problems. He suggests that, for instance, Albert Schweitzer's missionary energy was misplaced:

"If he had worked as energetically for as many years in a laboratory of tropical medicine, he would almost certainly have made discoveries which in the long run would help - not thousands - but literally billions\* of people."  
Skinner (ibid).

Skinner describes two different flights to the 'inner man'. The first of these would be represented by an interest in 'inferred events' going on 'inside' the organism which cannot be directly observed from the outside. Skinner's argument here is his familiar one (see. e.g. Skinner 1953; 1963) that such 'hypothetical entities' must be regarded as 'epiphenomena' which should be ascribed no causal or explanatory significance. The second 'diversion' which Skinner describes under this heading is of less relevance to our present concerns and comprises an interest in the physiological study of the nervous system. Whilst he accepts that physiology represents a perfectly valid discipline in its own right, he implies that in carrying-out such research psychologists are abandoning their own legitimate subject matter, the study of behaviour. It should perhaps be noted that here again Skinner seems unaware that research into physiological psychology is almost exclusively experimental and laboratory-based in character.

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\*Italicised in original.

Skinner's final target is what he terms 'the flight to layman-ship'. By this he means a move towards abandoning the jargon of scientific psychology in favour of a return to the psychological language and concepts of the layman. He parodies this argument in the following way:

"'Enough of the lingo of the laboratory!' the argument runs. 'Enough of clinical jargon! Enough of frightening equations! A plague on all your houses! Let us go back to common sense! Let us say what we want to say about human behavior in the well-worn but still useful vocabulary of the layman!'"

Skinner (ibid)

His argument here is that the experimental analysis of behaviour has clearly demonstrated the practical and explanatory advantages of abandoning common-sense notions in psychology, and that to leave the laboratory for this reason could only represent a hinderance in the search for an effective psychology. He implies that such an appeal to everyday language is essentially anti-intellectual, and maintains that instead psychologists:

"..... must be willing to resort to experiments which quite possibly involve complicated techniques and to theoretical treatments quite possibly expressed in difficult terms."

Skinner (ibid)

Although Skinner's views are expressed particularly candidly, and although in a number of other ways Skinner may be regarded as something of a maverick amongst his fellow experimentalists, the argument which he advances in this paper does seem to represent a particularly clear statement of what one might term the psychological zeitgeist which has exerted the greatest influence over research since, at least, the Second World War, and which has been described in some detail by Hudson (1972). Central to Skinner's argument is the assumption that only an experimental laboratory-based approach



towards research could ever yield a truly scientific paradigm. Any other approach, the argument goes, would essentially represent an unscientific diversion which would have little pay-off in terms of increasing our understanding of human psychology. In this view, which it would seem fair to say has traditionally been the dominant one in psychology, science may be more or less exactly identified with experimentation. During the late 1960's and throughout the 1970's, however, an increasing number of psychologists, and interested philosophers and sociologists of science, came to challenge this view.

#### 1.1.2. The Case for Taking 'Human Agency' Seriously

The argument which has been advanced, in one form or another, by a number of contemporary writers who have challenged the established supremacy of experimentalism in psychology has essentially involved two elements. Firstly, such writers have challenged the model of science through which many experimental psychologists have justified their approach towards research at the expense of others. Commentators have suggested that in fact experimental psychology has, by and large, wedded itself to an interpretation of scientific activity which is no longer adopted in other disciplines. Experimental psychologists have, in this view, based their approach on a view of science which certainly no longer accurately applies to the established physical sciences which they have so slavishly sought to mimic, if indeed it ever did so; and which is no longer widely held by philosophers and sociologists of science.

Secondly, writers who have taken-up this theme have gone on to argue that this adherence to a demonstrably inaccurate model of



scientific method has had a seriously debilitating effect upon the 'progress' or evolution of psychology as an academic discipline, in that it has imposed upon research an image of man, or of human activity, which fails to incorporate much of our 'common knowledge' of human experience. Given that this deficiency can no longer be justified through an appeal to 'the necessities imposed by scientific method', the argument runs, it becomes important that psychology should seek to develop conceptualisations of human action, and appropriate research methods, which seek to build upon and refine such 'common knowledge' rather than to implicitly consign it to the waste-bin.

Probably the most influential, and certainly the most cogently and persuasively argued of all the publications which have propounded this theme has been Harré and Secord's The Explanation of Social Behaviour (1972), which represents a collaboration between, respectively, a leading philosopher of science and a noted social psychologist.

Harré and Secord are especially concerned with 'reconstructing' social psychology. Their basic criticisms of the existing 'state of the art', however, can be applied equally appropriately to most areas of contemporary psychological research. In this context, they seek to establish that the predominance given to experimentalism represents one consequence of mistaken assumptions about the nature of scientific inquiry in other disciplines:

"..... we (have) identified a number of mistaken background assumptions in the way in which social psychology has been studied. Amongst these was the positivistic methodology of science, in which the job of an experimental scientist is seen as the collection of correlations among observables and the job of theoreticians as the construction of deductive systems of laws from which these correlations could be deduced under the sole constraint of logical consistency. Real science is a much more complex and promising affair."

Harre and Secord (1972) p. 68

Harre and Secord go on to give their own account of the 'advanced sciences', which has little in common with the methodology advocated by Skinner, and which is illustrated by examples drawn from the history of science.

They see common knowledge of non-random patterns of events as the starting point for what they term 'real science'. In practice, they argue, scientific activity begins by taking such 'common knowledge' seriously and seeks to refine and elaborate upon it by, firstly, extending what is known through common observation; and secondly, by critically checking the authenticity of what is thought to be known. Both of these tasks represent valid areas of scientific empirical study. Harre and Secord put forward the term 'exploration' to describe the first set of tasks. Only the second category of task involves 'experimentation'. Not only do Harre and Secord maintain that both sorts of activity are equally valid as scientific enterprise, but they also argue that the history of science shows exploratory research to be the more characteristic endeavour:

"Much of empirical science is exploratory, and involves a methodology not at all like the traditional idea of hypothesis, prediction and test. In exploratory studies a scientist has no very clear idea of what will happen, and aims to find out. He has a feeling for the 'direction' in which to go (increase the pressure and see what happens) but no clear

expectations of what to expect. He is not confirming or refuting hypotheses. This is the pattern of chemistry, and is beautifully exemplified in much of physics (.....). The conjecture/experiment pattern is very much more rare. In the whole of the Nineteenth Century it occurred only once in chemistry, in the prediction of new elements by Mendeleev, and testing of the hypotheses he formulated as to their likely behaviour."

Harré and Secord (ibid) p. 69

Harré and Secord maintain that in the 'advanced sciences' such exploration and experimentation has always been a 'means towards an end' rather than a self-justifying activity. The true objective of natural scientists has been the rational explanation of the non-random patterns of events which empirical study have elaborated and uncovered. Such explanation represents, for Harré and Secord, the province of 'creative' science. In their view the concept of 'modelling' is central to scientific explanation:

"The key to the understanding of the epistemology and logic of creative science, and thus to understanding its methodology is to be found in the notion of a model\*. A model is a realistic representation of something, like its subject in some ways and unlike it in others, often leaving some of its relations with its subject uncertain."

Harré and Secord (ibid) p. 73

They maintain that in science the most effective forms of model belong to the category of 'iconic paramorphs'. As the reference to the word 'icon' suggests, such models involve the representation of a real thing or process by another real or imagined thing or process. An iconic paramorph represents a model where the object or process which is being modelled is used to represent it. In short 'iconic paramorphs' rely upon a process of analogy for their explanatory

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\*Italicised in original

power. As one example of such an analogy, Harré and Secord refer to San Felicé and Bordet's introduction of the notion of the 'virus' to explain the occurrence of diseases such as Influenza and Polio-myelitis. They postulated the, at that point, unobserved existence of viruses as entities which one should look on as being in some important senses like bacteria - although it was known that bacteria were not responsible for the diseases which San Felicé and Bordet sought to explain. The essence of this example, and of the use of analogies in scientific explanation generally, is that one is invited to regard the, at that stage, hypothetical entity of the 'virus' as being both like and unlike the already understood properties of a bacterium. Harré and Secord argue that the use of such iconic paramorphs represents a key distinction between creative science and mere 'critical description'.

This view of science is radically different from that advanced by Skinner and vitiates against the scorn which he pours upon the 'flights' towards 'laymanship' and the 'inner man', and most damagingly against the unique importance which he places upon laboratory experimentation.

As we have seen, Harré and Secord quite clearly demonstrate that experimentation is not the only, or even the most, scientifically acceptable form of empirical research. Further, they show that in practice the physical sciences take 'lay' conceptions of their subject matter seriously and treat them with respect rather

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\*Harré has discussed this view in greater detail elsewhere - see, for instance, Harré (1965) and Harré (1972).

than derision. Skinner, on the other hand, implies that scientific conceptions are essentially different from 'common knowledge', and are always to be preferred. In effect, his 'radical behaviourism' consists of a wholesale rejection of our common-sense knowledge of human experience in favour of a model of human activity which may be constructed solely from laboratory experimentation and which will hence be shaped by the limitations of such experiments. As has been suggested in Section 1.1.1., Skinner perhaps goes further in this direction than many other experimentalists, but his arguments do share the flavour of the prevailing zeitgeist discussed by Hudson (1972). Such a view represents a radical and challenging programme - but, as Harré and Secord show, there is nothing particularly scientific about it. Ignoring our common-sense awareness of human experience and activity represents a self-denying ordinance which is not paralleled in other more 'advanced', or established, sciences. It is therefore difficult to see quite what there is to recommend it!

Skinner's characteristic rejection of the 'flight to the inner man' on the grounds of its appeal to hypothetical entities which can have no explanatory power is also seriously at variance with Harré's view of the central importance of argument by analogy in the scientists' primary task of explanation. One wonders whether Skinner would reject the notion of a 'virus' as being 'unscientific'? Given Harré and Secord's model of science, it is hard to imagine that a scientist could seriously pose the question 'are theories of learning necessary?' - as Skinner (1950) has done.

It is important to notice that the view of science which Harré and Secord propound can not be seen as a totally revolutionary one which has been concocted to support a particular view of psychological research. There is nothing at all unique in Harré's rejection of a 'positivistic' view of science, and indeed it would seem fair to argue that the implicit model of science which most experimental psychology appears to be based upon is one which would be rejected by most contemporary writers on the logic of science and the actions of scientists. Very similar criticisms of experimental psychology could be advanced, for instance, on the basis of the somewhat different models of science propounded by Toulmin (1961; 1967) or Kuhn (1962). To pursue this point any further would, however, be outside of the scope of this chapter.

We have seen that the traditional experimentalist approach towards psychology has been criticised not only on the grounds of having made invalid assumptions about the nature of scientific research, but also on the grounds that it imposes a seriously deficient view of man and of human action. Harré and Secord argue that in the development of an adequate social psychology it is both legitimate and desirable to begin with 'common knowledge' assumptions about human experience and activity. The argument is that to do otherwise would represent a totally unnecessary waste of 'knowledge' or 'data' which are already to hand. Harré and Secord go on to suggest <sup>w</sup> ~~that~~ such a model of man might look like.

They take as the basis for their exploratory model contemporary conceptions of 'personhood' or 'human agency', and in particular they pay specific attention to contemporary ideas in this area put

forward by analytical philosophers who may be seen to have carried out an initial refinement of lay notions. They give their model the rather polemical title of 'the anthropomorphic model of man'; playing upon the idea that existing psychological models have failed to pay sufficient attention to the uniquely 'human' characteristics of our behaviour. They reinforce this theme by prescribing that psychologists should 'for scientific purposes treat people as if they were human beings':

"We shall find our model in the most recent developments in analytical philosophy which have centred around the elucidation of the concept of a person\* as this concept appears in ordinary language and in philosophy. The model we will construct on the basis of recent work in philosophy we shall call the 'anthropomorphic model of man'. It is the heart of what we take to be the most radical proposal of this book, that we should treat people for scientific purposes, as if they were human beings\*, as we know and understand them in everyday life."

Harré and Secord (ibid) p. 87

The power of Harré and Secord's model hence hinges upon what they mean by a 'human being'. The defining properties to which they refer in clarifying their position in this context are by no means the same as those which would be used by a zoological taxonomist in describing a representative member of the genus Homo! For this part of their analogy they borrow heavily from the description of the powers of human beings put forward by the philosopher Stuart Hampshire (see, for example, Hampshire 1965). As interpreted by Harré and Secord the key assumptions made by this model are; firstly, that human beings are agents - that is that they have the power and potential to initiate action; and, secondly, that they are capable

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\* My underlining

of watching, commentating upon and criticising their own actions. In Harré and Secord's version, Hampshire's model may be reduced to three basic propositions:

- "A. A human being is characteristically an agent\*, that is has the power to initiate change and control the manner and goal of his performances.
- B. A human being can perceive things, that is can be aware of things other than himself, and is aware of that awareness.
- C. A human being has linguistic powers, particularly the power of speech.\*\*"

Harre and Secord (ibid) p.95

We should notice that the use of the word 'action' as opposed to the, to the psychologist, more common word 'behaviour' is held to be of considerable significance. The two words are not intended to be co-extensive. For writers in this vein the word 'action' carries the important additional connotation that the particular activity exhibited by the individual was initiated by that person and that he or she had, at a conscious level, an objective - for which the human agent could give some sort of explanation. An 'action' is held to differ from a 'behaviour' in that it expresses an intention (see e.g. Shotter 1970, pp.235-242).

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\* My underlining

\*\* Given these defining characteristics of a human being the question arises as to whether Washoe and her fellow 'AMSLAN' signing chimpanzees qualify as human beings! Harré and Secord allow that she represents a borderline case. Certainly Washoe satisfies criterion A, as Harré and Secord acknowledge, any live animal in fact fulfils this condition. It is also extremely hard to deny criterion C to her. One wonders whether a true test might revolve around what she and her colleagues would make of the West-End play 'Children of a Lesser God'! These difficulties would not arise if the criteria used by a taxonomist - which would take into account factors such as Washoe's inter-membral index, her skull structure, and her hirsute appearance - were employed.



The concept of human agency hence goes considerably beyond the model of man which is proposed by radical behaviourists and which is implicitly accepted as being 'scientifically respectable' by many other experimental psychologists. The traditional view of psychology, represented in this chapter by Skinner, would tend to outlaw such allegedly hidden phenomena as intentions and objectives as having no real place in a scientific discourse. The 'anthropomorphic model of man', on the other hand, rejects this appeal to the requirements of 'scientific purity' as being essentially bogus; and instead posits that, if psychology is to attain the level of the 'advanced sciences', then it should take into account our common knowledge of human action. In this view, far from following Skinner's rejection of the flights to 'real people' and to 'laymanship', psychology must, if it is ever to resemble other 'real sciences', take seriously the concept of human agency, that is the notion of human beings as entities empowered to initiate actions, and also the notion that human beings may comment upon and, in some senses, 'explain' their self-initiated actions. It is this concept which lies at the heart of Harré and Secord's notion of the 'anthropomorphic model of man', and it also represents a central assumption behind the research reported in this thesis.

#### 1.1.3. Maslow's Third Force

The work of Harré and Secord (1972) has been used to introduce the notion that a truly effective and scientific psychology would have at its core the concept of human agency because, in this author's opinion, this book represents the clearest and ultimately the most influential version of the essential argument. We have already stated, however, that during the late sixties and throughout

the seventies, many variations on this basic theme were advanced and we have already referred, albeit in passing, to Liam Hudson's book The Cult of the Fact (1972). Specific mention should also be made of John Shotter's short monograph Images of Man in Psychological Research (1975) which proposes that psychology should be transformed from a 'science of behaviour' to 'a moral science of human action'. Also influential have been: Fransella (1975); Harré (1979); Hudson (1975); Joynson (1970; 1972); Reason and Rowan (1981); and the rather earlier Koch (1964), which although American in origin has been explicitly acknowledged as an influence by the later British writers.

It would be quite misleading, however, to give the impression that all preceding psychology may be 'tarred with the same brush'. Although it has represented the dominant theme hitherto, particularly one would suggest in Great Britain, experimentalism has by no means always had it all its own way. Despite persistent assaults from the positivists, the broadly Freudian 'psychodynamic' tradition has persisted throughout the Twentieth Century, and has probably had considerably greater influence than many of its antagonists would care to admit. The view of an effective psychology adopted in this thesis is seen as being broadly compatible with this tradition and reference is made to ideas derived from this source, particularly, in Chapter Four.

The methodology which forms the unifying theme of the research which is reported here, however, is derived more directly from the tradition which Maslow (1970) has referred to as the 'Third Force in American Psychology'. It is a central tenet of this thesis that writers belonging to this tradition - especially Kelly, and to a somewhat lesser extent Rogers - have already gone a significant way.

towards establishing an approach towards psychological research which, congruent with the views exemplified by Harré and Secord, takes as its starting point the idea of human agency.

The central notion which unifies such writers as Maslow, Rogers and Kelly is that psychologists should seek to deal with the whole person, and to pay particular attention to people's notions of self. It is argued here that the conceptions of 'personhood' put forward, particularly, by Carl Rogers are closely allied to the views of human agency put forward by writers such as Harré and Secord - it should be noted that the latter writers in fact use the words 'person' and 'human being' almost interchangeably. Whilst it is certainly the case that the contributions made to psychology by the 'third force' have been more focussed upon the practice of guidance and counselling than empirical research, these writers have attempted to legitimate the position of the study of the person or human agency within a scientific framework and it would seem to be appropriate to pay some attention to this work at this point. Of particular relevance is Rogers' 1964 paper Towards a Science of the Person, which puts forward an argument strongly reminiscent of that outlined in Section 1.1.2.

This paper represents a contribution to a symposium based around the relative merits of behaviourism and phenomenology as 'bases for modern psychology' (Wann, 1964). Rogers argument is that any science should take into account not only objective knowledge, but also one's subjective hypotheses and empathic 'inter-personal' knowledge. His fundamental argument against behaviourism is that it has limited itself, unjustifiably, to only the first

'objective' form of knowing, and has hence unnecessarily restricted its underlying image of man:

"Valuable as have been the contributions of behaviorism, I believe that time will indicate the unfortunate effects of the bounds it has tended to impose. To limit oneself to consideration of externally observable behaviors, to rule out consideration of the whole universe of inner meanings, of purposes, of the inner flow of experiencing, seems to be to be closing our eyes to great areas which confront us when we look at the human world. Furthermore, to hold to the beliefs, which seem to me to characterise many behaviorists, that science is unpersonal, that knowledge is an entity, that science somehow carries itself forward without the subjective person of the scientist being involved is, I think, completely illusory.

In contrast, the trend of which I am speaking will attempt to face up to all\* of the realities in the psychological realm. Instead of being restrictive and inhibiting, it will throw open the whole range of human experiencing to scientific study. It will explore the private worlds of inner personal meanings, in an effort to discover lawful and ordinary relationships there. In this world of inner meanings it can investigate all the issues which are meaningless for the behaviorist - purposes, goals, values, choice, perceptions of self, perceptions of others, the personal constructs with which we build our world, the responsibilities we accept or reject, the whole phenomenal world of the individual with its connective tissues of meaning."

Rogers (1964) p. 119

Rogers goes on to argue that a scientific psychology which accepted the importance of individual or interpersonal experience would, in fact resemble more closely the established sciences such as physics and chemistry than does contemporary behaviourism. He characterises behaviourism as having modelled itself on Newtonian physics and quotes Oppenheimer's (1956) description of this world as 'a physics

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\*My underlining.

which is not there anymore, which has been quite outdated'. Rogers tries to illustrate that 'a science of the person' is indeed possible by references to a number of empirical studies which have been made of broadly phenomenological variables - such as Osgood, Suci and Tannenbaum's (1957) work with the Semantic Differential, or Stephenson's (1953) introduction of the Q-Sort Technique.

Roger's paper is a significant one because it shows that the later and more philosophically and historically sophisticated arguments advanced by writers such as Harré had been anticipated within psychology itself. In effect, it establishes that the kind of approach towards psychological research which the contemporary, mostly British-based, writers have advocated does have its parallels in a live tradition within psychology. Rogers argued that a science which based itself upon the notion of human agency was both possible and desirable. It would seem fair to argue, however, that the challenge which he threw down to psychological research was not initially accepted. Rogers introduced the promise of a science of the person - he could not be said to have actually ushered in that science.

#### 1.1.4. A Flight Worth Taking!

The two principal conclusions of this section should by now be clear. They are:

- i) That an effective science of psychology would have as its aim not the restrictive goal of an understanding of the 'behaviour of organisms', but the more inclusive one of an understanding of the experience of being human, which would

take as its starting point our everyday knowledge of human action.

- ii) That such a science would not necessarily be primarily 'experimental' and laboratory-based but would, like other disciplines, also permit and encourage 'exploratory' research which would expand and refine our existing common sense knowledge.

Such an endeavour is necessary and desirable if psychology is truly to emulate the more established scientific disciplines such as physics, chemistry and biology. There is at least one important sense, however, in which psychology is in a rather different position from these other sciences, and which is relevant to our present discussion. This is the view that psychology is special in that our common knowledge of the subject is particularly extensive and peculiarly accurate. We all know what it is like to be a human being! Further, the most familiar social encounter, such as a brief conversation with friends in a pub, would not be possible unless our intuitive predictions about human action were particularly accurate and subtle. We would, for instance, not know when to butt into the conversation or when to listen, nor; for that matter, would we recognise when it was our turn to buy a round! In other sciences we are prepared to accept that a particular theory may not tell the whole story. With human experience, however, we tend to object strongly when it appears that a particular model is denying some important aspect of our personal knowledge. Hence the quite common objection that experimental psychology is somehow immoral or de-humanising. It is not so much that behaviourism represents an

unnecessary simplification of the human condition; but rather that the psychologist, as a human agent him- or her-self, is often acutely aware of its degree of oversimplification, and the extent to which it fails to utilise our common knowledge.

It is as if the psychologist is in the position of a school-girl working her way through an arithmetic text where the answers to the problems which it sets are printed at the back of the book. So long as her teacher does not see her, she can always look-up the answer when she is half-way through a problem to see if she is on the right track - if it does not look as if she is going to arrive at the right answer she can try to find a different way of tackling the problem. Psychologists, more than other scientists, are in a rather similar position in that we can 'check' our theories against our own experience of being a person to see if it looks as if we are heading in the right direction. The simple answer is that for many it no longer looks as if the experimentalist tradition in psychology is likely to yield answers compatible with those 'at the back of the book'. It is time instead to tackle the problem in a rather different way. It is time, in short, to forget Skinner's advice and to 'flee the laboratory' in search of research methods which build upon our everyday knowledge of human action and which allow us to look at the people we study 'as if they were human beings'.

This thesis is concerned with the potential of one candidate for such a methodology: the Self-Identification Form of Kelly's Rep. Test.

## 1.2. Personal Construct Theory and the 'Rep. Test'

In section 1.1. it has been argued that psychological research should not be restricted to the kinds of methodologies traditionally characteristic of laboratory research in this area, and should instead consider approaches which may be 'exploratory' rather than 'experimental', and which are consistent with a model of man which takes into account the concept of human agency. The crucial question, which is addressed by the remainder of this thesis, is whether methods are available which meet these criteria and yet which also permit the conduct of empirical research yielding data which are acceptable to the scientific community. It is argued here that this question is one which acquires considerable force in the light of the contemporary literature, discussed in Sub-Sections 1.1.2. and 1.1.3., which has seriously questioned the validity of many of psychology's accepted research methods. What follows in this thesis therefore hinges

x <sup>on</sup> ~~around~~ two basic questions:

- i) Is it in fact possible to construct an empirical psychology which is in accord with the views of scientific endeavour which have been exemplified by Harré and Secord?
- ii) Further, can such research methods build upon not only our 'common knowledge' of human action, but also upon the insights which have been gleaned through existing or traditional approaches?

Although it is a subsidiary question, the second point is here held to be a very important one. To this author at least, it does seem to be desirable to make use of existing models and data and existing paradigms within the discipline of psychology. Whilst it



may well be the case that existing approaches can now be shown to have been needlessly restrictive, and hence seriously inadequate, it certainly need not follow that the previous hundred and more years of psychological research will hence prove to have been worthless. It would present a very daunting picture indeed if, in the attempt to rescue our common knowledge of human action from the positivist waste-bin, we should find that we have instead created our own rubbish-pile - comprising the mass of empirical data gathered by previous psychologists - to take its place. This thesis takes it as axiomatic that in 'fleeing the laboratory' the attempt should be made to carry-out empirical work which makes sense of existing traditions within the discipline and, indeed, which makes some sense in light of these traditions.

The methodologies proposed by Harré and Secord, in line with the general focus of their book, centre around the study of social behaviour and, in particular, around the interpretation of 'social episodes'. This thesis is more concerned with the utility of a technique which, whilst firmly accepting and acknowledging the individual's social context, permits a focus upon the individual person. This method - a modification of Kelly's Rep. Test which places especial emphasis upon the individual's self/other perception - arises out of an approach towards psychological theory and research which has its roots in Maslow's 'Third Force in American Psychology' (see Sub-Section 1.1.3.) and which can also be said to have made significant contributions to the contemporary, and largely British-based, critique of experimentalism (see particularly: Bannister 1966; and Shotter 1975, pp.116-118). This is Kelly's 'Personal Construct Theory' and we now turn to give some attention to the principal

tenets of this approach and to introduce some of the research techniques which it has spawned.

#### 1.2.1. Man the Scientist

Personal Construct Theory was introduced by George A. Kelly in his book The Psychology of Personal Constructs (1955) and has largely been popularised in this country by Bannister (see e.g. Bannister 1966; Bannister 1970; Bannister and Fransella 1971). The core concept in Kelly's theory is the image of 'Man the Scientist'.

This phrase represents a challenge to the psychologist to look upon the actions of the people he or she is studying as having crucial elements in common with his or her own attempts to study them. In Kelly's view it will be profitable to interpret human action as representing attempts by the person to develop an understanding of the world of which he or she is a part, and to find increasingly efficient and reliable ways of dealing with recurrent elements and events with which he or she is confronted. Kelly's analogy suggests that these attempts to anticipate events are essentially the same as the characteristic activities and endeavours of the scientist. Kelly introduces this analogy in the following terms:

"Let us see what it would mean to construe man in his scientist-like aspect. What is it that is supposed to characterise the motivation of the scientist? It is customary to say that the scientist's ultimate aim is to predict and control\*. This is a summary statement that psychologists frequently like to quote in characterising their own aspirations. Yet, curiously enough, psychologists rarely credit the human subjects in their experiments with having similar aspirations. It is as though the psychologists were saying to himself, 'I, being a psychologist\*, and therefore a scientist\*, am performing this experiment in order to improve the prediction and control of certain human phenomena; but my subject, being merely a human organism, is obviously propelled by inexorable drives welling up within him, or else he is in gluttonous pursuit of sustenance and shelter.

Now what would happen if we were to re-open the question of human motivation and use our long-range view of man to infer just what it is that sets the course of his endeavour? Would we see his centuried progress in terms of appetites, tissue needs, or sex impulse? Or might he, in this perspective, show a massive drift of quite a different sort? Might not the individual man, each in his own personal way, assume more of the stature of a scientist, ever seeking to predict and control the course of events with which he is involved? Would he not have his theories, test his hypotheses, and weight his experimental evidence?"

Construct theorists make much of the so-called 'reflexivity' of this model (see for instance Bannister and Fransella, 1971 p.16) by which they suggest that an important, and in their view unique, aspect of Kelly's theory is that in itself it represents an 'act of construing' which may be explained in terms of its own postulates. To this author this emphasis appears to be rather misplaced. It is by no means a unique feature of construct theory; for instance,

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\* Italicised in original

Skinner, albeit in his almost characteristic tongue-in-cheek manner, has attempted to account for his scientific activities in terms of 'competing behaviours' and 'reinforcement contingencies' (see, Skinner 1961, pp i-ii). Neither is it especially clear as to why this should be regarded as such a highly-prized characteristic of a scientific theory.

The so-called 'reflexive' characteristics of the model of 'man the scientist' do have the important implication, however, that construct theory becomes, as Kelly (1970) acknowledges, almost as much a model of science as a model of man. As this author has pointed out elsewhere (Knasel, 1977) to put forward a model of 'man the scientist' involves also putting forward an interpretation of scientific activity. It is relevant to notice here that Kelly's interpretation of science owes considerably more to the philosophical pragmatism of John Dewey (see, e.g. Dewey 1910) than it does to the positivist tradition, and hence differs significantly from the kind of interpretation which has been criticised in Sub-Section 1.1.2. In particular, Kelly places considerably more emphasis upon the significance of what Harré refers to as the 'creative' aspects of theory-building in science and he is also scornful of the 'hypothetico-deductive' method and the primacy of experimentalism in psychology. Something of this flavour is suggested by the following extract, in which Kelly refers to his position under the title of 'constructive alternativism'.

"This philosophical position we have called constructive alternativism\*, and its implications keep cropping up in the psychology of personal constructs. It can be contrasted with the prevalent epistemological assumption of accumulative fragmentation\*, which is that truth is collected piece by piece. While constructive alternativism does not argue against the collection of information, neither does it measure truth by the size of the collection. Indeed it leads one to regard a large accumulation of facts as an open invitation to some far-reaching reconstruction which will reduce them to a mass of trivialities."

Kelly (1970) p.2

It would be wholly inappropriate to suggest that Kelly introduced a 'model of science' which is comparable in its sophistication to those proposed by philosophically and historically orientated writers such as Harré, or, for that matter, Toulmin or Kuhn. It does appear to be significant to state at this point, however, that Kelly's 'constructive alternativism' does appear to be broadly compatible with the views of science which have been discussed in Sub-Section 1.1.2.

For reasons which are rather obscure to this author, Kelly adopted a highly formalised method in presenting his theory. This presentation comprised one 'fundamental postulate', which was elaborated through a series of eleven 'corollaries', each of which were expressed in highly abstract terms. For present purposes it will be sufficient to consider Kelly's 'fundamental postulate' without any detailed examination of its attendant 'corollaries'. This states that:

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\*Italicised in original

"A Person's Processes are Psychologically  
Channelised by the Ways in which He  
Anticipates Events."

Kelly was later to stress the importance of his use of the word  
'person' in this statement.

"We start with a PERSON\*. Organisms, lower animals,  
and societies can wait. We are talking about  
someone we know, or would like to know - such as you  
or myself.\*\* More particularly, we are talking about  
that person as an event - the processes that express  
his personality."

This passage is clearly highly congruent with the model of  
man which has been introduced in earlier sections of this chapter.  
It is quite clear that Kelly's concept of a person as 'someone  
we know' is strongly reminiscent of the more elaborate descriptions  
of human agents which we have already referred to and, further, it  
is also clear that Kelly's reference to 'someone we know' may be  
held to represent an appeal to 'common knowledge'. Additionally it  
should be stated that by using terms such as 'person', 'anticipation'  
and 'event' Kelly is employing the language of human agency rather  
than that of the experimentalist.

The key building block of Kelly's theory is the 'personal  
construct'; it is from this concept that Kelly derives his model  
of 'man the scientist' actively interpreting and anticipating the  
events which surround him, and of which he is a part, and which  
also provides the starting-point for his Role-Construct Repertory  
Technique which provides the basis for this thesis.

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\*Italicised in original

\*\*My underlining

In Kelly's terms a personal construct is a way of 'construing' or interpreting the world. The argument is built around the notion that people differ significantly in the ways in which they perceive phenomena. For example, given the same two books, one person might well say that one is darker in colour than the other, a second person might say that one is bigger than the other, whilst a third person might point out that one book is on cricket whilst the other is on hot-blooded dinosaurs. In accord with the model of 'man the scientist' these personal perceptions or 'constructs' are seen as determining the individual's expectations about a given situation. As we have seen, in Kelly's view the objective of this constant process of 'construing' and 'construct formation' is to enable the person to anticipate events successfully\*, hence, to follow the above example, he or she will be able to deal with a particularly voluminous book more efficiently if he or she thinks of it as being 'heavy' when faced with the task of picking-it up.

It is clear that Kelly characterises 'construing' as essentially a process of drawing contrasts between different elements. It follows logically from this that he viewed all constructs as dichotomous, each having a similarity and a contrast 'pole'. At least three 'elements', or events to be construed, are necessary to form a construct; two of the elements of the construct must be perceived as similar to each other, and the third element must be perceived as different from these two. Following Kelly's argument, we cannot, for instance, know what the adjective 'moral' means to a person until we know what events the person includes under this pole of the

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\*In Kelly's terms, 'to anticipate events by construing their replications'.

construct 'moral versus immoral'\* and which elements he or she would subsume under the description 'immoral'.

In further articulating his approach Kelly goes on to categorise constructs in a number of ways. Hence constructs may be seen as 'core' or 'peripheral', 'permeable' or 'impermeable' - according to the extent to which they allow new elements, or 'tight' or 'loose' dependent upon the variability of the predictors which they give rise to. Kelly also sees the individual's different constructs as having specific interrelationships. People differ not only in their constructs but also in their organisation of these constructs.

Having established the basic similarities between the image of man presented by Personal Construct Theory and the view of human agency advocated in Section 1.1.; and having paid some brief attention to the details of the theory we are now in a position to introduce one of the methods for empirical investigation which Kelly derived from his theory; the Role-Construct Repertory Technique or 'Rep. Test'.

#### 1.2.2. The Rep. Test

'Role Construct Repertory Technique' represents one of Kelly's attempts to provide an instrument for eliciting and analysing personal constructs. Bannister and his co-workers have provided later and more detailed discussions of this family of techniques

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\* Or under other constructs involving the concept of morality - such as, for example, 'moral versus political' or 'moral versus physical'.



together with instances of their use in psychological research (see particularly: Bannister and Mair 1968; and Fransella and Bannister 1977). The most widely known version of this technique goes under the title of 'the Rep. Test'. This represents a method for eliciting constructs from individuals.

The participant is first presented with a list of 'role titles'; chosen to sample a wide range of the people and relationships which may have been important in any person's life. Examples of such role titles suggested by Kelly include: 'A Teacher you dislike'; 'Wife' (or Girlfriend); 'Mother'; and 'the most interesting person you know personally' (sic). Precisely which titles are presented may be decided by the individual investigator. The participant or client\* is then required to supply a different person's name to fit each role title. The names of each person suggested by the participant are written on separate cards. The investigator now picks-out three of these cards and shows them to the participant asking him or her to suggest some way in which two of them are alike, as distinct from the third person. The investigator then points to the third card and asks how this person is different from the other two. This procedure is then repeated with different sets of three cards. There is no prescribed number of triads to present, but Bannister and Mair (q.v.) state that '10 to 15 seems a common range'.

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\*It is important to bear in mind that the Rep. Test was originally developed for use in clinical settings.

The end product of this procedure is hence a set of dichotomous descriptions, elicited in a way which is held to be entirely consistent with Kelly's theoretical statements concerning personal constructs. It should be noted that it is, in a sense, misleading to refer to this as a 'test' in that there is no one standardised format and content. Rather, the 'Rep. Test' represents a label for a flexible phenomenological method or technique.

The mere elicitation of constructs will often be extremely useful, indeed it is used in two of the three studies reported here; but for many purposes it is desirable to derive data more amenable to detailed, particularly numerical, analysis. From the point of view of Personal Construct Theory it is also desirable to be able to sample the way in which an individual employs a construct over a wider range of instances than just three elements. These needs are met by the 'repertory grid' technique, which represents an extension of the basic Rep. Test.

Constructs are elicited in the same way as for the Rep. Test. In addition, however, for each construct participants are required to indicate at which pole they would rate not only the three elements of the original eliciting triad, but also all of the role titles originally presented to them. Conventionally these decisions are recorded on a 'grid', consisting of squared paper along the top of which are represented the various role titles and down the side of which are recorded the constructs as they are elicited. In the simplest variant of the grid technique the investigator places 'ticks' and 'crosses' in the appropriate cells of the grid to indicate which role titles the participant subsumes under the

'emergent'\* pole of the elicited construct. Hence, at the end of the procedure the investigator has a list of constructs employed by the participant, and a record of how he or she uses these constructs with regard to a set of 'significant others' in his or her life.

In analysing this grid data it is possible to obtain measures of the degree of similarity in the participant's ways of construing the different role titles, or in the way he or she construes the constructs he or she has provided, by computing 'matching scores'. In the simplest form, these may be obtained by taking pairs of either rows or columns on the grid and counting the number of times in which the ratings in paired cells coincide. Hence in Figure 1.2.(a) below there would be a 'matching score' of six between Construct A and Construct B.

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Figure 1.2.(a): A Bi-Polar Grid

		Role Figures									
		1	2	3	4	5	6	7	8	9	10
CONSTRUCT 'A'		X	X	✓	✓	X	✓	✓	X	X	X
CONSTRUCT 'B'		X	✓	X	✓	X	X	✓	✓	X	X

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It is clear that these 'matching scores' constitute a crude index of correlation. By employing more involved methods of 'scaling' it is in fact possible to derive more conventional

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\*i.e. the 'similarity pole' as exhibited with the original elicited triad

co-efficients of correlation from the repertory grid. Hence, instead of merely asking the participant to provide a simple 'yes' or 'no' in assessing the appropriateness of the two poles of a given construct to a given element, and recording the decision in the form of a tick or a cross, we might ask the participant to give some numerical assessment of the two pole's relative appropriateness. This could be done, for example, by asking him or her to rate the emergent pole's appropriateness on say a seven-point scale (Figure 1.2.(b)).

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Figure 1.2.(b): A 'Ratings Grid'

		Role Figures											
		1	2	3	4	5	6	7	8	9	10		
Liberal		4	3	4	1	1	2	3	3	7	5	Repressive	
Spin Bowler		7	1	1	3	3	4	4	1	7	7	Pace Bowler	
Frigid		3	2	3	4	3	5	2	5	1	3	Loving	
Content		3	4	3	4	5	2	4	3	3	1	Restless	

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or to rank numerically the appropriateness of the different presented role figures (Figure 1.2.(c)).

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Figure 1.2.(c): A 'Rankings Grid'

		Role Figure											
		1	2	3	4	5	6	7	8	9	10		
Liberal		3	7	4	5	1	6	8	2	10	9	Repressive	
Spin Bowler		9	10	2	8	6	1	5	4	7	3	Pace Bowler	
Frigid		7	3	5	4	6	1	2	8	9	10	Loving	
Content		1	2	8	9	10	6	4	5	7	3	Restless	

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Such numerical data may clearly be used to derive parametric measures of correlation; and the resultant correlation matrices may themselves be subjected to 'higher-order' analysis techniques - notably to factor analysis. Such techniques have indeed been very widely used in repertory grid research (see Fransella and Bannister q.v.). It should be stressed, however, that they are by no means an inevitable or necessary refinement of Repertory Grid Technique - and indeed numerical scaling is not employed in any of the studies reported in this thesis. The amenability of repertory technique to such 'sophisticated' mathematical treatments is significant, however, in illustrating the techniques' suitability in providing data which are in a form traditionally dealt with by psychological researchers.

It should be noted that so far mention has been made only of using people known to the individual as the elements to be construed. Again, this is by no means a pre-condition of the technique. One could use any class of elements in this way. Hence, Bannister and Salmon (1966) presented individuals with the elements 'drawing pins', 'bowler hats', 'loaves of bread' and 'washing machines'. If the particular research interest focussed around, for example, young people's perception of different occupations which they might enter, it would be possible to ask individuals to construe occupational titles such as 'dustbin man', 'bank manager' or 'actuary' and hence to sample constructs particularly relevant to the way in which the individual views occupations - some researchers have in fact done this, for instance Healy\* (1968) and Shubsachs (1975).

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\*Healy gave the acronym ORT (Occupationally Relevant Traits) to one such modification which used 'supplied' constructs seemingly oblivious to the existence of the Object Relations Test.

Kelly's Role Construct Repertory Grid Technique hence represents a family of methods for deriving phenomenological data meaningful to the individual which at the same time are open to 'objective' statistical analyses at both idiographic and nomothetic levels. As such they are of great relevance to our present concerns. Generally they seem to have received a 'good press', as is shown in the broadly positive appraisals given, for instance, by Vernon (1963) and by Bonerius (1965). The concepts of 'reliability' and 'validity' against which psychological measures are generally assessed are not totally appropriate to these techniques. Given their flexibility, to talk of the reliability or validity of the Rep. Test is rather more akin to, for instance, discussing the characteristics of 'psychometric' measures in general rather than those of, say, Eysenck's Personality Questionnaire. Nevertheless, Bannister and Mair (q.v.) do give a lengthy discussion of these ideas in relation to grid techniques, which tends to endorse their general suitability for research purposes.

#### 1.2.3. The Self-Identification Form of the Rep. Test

Kelly's Rep. Test may hence be seen as a prime candidate in fulfilling the criteria suggested on pages 19-22; that is in providing a methodology which permits empirical research which takes into account our notions of human agency and yet which yields data comparable to those traditionally dealt with by psychological researchers. As we have seen, it represents a family of techniques derived from, and consistent with, a theory which itself provides scope for common knowledge ideas of human action and 'personhood', and at the same time allows the collection of data which should satisfy the requirements of the scientific community. In short, in prima facie terms

it appears to resemble a methodological tool which may be useful in moving towards a true empirical science of human action.

The remainder of this thesis represents an examination of the potential in this context of a particular variant of the technique which appears to be especially relevant in taking into account the individual's own sense of 'personhood'. This is Kelly's self-identification' format of construct elicitation.

This differs from the familiar 'minimum context form', which has already been described, in that the participants own name is always present as one of the names used in each of the eliciting triads of cards.

The constructs which emerge from this procedure are hence all ones which the person uses in describing his- or her-self in contrast with, and in relationship, to other people with whom he or she is familiar. To use a currently fashionable phrase, the technique hence allows us to sample an individual's 'self/other perception'; and the end product hence amounts to a self-description in terms which the person him- or her-self habitually uses. For this reason this variant of the Rep. Test appears to this author to be particularly appropriate for what one might term 'person-focussed' research. A look at the available literature, however, suggests that it has so far attracted little or no attention from researchers.

Two possible reasons for this state of neglect occur to the writer. Firstly, attention has tended to focus on an alternative technique which Kelly devised for eliciting self-description

- the 'Self-Characterisation' approach. This consists of a contextual analysis of a brief autobiographical sketch provided by the subject. Secondly, as it stands, the Self-Identification Form does not link-up well with grid methods. Since all the constructs are elicited in reference to 'the self' this is likely to give any grid a possibly artefactual bias - one might expect 'the self' to emerge as a 'superordinate' element due to the technique employed, irrespective of whether or not this reflects a tendency in the individual's own construct system.

This neglect appears to be rather unfortunate, and the present thesis represents an attempt to explore the potential of this variant of repertory technique through its application to three quite distinct and separate areas of psychological description; in each case employing the technique in a rather different way, in an attempt to explore its claimed flexibility. The underlying objective, as should by now be apparent, is to investigate the possibility of using such techniques in carrying out research which is consistent with the concept of an 'effective psychology' which has emerged from this chapter.

### 1.3. About this Thesis

Accordingly, the next three chapters of this thesis each represent separate studies of the potential utility of the self-identification form of the Rep. Test. Some explanation of the reasons for selecting each of these particular areas of research is given in the body of each of the respective chapters. Frequently these reasons centre around the practicalities of the accessibility of relevant participants to the investigator. One underlying theme in the selection of these research topics should be stated at this



point however. An attempt was made to find topics exemplifying each of the three 'basic' areas of 'applied psychology' - that is Occupational, Educational and Clinical description. Accordingly, the three topics dealt with are career choice (Chapter Two); reading and spelling disability (Chapter Three) and recurrent abdominal pains in childhood (Chapter Four). One other theme runs through this choice of topics. This was that in each case the broad problem to be investigated should have some significance for the layman. This is a theme which is dealt with in greater detail in the final chapter (see pp 249-251).

The intention was that each of the studies carried out as part of this research should be complete in itself. Accordingly, Chapters Two to Four are written in such a way that they are basically self-contained, and that they may be largely understood in isolation from each other. The relationships between the three studies, and their overall relevance to the issues raised by this first chapter are discussed in the fifth and concluding chapter.

## Chapter Two: Students' Self-Constructs and Occupational Stereotypes

### 2.1. Introduction and Literature Review

#### 2.1.1. A Place for the Study of Occupational Experience

The study reported in this chapter is concerned with the relationship between individuals' perceptions of themselves and their perceptions of occupations. It is thus an attempt to study a topic which has been regarded as an important factor in occupational experience. Historically psychology, as an academic discipline, has not been particularly concerned with the description and explanation of occupational behaviour and experience. Occupational psychology is frequently conspicuous by its absence from the undergraduate syllabus, and is given only very cursory attention by introductory text-books such as Morgan and King (1966) and Hilgarde, Atkinson and Atkinson (1972). This is most unfortunate. 'Work' accounts for a very large percentage of human behaviour; and represents an almost universal experience amongst modern man. This is a platitude which psychologists should not ignore. The discipline would present a very strange view of man if it concentrated totally on the unusual or exotic at the expense of the obvious and common-place. If psychology is to present accounts of human action which are in any way balanced reflections of individual experience, then it is important to pay attention to the fact that work is a very significant component in our society.

The study of career development is also a very important area in which research can contribute to practice. The topics of careers guidance and careers education are recognised as important ones (see

e.g. Schools Council, 1972; Department of Education and Science, 1973) and increasing attention is being paid to them. Careers education is, however, a very young discipline and it urgently requires an understanding of the phenomena of career development which will assist in the evolution of useful teaching methods and materials.

Research into occupational experience is therefore potentially a worthwhile pursuit both with regard to the development of psychology; and as a source for the development of improved pastoral care techniques in education.

#### 2.1.2. Super's 'Self Concept' Theory

Theories of career development have been many and varied, as has been emphasised by Taylor (1969) and by Crites (1969). One aspect of this literature which has generated considerable attention has been the role which the relationship between the individual's perception of himself and his perception of various occupations may play in determining the individual's occupational entry. In particular, writers have suggested that there will be a 'matching' process between the individual and his occupational environment. This is to be seen in the somewhat simplistic account given by Holland (e.g. 1959; 1966) and in the perhaps rather more subtle, and certainly more all-embracing, approach of Super and his co-workers (e.g. Super, Starishevsky, Matlin and Jordaan, 1963). Super's work is used as the starting point for the research in this chapter because it has given scope to a phenomenological approach and has concentrated attention of self/other perception, the central theme of this thesis.

Super has been concerned to develop a model which will describe the course of an individual's 'career' from adolescence through to eventual retirement. He has successively developed this formulation over the last 25 years: see for example Super (1953); Super, Starishevsky, Matlin and Jordaan (1965) and Super (1967). The review of literature which follows concentrates on one particular aspect of Super's work, but it is important to emphasise that Super has adopted a very broad approach, delineating a series of 'life stages' which describe an individual's career development throughout adulthood. Besides offering a model of the process of occupational choice, he and his associates have dealt with a large variety of concepts relating to career development; such as 'vocational maturity' - the extent to which an individual possesses the competences and attitudes required of him at a given point in his career, (e.g. Super and Overstreet 1960; Super 1974; Super and Kidd 1979; Super and Knasel 1981); or 'work salience' - the importance which an individual attaches to work in his life (Super 1976(b); Knasel, Super and Kidd, 1981). Recently, (Super 1976; Watts, Super and Kidd 1980) he has developed a 'life span, life space' model which amounts to an extension of role theory to the sphere of career development. Super has always aimed to provide a theory which has a wider focus of convenience than the specific topic of career choice, which is the aspect of his work treated here in isolation.

Central to Super's account of occupational choice is the part played by the individual's 'self concept'. In his seminal paper published in 1953, Super presented his theory as a set of ten propositions. The importance ascribed to the self concept is summarised by his eighth proposition, which states:-

"8. The process of vocational development is essentially that of developing and implementing a self-concept: it is a compromise process in which the self-concept is a product of the interaction of inherited aptitudes, neural and endocrine make-up, opportunity to play various roles and evaluation of the extent to which the results of role playing meet with the approval of superiors and fellows."

Essentially, it seems that Super's explanation of occupational choice rests largely on an implicitly Rogerian-style conception of self-congruence. The individual is seen as seeking occupations which are perceived as consistent with his or her self-concept. The occupation hence becomes a way for the individual to implement self-perceptions - to express him- or her-self through work.

This obviously represents a somewhat optimistic view of the world of work! It has, in fact, been suggested that 'self-concept theory' may be particularly appropriate to middle-class occupations (e.g. Roberts 1981). However, accepting that this theory's range of convenience may not encompass all the occupations which exist in our society, this model does have utility as a stimulus for research, in that it provides a working 'hypothesis' around which research may be focussed. This 'hypothesis' states that an individual's self-concept will be more similar to the concept he holds of his chosen occupation than to those he holds of other occupations.

#### 2.1.3. Studies of Self-Occupation Congruence

A number of American workers have attempted to investigate this hypothesis, or slightly more specific variants of it. For instance, Blocher and Shulz (1961) tested the hypothesis that the agreement

between the self and occupational concepts of adolescent boys would be greater for occupations in which they expressed interest than for those in which they had little interest. A 180-item checklist was administered to 135 12th-grade boys and they were asked to use it to describe their actual and ideal-self concepts together with concepts of most and least liked of the 45 occupations then listed in the Strong Vocational Interest Blank. Their descriptions of the liked occupations agreed more with the actual self concepts than did the descriptions of the occupations that were least liked. Other American workers, such as Morrison (1962) and O'Hara (1967) have reported essentially similar results.

A slightly different approach was adopted by Englander (1960), who administered a Q-sort instrument of 80 personality-items to 126 women studying education, asking them to describe themselves and an elementary school teacher. From these data, Englander was able to differentiate 62 women specialising in elementary education from 31 women specialising in secondary education and from 33 women not specialising in education. The self concepts of those in elementary school teacher than were the self perceptions of the other two groups of specialists.

Research into this area in the United Kingdom has been rarer. One study of interest was that of Haystead (1977) who conducted a longitudinal study of 10 boys and girls from the end of their second year at school until the then statutory leaving age of 15. Dimensions of self images were derived from interviews in which the pupils were asked to describe how they saw themselves and the requirements of people in various jobs. The dimensions of self and

occupational perceptions were transferred to cards and the children asked to arrange the cards according to how true they thought the items were of themselves, and how important they thought it was that someone in the job they wanted to obtain should possess these qualities. Significant associations were found between self concept items and the perceived requirements of occupations, although it is reported that these relationships held only in the negative sense that pupils who rated themselves as not having characteristics 'generally accepted as desirable' (e.g. good at giving orders; intelligent; strong) tended to say that it was not important that people in their preferred occupation should possess them.

#### 2.1.4. Starishevsky and Matlin's 'Translation' Model

So far we have considered only Super's earliest and simplest formulation, to which the above studies are relevant. Super has, however, provided rather more detailed theoretical accounts of the processes he is describing. In his later works, Super identified three processes relating to self-conceptualisation - 'Formation', 'Translation' and 'Implementation'. (Super 1963, Super and Bohn 1971).

Super's ideas about self-concept formation are not in any way original, and although he stresses its importance it is, as a subject in itself, somewhat peripheral to his theory.

He argues that the 'translation' of self-concepts into occupational terms takes place via one or more of three processes - 'identification', which involves an individual choosing a model and saying "I am like him" or "I want to be like him" without considering

specific traits piecemeal; - 'experience' via which opportunities are provided for the discovery of vocational aspects of the self-concept; and 'observation' through which the individual learns that given aptitudes and interests are important in certain occupations. It is this translation stage which is particularly relevant to the description of the processes by which an individual specifies his occupation.

Finally, 'implementation' involves the conversion of self-concepts into realities. The individual either seeks the necessary training for the chosen occupation which he or she sees as consistent with his or her self-concept or finds employment in it.

An important step forward in the articulation of the concept of 'translation' was taken by two of Super's associates, Starishevsky and Matlin, who proposed an explicit model of the process (1963). They argue that implicit in the concept of translation is an analogy involving 'two realms of language'; an area of psychological statements; and an area of occupational terms. To label these areas, they use two words, 'Psychtalk' and 'Occtalk' which are unfortunately reminiscent of Orwell's 'Newspeak'. 'Psychtalk' is said to consist of 'all statements whose predicates are dimensions a person habitually used to differentiate people'. It should be noted that this concept is idiographic in that each individual will have his or her own psychtalk. Self-concepts are central to psychtalk; Starishevsky and Matlin define self-concepts as 'all statements in psychological terms that the subject is prepared to make about himself or others'. They define 'occtalk' as consisting of all statements an individual is willing to make of himself or others, of which the predicated are



names of occupations or their equivalents. Again, the concern here is with individual perceptions rather than with any shared reality. An individual's occtalk will include any phantasy he might have about occupations, irrespective of whether they are reality consistent.

With regard to translation they argue that any statement which an individual makes 'in occtalk' may be translated into a psychtalk equalivalent. For example, "I want to be a lawyer" (occtalk) might 'mean' - "I am socially minded, aggressive, and concerned with upholding society's standards", (psychtalk). Again, the exact translation will vary from person to person. Starishevsky and Matlin introduce the analogy of individuals having their own dictionaries, consisting of listings of predicates in psychtalk equivalent to predices in occtalk.

This leads to the concept of 'incorporation'. They argue that all occtalk statements, such as "I am a Salesman", "I want to be an Optician" or "I am going to be an Opium Smuggler" imply a psychtalk translation which is, itself, a part of the individual's self-concept. The degree to which an occtalk statement involves elements of the individual's self-concept they term its 'level of incorporation'. They argue that individual occupational statements will differ with regard to their level of incorporation of the totality of their occtalk statements. Adopting this model's terminology the earlier hypothesis may be restated as - "Individuals will exhibit a higher level of incorporation with regard to their chosen occupation than will be the case with other occupations".

Starishevsky and Matlin close their discussion by suggesting that repertory grid technique will prove to be particularly appropriate to testing this kind of hypothesis. This does not come as a tremendous surprise. Generally there appear to be great similarities between their model and personal construct theory. This is particularly apparent when one considers the emphasis which these authors place upon the ipsative nature of an individual's language system, the importance which they place upon statements being used to differentiate phenomena, and their argument that it is necessary to consider the scope which an individual ascribes to a statement - all of which is highly reminiscent of Kelly.

It is understandable, therefore, that a number of studies were carried out by doctoral students at Super's department at Teacher's College, Columbia University, employing an adaptation of repertory grid technique to test hypotheses derived from Starishevsky and Matlin's model. (e.g. Shiner 1963; Oppenheimer 1966; Bingham 1966)

These studies employed an adaptation of Kelly's technique christened 'The Modified Rep. Test (M.R.T.)'. The M.R.T. employs the normal Rep. Test method for eliciting bi-polar constructs, as described by Kelly (1955, pp. 232 + ff) and by Bannister and Mair (1968, pp. 48). Subjects are then asked to rate themselves and various occupations along seven-point scales, at either end of which are the poles of the previously elicited constructs; and this has the effect, as the authors point out, of incorporating aspects of Semantic Differential Technique (Osgood, Suci and Tannenbaum, 1958). 'Level of Incorporation' is defined, in terms of the M.R.T., as the degree to which these ratings manifest agreement between the

description of self and the description of the occupation.

Shiner (q.v.) investigated the relationship between self-concept and change of occupation. In a cross-sectional study of 32 'National Defence Education Act' (N.D.E.A.) counselling and guidance enrollees at Teacher's College he used the M.R.T. to measure self concepts and occupational concepts. He found significantly higher levels of incorporation between self concepts and the concept of the chosen occupation, than between self concept and the concept of the relinquished occupation. This represents clear-cut support for Starishevsky and Matlin's model.

Similarly, Oppenheimer (1966) administered the M.R.T. and an Occupational Preference Scale (derived partly from the Strong Vocational Blank) to 81 male liberal arts students at nearby Rutgers University. For each participant the correlation between an occupational preference hierarchy derived from the agreement between self and occupational rating as shown by the M.R.T. and the preference hierarchy expressed by the occupational preference scale was calculated. He reports that the median correlation was 0.52. This again provides empirical support for Starishevsky and Matlin's model. This with a group whose occupational interests could be reasonably expected to be less homogeneous than that studied by Shiner.

Bingham used the M.R.T. to study 'regnancy' - a 'meta-dimension' of an individual's self concept. Basically, the hypothesis in this context was that when an individual changes occupation the constellation of self-attributes which he or she considers relevant to performance in the new occupation is perceived as more important than the

constellation of self-attributes he or she considers relevant to performance in his or her previous occupation. Like Shiner, Bingham studied individuals changing from teaching to counselling, and enrolled on counselling courses. He also looked at 'level of incorporation', essentially replicating Shiner's study, but increasing the size (to 82) and geographical representativeness of the sample, making the study longitudinal and employing a control group, (consisting of 44 employed secondary school teachers). The hypothesis that change of occupation is a function of the regnancy of occupational self-constructs was supported by three observations; enrollees manifested a significantly higher level of regnancy for their chosen occupation - counsellor - than for their relinquished occupation - teacher; they also manifested significantly higher regnancy for counselling than did controls; and they manifested greater difference in regnancy for the two occupations than controls. These three findings were, however, highly correlated. No difference in predictive efficiency for 'regnancy' as opposed to 'level of incorporation' was observed.

A fourth study was carried out by Healy (1968) who, as mentioned in Chapter One, compared the usefulness of the M.R.T. technique to that of a more 'exogenous' instrument, the 'O.R.T.' (sic). He administered both instruments to a total of one-hundred-and-forty-seven subjects, taking courses in medicine, accountancy and engineering. The agreements between the self and occupational concepts of the medical and accounting subjects were, as expected, higher for their own respective occupations, but the engineering students did not show higher agreement between self- and chosen-occupational concepts. Results were very similar for both instruments.

This study hence gives support to the translation model - albeit whilst perhaps suggesting a limit to its usefulness. It also provides a measure of concurrent validity for the repertory technique used in the previous studies\*.

Super and Steer (unpub.) attempt to explain the failure of the M.R.T. to prove to be more efficient than the more conventional measure used by Healy. They argue that:

"in using Kelly's varied social roles as stimuli in the first part of the M.R.T., Healy may have obtained traits highly relevant for describing a broad spectrum of interpersonal roles, but not very suitable for describing occupational roles."

They suggested that this difficulty could have been avoided by using the 'Occ. Rep.', a term which refers to a modification of the M.R.T. suggested originally by Bingham, whereby occupational rather than social roles are used at the construct elicitation stage.

This technique was employed by Grundfest (1970) working at Rutgers University. In addition to testing the general translation hypothesis, she speculated that women in their final stages of teacher training who had experienced teaching practice would show more agreement between self concepts and teaching than those in the early stages. The juniors were also expected to show more agreement between concepts after teaching practice than before. These students did exhibit a higher level of incorporation for teaching than for other jobs, but neither of her subsidiary hypotheses were confirmed.

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\*Although Healy did not interpret his findings in this way.

These studies illustrate that Starishevsky and Matlin's model has proved to be a fruitful basis for research. The model certainly does not generate any direct predictions which were not already apparent in Super's own writings, but it does provide a useful (if not aesthetically pleasing!) vocabulary for dealing with the problem and, by establishing links with construct theory it has provided what has proved to be a reasonably convenient and successful technique for investigating the process. Having said this, there are a number of problems which are inherent in the M.R.T. and its derivative the 'Occ. Rep.'.

Firstly, by using the normal 'Minimum Context Form' of elicitation, constructs are obtained which the individual would not necessarily apply to himself given the choice. In effect, this procedure is sampling the individual's person perception in general rather than his self-perception in particular. The two areas of judgement do not necessarily involve the same construct repertoires. To some extent, this problem was invited by Starishevsky and Matlin's distinctly idiosyncratic definition of the self concept, which includes 'all statements in psychological terms that the subject is prepared to make about himself or others\*.'

Secondly, one may question the need to employ 'borrowings' from Semantic Differential technique, rather than a more conventional Repertory Grid procedure.

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\*My underlining

Thirdly, the M.R.T. takes the form of a self-administering, paper-and-pencil group test. It is worth noting that Bingham did not even administer the test himself, but rather sent them to various colleges throughout the United States. This self-administering procedure places considerable extra cognitive demands on the participants, and in fact M.R.T. subjects experienced enormous difficulties in completing the materials. (Super, pers. comm.). Finally, although the M.R.T. procedure derives data from a variety of occupations in a number of studies, it was only the materials from the 'relinquished' and 'new' occupations which were analysed by the investigators. This seems to be a waste of materials; and of the subjects' time.

In an earlier study (Knäsel, 1976), an attempt was made to overcome those problems inherent in M.R.T. technique via the use of a repertory technique combining a self-identification form of construct elicitation with an adaptation of conventional grid technique.

The method employed involved a 21 triad self-identification elicitation procedure. This was followed by the administration of a grid on which the subject was required to employ the constructs previously elicited to rate, on a 'match/no match' basis, a series of 11 occupational roles, such as 'Banker' and 'Journalist' including the subjects' chosen occupation. An index of 'Level of Incorporation' was derived by calculating matching scores between self (as described by the subject at the elicitation stage) and each of the occupations.

An additional aspect of the methodology was the prior administration of an occupational interests guide - the Thurstone Interests Schedule\* - which gave scores on ten interests categories. Each of these categories were represented by an occupational role at the grid stage of the procedure. To give two examples, Accountant was chosen to represent 'computational' occupations whilst Child Welfare represented 'humanitarian' ones. The additional, eleventh, role was the subjects' chosen occupation; i.e. Teacher.

The use of the self-identification procedure avoided the problem of the subject providing constructs which she would not necessarily apply to herself. The grid procedure avoided the use of semantic differential technique, and, importantly, allowed data to be collected from a variety of occupations. The repertory materials were administered on an interactive, individual basis. Thus, all four of the major pitfalls associated with the M.R.T., and related techniques, were avoided by this procedure.

The subjects in this exploratory study were 34 female teacher training students at a College of Education in the west of England. Twenty-one were completing their first year of training, the remaining 13 had just finished their third and final year and were about to leave the college. Three 'hypotheses' were formed, these were:

- "a) That the 'occupational grid' would reveal a higher 'level of incorporation' (as shown by matching scores between the various occupations and the individuals' 'self concept' - derived from the construct elicitation) for the subjects' chosen occupation of 'Teacher' than for the other occupations presented.

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\*Described in Section 2.2.2.



- b) That this difference would be more marked with third year students than with first year students.
- c) That there would be a positive correlation between the interest scores as shown on the Thurstone Interests Schedule, and the 'matching to self' scores on the occupational grid."

There was strong support for the first hypothesis. T-tests revealed that 'Teacher' derived significantly higher matching-scores with self than did the next highest rated occupation. Contrary to hypothesis (b) however, this effect was less marked amongst final year students than amongst the first years. Hypothesis (c) was strongly supported - the obtained correlations between interest scores and matching-to-self scores were 0.71 for first year students, 0.73 for third year students and 0.89 for all subjects considered together.

This support for hypotheses (a) and (c) can be seen as giving support to the translation model. Hypothesis (b) was not derived directly from this model.

In general, this study suggested that the self-identification methodology adopted offers promise as a research instrument in this area, in that it allows a direct test of hypotheses derived from the Starishevsky and Matlin model, whilst avoiding the problems associated with the M.R.T. In permitting the operationalisation of hypothesis (c), it in fact allowed a broader focus than that given by the earlier technique. The correlation data between interest scores and matching-to-self scores also provide encouraging evidence of the technique's concurrent validity.

Some minor technical problems were encountered, however. Firstly, subjects were only asked to supply the emergent pole of each construct - it was thought that this might make the task easier. In fact, many subjects said that it was easier for them to find differences than similarities. Secondly a large number of 'trivial' or inappropriate constructs, such as references to eye-colour, or an inability to make tea acceptably, were elicited. It was thought that this had been encouraged by the instructions given to the subjects, which stated that subjects were not to be worried about producing obscure or frivolous descriptions. Finally, the bi-polar 'match/no-match' rating procedure gave a number of subjects difficulty. There appeared to be a need for a third neutral, response option.

These methodological difficulties, however, are easily rectifiable and the exploratory study described above does provide grounds for regarding the technique described as providing a useful tool for investigating the phenomena of Self-Occupation matching, and suggests that the same procedure could be applied to comparable groups of subjects in the process of implementing different occupational choices. The series of studies described in the remainder of this chapter achieve this by applying the technique to students of a variety of 'occupationally relevant courses'.

## 2.2. Design of the Study

The study described in this chapter represents a logical extension of that reported by Knasel (1976). Essentially the same hypotheses were investigated, using a sample of young people engaged in a wider range of courses which could each be said to lead to clear

'occupational distinctions'; in the same way that teacher training 'prepares' a person for a 'career' in teaching. Additionally, certain detail refinements were made to the repertory procedure used, in line with lessons drawn from the earlier study.

#### 2.2.1. Selection and Recruitment of Subjects

For the purpose of this study it was decided to recruit participants from amongst those people following four occupationally relevant courses which were available within the Bangor area. The four courses chosen were: a degree course in Forestry; a degree course in Accountancy; a course leading to a Post-Graduate Certificate in Education (PGCE); and a course for prospective State Registered Nurses (SRN's).

It should be noticed that both Forestry and Accountancy could be said to represent unusual degree choices. U.C.N.W. Bangor is, in fact, the only British institution offering an Honours degree course in Forestry. At the time when this study was carried out the idea of a degree in Accountancy was regarded as very innovatory - such courses were only instituted during the mid-seventies. In both cases staff involved in running these courses stated that almost all of their graduates sought jobs in the relevant profession. At the time of this study both of these courses had a very good 'placement record' in that it was extremely rare for successful Honours graduates to fail to secure employment in their chosen profession.

The Post-Graduate Certificate in Education represents an initial teaching qualification for those people who have already obtained a university degree (or equivalent). It is a one-year course and

involves intensive periods of teaching practice.

All three of these courses were offered at U.C.N.W. Bangor. Student nurses were recruited from the SRN course based at St. David's Hospital, Bangor\*. This is a three-year course, which is very largely carried out 'on-the-job'.

The age and sex of the participants in this study are presented in Table 2.2.1.

Accountancy students were all recruited during the spring term of their final year on the course. Initial contact was made through the investigator describing the broad objectives of the study and making an appeal for volunteers at one of their routine lectures. Seven participants were recruited during the spring of 1977, and a further 13 during the corresponding period of 1978. The 'accountants' recruited to the study were predominantly male, but five women were included amongst the total sample of 20 students.

Of the 11 'foresters', five were second-year students and the remaining six were first-years. All were recruited - using the same kind of initial contact as with the accountants - during the summer of 1978. All participants were male; it is very rare for women to take this course.

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\*I should like to thank Dr R.H. Davies of St. David's Hospital for his help in arranging access to this department.

Table 2.2.(1): Individual Subject Characteristics

	N	SEX		AGE (In Years)		
		MALE	FEMALE	RANGE	MODE	
ACCOUNTANTS	20	15	5	19 - 23	20	ACCOUNTANTS
FORESTERS	11	11	--	19 - 26	19	FORESTERS
TEACHERS	13	5	8	21 - 24	21	TEACHERS
NURSES	15	2	13	18 - 21	19	NURSES

All the PGCE students were recruited, in the same way as the previous two groups, during the spring of 1978. There was a slight preponderance of women amongst this group. All participants had degrees in 'arts' subjects (English being the most common), which had been completed at U.C.N.W. Bangor immediately prior to joining the course.

Student nurses were recruited from three different stages of the course, five each from: the initial six-week preliminary course; the final two months of the first year on the course proper; and the corresponding period in the third and final year of the course. All of these interviews were conducted during the autumn of 1977. Second-year students were not available at this stage of their course as they were working full-time in the hospital. Tutors selected participants for the study - all participants were, however, told that their involvement in the study was entirely voluntary and one in fact declined to take part. The two male nurses who participated in the study were both in the final year of the course.

#### 2.2.2. Materials

The repertory procedure employed in this study involved two basic stages. Firstly, subjects were required to respond to 21 'triadic sorts', in each of which 'self' was combined with two significant others taken from the following list of seven role titles:

MOTHER  
BOY (OR GIRL) FRIEND  
BEST FRIEND  
FAVOURITE TEACHER  
A PERSON YOU DON'T LIKE  
A SUCCESSFUL PERSON  
A PERSON YOU ADMIRE

these were selected from amongst those suggested by Bannister and

Mair (1968, pp 53). A standard randomised order for the presentation of these 21 triads was devised using a random numbers table.

In the second stage of the procedure, participants were required to apply the 21 constructs elicited during the first phase to a range of occupational role titles. In each case these included the following ten titles:

CHILD WELFARE  
SONG WRITER  
BANKER  
HOTEL MANAGER  
DOCTOR  
JOURNALIST  
ACCOUNTANT  
CIVIL ENGINEER  
CARTOONIST  
POLITICIAN

Each of these role titles was chosen to represent one of the ten occupational categories used in the Thurstone Interests Schedule. Additionally, 'foresters', 'teachers' and 'nurses' were asked to evaluate their own chosen occupation as one of the role titles which were presented. This stage of the procedure was presented in the form of an 11 (or, in the case of accountants, 10) by 21 squared grid; with the occupational role titles printed across the top of the grid, and with the 21 elicited constructs written-in by the investigator down either side of the grid. The constructs did not appear on the grid in the same order as they were elicited - that is the first elicited construct was not written into the spaces nearest the top of the grid. Rather, the investigator entered the constructs into the grid according to a standard randomised order.

This procedure differs from that employed in the earlier study in two principal ways, viz:

i) Participants were required to provide both a 'similarity' and a 'contrast' pole for each construct.

ii) In completing the grid, participants had available three rather than two response categories. They could use '+', signifying the similarity pole; '-' signifying the construct pole; or '0' signifying that neither description was relevant.

Participants were also asked to complete the Thurstone Interests Schedule. This 'instrument' was introduced in 1947 to replace the earlier Thurstone Vocational Interests Schedule. The manual (Thurstone, 1947), describes it as consisting of a single sheet of paper, 11" by 17", in the form of a four-page folder. On the double inside page is printed a large rectangle which is divided into ten rows and ten columns. In each of the resultant one hundred cells is printed a different pair of occupations. These occupations are chosen to represent ten vocational fields, viz:

PS	PHYSICAL SCIENCE
BS	BIOLOGICAL SCIENCE
C	COMPUTATIONAL
B	BUSINESS
E	EXECUTIVE
P	PERSUASIVE
L	LINGUISTIC
H	HUMANITARIAN
A	ARTISTIC
M	MUSICAL

Perhaps surprisingly, in view of the schedule's author, these ten fields do not represent the result of a factor analysis; rather they were chosen so as to include the 'Spranger Life Interests' as well as 'those categories which are in current use'. Respondents are asked to mark their preference with regard to each pair. He or



she is encouraged to mark both items if he or she 'likes' both of them. Similarly, if neither alternative appeals, the respondent is allowed to cross-out both of them. This procedure hence avoids the criticism which was later levelled at measures which employ a 'forced-choice' technique, such as the Kuder General Interests Survey (Kuder, 1970), by Stahman (1971).

Thurstone states that the schedule 'seldom requires more than ten minutes, and usually less'. This represents a considerable time-saving over most such schedules; the Strong Vocational Interests Blank (Campbell, 1971), for instance, can take over an hour to administer.

Thurstone claims 'split-half' reliabilities of between 0.90 and 0.96 for all ten of the scales. He also presents data concerning what he terms 'item validities'. These represent the correlation between the individual item score and the profile score to which the item contributes. The average item validities on each of these scales range between 0.64 and 0.85. Thurstone presents no normative data. He argues that such data are not necessary because the scores on the ten scales are directly comparable.

The schedule was reviewed in the Fourth Mental Measurements Yearbook (Buros, 1953) by Frederickson, and by Super. Both reviewers are critical of the lack of normative data. Frederickson, however, argues that this criticism does not detract seriously from the schedule's usefulness, and argues that, due to the ease with which it may be administered and scored, it represents a valuable tool for the careers counsellor. Super is more critical, and argues that

until more investigations are carried out into the validity of the instrument, its use should be restricted to research - which would, presumably, include studies like that reported in this Chapter.

Besides the lack of normative data, the most important criticism which might be levelled at the Thurstone Interests Schedule would be its age. There have been some significant changes in the titles used within the general job market over the last 30 years and there is an argument that a revision of the schedule would be valuable; such that it could, for instance, incorporate newer occupations such as Computer Operator or Systems Analyst - both of which would fit well with the schedule's Computational category.

Having acknowledged these reservations, the schedule was chosen for this and the earlier study for two main reasons. Firstly, its simplicity and the speed with which it may be completed. This was considered to be an important plus, allowing the participant to complete the schedule in his or her own time prior to completing the Rep. Test materials. Secondly, the occupations used in the schedule are likely to be perceived as being of approximately the same 'occupational level' (Frederickson argues that the occupations chosen are particularly relevant to 'higher level jobs'). This consideration was thought important in deriving the occupations to be used in the repertory grid.

The major hypotheses to be investigated using these materials were essentially the same as those employed in the earlier study, viz:

i) That the 'occupational role titles' grid would reveal a higher 'level of incorporation' - as shown by matching-scores between the various occupations and the individuals' 'self-concepts', as derived from the construct elicitation stage - for the participants' 'chosen' occupation than for the other occupations presented on the grid.

ii) That there would be positive correlations between interests scores for the categories on the Thurstone Interests Schedule, and the 'matching-to-self' scores on the repertory grid.

### 2.3. Conduct of the Study

On agreeing to participate in the study, subjects were given a Thurstone Interests Schedule to complete prior to their 'interview' which was arranged for a time a few days later.

Interviews were always conducted in conditions of quiet and privacy. With 'accountants', 'foresters' and 'teachers' many interviews were conducted in a room within the U.C.N.W. Department of Psychology. In a smaller number of cases they were conducted at either the participant's or the investigator's own room in his or her Hall of Residence. Student nurses were interviewed during their normal class hours. These interviews were conducted in the library of the teaching department of the hospital. In every case, however, interviews were conducted in a comfortable setting which was free from unnecessary interruptions.

After an initial brief 'chat' about some topic irrelevant to the study - such as the previous night's soccer results, or the participant's taste in rock music - designed to put the participant at ease, the participant was asked whether he or she had brought along the interests schedule. This was put to one side, and it was

explained to the participant that the session would be in two parts. He or she was then shown the list of significant others, and asked to think of particular people, known to him or her, to whom the titles (listed on p. 60) could be applied. It was explained that the participant was to choose one person corresponding to each title. Participants were invited to write the names of the people next to the appropriate title if they felt that this might help them; whether or not the subjects did this was up to them.

The investigator told participants that they would be shown sets of three cards - one of which would have YOURSELF written on it, whilst on each of the other two cards would be written one of the titles they had been shown. They were to take these cards as representing the people who had been chosen. They were told that their task would be to draw comparisons between the people presented to them - they were to find some way in which two of the people were alike whilst the third person was different - something which they had in common which was not shared by the third person. Examples were given such as - 'I might say that my MOTHER and BEST FRIEND are alike in that they are outgoing whereas I'm rather shy; or again if given MYSELF, BEST FRIEND and FAVOURITE TEACHER, I might say that MYSELF and BEST FRIEND are alike in that we are both post-graduate students whereas my FAVOURITE TEACHER hasn't been'. Participants were told that a different description was required for each set of three cards presented.

The subject was then presented with the first three cards, viz: SELF, FAVOURITE TEACHER, MOTHER. When the subject had provided both a 'similarity' and a 'contrast' pole the investigator wrote these in

the space provided on the construct elicitation record and also in the relevant spaces on the occupational ratings grid.

The participant was then shown the second triad of cards, and this procedure was repeated until all 21 triadic sorts had been completed. If the participant was plainly having difficulties in finding an appropriate description, he or she was asked if he or she would like to go on to the next three cards and leave the present ones until later.

On completion of the twenty-first triad, the participant was told that the first part of the procedure was now complete. The participant was then shown the occupational role title grid. The investigator explained that the task was to rate each role title against each of the descriptions.

The similarity pole - i.e. the description which had been applied to the two significant others who were 'alike' - was always written into the left-hand side of the grid and the difference pole into the right-hand side. Participants were asked to put a plus sign (+) into the relevant cell of the grid if they thought that the left-hand (i.e. similarity) description fitted the particular job title best, and a minus sign (-) if they regarded the right-hand description as the most appropriate. They were to put a zero (0) if they felt that neither description was appropriate. Participants were allowed to fill in the cells of the grid in any order in which they chose. If a participant enquired what a given occupational role entailed he or she was asked to make use of his or her own knowledge.

Table 2.4.(1): 'Matching to Self' - Summary of Group Mean Scores

	N			CW	SW	B	HM	D	**	J	A	CE	C	P		N	
ACCOUNTANTS	20			8.70	7.45	8.35	7.05	9.35	11.40	7.05	11.40	7.70	7.65	7.10		20	ACCOUNTANTS
FORESTERS	11			7.91	5.18	6.64	6.18	7.45	10.00	6.72	6.91	8.54	6.00	5.80		11	FORESTERS
TEACHERS	13			8.77	8.31	6.31	6.62	8.00	11.46	7.77	6.62	5.85	6.23	7.15		13	TEACHERS
NURSES	15			11.20	9.73	8.93	9.13	10.73	13.53	8.27	8.47	9.73	9.13	7.73		15	NURSES

Key to Occupations

CW = Child Welfare  
 SW = Song Writer  
 B = Banker  
 HM = Hotel Manager  
 D = Doctor

J = Journalist  
 A = Accountant  
 CE = Civil Engineer  
 C = Cartoonist  
 P = Politician

\*\* = Chosen Occupation

Table 2.4.(2): Interests - Summary of Group Mean Scores

	N			PS	BS	C	B	E	P	L	H	A	M		N	
ACCOUNTANTS	20			5.8	5.25	13.90	11.60	7.75	8.75	9.65	3.50	5.15	4.10		20	ACCOUNTANTS
FORESTERS	11			9.27	13.36	2.91	8.45	7.82	4.13	10.36	6.91	8.91	5.91		11	FORESTERS
TEACHERS	13			6.15	10.31	3.08	4.46	7.92	8.15	14.08	10.54	12.38	11.85		13	TEACHERS
NURSES	15			7.13	11.27	2.13	2.40	5.53	5.53	8.13	9.00	7.40	5.53		15	NURSES

Key to Interests

PS = Physical Science  
 BS = Biological Science  
 C = Computational  
 B = Business  
 E = Executive

P = Persuasive  
 L = Linguistic  
 H = Humanitarian  
 A = Artistic  
 M = Musical

On completion of the grid the ideas behind the study were explained to the participant, as far as possible.

Interviews were not timed. There were, however, clear variations in the times taken to complete the materials, ranging between about 35-90 minutes; with 45 minutes representing a rough average.

Obviously, due to the essentially interactional quality of the sessions it was not possible to employ rigidly standardised instructions for all participants, and there were certain variations. All sessions, however, followed the general outline given above.

#### 2.4. Findings

The principal findings to emerge from this study are presented in Table 2.4.(1) and 2.4.(2) (pp 68-69). Table 2.4.(1) presents the mean group matching-to-self scores for the four different groups of participants in the study - i.e. data indicating the mean level of 'self-incorporation' for each of the occupations presented to the participants. In each case the chosen occupation emerges with the highest matching-to-self score, and in each case one-tailed Mann-Whitney 'U' Tests reveal this score to be statistically significantly greater than the next highest score, at the 0.05 level.

The data summarised in these tables are presented in more detail in subsequent tables and figures. Tables 2.4.(3), 2.4.(5), 2.4.(7) and 2.4.(9) present individual matching-to-self scores for each participant, broken down according to the course of study which they were pursuing. Similarly Figures 2.4.(a), 2.4.(c), 2.4.(e) and 2.4.(g) present histograms summarising the mean matching-to-self



Table 2.4.(3): 'Matching to Self' Scores, Accountants

SUBJECTS	CW	SW	B	HM	D	J	A	CE	C	P
1	6	9	16	14	14	13	16	10	10	14
2	7	7	9	11	11	11	13	10	7	5
3	15	9	6	14	14	6	10	9	8	5
4	15	5	8	12	12	6	16	4	12	5
5	12	8	14	12	12	7	15	14	9	7
6	17	10	10	13	13	11	13	7	10	10
7	4	5	11	5	5	7	9	3	5	10
8	6	4	10	9	9	6	10	9	3	3
9	10	4	3	4	4	6	6	3	3	2
10	10	14	8	18	18	14	8	9	11	13
11	5	8	8	3	3	8	9	5	11	8
12	12	13	7	8	8	12	7	8	12	8
13	5	10	12	8	8	14	13	13	10	7
14	9	16	9	9	9	11	10	12	13	9
15	4	3	6	14	14	8	8	6	4	4
16	10	6	3	5	5	6	10	8	5	11
17	8	8	13	9	9	6	15	14	6	7
18	9	5	11	8	8	6	11	4	8	3
19	6	1	6	4	4	2	3	1	0	4
20	9	4	7	7	7	5	8	5	5	7
TOTALS	174	149	167	141	187	141	228	154	153	142
MEANS	8.70	7.45	8.35	7.05	9.35	7.05	11.40	7.70	7.65	7.10

TOTAL SAMPLE SIZE = 20

Figure 2.4.(a): Mean 'Matching to Self' Scores, Accountants

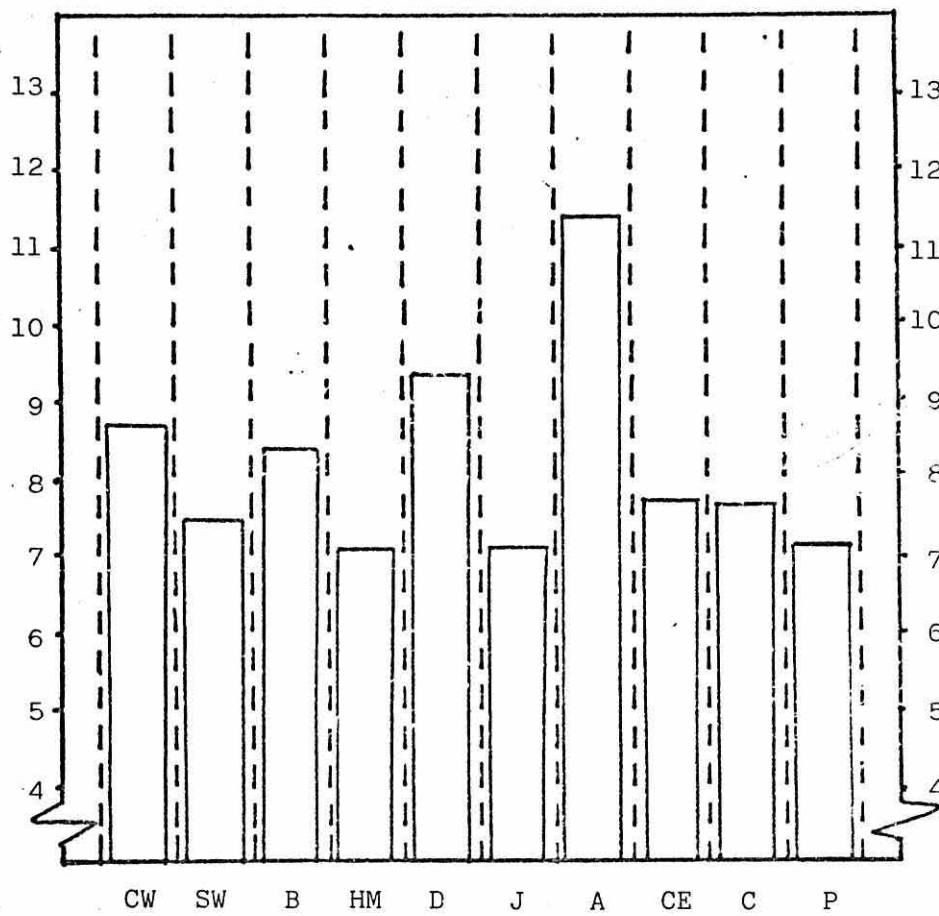


Table 2.4.(4): Interests Scores, Accountants

SUBJECTS	PS	BS	C	B	E	P	L	H	A	M
1	0	1	17	10	5	17	17	4	5	1
2	13	4	17	17	8	13	8	0	1	4
3	1	3	14	15	13	6	1	12	3	2
4	7	4	20	12	6	10	10	1	8	5
5	1	11	9	4	8	3	10	0	0	4
6	10	4	12	17	10	6	12	1	5	10
7	2	1	8	13	9	8	10	0	1	0
8	9	17	12	14	7	10	15	0	7	1
9	3	5	15	8	8	14	11	5	1	0
10	8	13	11	1	5	0	5	13	11	15
11	2	2	15	7	10	14	18	7	11	7
12	0	0	16	13	0	2	0	1	0	0
13	7	10	13	12	8	9	15	6	17	11
14	15	2	14	16	6	7	9	0	2	8
15	10	17	12	8	10	8	10	12	13	0
16	12	4	16	14	12	6	5	2	2	3
17	13	6	16	12	10	11	7	0	18	3
18	3	1	20	18	8	6	1	3	4	5
19	0	0	14	17	5	9	16	1	2	0
20	0	0	7	4	15	11	13	2	2	3
TOTALS	116	105	278	232	155	175	193	70	103	82
MEANS	5.8	5.25	13.9	11.6	7.75	8.75	9.65	3.5	5.15	4.1

TOTAL SAMPLE SIZE = 20

Figure 2.4.(b): Mean Interests Scores, Accountants

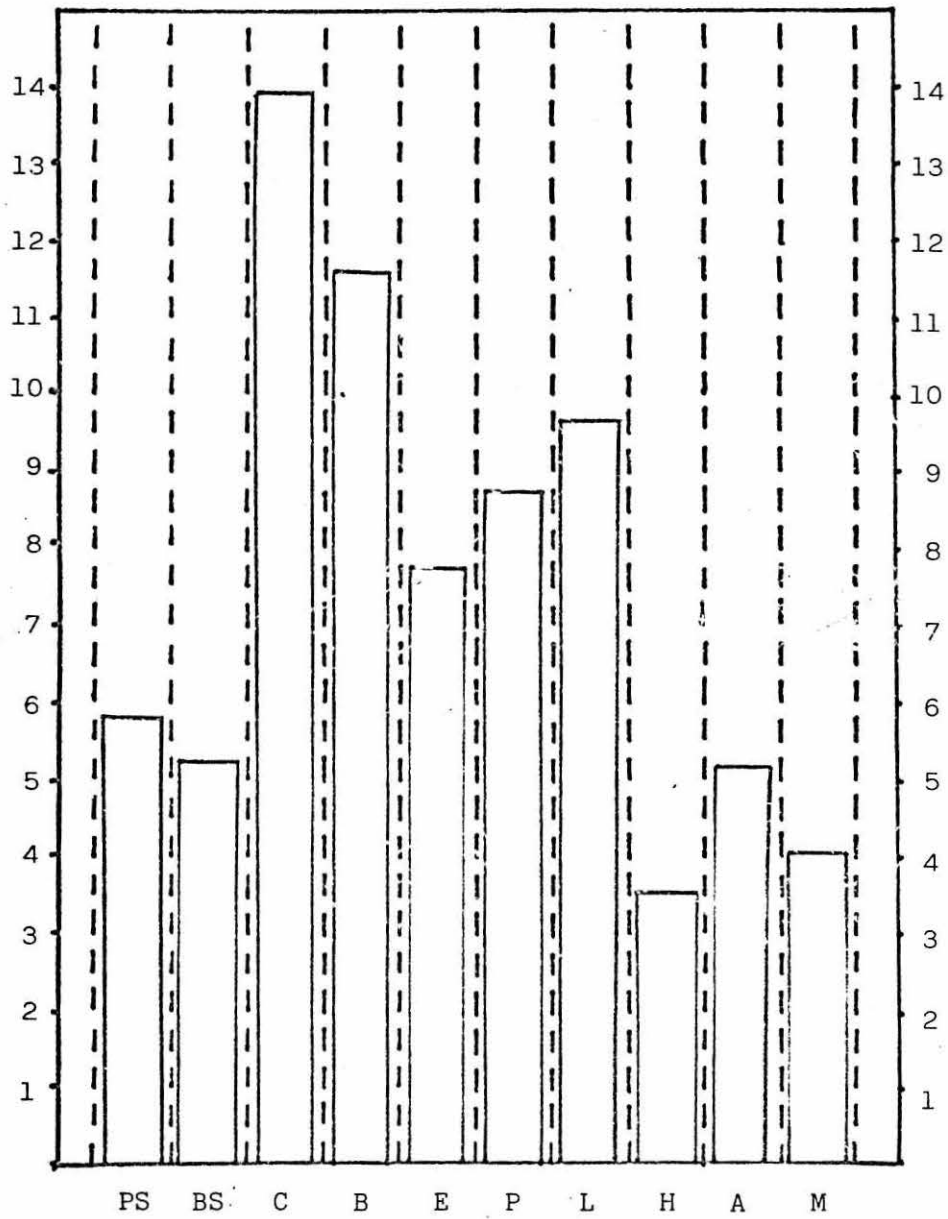


Table 2.4.(5): 'Matching to Self' Scores, Foresters

SUBJECTS	CW	SW	B	HM	D	J	F	A	CE	C	P
1	7	5	4	6	6	5	9	9	6	6	4
2	9	4	5	5	8	3	9	6	8	4	5
3	9	6	7	6	6	4	12	6	7	4	3
4	9	7	9	3	3	6	5	10	10	4	12
5	4	3	6	3	6	5	4	3	4	3	3
6	10	4	5	4	4	5	5	4	5	1	2
7	9	4	2	9	10	14	12	6	17	15	9
8	11	10	7	10	11	13	14	9	14	11	15
9	6	3	7	4	10	6	14	8	8	6	6
10	7	5	5	9	11	9	11	4	8	8	1
11	6	6	10	9	7	4	15	11	10	4	4
TOTALS	87	57	73	68	87	74	110	76	94	66	64
MEANS	7.91	5.18	6.64	6.18	7.45	6.72	10.00	6.91	8.54	6.00	5.80

TOTAL SAMPLE SIZE = 11

F = FORESTER

Figure 2.4.(c): Mean 'Matching to Self' Scores, Foresters

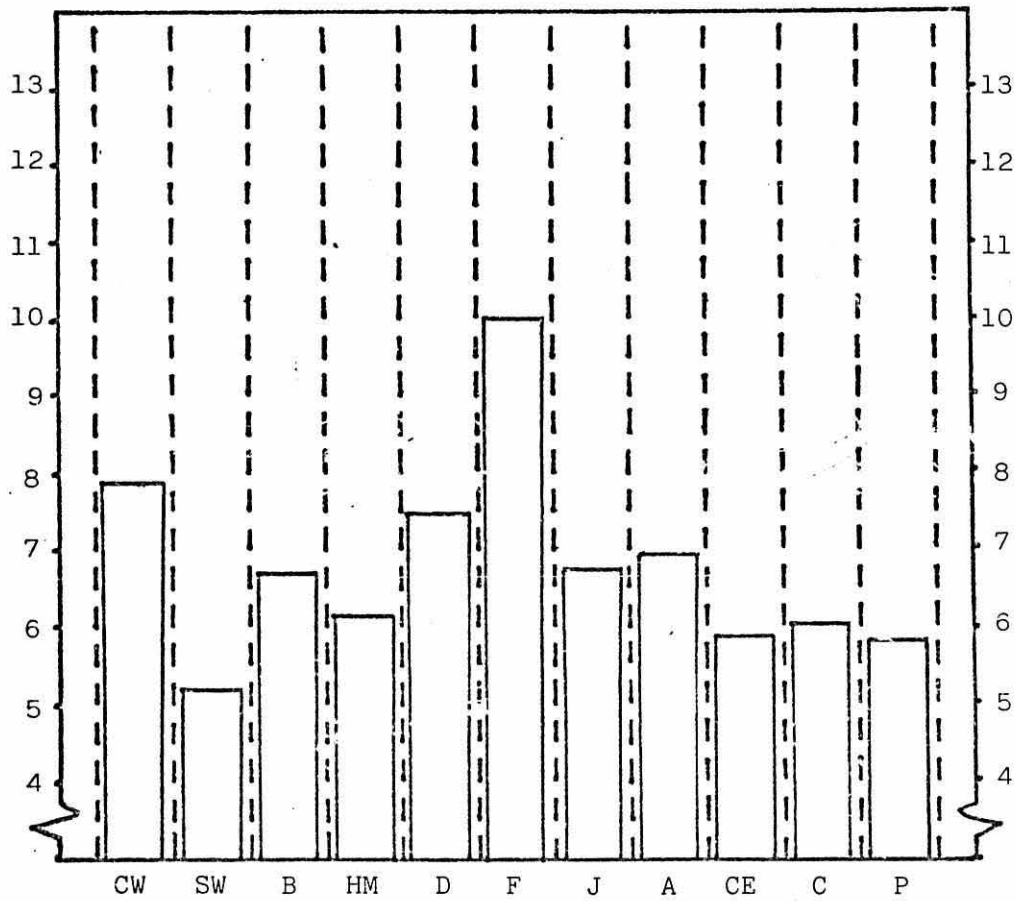


Table 2.4.(6): Interests Scores, Foresters

SUBJECTS	PS	BS	C	B	E	P	L	H	A	M
1	14	19	1	3	4	1	2	2	2	1
2	14	5	9	11	8	4	5	10	2	1
3	9	9	3	3	5	1	12	11	14	16
4	3	7	3	14	6	5	14	14	14	7
5	8	13	0	2	9	2	15	8	11	7
6	12	16	12	18	4	1	8	0	1	0
7	7	20	0	7	9	8	15	12	12	7
8	7	10	2	6	11	7	16	4	15	7
9	13	19	3	11	12	8	11	5	13	2
10	7	13	8	18	12	9	13	6	9	5
11	8	16	0	0	6	0	2	4	5	12
TOTALS	102	147	32	93	86	46	114	76	98	65
MEANS	9.27	13.36	2.91	8.45	7.82	4.18	10.36	6.91	8.91	5.91

TOTAL SAMPLE SIZE = 11

Figure 2.4.(d): Mean Interests Scores, Foresters

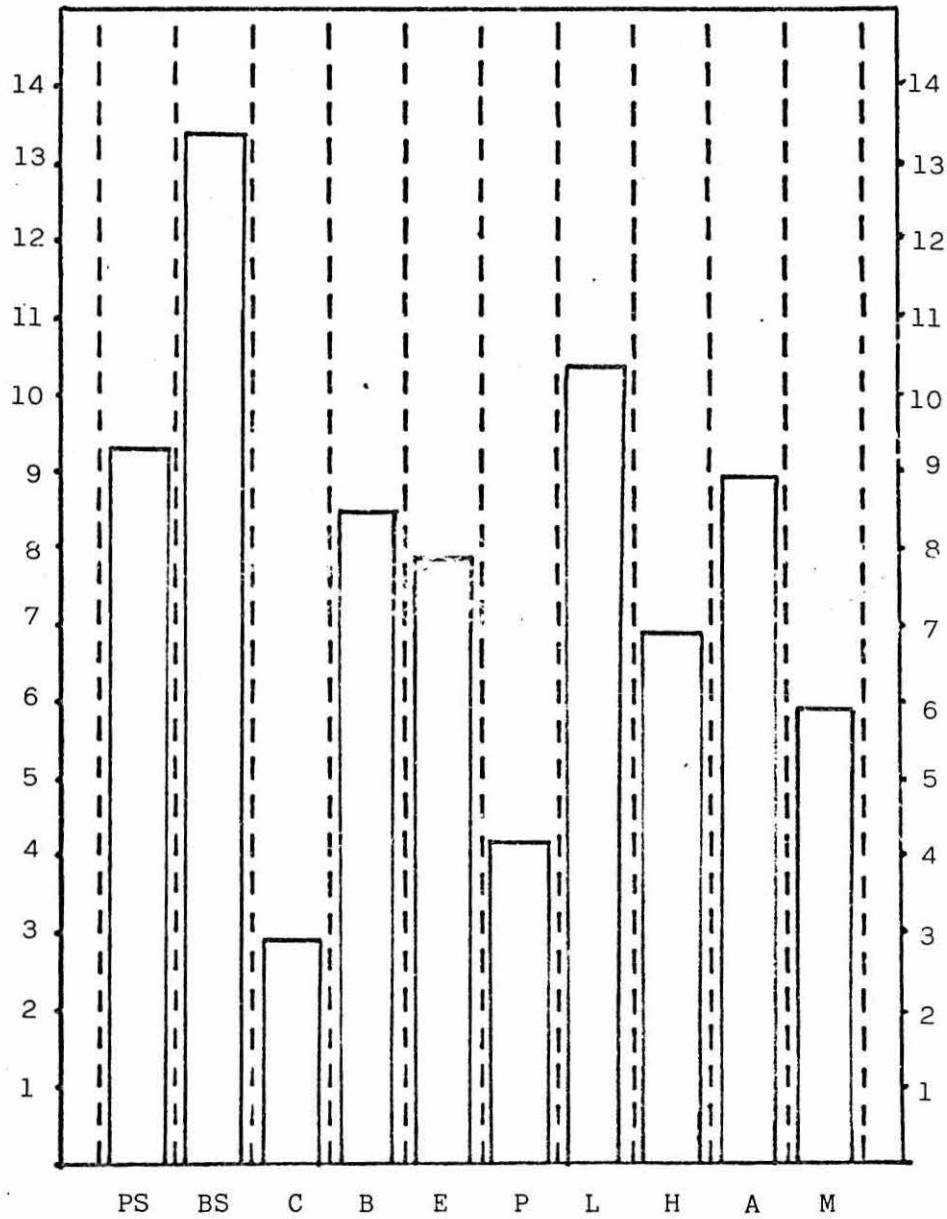




Table 2.4.(7): 'Matching to Self' Scores, Teachers

SUBJECTS	CW	SW	B	HM	D	J	T	A	CE	C	P
1	9	11	11	10	12	10	12	14	14	11	8
2	10	11	8	7	8	14	14	10	10	11	6
3	9	9	12	11	10	11	12	9	9	11	10
4	7	4	5	2	5	1	9	4	3	4	5
5	12	9	6	0	0	7	13	3	2	1	1
6	13	12	11	15	14	12	17	13	14	16	15
7	6	5	1	3	3	5	10	1	1	1	1
8	10	10	10	7	11	10	12	9	7	5	9
9	5	1	2	6	6	4	5	1	2	1	5
10	9	8	3	5	11	4	13	9	1	11	5
11	4	3	3	3	6	3	6	2	5	3	4
12	12	14	10	6	14	13	14	6	9	14	13
13	8	11	3	5	4	7	12	5	4	9	11
TOTALS	114	108	82	86	104	101	149	86	76	81	83
MEANS	8.77	8.31	6.31	6.62	8.00	7.77	11.46	6.62	5.85	6.23	7.15

TOTAL SAMPLE SIZE = 13

T = TEACHER

Figure 2.4.(e): Mean 'Matching to Self' Scores, Teachers

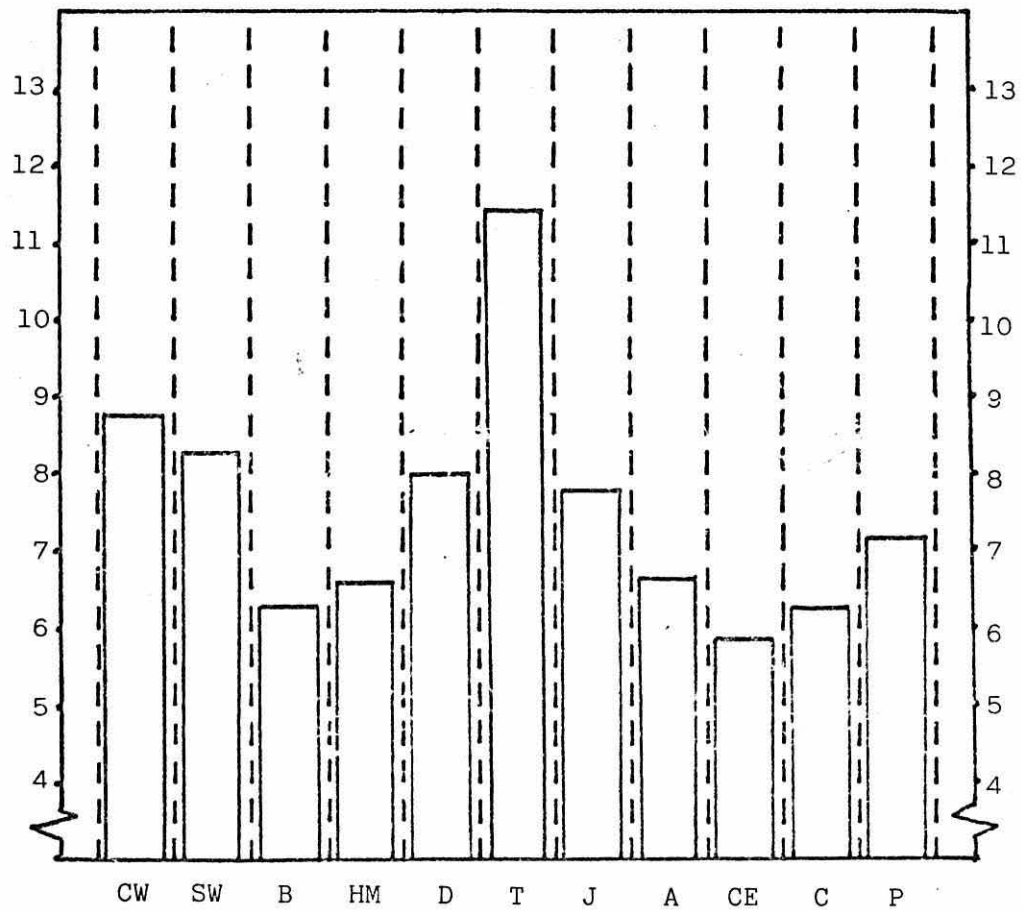


Table 2.4. (8): Interests Scores, Teachers

SUBJECTS	PS	BS	C	B	E	P	L	H	A	M
1	8	18	1	0	2	1	13	7	11	9
2	8	14	5	6	8	7	16	10	10	16
3	4	4	0	8	16	6	12	10	12	3
4	10	10	7	7	6	10	16	7	12	11
5	0	0	0	0	5	10	15	14	9	20
6	10	9	9	14	17	14	11	9	18	16
7	1	7	1	2	8	4	13	18	14	8
8	5	13	0	0	10	9	19	17	16	13
9	7	20	14	4	9	3	8	17	3	8
10	8	5	0	1	5	9	14	14	11	19
11	17	18	2	6	5	11	12	1	11	12
12	2	8	0	1	2	8	15	6	20	10
13	0	7	1	9	10	14	19	7	14	9
TOTALS	80	134	40	58	103	106	183	137	161	154
MEANS	6.15	10.31	3.08	4.46	7.92	8.15	14.08	10.54	12.38	11.85

TOTAL SAMPLE SIZE = 13

Figure 2.4.(f): Mean Interests Scores, Teachers

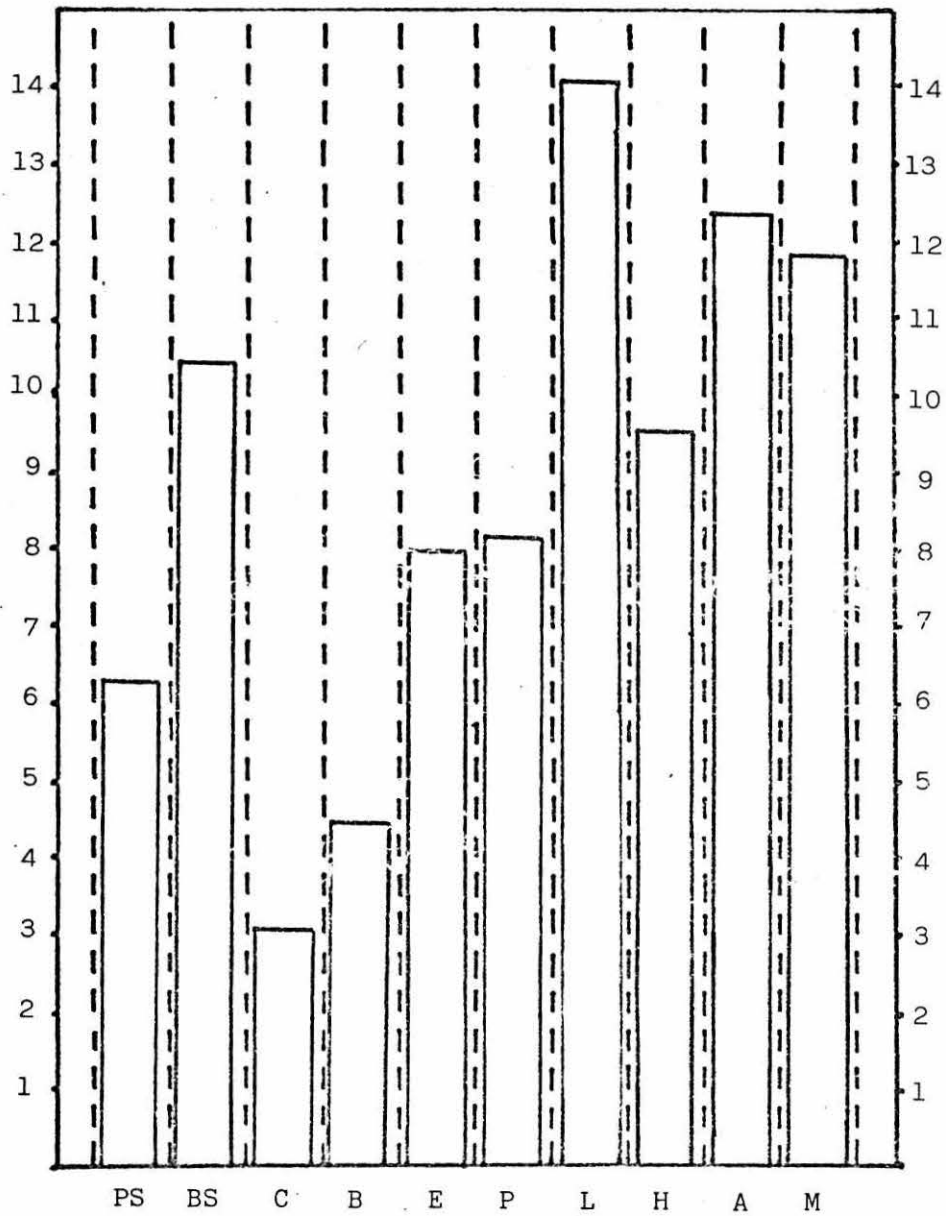


Table 2.4. (9): 'Matching to Self' Scores, Nurses

SUBJECT	CW	SW	B	HM	D	J	N	A	CE	C	P
PA	13	10	7	8	10	10	15	10	12	10	7
PB	11	9	9	8	4	8	14	10	9	11	9
PC	4	2	5	4	10	4	9	5	4	4	5
PD	14	7	11	15	13	6	16	6	9	10	8
PE	17	15	12	16	17	8	18	15	15	14	15
1A	13	12	7	6	10	9	13	9	11	10	7
1B	13	12	11	13	12	12	15	8	13	13	6
1C	8	10	4	8	2	9	13	10	9	9	6
1D	13	13	13	10	10	6	11	14	13	10	6
1E	13	6	10	8	8	9	15	9	8	8	8
3A	12	7	11	6	17	15	18	9	11	10	7
3B	3	7	2	5	3	5	6	3	4	5	4
3C	11	11	12	9	11	10	10	10	12	9	9
3D	12	7	10	11	10	7	17	7	6	7	4
3E	11	8	10	10	14	6	13	4	5	7	5
TOTALS	168	146	134	137	161	124	203	127	141	137	116
MEANS	11.20	9.73	8.93	9.13	10.73	8.27	13.53	8.47	9.73	9.13	7.73

Sub-Table 2.4. (9a): Mean 'Matching to Self' Scores for Each Year Group

	N	CW	SW	B	HM	D	J	N	A	CE	C	P	N	
PRELIMINARY COURSE	5	11.8	10.6	8.8	10.2	10.8	7.2	14.4	9.2	9.8	9.8	8.8	5	PRELIMINARY COURSE
FIRST YEAR	5	12.0	10.6	9.0	9.0	10.4	9.0	13.4	10.0	10.8	10.0	8.6	5	FIRST YEAR
THIRD YEAR	5	9.8	8.0	9.0	8.2	11.0	8.6	12.8	6.2	7.6	7.6	5.8	5	THIRD YEAR

TOTAL SAMPLE SIZE = 15

N = NURSE

Figure 2.4.(g): Mean 'Matching to Self' Scores, Nurses

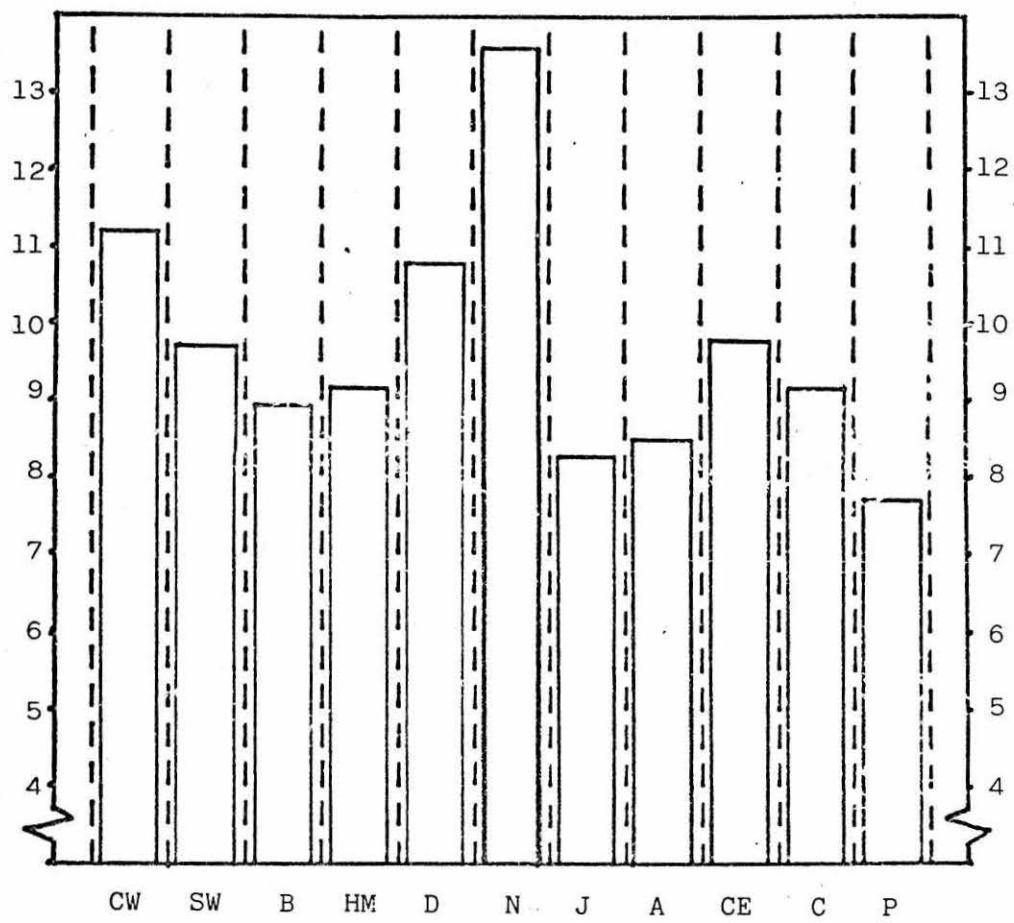


Table 2.4.(10): Interests Scores, Nurses

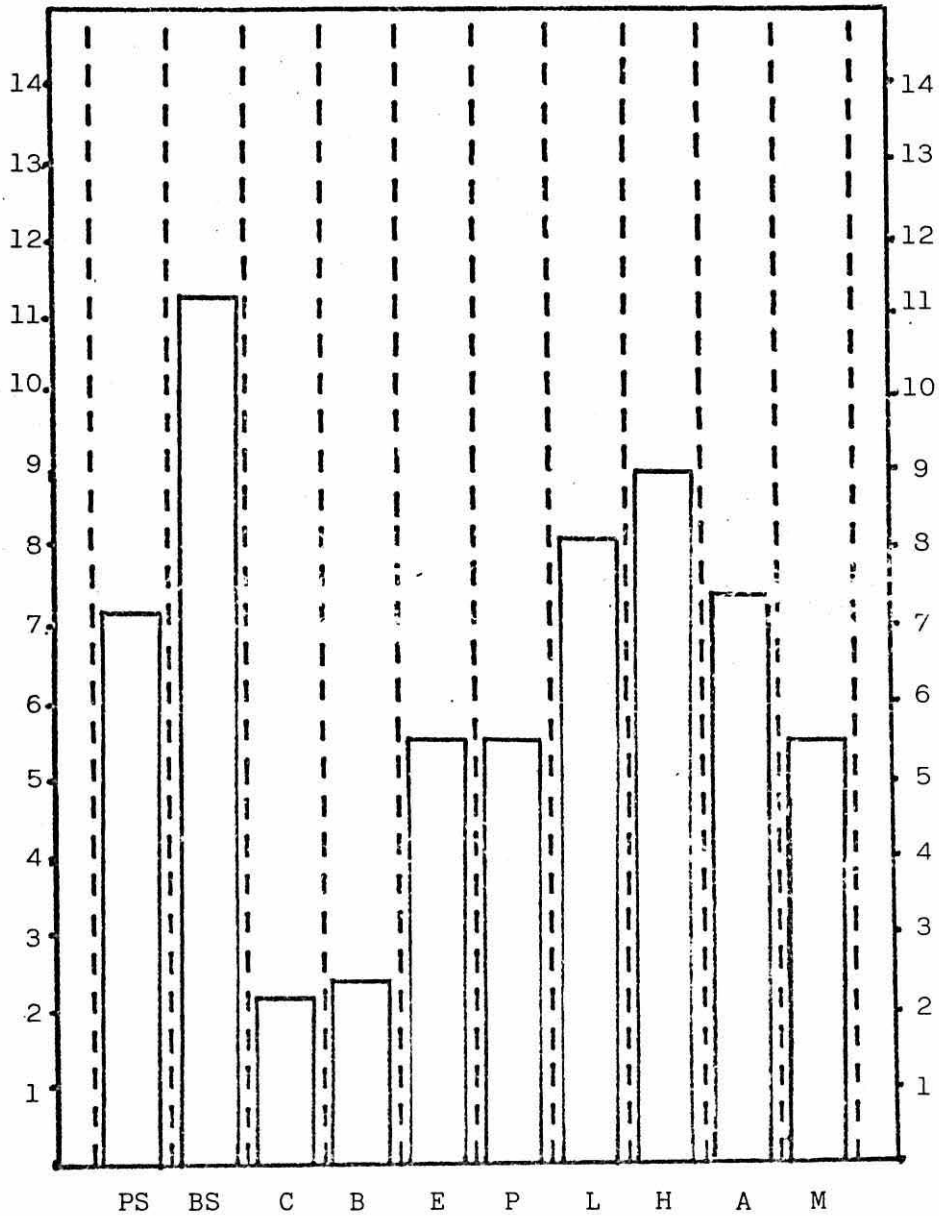
SUBJECT	PS	BS	C	B	E	P	L	H	A	M
PA	9	14	8	7	10	9	11	12	17	3
PB	2	14	1	6	6	5	4	13	6	5
PC	11	13	0	0	5	7	11	12	9	5
PD	7	12	3	5	10	0	2	9	4	12
PE	2	9	0	0	3	7	8	7	3	5
1A	13	19	0	0	6	2	5	11	1	0
1B	7	17	0	1	9	3	8	14	6	17
1C	4	8	1	3	5	6	13	13	16	9
1D	10	11	0	4	5	2	11	2	12	4
1E	7	12	6	2	5	5	10	10	1	0
3A	9	20	0	0	9	10	10	12	16	0
3B	3	7	0	0	0	3	1	2	0	20
3C	10	9	0	4	5	10	12	5	18	0
3D	0	1	13	2	8	11	14	12	2	3
3E	13	3	0	0	2	3	2	1	0	0
TOTALS	107	169	32	36	83	83	122	135	111	83
MEANS	7.13	11.27	2.13	2.40	5.53	5.53	8.13	9.00	7.40	5.53

Sub-Table 2.4.(10a): Mean Interest Scores for Each Year Group

	N	PS	BS	C	B	E	P	L	H	A	M	N	
PRELIMINARY COURSE	5	5.8	12.4	2.4	3.4	6.8	5.6	7.2	10.6	7.8	8.0	5	PRELIMINARY COURSE
FIRST YEAR	5	8.2	13.4	1.4	2.0	5.8	3.6	9.6	10.0	7.2	6.0	5	FIRST YEAR
THIRD YEAR	5	7.0	7.6	2.6	1.2	3.8	7.4	7.8	6.8	7.6	4.6	5	THIRD YEAR

TOTAL SAMPLE SIZE = 15

Figure 2.4.(h): Mean Interests Scores, Nurses





scores for each of the four courses. Tables 2.4.(4), 2.4.(6), 2.4.(8) and 2.4.(10); and Figures 2.4.(b), 2.4.(d), 2.4.(f) and 2.4.(h) present the corresponding data derived from the Thurstone Interests Schedule.

Table 2.4.(11) presents Spearman Rank Correlation Co-efficients computed between mean matching-to-self scores and interests scores for each of the four categories of participants. Only one of these positive correlations is statistically significant at the 0.05 level (one-tail test) and, indeed, a co-efficient of 0.00 is recorded for accountancy students!

Table 2.4.(12) attempts to express the degree of interrelationship between the four different course groups in terms of their matching-to-self and interests scores. Sub-Table 2.4.(12a) presents Spearman Rank Correlation Co-efficients computed between each of the different course groups. Only one positive correlation is statistically significant at the 0.05 level (one-tail test); this is between accountants and foresters.

Sub-Table 2.4.(12b) presents corresponding data derived from the interests schedule. Statistically significant positive correlations emerge between nurses and foresters and also between nurses and teachers. A negative correlation of -0.62 is recorded between nurses and accountants, but this has not been accorded statistical significance as a one-tail criterion was employed; the a priori assumption being that positive correlations would be the most likely.

Table 2.4.(11): Spearman Rank Correlation Co-efficients between  
'Matching to Self' and Interests Data

ACCOUNTANTS	0.00	N/S
FORESTERS	+0.39	N/S
TEACHERS	+0.15	N/S
NURSES	+0.57	*

\* = Significant at the 0.05 level (single-tail test)

N/S = Not Significant

Table 2.2.(12): Inter-Group Rank Correlations

Sub-Table 2.4. (12a) Matching to Self Scores

	NURSES	TEACHERS	FORESTERS
ACCOUNTANTS	+0.42	+0.04	+0.59*
FORESTERS	+0.44	-0.06	
TEACHERS	+0.33		

Sub-Table 2.4.(12b): Interests Scores

	NURSES	TEACHERS	FORESTERS
ACCOUNTANTS	-0.62**	-0.50	-0.13
FORESTERS	+0.64*	+0.54	
TEACHERS	+0.61*		

\* = Association significant at the 0.05 level (one-tail test)

\*\* = Given a 'one-tailed' criterion this negative correlation cannot be accorded statistical significance as an a priori grounds one would anticipate a positive association

## 2.5 Discussion of Results

The data presented in Table 2.4.(1), and illustrated in Figures 2.4.(a), 2.4.(c), 2.4.(e) and 2.4.(g) present clear evidence in favour of the first hypothesis under investigation in this study - namely that each group of 'students' would exhibit a higher level of self-incorporation of their 'chosen' occupation; that is, for the occupation their course of study was preparing them for, than for any of the other occupational role titles presented through the repertory grid procedure. As such, this study provides definite support for the Starishevsky and Matlin 'translation model' of self concept /occupational concept matching, and thereby for Super's more general ideas about the significance of self concepts within the process of career development.

In this context the study also provides a replication of the results reported by Knasel (1976) and shows that the same general findings emerge from a wider range of occupationally relevant courses than that originally investigated. It should be noticed that in fact the support for this hypothesis is more clear-cut in this study than in the earlier one. In the latter case, although the difference between the level of self-incorporation for the chosen occupation of teacher was statistically significant (at the 0.05 level) for the sample as a whole this was not the case for the sub-sample of 13 third-year subjects - where the difference, although apparent, was too slight to be accorded such significance. Although there can be no evidence to substantiate this view, it is tempting to ascribe the more clear-cut nature of the findings of the present study to the refinements made to the grid procedure employed.

It must be emphasised that whilst these data show that there is a significant degree of congruence between the perception of self and the perception of the individual's chosen occupation they do not, of themselves, point to any specific details of the role which self/other perception may play in the actual process of occupational choice. We cannot, from these data, infer that self/other perception plays any 'causal' role in the process of selecting an occupation - or, to relate the point more specifically to the present study, to the process of selecting an 'occupationally relevant' course. The argument being used here is not a Skinnerian-style one along the lines that a phenomenological process such as self/other or self/occupational perception cannot be assigned causal significance but on the contrary must be regarded as 'epiphenomena'. Rather, the view expressed here is that the present study presents us with only a 'snap-short' of an individual's self/occupational perception, after a particular occupation decision - that of enrolling in a course of study which will greatly enhance the individual's chances of entering a particular profession, and at the same time perhaps tend to 'close-down' other career options - has been made. We know nothing of the individual's perception of themselves in relation to other people or to occupational role titles before the choice was made. It should be recognised that a number of different kinds of change may have occurred after enrollment in the chosen course which might <sup>a</sup> effect self/occupation matching. In particular, the individual might have 'altered' his or her perception of self taking into account experience of beginning to play the chosen occupational role, - that is by beginning to 'become' accountants, foresters, teachers or nurses - in such a way as to increase measured self/occupational congruence. Conversely, the individual's view of the chosen occupational

role may have altered in such a way as to incorporate his or her own self-perceived personal characteristics. Indeed, both of these processes could conceivably have been at play in a complementary way, producing a degree of self/occupational matching which may not have been apparent when the original decision to enrol in the course was made.

The limitations inherent in such cross-sectional studies in this area have been noted by Kidd (1981). To truly evaluate the significance of self-other perception in the process of occupational choice it would be necessary to carry-out a much larger scale longitudinal study which could allow comparisons to be made between levels of self-incorporation before and after significant career decisions had been implemented. Such a study would have been outside the scope of the present thesis, but it could very well be argued that this and the earlier study by the present author have pointed to the potential utility of the present methodology employing the Self-Identification Form of the Rep. Test in any such study.

From the viewpoint of career development theory, probably the most interesting findings to emerge from this study are to be found in Table 2.4.(11) which presents Spearman Rank Correlation Coefficients between group mean matching-to-self scores for the different occupations presented in the repertory grid and for the ten occupational categories of the Thurstone Interests Schedule. The second main hypothesis under test in this study was that there would be significant positive correlations between these two sets of data - and indeed there was strong support for the equivalent hypothesis in the earlier study using teacher training students. Table 2.4.(11)

reveals that this was not the case with three of the four groups who took part in this study. Only mild positive rank correlations exist between the two sets of data for foresters and teachers, and there is no evidence of any association whatsoever for accountants. An anticipated significant positive correlation only emerges with the sample of student nurses.

The finding is therefore that for three of the four groups of participants in the study there is a clear degree of self-incorporation apparent for the individuals' chosen occupation - but the same tendency does not emerge when the level of self-incorporation is matched against expressed 'interests' in a range of occupations which the individuals in question have not chosen to enter. It should be noticed that the Thurstone Interests Schedule in fact requires the individual to express preferences between pairs of occupational roles. It might therefore be reasonable to refer to the Schedule as a preference inventory.

We could therefore express the findings of this study as showing, that for accountants, foresters and teachers, a positive relationship exists between the individual's self-perception and his or her perceptions of an occupational role for which he or she has chosen to enter a period of preparation, but that no such relationship obtains between the level of self-incorporation and expressed preferences between a range of other occupations.

The distinction between occupational choices and occupational preferences is one which has recently received some attention from writers in this area (e.g. Kidd 1981; Super 1981). In particular,

Super has suggested that the vast majority of studies which have focussed on Holland's approach to career development have in fact confused the two issues, and have used data on comparative career preferences to support propositions about the process of career choice (Super 1981, pp 20-21). The distinction to be drawn in this study is a particularly clear one - as explained earlier in this section the participants in this study had, in an important sense, already made a career choice. The data in Table 2.4.(11) therefore relate to expressed preferences between occupations which the individuals have, for the time being at least, chosen not to enter. The present study could hence be said to have illustrated a significant difference between occupational choices and occupational preferences - suggesting that for certain people self/occupational matching does not necessarily relate to preferences between non-chosen occupations. This interpretation of these findings therefore supports the distinction drawn by Super in his criticisms of the body of research derived from his principal rival's theoretical position. At the same time, it must also be held to point to a limitation to the significance of self-other perception in the process of career development as Super himself has described it.

A methodological point could be raised against the above interpretation of the findings of this study. It could be argued that the low (or, in the case of accountants, non-existent) correlations reported in this study, rather than allowing a distinction to be drawn between choice and preference, are in fact a product of selecting only one occupation for the repertory grid to stand for a range of occupational roles employed in each category of the X Thurstone Interests Schedule. Against this view may be <sup>CITED</sup> sighted the



very high 'item validities' reported by Thurstone (see p. 63); and the fact that significant positive correlations do occur for the sample of student nurses who took part in this study, and the trainee teachers involved in the earlier study. It seems extremely unlikely that the clear distinction between accountants and nurses revealed in Table 2.4.(11) could be explained away in methodological terms.

Having said this, a viable explanation of the difference does not come readily to hand on the basis of this study. Taken together this and the earlier study have investigated individuals involved in five different occupationally relevant courses. For two of these groups of participants there appears to be a relationship between 'level of self-incorporation' and expressed preferences which is not replicated with the remaining three courses. The only distinguishing factor between these two sets of courses which occurs to this author is that the three courses for which there are no correlations are university-based courses, whilst the other two are not. It is not clear, however, whether any significance can be read into this distinction.

Finally, Table 2.4.(11) presents inter-group correlations for matching-to-self scores and for interests scores. The main importance of these two sub-tables is to demonstrate that there are between group trends apparent in both sets of data. Of these, the clearest trend to emerge is that accountants interests scores tend to be quite different from those of any of the other groups included in this study.

### Chapter Three: 'Failure at School'

#### 3.1. Introduction

This chapter presents a very small scale study of a series of children who could be said to be experiencing some degree of difficulty with their school work. More specifically, it is concerned with the possibility that such children might adopt particular strategies in their school work which are designed to minimise their chances of 'failure' and to maximise their chances of 'success'; and that these strategies might be reflected in the children's performance on a simple modification of the Self-Identification Form of the Rep. Test

A number of writers - perhaps most notably Holt (e.g. 1965; 1967) and Skinner (1958) - have suggested that, in many instances, conventional methods of education place greater emphasis upon children learning through their mistakes or failures than through their successes; in Skinner's terms they rely more on strategies based upon 'aversive control' than upon the use of positive reinforcement. Both Holt and Skinner have argued that such an emphasis can have a debilitating effect upon a child's degree of genuine learning. In particular, Holt has argued that school children frequently come to adopt strategies which are designed to limit their exposure to possible instances of failure; that, in effect, they will 'play safe' - by for example avoiding situations where they are required to provide a new answer to a particular problem. The present study investigates whether children who are experiencing particular difficulties in their school work will adopt such a

'minimax' strategy in completing a variant of the Rep. Test which has been designed to provide scope for such a 'conservative' style of performance.

Whilst such writers as Holt and Skinner have suggested that an emphasis on 'aversive control' is characteristic of most instances of 'conventional' education\*, and hence imply that such a style of performance might be characteristic of most school children, this study rests upon the hypothesis that the adoption of such a strategy will be more marked with a series of children who are experiencing particular difficulties in their school work than will be the case with a 'control' series of children who are held to be doing relatively well at school. The children who formed this first series were recruited from amongst those who had been referred to the Bangor Dyslexia Unit (based at the Department of Psychology, UCNW) and who, on testing, had been characterised as being 'dyslexic'; that is, as exhibiting a marked discrepancy between their ability on verbal tasks and in other areas of intellectual performance.

Children who met these criteria were selected for this purpose for two reasons. Firstly, 'dyslexics' represent an almost ideal criterion group in that these are children whose performance in a particular area of their school work - spelling and/or reading - is clearly giving parents or teachers some cause for concern but who may otherwise show marked ability in other academic tasks. Any

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\* In fairness to the British education system, it should perhaps be noted that both of these writers were concerned essentially with schooling in the United States.

discrepancy in their performance on the Rep. Test as compared to a control series may not therefore be easily attributed to an overall difference in the two series' relative academic ability. Secondly, the series was readily available to the researcher, as the Bangor Dyslexia Unit represents one of the principal centres for the study of this alleged group of learning disabilities.

### 3.1.1. The Concept of Dyslexia

The notion of 'dyslexia' - (that is, of a group of specific learning disabilities centring around a relative inability and lack of attainment in spelling and reading when compared with other areas of intellectual performance) - is one which has aroused considerable interest and controversy. Whilst psychologists have disagreed amongst themselves over the validity of 'dyslexia' as a scientific concept, the term has also engendered considerable debate between certain psychologists and members of the teaching profession over its practical utility in education. The idea was, however, originally introduced by neurologists and was initially conceived of as a concomitant to the well-known acquired disorder of 'Aphasia' or speech-loss.

In his historical review of the concept, Critchley (1970) describes how neurologists recognised early on that 'Aphasia' or speech-loss could be accompanied by a marked reduction in the patient's capacity to attach meanings to printed or written verbal symbols. It should be emphasised that Aphasia is an acquired disability brought about by disease or trauma to the brain. Critchley shows that by the second half of the Nineteenth Century descriptions of Aphasia accompanied by an inability to comprehend the printed or written

word had become relatively commonplace, and attributes the introduction of the term 'dyslexia' to describe this acquired disability to Berlin (1887). Critchley himself favours the term 'alexia' to describe this condition, reserving the term dyslexia to refer to an inborn or developmental disability. He describes 'alexia' in the following terms:

"Hence it is more correct to regard alexia as that variant of aphasia where the most conspicuous feature consists in an extreme difficulty in the interpretation of verbal or literal symbols by way of visual channels."

Critchley (1970 p.3)

At around the turn of the Nineteenth Century a number of British medical practitioners, notably Hinshelwood (e.g. 1895; 1896); Pringle-Morton (1896) and Kerr (1896), began to introduce the notion of a similar relative inability in terms of spelling and reading which affected school children and which was developmental rather than traumatic in origin. Of these authors, Hinshelwood is generally regarded as the most significant (see Critchley 1970; Miles and Miles 1975). In his two monographs on the subject (Hinshelwood 1900; 1917) he referred to this condition as 'word blindness'. He suggested that children exhibiting this condition were 'blind' to the 'look' of the words which they had to read and write, as if printed or written words were merely meaningless marks on paper rather than semantic symbols. Emphasising the neurological basis of the notion, Hinshelwood argued that such 'congenital word blindness' represented a blockage in the development of the nervous system. This explanation has been one which has become known as the 'maturational lag hypothesis' (see Critchley 1970; Naidoo 1972) and has received considerable attention

from later writers (e.g. Birch 1962) and suggests that dyslexia has its origins in a delay in the completion of those areas of the child's neural net which are involved in verbal and letter recognition and production.

An essentially similar, but rather more detailed, neurological thesis was advanced by Orton (1937). He argued that many children who exhibited specific reading and spelling difficulties seemed to find it easier to write from right to left rather than in the more conventional direction, and that sometimes letters or even whole words were written the wrong way round, a tendency which occasionally culminated in examples of frank mirror-reading or mirror-writing. Orton also stated that in his experience an unusually high proportion of these children were left-handed or ambidextrous.

He proposed that each of these phenomena evidenced an underlying lack of a clear dominance or specialisation in the functions of the individual's cerebral hemispheres. Orton (1938) summarised his view in this way:

"The view presented here (is) that many of the delays and defects in development of language function may arise from a deviation in the process of establishing unilateral brain superiority in individual areas....."

Orton (1938)

He introduced the word 'strephosymbolia'\* to describe this general

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\* As Critchley (ibid) points out, an almost bewildering array of words and phrases have been used to describe the phenomena referred to here under the heading of 'dyslexia'. Amongst these he lists 'legasthenia'; 'word amblyopia'; 'typholexia'; 'analphabetia partialis'; 'bradylexia'; and 'script blindness'. Mercifully few of these assaults upon the English language have received any currency.

syndrome. Whilst this term has not gained favour in describing the group of learning disabilities as a whole, it is often used to refer to the kinds of mirror-writing and mirror-reading which he described (see e.g. Miles and Miles 1975; Newton and Thompson 1975).

The Orton Society, which operates in the United States, continues to co-ordinate studies into developmental defects in language and to organise appropriate facilities for teaching children suffering from these defects. Orton's cerebral dominance explanation of the origins of dyslexia has been very influential, and indeed continues to influence research in this area (see, for instance, Newton and Thompson 1975).

We have seen that the concept of 'dyslexia' was originally propounded by neurologists, and it is hence not surprising that the original theoretical conceptions of the disorder emphasised the possible significance of neurological factors. More recently however, psychologists have become heavily involved in research work in this area, and British writers, such as Miles (1976) or Naidoo (1972), whilst by no means denigrating the pioneering work of the neurologists, nor totally dismissing the possible importance of neurological factors, have placed a greater emphasis upon the performance aspects of dyslexia.

Such work has generally taken two complementary directions. Firstly, attention has focussed upon the use of broadly 'psychometric' methods in diagnosing particular children as being 'dyslexic'. In such work at the Bangor Dyslexia Unit, the Wechsler

Intelligence Scale for Children has proven particularly useful, especially when attention is paid to sub-test scores, and when the scale is used in conjunction with non-verbal measures of intellectual ability such as Raven's Progressive Matrices. The second principal line of development has been psychological research which has attempted to provide laboratory demonstrations of differences in cognitive functioning between dyslexic and non-dyslexic subjects (see for example, Miles and Wheeler 1977). This work is seen as having utility in establishing the scientific validity of the concept as well as in possibly pointing towards areas to which teachers of dyslexic children should pay especial attention. For the purpose of the present study, however, it is necessary to give a summary of the clinical picture of dyslexia which seems to have emerged over the years, rather than a detailed examination of such research work. The description which is given below follows that provided by Miles (1970).

The most significant diagnostic feature of the syndrome of dyslexia is that the child should exhibit a relative 'failure' in terms of his or her performance at spelling and/or reading which appears to represent a marked discrepancy with his or her performance in other areas of school work and which cannot therefore be attributed to a general lack of ability. Similarly, the child's poor performance at spelling and/or reading should not be attributable to extraneous factors, such as persistent absence from school, for whatever cause, or to physical defects in the child's eyesight or hearing.



The particular kinds of errors which dyslexics make are held to have some diagnostic significance. The spelling of dyslexics is frequently held to be characteristically 'odd' or 'bizarre'. The kind of reversal errors which Orton pointed to are held to be especially common amongst dyslexics. As with earlier writers, Miles (1970) points out that many young children will, for example, misread a letter 'b' for a 'd' or a 'p' for a 'q'; or make the corresponding error in writing them down. He argues that such difficulties are likely to persist with the dyslexic child, who may still make such 'strephosymbolic' errors when aged nine or ten, or even older - long after most children are held to have 'grown out of them'. Additionally, dyslexics are said to frequently produce attempts at spelling words which make little or no sense to anybody else. Hence, Miles gives the example of a boy who sepl't the word 'FIVE' as 'FEQUP'; and more bizarrely, another who, in the same sentence, spelt the word 'SUBSTANCE' as 'SEPEDNS' and 'SOPSTS'. The argument here is that such errors fall outside of the range of those produced by 'normal' spellers including, presumably, bad spellers who would not be characterised as dyslexic; and are hence illustrative of a special form of disability.

One should notice that, whilst many dyslexics experience difficulties with both spelling and reading, as Naidoo's (1972) research has emphasised many children who exhibit characteristically 'dyslexic' spelling are quite competent at reading tasks. Conventionally, therefore, comparative competence at reading as opposed to spelling would not preclude a diagnosis of dyslexia.

Miles (*ibid*) describes a number of secondary symptoms or 'additional signs' which may be present in some cases of dyslexia. There appears to be some evidence that dyslexics are unusually prone to 'clumsiness' and, in line with Orton's ideas, there is also some suggestion that they are especially likely to be either left-handed, or to show no clear dominance - in the sense not simply of being ambidextrous but also, in some cases, of showing 'mixed preference', for example left-handedness coupled with right-footedness. Miles also suggests that dyslexics are especially poor at spotting rhymes:

"In addition I have known some dyslexic children who completely failed at giving rhymes; I do not mean merely that they could not, for example, say cat\* if asked to give a rhyme for bat\*, but that they did not seem to have the least idea what a rhyme was."

Miles (1970) p.8

Other such 'additional signs' might include poor handwriting; difficulties in copying complex abstract shapes and designs; and difficulties in orally repeating longer words such as 'discrimination' or 'preliminary' with the syllables in the correct order.

There is considerable evidence that substantially more boys than girls are diagnosed as dyslexic. On the basis of 19 studies conducted between 1927 and 1968, Critchley (1970) states that 'we can therefore assume that about four males to one female may be accepted as a reasonable figure.' Critchley interprets such findings as illustrating a genetic basis to dyslexia, and on the same theme also points to a considerable degree of evidence that dyslexia 'may run in families'.

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\* Italicised in original

Miles (1970) has summarised the 'clinical' picture of dyslexia in the following way:

"To sum up: a dyslexic child is one who has special difficulty with spelling and usually with reading also, his performance falling a long way short of his intellectual level. Often he continues to reverse\* letters and words at an age when other children no longer do so, and his spelling is frequently bizarre (.....) Whenever you come across a child who fits this general picture, you would be right to label him as a dyslexic, and to realise that his problems merit special attention."

Miles (ibid) p.9

Within this context of the present study we should notice that the concept of dyslexia does not refer simply to any child who finds spelling or reading difficult, but rather to those where there is a clear discrepancy between their performance at literacy tasks and in other areas of intellectual performance. Those who write on the topic of dyslexia frequently imply that such children are, in other respects, of high academic ability. Indeed, one of the more tiresome aspects of books on this subject is their tendency to list very famous figures from history, such as William Shakespeare, Albert Einstein, Hans Christian Anderson and William James\*\* who, on the basis of very circumstantial and equivocal evidence, are suspected of having been dyslexic. At a more serious level, there are certainly

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\* Words underlined in this section are italicised in the original

\*\* It is interesting that psychologists should want to include one of their own 'founding fathers' in such company; perhaps the fact that dyslexia is said to run in families might have something to do with his brother Henry's idiosyncratic sentence constructions! To try to offset the balance, Thompson (cited by Critchley 1970) has suggested that Lee Harvey Oswald may have been a dyslexic.

many examples of children who have been diagnosed, on the basis of 'standardised' tests, as being dyslexic who have gone on to successfully complete GCE 'O' level and 'A' level courses and, indeed, have been very successful at University (see for instance Miles 1976). For the purposes of the present study, the important point to notice is that dyslexia is seen as representing an area of specific failure within the child's school work; such children are not held to be lacking in overall academic ability.

It would not be appropriate to close this very brief consideration of the concept of dyslexia without some reference to the high degree of controversy which surrounds it. In particular, those such as Miles and Newton who have advocated that dyslexia represents an area of special educational need - and that special attention should accordingly be paid to the teaching needs of dyslexic children - have often met with unusually fierce opposition from some members of the teaching profession. At an official level, many Local Education Authorities have made little or no specific provision for dyslexic children; and whilst many examination boards are willing to take into account evidence of dyslexia, there has been little or no formal recognition of the condition of developmental dyslexia by central government.

The controversy rages particularly fiercely at the popular level. For example, the 'Guardian' newspaper and the magazine 'New Society' have frequently carried articles both for and against granting wider recognition to this alleged disorder. In the experience of this author there does appear to be a strong 'folk-lore' amongst many school-teachers that dyslexia represents a case

of 'special pleading' on the part of the parents of pupils who either have been badly taught or who are not living-up to the wildly unrealistic expectations of their parents. Whilst it does seem to be the case that writers such as Miles and Newton do not appear to be particularly sensitive to the wider range of educational problems which confront practising teachers; teachers themselves seem to be 'blind' to the argument that it is not that dyslexics cannot be taught to spell but rather that they require special attention and help which is tailored to their own particularly idiosyncratic needs.

One of the arguments which is most frequently put forward against the proponents of dyslexia is that it is a peculiarly 'middle-class disease'. There are two aspects to this argument, which is of some relevance to the present study (as will become clear in Section 3.1.3.). One argument, which fails to convince this writer, is that the sub-scales of intelligence measures on which dyslexics are supposed to frequently score well are precisely those which are least 'culture-fair' - that is that 'working-class' children will be unlikely to show-up on testing as being dyslexic, because their scores on 'literacy-free' indices of intelligence are likely to be 'depressed' since they have not had access to a facilitative 'middle-class' environment - and hence will be less likely to show a significant discrepancy between their reading and spelling attainments and their ability at other intellectual tasks. Secondly and perhaps more plausibly, it is argued that, given an educational system where it is left largely to individual parental effort for a dyslexic child to gain some institutional recognition of his or her 'special disability', it will be middle-class parents who will be particularly likely to make the considerable effort

required to gain such recognition by, for instance, contacting organisations such as the Bangor Dyslexia Unit to obtain a diagnosis of dyslexia which will carry weight with an examination board. Such an argument does square well with overwhelming sociological evidence that middle-class parents are more likely to make effective use of a bureaucratic system, although it is hard to see how, of itself, it could justify withholding such recognition.

The present study is not intended as a contribution to this debate. Dyslexics are employed in this study to test a more wide-ranging hypothesis concerning the effects of 'negative' educational experiences. Indeed, as will become clearer in Section 3.1.3., it is argued that the assumptions made in the present study do not ultimately rest upon the scientific validity and educational utility of the concept of 'dyslexia'. Rather, the assumption which is made is that children who have been diagnosed by the Bangor Dyslexia Unit as being 'dyslexic' are likely to have experienced a degree of relative 'failure' in their school-work which cannot be attributed to a general lack of intellectual ability which could be likely to affect their performance on a simple modification of the Rep. Test. It is argued that the discussion of the concept of dyslexia which has been presented in this section, and particularly the conclusions drawn on page 106, go some way towards justifying this assumption.

### 3.1.2. 'How Children Fail'?

The principal hypothesis to be tested in this study is derived from the work of John Holt. His basically anecdotal books 'How Children Fail' (1965) and 'How Children Learn' (1967) are based upon

observations made whilst he was teaching in the US equivalent of a primary school. They are essentially concerned with the thesis that many aspects of conventional schooling in fact lead a majority of students to adopt strategies which, far from increasing their capacity to learn, instead are designed to minimise their exposure to instances of 'failure' - that is to situations where they can be seen to be 'wrong'.

In this sense Holt argues that most school children may be characterised as failures:

"But there is a more important sense in which almost all children fail: except for a handful, who may or may not be good students, they fail to develop more than a tiny part of the tremendous capacity for learning, understanding, and creating with which they were born and of which they made full use during the first two or three years of their lives.

Why do they fail?

They fail because they are afraid, bored and confused.

They are afraid, above all else, of failing, of disappointing or displeasing the many anxious adults around them, whose limitless hopes and expectations for them hang over their heads like a cloud."

Holt (1965) p.9

His argument is that in their school-work many children are not concerned with any desire to gain a better understanding of the world and a wider grasp of the knowledge which their teachers are seeking to give to them, but rather with the more mundane task of getting through their school-day with the minimum amount of effort and, more importantly, with avoiding the unpleasant consequences of producing the wrong answers to questions or problems which are put to them. He argues that, to this end, many pupils will try to avoid situations where they are required to give any answers which might prove to be 'wrong'. In Holt's view, it is not learning which

provides a challenge to school children, but rather the avoidance of failure:

"Children in school are like children at the doctor's. He can talk himself blue in the face about how much good his medicine is going to do them; all they think of is how much it will hurt or how bad it will taste. Given their own way they would have none of it.

(.....)

For children the central business of school is not learning, whatever this vague word means, it is getting these daily tasks done, or at least out of the way, with a minimum of effort and unpleasantness. Each task is an end in itself. The children don't care how they dispose of it. If they can get it out of the way by doing it, they will do it; if experience has taught them that this does not work very well, they will turn to other means, illegitimate means, that wholly defeat whatever purpose the task-giver may have had in mind."

Holt (ibid) p.37

He argues that this attitude is particularly common amongst those who have had particularly persistent experiences of such 'failures'; and that they will come to adopt strategies in their school work which will minimise their potential exposure to such instances of failure. He suggests that due to their repeated experiences of failure such children reach a point where they are no longer engaged in the same kinds of tasks in their school-work as their more successful peers:

"Until recently it had not occurred to me that poor students thought differently about their work than good students; I assumed they thought the same way, only less skilfully. Now it begins to look as if the expectation and fear of failure, if strong enough, may lead children to act and think in a special way, to adopt strategies different from those of more confident children."

Holt (ibid) p.38



Such strategies may be designed to avoid exposure to such instances of potential failure in the first place, or to avoid the production of a definite unequivocal answer which can clearly be seen, by teachers and peers, to be either right or wrong. Holt argues that many school children readily become experts in such strategies:

"Game theorists have a name for the strategy which maximises your chances of winning and minimises your losses if you should lose. They call it 'minimax'. Kids are experts at finding such strategies. They can always find ways to hedge, to cover their bets."

Holt (ibid) p.28

He provides numerous examples of children, in classes which he either taught himself or observed, who adopted such 'fail-safe' strategies. For example, he refers to one young girl who when required to give an oral answer to an arithmetic problem chose to whisper her answer, such that it was inaudible both to her teacher and to her class-mates. Similarly, the same girl was asked to place a weight on a beam in such a way that the beam would balance when allowed to pivot on a fulcrum some way from the centre. To 'hedge her bets', having placed the weight she then announced that she did not think that it would balance. When the beam was allowed to pivot, and it failed to balance, Holt reports that 'she almost seemed to feel vindicated'. As another example, amongst many, Holt refers to children playing the game 'Twenty Questions'. He reports the impression that, when playing the game in class, rather than seeking to discover the 'hidden thought', many children will be more concerned to find a question which does not look 'silly' and which is more or less bound to produce the answer 'yes' - he maintains that their experiences of schooling have not prepared them for the idea

that in such a game the answer 'no' can be every bit as informative - and in some circumstances distinctly more so - than the answer 'yes'. Holt's central argument is that such strategies, adopted by children who have had considerable experience of 'failure'\*, are intended, if possible, to avoid the necessity of providing an answer or a solution, and, in situations where such a necessity cannot be avoided, to provide a 'safe' answer which avoids the possibility of being wrong.

Holt summarises his view of the child who has failed at school in the following way:

"He cannot stand uncertainty or failure. To him, an unanswered question is not a challenge or an opportunity, but a threat. If he can't find the answer quickly, it must be given to him, and quickly; and he must have answers for everything. Such are the children of whom a second-grade teacher once said, "But my children like\*\*to have questions for which there is only one answer." They did; and by a mysterious coincidence so did she."  
Holt (ibid) p.164

The essentially anecdotal ideas of Holt find strong parallels in the allegedly more 'scientific', and certainly more formal and jargonised work of B.F. Skinner; and in particular in his writing on the 'technology of teaching' (e.g. Skinner 1954; 1958).

In these papers Skinner propounds the idea that most classroom situations rely more heavily upon techniques of aversive control than upon positive reinforcement. He argues that much teaching is based more upon the exposure of children to instances of failure -

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\* It should be stressed that in Holt's view most school-children would fall into this category.

\*\* Italicised in original.

where they receive punishment contingent upon producing inappropriate responses - than upon the structured presentation of positive reinforcement which may be used to shape appropriate learning. He argues that because teachers typically take a comparatively long time to mark written work the contingencies between 'appropriate' learning and reinforcement are frequently broken down, leaving a reliance upon aversive control in the class-room; which frequently results in school-children learning 'avoidance reactions' rather than the responses intended by the educator. It was this argument which led Skinner to introduce his notion of teaching-machines based upon linear programmes which ensure that the pupil receives regular 'positive reinforcement' in the form of confirmation that he or she has provided the 'right' answer.

Although it seems self-evident that Holt and Skinner approach education with quite different 'images of man', and although they go on to suggest radically different solutions, their ideas do seem to involve some close similarities. Holt's view of the overwhelming significance of 'failure' in the class-room and Skinner's emphasis upon the prevalence of 'techniques of aversive control' appear to be closely parallel; and it would not be difficult to reconcile the radical behaviourist's views on 'avoidance learning' with Holt's notion of 'strategies'.

### 3.1.3. 'To Repeat or Not to Repeat\*'

It is these strategies which form the primary focus of this study. The main hypothesis under consideration is that, when

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\* This phrase is taken from Shubsachs (1975).

confronted with a variation of the Rep. Test which allows them the unstated option of presenting a minimal number of 'novel' replies, children who may be assumed to have encountered repeated instances of 'failure' in their school work will be more likely to adopt this 'safe' strategy than will a second series of school children who have been selected as being reasonably 'successful'.

In this author's experience, one of the aspects of the repertory procedure which provides respondents with the most difficulty is the convention that they should not be allowed to repeat constructs which have been elicited by preceding triads to which they have already responded. Often individuals will state that a particular description which they have already used is also the most appropriate one to describe the three figures with which they are presently confronted. Shubsachs (1975) has suggested that this difficulty is particularly common when it is children who are required to complete the construct elicitation procedure, and suggests that it might sometimes be advisable to abandon this convention, and to allow children to repeat constructs which have already been elicited.

It was decided to allow participants in this study the unstated possibility of repeating constructs; the hypothesis being that children who had had experience of persistent failure would be more likely to adopt this strategy than would a comparable series of children who were relatively successful at school. The assumption is therefore made that the frequent repetition of constructs would represent an example of the kind of 'minimax' strategy to which Holt refers. Certainly a high degree of repetition would allow the child

to provide a minimum number of 'novel' answers and would allow him or her to complete the task posed by the Rep. Test with the minimum amount of effort.

Children who had been referred to the Bangor Dyslexia Unit were chosen as representing a series of children who could be assumed to have encountered some degree of persistent failure in their school-work. There are two bases to this assumption. Firstly, these children could be reasonably assumed to have experienced persistent relative failure in their school work involving both spelling and, in many cases, reading. In most cases clear evidence was available that this was the case. Secondly, as implied earlier, (see particularly page 106) the fact that these children had been referred to the Unit could be taken as evidence that their progress at school had caused adults, either their parents or in some cases their teacher, some concern and that these children had so far failed to live-up to these adults' expectations of them, however realistic or unrealistic these expectations may have been. This is clearly closely similar to the kind of situation which Holt has described where the pupil's 'fear of failure' becomes more important than his or her attempts to learn.

An additional hypothesis was examined in this study via the use of 'supplied constructs' which had been chosen in advance by the investigator. This was the notion that the 'dyslexia' series would be more likely to have a negative self-evaluation than would be the case with the children who were being relatively successful at school. Writers on the topic of dyslexia (see, for instance,

Miles 1975, pp 5-14) have frequently suggested that one of the major problems found by the dyslexic child is that he or she is likely to have 'low morale'. A series of nine pre-selected constructs were administered to each of the participants in the study in an attempt to investigate this hypothesis.

### 3.2. Design and Conduct of the Study

#### 3.2.1. The Self-Identification Form of the Rep. Test in this Study

The variant of the Rep. Test used in this study employed 21 triadic sorts, in which 'Self' was successively combined with pairs of 'significant others'. This procedure hence employed seven 'significant other' cards. These were: 'mother'; 'brother or sister' (where appropriate)\*; 'favourite teacher'; 'least favourite person'; and three 'friends'. The triads were presented in a standard randomised order. Participants were required to supply their own constructs in response to the first eleven sorts. In the instructions given to the children, no mention was made of the possibility or otherwise of repeating constructs. No comment was made when constructs were repeated.

The remaining ten constructs were supplied by the investigator. The first nine of these were chosen, on a largely intuitive basis, as being likely to give some indication of the participants 'morale' or 'self evaluation'. These are listed, in the order in which they were presented in Table 3.2.(1).

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\* Where the child had no siblings, he or she was instead asked to supply the name of four rather than three friends.

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Table 3.2.(1): Supplied Constructs

a	'Like indoor games' vs 'Like playing outside'
b	'Tidy' vs 'Messy'
c	'Popular' vs 'Like being alone'
d	'Confident' vs 'Nervous'
e	'Lucky' vs 'Unlucky'
f	'Lazy' vs 'Hardworking'
h	'Clever' vs 'Average'
j	'Not keen on school' vs 'Like school'
k	'Failure' vs 'Successful'

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The final construct employed was 'like soccer' versus 'prefer cricket' and was included so that the participant would be able to finish the test on a 'non-threatening' note.

Each pole of a particular supplied construct was presented to the child on a card of the same size as those used for 'self' and the 'significant others'. The participant's task was to assign two of the elements to one of these construct cards and one element to the remaining card.

### 3.2.2. Selection and Recruitment of Participants

The 'dyslexia series' of participants consisted of 20 children who had been referred to the Bangor Dyslexia Unit and had been diagnosed as 'dyslexic' on the basis of a standard battery of tests used by the Unit to give a picture of the child's reading and spelling attainments and of their relationship to his or her ability in other areas of intellectual performance, and which also includes some 'checks' on possible associated symptoms such as 'handedness', 'ability to detect rhymes' and so on (see page 103).

These children were drawn from many areas of England and Wales - and notably from the Wirral, where there was an especially active local dyslexia association. Only a small minority of these children lived in the Bangor area, and only three were 'bi-lingual' in that they spoke Welsh in addition to English. The participants' individual characteristics, in terms of age and sex are discussed in Section 3.3.1. below.

The 'school-series' comprised 20 children who had been 'matched' with the 'dyslexia series' in terms of age and sex, and who had - in their teachers' opinion - shown no signs of any special literacy problems.

These children were recruited from two Hampshire schools: New Alresford County Junior School and Eggar's Comprehensive School, Alton. These were both schools at which the author had been a pupil. It was decided not to approach schools in the Bangor area as many of the pupils recruited from these schools would have been bi-lingual, which could have introduced an extraneous variable in comparing the two series.



In recruiting these participants, the investigator requested to see children who matched the age and sex characteristics of the participants in the dyslexia series, and additionally stressed that it was important that they should be regarded by their teachers as being quite successful in their school, and as having no particular problems with spelling and reading. It was also emphasised that this did not mean that the investigator necessarily wanted to interview children who were 'at the top of their class' - it was more important that they were generally 'living up to expectations'.

### 3.2.3. Completion of the Rep. Test

The 'dyslexia' series were tested at the Department of Psychology, U.C.N.W., shortly after they had completed the Dyslexia Unit's assessment battery. The 'school' series were tested at their own schools. In each case the children were seen individually, and completed the Rep. Test in a room which had been temporarily loaned to the investigator for the purpose.

The Rep. Test was introduced to the child as a 'game to play'. To familiarise them with the procedure two 'dummy runs' were carried out using the names of animals as the elements to be construed. The investigator first laid out in front of the participant three cards on which were written the words, 'PANDA'; 'TIGER' and 'EAGLE'. The investigator then said "I might say that Panda and Tiger are alike because they've both got four legs, and an Eagle has only got two. The participant was then presented with another three cards 'DOG'; 'RABBIT' and 'CROW', and was asked to say how two of these were alike and one was different. The names of the 'significant others' to be construed were then elicited from

the participant, and the first eleven triads were presented to the participant in sequence. No reference was made to repeating descriptions in any of the instructions which were given, and no comments were made if and when a participant did repeat a construct. For each triad the investigator read aloud the names which had been written on each of the cards.

To familiarise the participants with the procedure for the ten 'supplied constructs', the first 'animal triad' was again presented to the child together with two additional cards on which were respectively written 'HAS FUR' and 'HAS FEATHERS'. The investigator explained that he would group the 'PANDA' and 'TIGER' cards with 'HAS FUR' and the 'EAGLE' card with 'HAS FEATHERS'. The remaining three animal cards were re-presented to the child who was asked to show how he or she would group them with cards with 'WALKS' and 'FLIES' written on them. The participant then completed the ten supplied constructs, and again in each case the words written on each card were read out aloud by the investigator.

### 3.3. Findings

#### 3.3.1. Subject Characteristics

The age and sex of individual participants in both the 'Dyslexia' series and the 'School' series are presented in Table 3.3.(1). In each series the twenty participants comprised 14 boys and 6 girls. This finding is consistent with most earlier investigations of dyslexia which (see Section 3.1.1.) have shown a clear predominance of boys amongst those children falling into this category. The mean age of the participants in this study was 11

Table 3.3.(1): Individual Characteristics

SUBJECT NUMBER	AGE (Years and Months)	SEX
1	9.1	F
2	12.2	M
3	10.9	M
4	9.7	M
5	12.0	M
6	9.5	M
7	10.6	M
8	8.5	F
9	10.9	M
10	13.9	M
11	11.3	M
12	12.2	M
13	10.3	M
14	10.0	M
15	11.0	F
16	10.6	M
17	15.7	F
18	15.4	F
19	15.8	F
20	15.11	M

NB. These figures apply equally to both 'Dyslexia' and 'School' series. Samples were matched for these characteristics.

years 6 months (boys, 11 years 3 months; girls, 12 years 6 months)..

### 3.3.2. Rep. Test Findings

Table 3.3.(2) presents the number of constructs elicited from each of the participants in this study. The 'scores' presented here were determined by a colleague\* at the Department of Psychology, U.C.N.W. who was not familiar with the hypothesis under test in this study, and who had not been informed as to which of the participants in the study had been diagnosed as being dyslexic. The data were hence scored 'blind'. The mean number of constructs elicited for the two series were: 'Dyslexia' series 5.6; 'School' series 7.8. Using the Wilcoxon Matched-pairs Signed-ranks test this difference between means was found to be statistically significant at the 0.01 level.

Table 3.3.(3) presents data derived from the first nine 'supplied constructs' - data for the tenth construct, 'Like-Soccer' versus 'Prefers Cricket' were not analysed and are not presented here. These data were analysed using the Chi Square test, in accordance with the criteria put forward by Siegel (Siegel 1956, p. 110). No differences between the 'Dyslexia' series and the 'School' series statistically significant at the chosen alpha level of 0.05 emerged from these supplied constructs.

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\*I should like to thank Sara Hughes for her help in this context.

Table 3.3.(2): Number of Constructs Elicited

SUBJECT NUMBER	'DYSLEXIA' SERIES	'SCHOOL' SERIES
1	3	11
2	4	9
3	7	7
4	5	4
5	8	9
6	4	10
7	4	11
8	9	9
9	9	9
10	6	7
11	6	3
12	5	9
13	3	5
14	4	11
15	6	10
16	3	5
17	9	6
18	7	8
19	4	6
20	6	7
TOTALS	112	156
MEANS	5.6 *	7.8*

\* Using the Wilcoxon Matched-pairs Signed-ranks test, this difference is significant at the 0.01 level, for a 1-tail test.

Table 3.3.(3): Supplied Construct Data

CONSTRUCT	SELF-DESCRIPTION	
	'DYSLEXIA' SERIES	'SCHOOL' SERIES
INDOOR vs OUTDOOR	4 16	0 20
TIDY vs MESSY	10 10	11 9
POPULAR vs ALONE	9 11	11 9
CONFIDENT vs NERVOUS	6 14	7 13
LUCKY vs UNLUCKY	10 10	11 9
LAZY vs HARD-WORKING	11 9	10 10
CLEVER vs AVERAGE	2 18	3 17
LIKE SCHOOL vs DON'T LIKE SCHOOL	7 13	10 10
FAILURE vs SUCCESS	12 8	6 14

### 3.4. Discussion of Results

The data presented in Table 3.3.(2) provide clear evidence in support of the principle hypothesis under test in this study, namely that a series of children who had experienced particular difficulties, or failures, in school work involving spelling or reading would be more likely to repeat constructs on the Rep. Test than would a comparable series of children who had not experienced such marked difficulties. This evidence is interpreted as supporting the view, derived from the work of Holt, that such children who had experienced a considerable degree of relative failure would be more likely than otherwise comparable children to adopt a 'minimax' strategy - minimising their potential exposure to 'failure' by limiting the number of 'novel' responses which they volunteered and also minimising the degree of effort which they were required to put into the task with which they were confronted.

We should emphasise that in the context of the self-identification form of the Rep. Test individuals are not, in fact, confronted with any 'real' possibility of failure. In the construct elicitation procedure there are no such things as right or wrong, or even 'more acceptable', answers. It should also be emphasised that in this instance the Rep. Test was presented to the children as being 'a game to play' rather than a test; indeed with the children in the 'Dyslexia' series the investigator contrasted their 'game' with the tests which the child had just completed as part of the Bangor Dyslexia Unit's assessment procedure. It appears to be the case, therefore, that the data presented in Table 3.3.(2) illustrate children adopting such a 'safety-first' strategy in a wholly inappropriate situation. Parallels may be drawn between the instance provided by this study

and Holt's own examples of children who adopted such minimax strategies when playing 'twenty-questions' at school. It appears to be the case that not only are the kinds of strategy which Holt has described associated with the experience of failure, but such strategies also 'carry-over' into other areas of the children's behaviour.

The case could be advanced that the data presented in Table 3.3.(2) - rather than representing evidence of a strategy adopted by children 'seeking to avoid failure' - in fact are illustrative of an underlying relative inability on the part of dyslexics to provide a sequence of novel answers when confronted by the Rep. Test construct elicitation procedure; i.e. that dyslexics are actually unable to provide a series of novel answers rather than merely reluctant to do so. To allow for this argument, a small control series of six children who had been diagnosed as dyslexic by the Bangor Dyslexia Unit were asked to complete the same Rep. Test modification as participants in the main study, except that they were not allowed to repeat constructs.\* All six of these children were able to complete this task and in fact did not give any impression of experiencing any greater difficulty with it than did a series of twenty children investigated as hospital patients in the study reported in Chapter Four of this thesis.

It should be emphasised that the hypothesis tested in this study was in fact somewhat different from the view put forward by

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\*In fact, three of these children were tested by my colleague Isobel Hargreaves, to whom thanks are due.



Holt. He maintained that most school children experience failure and will regularly come to adopt minimax strategies. The hypothesis which has been supported here is rather more 'refined' in that it postulates that children who have experienced relatively more 'failure' will be correspondingly more likely to adopt such strategies. In a sense, therefore, this study could be cited as evidence that 'dyslexics' can be distinguished from otherwise comparable school-children, and may hence represent an educational group worthy of special attention.

- X However, whether such children have <sup>QUALITATIVELY</sup>~~quantitatively~~ different educational needs distinct from those of other school children, and particularly from those of other poor spellers and readers, was not under examination in this study; and it could very well be argued that this represents the crucial question in assessing the concepts educational utility.

Similarly this study has produced no evidence either for or against the notion that 'dyslexia' has a neurological basis. Whilst this study has provided evidence of what could be loosely termed 'sociological or educational concomitants' of the basic concept of dyslexia, it has rested upon the assumption that children in the 'dyslexia' series had experienced particular difficulties with reading or spelling. The strategy demonstrated here is seen as a reaction to these difficulties - no light has been shed on their essential nature.

No statistically significant differences emerge between the two series' self-evaluation as revealed by the 'supplied' construct data presented in Table 3.3.(3). These data are discussed in more detail in Chapter Five, where comparisons are drawn with data derived from the study of children suffering from stomach pains described in Chapter Four.

## Chapter Four: Recurrent Abdominal Pain in Childhood\*

### 4.1. Introduction and Literature Review

'Recurrent Abdominal Pain' represents one of the most common problems which confronts family doctors and paediatricians. Abdominal pains in childhood may be the product of a number of disorders of known organic aetiology - Appendicitis and the various forms of Mesenteric Adenitis being obvious examples. In a large number of cases, however, no organic cause for the symptom may be found despite the child experiencing repeated attacks of severe pain over an extended period of, possibly, several years. This condition hence provides paediatricians with a major problem both in terms of its frequency; and in terms of the fact that, despite the existence of an extensive body of literature, built up over a period of sixty-five years, the nature of the problem is still very imperfectly understood. Many medical writers (e.g. Apley 1959; Friedman 1972; Heinild, Malver, Roelsgaard and Worning 1959, and MacKeith and O'Neill 1951) have argued that the condition represents an 'emotional' or 'psychosomatic' disorder. The amount of specifically 'psychological' investigation carried out in this area, has however, been minimal.

The investigation reported in this chapter was instigated by Dr R.H. Davies, Consultant Paediatrician at St. David's Hospital,

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\* The research reported in this chapter was carried out in collaboration with Dr R.H. Davies, Consultant Paediatrician at St. David's Hospital, Bangor. I should also like to acknowledge the help and co-operation of Dr A.H. Morris, Consultant Paediatrician, and of the nursing staff of 'Ward Y Plant', St. David's Hospital.

Bangor, who wrote to the Department of Psychology, U.C.N.W. to enquire as to whether any student might be interested in building-up a 'psychological' profile of a series of children suffering from this complaint. The author 'accepted the challenge' because it was felt that the investigation offered the opportunity of applying the self-identification form of the Rep. Test to a medical setting: because it presented the chance to assess the technique's usefulness as part of a 'battery of psychological tests'; and because it was felt that these children might provide an interesting group for comparison with the 'dyslexic' children under investigation (see Chapter Three).

#### 4.1.1. General Features of the Syndrome

In approaching the literature relevant to this topic it is worth noting that a number of labels have been applied to the group of symptoms referred to here as 'Recurrent Abdominal Pain in Childhood'. Among these have been 'Colicky Abdominal Pain' (Still, 1909); 'Irritable Bowel Syndrome' (Stone and Barbero, 1970); 'Periodic Abdominal Pain' (Pringle, Butler and Davie, 1966); 'Non-Specific Mesenteric Adenitis' (see Conway, 1951); 'Abdominal Epilepsy' (e.g. Still, 1912; Moore, 1945); and 'Abdominal Migraine' (e.g. Hurst, 1924; Farquhar, 1956). Of these, the latter three have certain theoretical implications which will be discussed below - 'Abdominal Epilepsy' and 'Abdominal Migraine' receiving particular attention owing to the amount of literature which these concepts have generated. At a more whimsical level, Apley, in the preface to his standard work on this topic (1975) referred to children who experience this pain as 'little belly achers', and this phrase has 'caught-on' amongst paediatricians.

The first study specifically into this area (as opposed to the passing references made earlier by Still 1909 q.v.) was reported by Moro in 1913\*. Before going into a selected review of subsequent literature it will be worth giving a brief description of this paper to introduce the nature of the syndrome, particularly as Heinild et al. (1959) state that, "As so often with 'first descriptions' posterity does not appear to have much to add to the clinical picture."

Moro reported some 18 cases of children aged between 4 and 14 years who suffered from frequently severe, apparently spastic, attacks of pain lasting from minutes up to hours. He labelled this complaint 'Rezidivierende Nabelkoliken' (Recurrent Umbilical Colica). The condition was, in Moro's experience, most commonly encountered in the age range from 5-7 years. Although the stomach pains were the most predominant feature, a number of other symptoms, such as periodic vomiting (frequently occurring before going to school), Migraine, Asthma, Enuresis, and Vasomotor Disturbances, as well as pains in locations such as the legs, the head, and between the shoulder blades were frequently present.

Some patients had had operations for suspected 'Chronic' or 'Grumbling' Appendicitis, but in the majority of these cases the appendix proved to be healthy. Moro also ruled out Mesenteric Adenitis as a cause of the condition.

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\*I should like to thank my friend Reinhart Sonnenberg of the Department of German and Teutonic Philology, U.C.N.W. for his help in translating this paper.

Very significantly, in terms of the present study, Moro, in this first clinical description of the syndrome, argued that the explanation for this condition lay in the children's 'neuropathic constitution', which he argued was common to all of the patients he had observed. He argued that these children were oversensitive and overexcitable and that this was due to their 'neuropathogenic domestic surroundings'. He observed that symptoms would frequently disappear, even if no other treatment were administered, if the children were moved to different surroundings (e.g. to hospital). He took this to illustrate the psychogenic nature of the 'illness'.

This description of Recurrent Abdominal Pain was based on a very small number of cases. The clinical picture which emerges from Moro's work has, however, been generally confirmed.

The most important work in this area has been by Apley (1959 and 1975) who details his own findings from a study of 1000\* school children, 108 of them classified as suffering from Recurrent Abdominal Pain, plus an additional group of 100 consecutive cases of children referred to hospital with Recurrent Abdominal Pains as their major symptom. Apley also refers to much of the existing literature on this topic.

The following general features of the syndrome which emerge from this literature will now be discussed: site, severity, frequency and time of occurrence of pain; associated symptoms; evidence of organic causation; incidence of the syndrome; age of onset and referral; and prognosis.

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\*Also reported in Apley and Naish, 1958.

The umbilicus has consistently emerged as the commonest location for pain in this disorder. Apley (1975) reports that in 66% of his 'hospital series' the pain was umbilical. Similarly Stone and Barbero (1970) report pain as being periumbilical in 49% of their patients (n = 102), Conway (1951) reports the pain as being umbilical in 58% of his cases (n = 200), and MacKeith and O'Neill (1951) report the umbilicus as being the site of pain for 12 of their 25 cases. The epigastrium seems to emerge as the next most frequent site - 19.8% in Stone and Barbero's sample and 15.5% in Conway's sample\*. Both Apley, and Stone and Barbero report that in about 10% of their cases the pain was too vague for the child to supply a location.

The severity of the pain varies greatly from individual to individual - Stone and Barbero state that the "pain varied widely from extreme levels of abdominal 'colic' to minimal levels of soreness." Authors agree that the degree and description of pain have no diagnostic significance.

No clear pattern emerges for either the duration or frequency of occurrence of the pain. Stone and Barbero quote the following figures for the duration of pain:

5 - 60 minutes	37%
1 - 3 hours	36%
more than 3 hours	27%

The 'upper limit' for the duration of an attack is unclear - Apley states that in rare cases it may be in the region of several days.

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\* Apley reports similar findings but does not give any precise figures.

Frequency of attacks also seemed to vary greatly; Conway reports that this may range from the occurrence of more than one attack per day to intervals free from pain lasting between three and six months. Stone and Barbero conclude that:

"The Abdominal Pain was characteristically erratic and variable, without any consistent pattern in time, duration and intensity. In fact, a well defined or predictable course warrants continual scrutiny for a specific etiology, although even this picture on investigation ultimately may not demonstrate a traditional organic entity."

Beyond this latter suggestion, neither the frequency, nor the duration of attacks appear to have any diagnostic importance.

Apley and Conway report on the time of occurrence of the pain. Apley reports that in half of his 'hospital series' pain occurred variably, at any time night or day. This also held good for 55.5% of Conway's patients. In addition, Apley reports that for 15% it occurred predominantly in the morning, and that for another 15% it occurred predominantly during the evening or night. 38% of Conway's patients only experienced the pain during the day. Apley reports a few patients who were woken from sleep by pain. This is replicated by Stone and Barbero.

In agreement with Moro's findings, a number of symptoms have been found to be frequently associated with attacks of abdominal pain. Anorexia, fever, headache, pallor and vomiting have been amongst the associated symptoms as reported by Apley and Naish (1958), Stone and Barbero (1970) and Conway (1951).

Table 4.1. (1): Symptoms Associated with the Syndrome

	CONWAY (1951)	APLEY and NAISH (1958)	STONE and BARBERO (1970)
ANOREXIA	56%	Not Quoted	25%
FEVER	36%	11%	16%
HEADACHE	23%	23%	50%
PALLOR	Not Quoted	38%	41%
VOMITING	44.5%	22%	34%

Other symptoms encountered in some cases may be limb pain, diarrhoea and constipation.

Of course, one of the most important features of this disorder is that no organic cause for the pain may be identified. The two most likely causes for abdominal pain of this nature would be Mesenteric Adenitis and Appendicitis. It is worth discussing the evidence that they are not implicated.

Acute Mesenteric Adenitis, caused by inflammation and infection of the mesenteric lymph glands produces symptoms similar to those of Appendicitis. Conway (1951), and MacKeith and O'Neill (1951) point out that at that time it was fashionable to explain recurrent abdominal pains in childhood as being the product of a chronic, non-specific, form of this condition. It has been argued in support of this view (e.g. by Fitzsimmons 1946) that on operation enlarged infected mesenteric glands can be found. The significance of such evidence, however, may be doubted, as it is recognised that large



mesenteric lymph nodes are often found incidentally on operation (Gross 1953) or at necropsy in children after sudden death (Penner 1949). Apley (1975) argues that the incidence of enlarged nodes has yet to be shown to be significantly greater in children with recurrent pains than in those without them. Stuckey (1950), in a study of 100 children suffering from recurrent abdominal pain who underwent appendectomy showed that the rate of cure was not related to the condition of the mesenteric glands. The evidence available seems to agree with Conway's argument that the conception of Non-Specific Mesenteric Adenitis lacks a sound pathological basis.

MacKeith and O'Neill conclude that:

"Possibly, these descriptions of non-specific mesenteric adenitis as a common cause of recurrent abdominal pain in childhood are an example of medical mythology in the making."

Wallis (1955) presents a series of cases of 'Tuberculous Mesenteric Adenitis', the symptoms of which he described as colicky central abdominal pain, nausea, vomiting, sweating, meteorism, diarrhoea, constipation, pallor, lassitude and loss of weight. Clearly this picture is very similar to that which emerges from studies of Recurrent Abdominal Pain - particularly when it is added that in Wallis' series many of the cases showed a history going over several months. It is, however readily diagnosable via the tuberculin test and radiology. The source of this tuberculous infection is almost always the drinking of 'raw' (i.e. untreated) milk. For this reason the condition is commoner in country areas than in cities, and it has become increasingly rare.

As MacKeith and O'Neill (1951) point out, the picture which emerges from studies of children suffering recurrent abdominal pains

is quite different from the classical picture of Acute Appendicitis - in which the pain precedes vomiting, the tongue is coated, the breath smells and there is tenderness in the right iliac fossa.

Appendectomy has been performed in many cases of recurrent pain, but in most cases a 'normal' appendix has been found, (Apley 1975; Stone and Barbero 1970). Fitzsimmons (1946) reports 100 consecutive cases of recurrent abdominal pain in which the children had their appendices removed. In 94 of these cases a non-inflamed appendix was observed. The evidence from Stuckey's (1950) series of 100 cases is, however, less clear. In only 25.9% of the cases examined in this series was the appendix found to be 'normal'. However, in only 30.6% of cases was the appendix found to be actually acutely inflamed. The difference of Stuckey's findings from those reported elsewhere may perhaps be partially explained in two ways. Firstly the appendices in Stuckey's study were subjected to much more detailed scrutiny than in other studies. Although this appears laudable, doubts may be cast in that Boyd (1947) has written "Today every appendix is condemned by some pathologist somewhere" - implying that if abnormalities in this area are looked for hard enough they can be found. Secondly, the proportion of cases in this study in which pain was localised in the right iliac (a symptom usually taken as indicating Appendicitis rather than 'Recurrent Abdominal Pain') was unusually high (24%) and there were an additional 27% of cases in which pain was characterised as being 'central plus right iliac'. On these grounds one might a priori expect a higher proportion of pathological appendices than in other studies.

Stuckey's findings with regard to rate of recovery are also somewhat paradoxical. He found that in 60% of his patients the

operation led to a 'complete cure' but that there was no relationship between this recovery rate and the pathology of the appendix. This finding has usually been interpreted as indicating the non-involvement of the appendix in the disorder (e.g. Apley 1975; MacKeith and O'Neill 1951); but it is worth noting that Stuckey interpreted it as suggesting the wisdom of appendectomy in these cases. A possible explanation for this aspect of Stuckey's results will be put forward in section 4.1.4.

In considering this area it is worth noting a point made by Apley:

"Nevertheless it is obviously important to remember that there is, of course, nothing to prevent acute appendicitis from occurring in one-tenth of all children who have recurrent abdominal pain, just as it may occur in others."

It has already been stated that Recurrent Abdominal Pain is a very common problem in paediatric practice. Some estimate of the incidence of the disorder is given by the number of cases found in Apley's series of 1000 unselected school-children - a total of 108 cases being identified. This incidence of the 10.8% seems quite high - but the figures produced by the National Child Development Study (Pringle, Butler and Davie, 1966), in which the progress of several thousand children is being followed are still higher - an incidence of 15.7% for girls and 14% for boys being reported. Apley seeks to explain the higher incidence found in this study in terms of methods of data collection. Pringle et al. relied upon questionnaire data and reports from local authorities as opposed to direct interviews.

In agreement with the figures given by Pringle et al. (q.v.) Apley found that girls were more commonly affected than boys, the incidences being; girls 12.3% and boys 9.5%. Conway's sample also shows a preponderance of girls (54% = n = 250), but this difference was not interpreted as suggesting a link between sex and 'Complaint of the Abdomen'. Stone and Barbero found a difference in the opposite direction, 62 of their 102 cases being boys. It is, perhaps, worth emphasising that Apley and Pringle et al. were quoting proportions from samples taken from the 'general population', whereas Conway, and Stone and Barbero were citing from cases referred to hospital.

Heinild, Malver, Roelsgaard and Worning (1959) present figures for the seven year period between 1949 and 1955 which indicate that 19.69%\* of the 11,392 patients referred to the paediatric clinic in Copenhagen at which they work suffered from Recurrent Abdominal Pain. How typical this is of clinics in general is, of course, not clear.

This set of findings relating to the incidence of the complaint should obviously be taken as giving some indication of the size of the problem rather than as being the 'last word' on the subject.

The ages of patients reported in the literature generally falls within the range of two years of age to 16 years. Investigators have frequently employed a 'minimum age criterion' owing to the

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\* The percentages for individual years range between 17.7% (1949) and 23.5% (1955).

problems inherent in very young children attempting to communicate the site and nature of their pain. Conway, and Stone and Barbero employed a lower limit of two years of age; Apley, and MacKeith and O'Neill a criterion of three years of age. The oldest children in MacKeith and O'Neill's sample were 16; those in Apley's school sample 15; Heinild et al.'s oldest patients were 14 years, as were those of Stone and Barbero; whilst Conway's series contained no children over the age of 12.

Heinild et al. present figures for the age of children at referral to their clinic, and these are shown in figure 4.1.(a) These figures are fairly representative of those reported, although the dramatic 'trough' found between the ages of seven and ten years is curious. Apley in fact reports a similar finding for girls in his hospital sample.

Apley presents data for the age of onset of the recurrent abdominal pains, as opposed to the age of referral, taken from a sample of 118 children. These are shown in figure 4.1.(b). There is a clear sex difference in these figures which remains unexplained.

Data on which the paediatrician can base a reliable prognosis in cases of this disorder are not very common. Apley points out that there is a widely held opinion that children will 'grow out of it'. Such data as there are, however, are rather less 'optimistic' in their indications than this.

Apley reports a study he himself carried out, involving 30 cases, who in childhood had attended hospital with recurrent abdominal

Figure 4.1.(a): Age of Children at Referral (from Heinild et al, 1959)

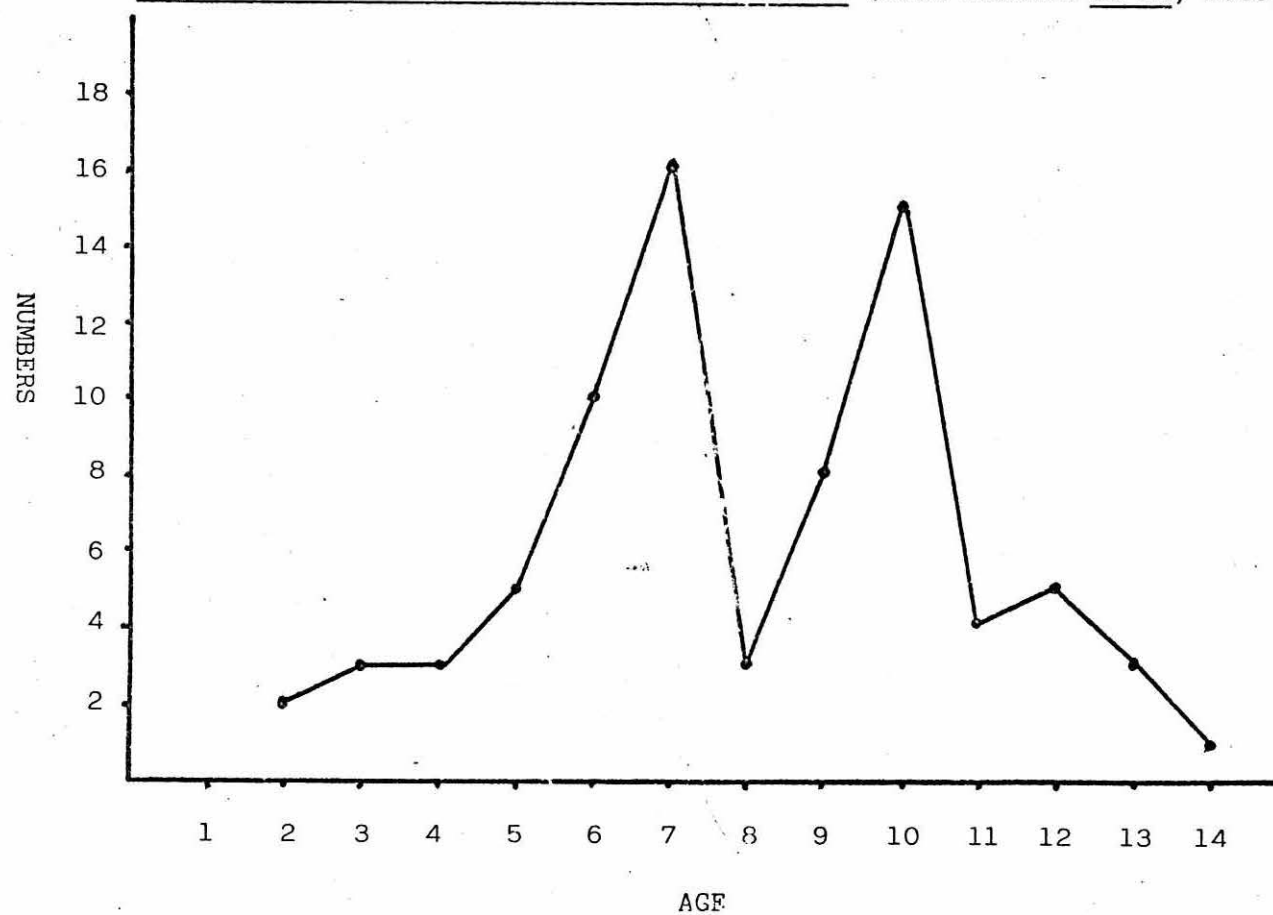
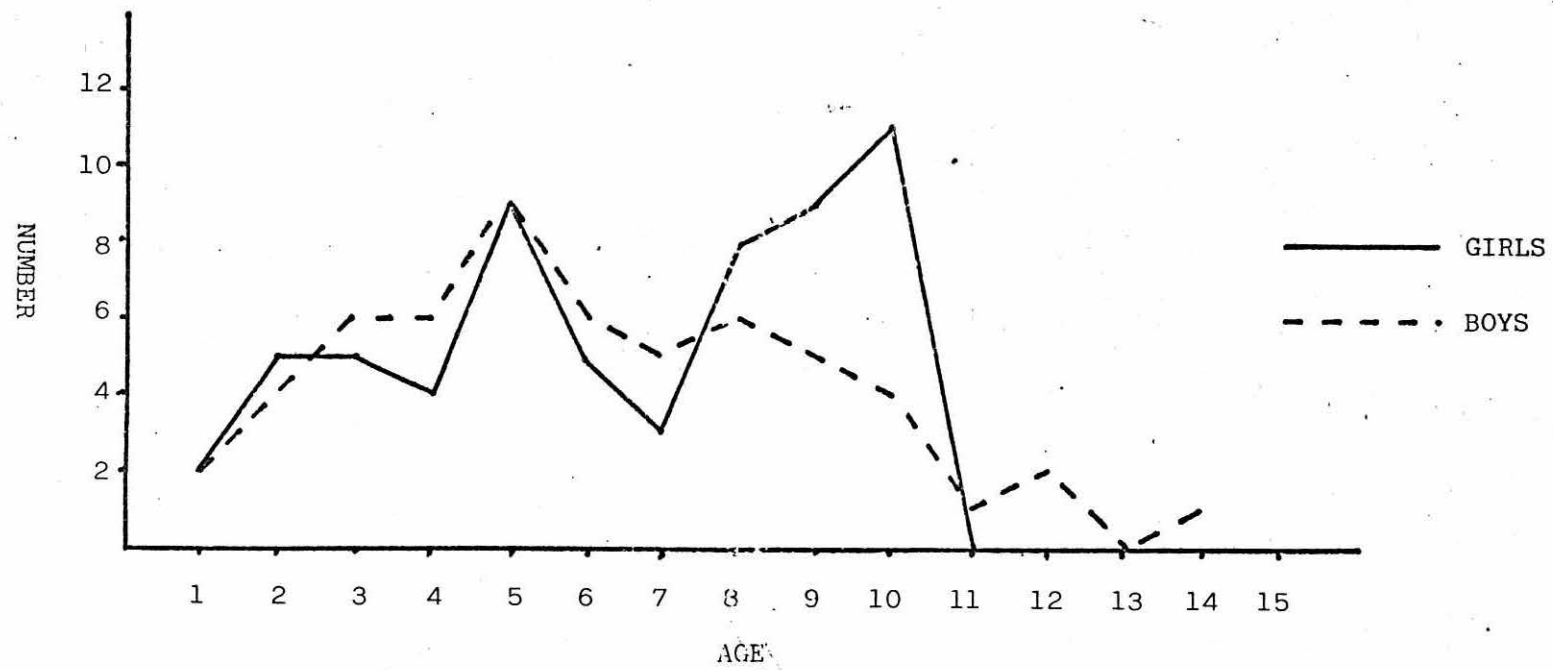


Figure 4.1.(b): Age at Onset of Pains (from Apley 1975)



pain and had received either no treatment or 'inadequate treatment'\*. In 18 cases Apley or his co-worker visited the patients' homes, after a period of between nine and 20 years since the hospital attendance. In the remaining 12 cases, the information was obtained from the family doctor - in these cases between eight and 12 years after the original complaint. In nine cases the patient was now symptom free, in nine cases the abdominal pain had ceased but other symptoms - often headache - had developed, (this was a much higher number than in a control series of 18 cases) and in the remaining 12 cases attacks of abdominal pain continued - although often less severely and less frequently.

Keyman, Hardoff, Berger and Winter (1973) report similar results from a series of 40 children hospitalised in Israel who were interviewed at least five years later. Five of these cases were reported to now show organic disease (three Peptic Ulcer, one Familial Mediterranean Fever, one Pyloric Stenosis). Twelve patients still complained of abdominal pain and several others of 'various symptoms'.

The main focus of Heinild et al.'s research was an X-ray study of the stomach and duodenum in these cases. They described changes in the patients' gastric mucosa and in peristalsis. Eighty one of their 135 cases were again examined after an interval of at least three years. They found that the X-ray changes in the stomach persisted in 75% of cases (this was not the case in a control series of 34 patients) although there was 'subjective' improvement - complete or partial disappearance of the pains - in two-thirds of their

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\*Apley (1975); also reported in Apley and Naish (1959).



cases. Heinild et al. conclude that

"X-ray changes subside more slowly than subjective complaints, if they do subside entirely"\*

Apley concludes with the statement

"In other words little belly achers are likely to grow up to be big belly achers."

This appears to be overstating the case a little. In 60% of his own cases the actual abdominal symptom had ceased. The recovery rate was slightly higher in Heinild et al. and Keyman et al.'s samples. However, in a still significant proportion of cases, according to these studies, abdominal symptoms do persist for some considerable time - perhaps (in some of Apley's cases) even into adulthood. Hence it would seem fair, given our current state of knowledge to revise Apley's statement and say that

"Little belly achers need not grow into big belly achers, - but they may do."

Certainly, it does not seem accurate to argue that they will simply 'grow out of it'.

In trying to sum up the clinical picture which emerges from the body of research into the general features of the condition referred to here as 'Recurrent Abdominal Pain in Childhood', we may say that in this disorder pain is usually located in the 'central abdomen' - around the umbilicus. Although by definition recurrent, the pain

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\* Stuckey (1950) and Friedman (1973) also present 'follow-up' data in their studies, but in these cases some form of 'treatment' had been given, and they are hence regarded as not being strictly relevant to this discussion. They are mentioned in Section 4.1.4.

may vary greatly in its severity and frequency both between and within cases. Associated symptoms such as pallor, fever, vomiting, anorexia and headache are common. Appendectomy has occasionally been performed in these cases, but (with the possible exception of Stuckey's findings) the appendix is usually found to be normal. The condition seems to be found in about one-in-ten children in an age group between about three and 15 years - and perhaps represents as many as one-in-five of all cases referred to hospital paediatricians. The first attack may occur at any age during this period, but five years of age seems to represent a modal figure. The condition, at least if left 'untreated', may continue for a long time - for as long as nine years in perhaps as many as one-third of cases. The condition is, in theory at least, quite open to differential diagnosis from such other 'abdominal pain conditions' as Mesenteric Adenitis or Appendicitis.

It will be seen that this mixture although, of course, much more fully articulated, squares very well with that presented in Moro's pathfinding paper of 1913. It will be worth noting, at this point, the criteria employed by Apley to determine whether a case should be included under the term 'Recurrent Abdominal Pain'. These required that children should have complained of at least three episodes of abdominal pains, severe enough to affect their activities, over a period longer than three months.

A number of explanations of the origins of this disorder have been put forward over the years. Three of these will be considered in some detail: two of these, which explain the disorder in terms of the pains representing the childhood manifestation of Epilepsy

or of Migraine, suppose an 'organic' basis of some kind to the disorder. The third 'explanation' which is undoubtedly the most common (on this side of the Atlantic at any rate), considers that the pains are 'psychogenic' in nature.

#### 4.1.2. 'Abdominal Epilepsy'

As Apley points out, it has long been recognised that paroxysmal visceral disturbances may be a part of, or the 'equivalent' of, the symptoms of Epilepsy - and it has also long been suggested that epilepsy may hence be one of the causes of recurrent abdominal pains in childhood. Still (1912) included 'Abdominal Epilepsy' as one of a list of possible causes of 'Colicky Abdominal Pain'. With the advent of reliable electroencephalographic (E.E.G.) equipment, and the usage of this technique in the diagnosis of epilepsy, shortly after the war, the concept of 'Abdominal Epilepsy', as an explanation of the kind of problem we are concerned with here, received great impetus. The seminal E.E.G. study in this tradition was published by Moore in 1945.

Moore (1945) gives a series of six case studies via which he attempts to delineate the concept of 'Abdominal Epilepsy'. It is worth noting that only two of these patients were children, and one of these, aged three years, showed signs of mental retardation.

Moore begins by stating that:

"What heretofore may have been considered unexplained abdominal pain obviously must have an explanation, and it is suggested that where this variety of pain exists study be directed to the possible existence of cerebral disease dysfunction."

In his six patients, recurrent paroxysmal abdominal pain was either the only or the dominant symptom. In three of the adult cases the

history of the pain dated back to childhood, and in fact in two cases appendectomy had been performed in childhood and had revealed a normal appendix. Moore argued that the pains were:

"revealed to be a type of epileptic variant (abdominal epilepsy) as shown by symptomatology, electroencephalography and response to anti-convulsant drugs."

The symptomatology was seen to resemble that of Epilepsy in that pains could be seen as taking the form of 'fugitive', paroxysmal, disorderly, uncontrolled and recurrent ..... attacks.

The E.E.G. recordings are reported for five of the patients. Of these, two showed no abnormalities, one showed clearly 'epileptiform' spike-and-wave discharge and two showed abnormal 'slow' activity.

The response rates to anti-convulsant drugs were probably the most impressive part of Moore's argument. In five of the cases the anti-convulsant diphenylhydantoin sodium was administered and produced a complete cessation of the abdominal symptoms. In one of these cases a placebo was substituted without the patient's knowledge, and the symptoms returned.

More (1950) gave the following description of the symptomatology of the syndrome:

"The abdominal pain is paroxysmal and may constitute the sole or dominating feature of the syndrome, or it may be the significant centre about which cluster one or more of such symptoms as 'peculiar feelings', nausea, occasionally vomiting, rarely diarrhoea, pallor, sweating, 'rumbling sounds', in the abdomen periodic attacks of abrupt behaviour disturbances, night-

mares, clonic movements of the abdominal muscles and rarely of the limbs, without loss of consciousness, and post-ictal 'achiness', exhaustion and sleep."\*

Clearly there is much similarity between this clinical picture, and that which emerges under the heading of Recurrent Abdominal Pain in Childhood. He also states that amongst published work - both his own and that of other authors - 67-80% of patients had abnormal electrograms. He contrasts the concept he is putting forward of abdominal pain as an epileptic variant in which overt Epilepsy may be absent or vary rare with those authors who have described patients manifesting gastro-intestinal disturbances as a secondary symptom to overt epilepsy.

In summary, Moore presents 'Abdominal Epilepsy' as being a clinical entity in which the main symptoms are recurrent attacks of abdominal pains which may or may not have associated symptoms such as vomiting, palor, etc. which is frequently associated with abnormal - even epileptiform - E.E.G. findings, and which is responsive to treatment with anti-convulsants. For our purposes however, it is worth noting some important points in Moore's argument and in the case-studies which he presents. Firstly, the syndrome is not seen as being characteristically a childhood complaint. In fact, most of Moore's own patients were adults, although he does cite Klingman, Langford, Greely and Hoefer's (1941) study of 12 children in whom abdominal pains were explained as being epileptic disturbances. Secondly, it is worth noting that although Moore feels justified in citing a high percentage of cases as having

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\* Moore 1950

'abnormal' E.E.G. patterns, these E.E.G.'s were certainly not always characteristically 'epileptiform'. Thirdly, the sample contained in his 1945 paper differs from those normally cited in studies of recurrent abdominal pains in that five or the six cases exhibited a clear organic background to their condition - in three of these cases there was a history of head injury. Further, as Apley points out, all but one of Moore's cases exhibited signs indicative of overt epilepsy. Finally, Moore does not give any indication of the incidence of his alleged syndrome.

Livingstone (1951), following-on Moore's work reports a study detailing a series of 14 children aged between five and 11 years suffering from symptoms very similar to those reported by e.g. Apley or Conway, i.e. the pain was periumbilical in location - radiating in some cases to the epigastrium, attacks lasted for periods ranging from five minutes to 36 hours, and symptoms such as nausea or vomiting were associated in eight cases.

E.E.G. analysis revealed definite abnormalities in ten cases. Of these four patients showed abnormally fast activity; four abnormally slow activity; one paroxysmal bursts of spikes and one high voltage spike-and-wave discharge. In one patient an attack of abdominal pain occurred actually during a recording session, and the record during this attack was characterised by the onset of high-voltage slow activity.

All the 14 patients were treated with the anti-convulsant Dilantin, which proved entirely successful in 11 cases.

Livingstone, in discussing his results, emphasises that, since abdominal pain is extremely common and may have many causes, intensive investigation should be carried out before a diagnosis of 'Abdominal Epilepsy' is arrived at. However, he does argue that although the three diagnostic criteria should be:

"1) recurrent paroxysmal attacks of abdominal pain followed by postictal-like exhaustion and sleep;

2) electroencephalographic abnormalities

and 3) a favourable clinical response to Dilantin therapy."

a diagnosis of abdominal epilepsy may be considered

"in patients who suffer with recurrent attacks of abdominal pain and yet do not present the typical triad of findings as previously mentioned."

Livingston's work, particularly when taken together with that of Klingman et al., clearly suggests that 'Abdominal Epilepsy' could provide an explanation of at least some of the cases considered under the heading of 'recurrent abdominal pain'. It is not clear how many cases there are from the papers we have so far considered, and this is obviously a crucial question. Other workers have carried out larger scale studies of children suffering from recurrent abdominal pains.

Apley, Lloyd and Turton (1956) report an E.E.G. study of 133 children meeting Apley's normal criteria for 'Recurrent Abdominal Pain' aged between three and 14 years, compared to a sample of 133 children free of pain. They were critical of the lack of control data, and the 'very small' samples involved in the work of Klingman et al., Moore, and Livingstone. In their study, they could find no

statistically significant differences in the E.E.G. records of the two groups. There was a slight preponderance of abnormalities in the 'Recurrent Abdominal Pain' group when hyperventilation and photic stimulation were employed. These abnormalities were made up largely of 'focal spikes', which the authors did not regard as being indicative of Epilepsy.

In considering definitely 'epileptiform' records alone, the children without pain showed rather more abnormalities than the 'belly-achers'. (Although, this difference was non-significant).

On the basis of their data, Apley et al. argue that 'As a sole manifestation of epilepsy, abdominal pain must be rare,' and that:

".....children with no symptoms other than recurrent abdominal pains should no more be considered 'epileptic' than children without them."

Papathiophilou, Jeavons and Disney (1972) report a study of 50 (30 female; 20 male) children referred to the E.E.G. department of a Birmingham hospital with diagnoses of Recurrent Abdominal Pain, Periodic Syndrome or Abdominal Migraine. They were aged between four and 13 years, with a mean age of 8.6 years.

Papathiophilou et al. are critical of the way in which many authors in this area have failed to distinguish between 'abnormal' and 'epileptiform' records. They also suggest that much confusion in the literature is due to various authors adopting different criteria of 'normality'.



Sixty-Four E.E.G. records were taken in the waking state, and 41 during sleep. If one of several waking records were abnormal, the patient was classified as having an abnormal waking E.E.G. Thirty-three of the 50 patients had a normal waking E.E.G., ten showed mild abnormalities, and definite abnormalities were shown in seven cases. Of these, four showed generalised spikes or spike-and-wave discharges and three had normal resting records but spike-and-wave was evoked by photic stimulation.

Of the 41 sleep records, 33 were normal, seven showed generalised abnormal discharges, and one showed focal or unilateral abnormality. Four of these patients with abnormal sleep records had shown normal waking records. Hence 21 of the 50 patients exhibited an 'abnormal' E.E.G. either during waking or during sleep, but in only 11 (22%) did the abnormality consist of spike or spike-and-wave, that is 'epileptiform' E.E.G. activity.

Papathiophilou et al. also report a follow-up study of 14 patients who 12-14 years previously had been referred to the E.E.G. department suffering from recurrent abdominal pains. Only one patient (who had exhibited spike-and-wave in his original E.E.G) had developed epilepsy. The authors conclude that:

"Recurrent abdominal pain, or 'periodic syndrome' seems to be an epileptic phenomenon in only a very small proportion of cases, and the finding of E.E.G. abnormalities, other than spikes or spike-and-wave, should not be taken as evidence of epilepsy."

It is generally accepted that epileptiform E.E.G. tracings are

obtained from between 70 and 80%\* of the epileptic population as opposed to 10-15% of the non-epileptic population. Hence, the proportion of epileptiform records reported by Papathiophilou et al. is about that which could be expected by chance.

The evidence of these last two studies clearly implies that if 'Abdominal Epilepsy' exists at all, it is not a very important element in the general picture of recurrent abdominal pains in childhood. One cannot, of course ignore altogether the work of writers such as Livingstone or Moore. However, it is worth pointing out that these studies dealt with selected groups of patients as opposed to a random sample of children with abdominal pains. It is also worth emphasizing that in Moore's samples, at least, an unusually large number of patients actually exhibited 'overt epilepsy'. It is quite conceivable that in a proportion of epileptics abdominal disorders rather than being associated symptoms may be the major manifestation of the disorder. The evidence given by Moore and others, particularly the response to anti-convulsants, may be taken as evidence of this. The condition, however, seems to be rare, and it would seem that in only a very small number of cases could the concept be used to explain cases of Recurrent Abdominal Pain in Childhood. One would assume that differential diagnosis, on the basis of E.E.G. tracings, should be possible, but given the findings of Apley et al., and Papathiophilou et al., the utility of this would seem debatable.

'Abdominal Epilepsy' might explain a small minority of cases of Recurrent Abdominal Pain in Childhood, but as an explanation of the syndrome in general it appears to be inadequate

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\*In fact Moore (1945) cites these figures in explaining a normal tracing found with one of his patients.

#### 4.1.3. Abdominal Migraine

A concept parallel to Abdominal Epilepsy has been that of 'Abdominal Migraine'. Here again, recurrent abdominal pains are seen as representing the 'childhood manifestations' of a more clearly understood disorder, common in adults, which is also of a periodic nature.

That a connection between Migraine and Recurrent Abdominal Pains in Childhood should be made is perhaps not surprising in view of the fact that even in adults abdominal disturbances are a frequent adjunct to migrainous headaches. In addition an association between so called 'cyclical (psychical) vomiting' and Migraine has long been noted (e.g. Hurst 1924; or Riley 1937). Further, in view of the literature reviewed in section 4.1.2., it should be noted that differential diagnosis between Epilepsy and Migraine is by no means easy in all cases,\* such that one might reasonably expect advocates of the entity 'Abdominal Migraine' as well as those of 'Abdominal Epilepsy'.

The concept has a long history, dating back at least as far as Buchanan (1921) and Blitzen and Buchanan (1926). It has, however, usually been seen as being a rather rare condition, and the concept has gained less currency than that of abdominal epilepsy. Apley (1975) in fact gives it only very cursory consideration.\*\*

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\*See, for instance, the discussion appended to Krupp and Friedman (1953).

\*\* Apley in fact dismisses the title 'Abdominal Migraine' as being self-contradictory - 'for Migraine comes from the Latin 'hemicrania' meaning '(pain in) the half of the head'. The argument seems unnecessarily pedantic, rather reminiscent of nineteenth century attempts to restrict the diagnosis 'Hysteria' to women as the term literally means 'condition of the womb'.

The paper which comes closest to putting forward 'Abdominal Migraine' as a label (if not as an explanation) for recurrent abdominal pains in childhood was published by Farquhar in 1956. He reports a series of 112 patients (61 boys, 51 girls) aged between 18 months and 15 years, diagnosed as suffering from Abdominal Migraine. Thirty-four of these children actually suffered from 'typical migrainous headaches'. A total of 84 suffered from recurrent abdominal pains, in most cases with associated symptoms of headaches, or vomiting, or both. There can be little doubt, from the symptomatology described by Farquhar, that the sample contained many children whom Apley (for one) would have diagnosed as 'little belly achers'.

Seventy-two of the parents of the children in the sample suffered from migraine (61 female; 11 male). A further 32 suffered from recurrent headaches 'which may well have been migrainous but did not meet the essential criteria'. Seemingly on this basis only, Farquhar argued that:

"If these are not attacks of migraine they can be said with confidence to occur in children of migrainous heredity and to be precipitated by the very same factors that bring on an attack of migraine.\* It would seem reasonable to consider that all the manifestations of this disease are but part of the clinical picture of migraine in childhood."

There seems to be little support for Farquhar's claims. Piper (1951) in reporting a case of Abdominal Migraine states that 'among the array of migrainous variants abdominal migraine is one of the rarest.' In a report of 50 cases of Migraine in children, Krupp and Friedman (1953) state that:

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\* Although, as we shall see, no-one is very clear quite what these factors are!

"... in the majority, the syndrome in children was similar to that in adults, except that in children the headache was less severe and abdominal complaints were more prominent (but no less frequent)."

Apley (1972) reports a history of migraine in 14% of the families of children with recurrent abdominal pains, as opposed to 3% of the families of the 'control' children. Although the difference here is clear, the total incidence still remains very small. Farquhar seems to be trying to extend the 'range of convenience' of the concept of Abdominal Migraine such that it includes a large number (70% of his sample) of children who do not suffer from migraine headaches, rather than simply referring to a small proportion of migraine variants. There seems to be little justification for doing this.

Even if 'Abdominal Migraine' were accepted as a label for the complaint considered in this chapter, it would hardly provide a causal explanation for the condition. Although the actual mechanism involved in an attack of Migraine is well understood in terms of vasoconstriction of the cerebral arteries followed by dilation and distention of these same arteries, the actual circumstances which bring this mechanism into play are not known. Farquhar talks in terms of 'nervous tension' and 'emotional disorder'. We now turn to consider the possibility that these factors may be involved in Recurrent Abdominal Pain in Childhood.

#### 4.1.4. Explanations in Terms of Psychological Factors

It was noted in Section 4.1.1. that Moro, in the first paper devoted to this problem, suggested that the pains might have a psychogenic basis. This has probably been the most commonly advanced explanation for the problem, at least in the UK.

Many medical writers have suggested that a large proportion of their samples of children with recurrent abdominal pains exhibit 'emotional disturbance'. For example, Heinild et al. (1959) report that 90% of their sample of 81 patients with 'abdominal colics' exhibited 'evident maladjustment' which was seen as:

".....ranging from unrest and restlessness, through defiance and jealousy reactions, enuresis, tics, eating problems, school problems, to real anxiety and compulsion neurosis."

Apley (1958) states that 'significant emotional disturbances' occurred in two thirds of his cases, as opposed to an incidence of one-in-five in his controls. Apley (1975) in fact gives a breakdown of expressions of emotional disturbance in terms of 'undue fears', nocturnal enuresis, sleep disorders and appetite difficulties. This is shown in the following table.

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Table 4.1.(2): Expressions of 'Emotional Fear'

	Percentage in Children	
	Without Pain	With Pain
Undue Fears	10	27
Nocturnal Enuresis	8	20
Sleep Disorders	18	39
Appetite Difficulties	21	37

From Apley (1975)

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He also gives assessments of the children's personality traits based on clinical impressions of the child, 'observations of the child-parent relationship', and 'information from inquiries about the child's attitudes and reactions in different circumstances'. He claims to be able to show that:

"As compared with the controls, significantly more of the children with recurrent abdominal pain were highly strung, fussy, excitable, anxious, timid or apprehensive. Most gave an impression of over-conscientiousness, as did many of their parents."

Often they were 'bad-mixers', but aggressive behaviour was uncommon, and, as a generalisation, the children could be classified as 'indrawn rather than outgoing'.

One of the most important papers in this area was that published by MacKeith and O'Neill in 1951. They studied 25 consecutive children attending a children's outpatient clinic at Guy's Hospital with recurrent abdominal pain as the presenting symptom, who were referred from the paediatric clinic for psychiatric examination and treatment. Children were given puppets to represent members of their family, school teachers, and friends, to enable them "... make their motives and feelings readily comprehensible to the observer." On this basis the authors concluded that in 21 children emotional disturbance (usually either anxiety or anger) determined the attacks, emotional excitement being one amongst several determinants in six cases, and the principal or sole determinant in 15. They stated that:

"In most of our cases the principal cause of this recurrent syndrome seems to be what Trotter (1941) termed 'disharmonious mental state' - i.e. state of unusual and protracted emotional excitement aroused by a failure to adapt to the stresses which the child had to meet."

Sixteen of the patients received psychotherapy 'at some length'. Eight ceased to have attacks altogether; six showed 'some improvement' and only two showed no improvement at all.

MacKeith and O'Neill argue that in their cases the time relationship between emotional excitement and an attack of pain was clear. For example, they cite the case of a child who was brought to hospital by train, and on every journey, took fright when the train passed through a tunnel and experienced a spasm of pain. Apley (1975) also claims that in many children there was a relationship between attacks and stressful situations. For example, he cites the case of a boy who experienced pain when being driven to boarding school by his mother at the beginning of each term.

As further evidence of a psychological basis to the disorder, Stone and Barbero (1970) report that in 53% of their cases there was a total abatement of intestinal symptoms during hospitalization, and that there was a decline in symptomatology in a further 38%. Moro (1913) also noted a reduction in symptoms if the child was removed from his domestic surroundings. This evidence shows that the disorder may be situation specific.

A number of authors have pointed to the child's family situation as playing an important role in the condition. Moro (1913) stated that:

"The majority of the children are over-sensitive, too easily excitable, they are spoilt (pampered) and fitful on account of their neuropathogenic domestic surroundings." \*

Other writers have also suggested the importance of the child's familial environment. Apley states:

"The behaviour of the child with abdominal pains is based both on his genetic make-up and on the atmosphere of the home. Statistics do not give the whole story, but in a considerable proportion of my cases the children come

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\*Translated by Reinhart Sonnenberg



from what I have called 'painful families' -  
Practising doctors will recognise ruefully  
what this means:

These are families with one or both parents  
suffering from recurrent pains and psycho-  
logical problems, recurrent illnesses and  
pseudo-illnesses. With a background of a  
'painful family' the child complaining of  
abdominal pains is most unlikely to grow up  
symptom-free."

Similarly, Stone and Barbero (1970) suggest that parental close-  
ness and sensitivity to the child may interfere with the ability of  
the parents to help the child to cope with what they (Stone and  
Barbero) term 'a symptomatic ailment', such as abdominal pain. They  
also point out, however, that no uniform characterisations of their  
patients' families emerge from their study.

The clinical picture which emerges from these medical writers  
hence presents us with 'emotionally disturbed' children who are often  
anxious and excitable. Frequently, the portrait continues, the attacks  
of pain will be tied to specific events, and further the symptoms may  
disappear altogether if the child is moved to a new environment (e.g.  
hospital). Moreover, it is suggested that the condition may be main-  
tained, in some way, by the child's family circle.

One of the most important contemporary developments within  
psychology, which has particular relevance to the study of 'psychoso-  
matic' medicine has been the study of 'autonomic conditioning' - pioneered,  
in particular by Neil Miller and his co-workers. This work is of  
importance to the present discussion because it enables us to look at  
autonomic disturbances, such as those which one may presume to be  
involved in stomach ache, as 'learned behaviours'; which provides a

possible route towards explaining the genesis of the condition, and also some of its important features.

The 'traditional' view of conditioning maintained that autonomic reactions were only susceptible to modification via classical conditioning, and that 'operant' modification of such responses was impossible. This would imply that autonomic functions may be responsive to only a very small range of specific stimuli; whereas 'voluntary' responses may occur in an almost limitless variety of situations. Kimmel (1974) traces the origins of this view back to the debate between Skinner and Konorski and Miller (S), which took place during the late 1930's. N.E. Miller (1969) argues that this model was based upon:

"..... failure to secure instrumental learning in two incompletely reported exploratory experiments and a vague allusion to the Russian literature."

During the 1960's, however, a large number of experiments have shown that this view needs drastic revision.

In particular Miller and his co-workers demonstrated that it was possible to train rats to produce a wide variety of autonomic responses; from blood pressure changes to changes in the rate of urine formation in the kidneys; using electrical stimulation of the brain, or the avoidance of electric shocks, as the reinforcers. These rats had been administered curare, which paralyses the animals skeletal muscles, and hence eliminates (or at least drastically reduces) the possibility of voluntary mediation of such responses. Much of this work is summarised by Miller (1969). Of particular relevance to the present discussion is an experiment conducted by Miller, Carmona and Demierre, in which changes in the amount of

blood present in the rats' stomach walls were learnt in relation to the avoidance of the electric shocks.

Nor are these findings restricted to 'animal preparations'. A large number of studies reviewed by Kimmel (1974) have shown that autonomic functions in humans may also be responsive to operant techniques\*. The implication of this work is that autonomic reactions may be learnt in relation to a variety of stimuli, in very much the same way as, say, motor skills.

The practical implications of such autonomic conditioning are immense, and have led to, for instance, the therapeutic techniques known as 'bio-feedback'. The theoretical implications, at an explanatory level, are also great, as Miller (1969) was quick to point out. Of considerable interest to our present purpose, he cites the following hypothetical example under the heading 'Cause of Psychosomatic Symptoms'.

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\*This review is particularly important in view of the difficulties which have emerged in attempting to replicate the animal work of Miller and his co-authors. (See, for instance, Miller and Dworkin 1974). Work with curarized rats is theoretically important in that it eliminates the possibility of 'voluntary mediation' - which could explain some human findings - and hence allows the study of 'pure' autonomic conditioning. For the purposes of the present discussion, however, this theoretical point is of only very slight relevance - the important point is to illustrate that autonomic responses may be learnt in relation to a wide range of stimuli. The literature reviewed by Kimmel shows this to be the case; whether such responses are 'mediated' by 'voluntary mechanisms', is, for our purposes, a side issue.

"For example, suppose a child is terror-stricken at the thought of going to school in the morning because he is completely unprepared for an important examination. The strong fear elicits a variety of fluctuating autonomic symptoms, such as a queasy stomach at one time and pallor and faintness at another; at this point his mother, who is particularly concerned about cardiovascular symptoms says, 'You are sick and must stay home.' The child feels a great relief from fear, and this reward should reinforce the cardiovascular responses producing pallor and faintness. If such experiences are repeated frequently enough, the child, theoretically, should learn to respond with that kind of symptom. Similarly, another child whose mother ignored the vasomotor responses but was particularly concerned by signs of gastric distress would learn the latter type of symptom."\*

By allowing us to look at abdominal pains as learned behaviours, 'autonomic conditioning' hence provides an explanation for the origins of the condition. Further, as is illustrated by the above quotation, it lays stress upon the child's home background - which, as we have seen, is thought to be an important ingredient of the syndrome. It further suggests an explanation as to why the disorder may be situation specific, as some data suggest. In effect, this approach sees the child's abdominal problems as being promoted by, and maintained by, his or her family environment. In those cases where the child's symptoms disappear upon hospitalisation, this may be because the familial stimuli maintaining the behaviour are no longer present.

It should perhaps be noted at this point that this model may go some way towards explaining the rather anomalous findings on

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\* My underlining

the effectiveness of appendectomy reported by Stuckey (1950, see Section 4.1.1.). Given that the stomach pains may be learned behaviours maintained by the patient's familial environment, it would be quite feasible to expect such drastic treatment as abdominal surgery to have some sort of placebo effect. Parents, if led to believe that the surgery may alleviate their child's condition, may well expect their child to recover and this expectation may lead to a change in the child's behaviour. Placebo effects are very common in clinical practice, and such an explanation may also account for findings such as those of Moore (1950 q.v.) on the effectiveness of anti-convulsants. It is interesting to note that Moro argued that his prescription of valerian tincture was 'obviously only having a suggestive effect.'

This model based on autonomic conditioning supplies an explanation of how the condition may be generated and maintained, but it does not explain why the mechanism should be brought into action. Apley (1972) uses the phrase 'listening to the child talking with his body' as a description of 'the sensitive physician trying to understand a child with recurrent pain.' Two important questions we need to ask are: "Why should the child need to 'talk with his body'?" and "Why should the abdomen be singled out for this role rather than any other part of the body." It should be noted that it is not always possible to answer these questions separately.

In trying to explain the origin of particular symptoms, Miller writes of mothers who are especially responsive to abdominal symptoms.

Although valuable in emphasising the roles of the family in the condition, this line of reasoning does little to explain the choice of the abdomen, rather it simply raises a secondary question - why should mothers respond to abdominal symptoms in particular? It seems unlikely that such an explanation will suffice for a condition as commonplace as Recurrent Abdominal Pain in Childhood.

Another line of speculation is put forward by Menninger (1947) who argues that the gastro-intestinal tract may, of itself, be particularly vulnerable to functional disturbances. He maintains that:

"Next to the skin, the gastro-intestinal system has more contact with the external world than any other part of the personality. It receives more direct demands for adjustment and accommodation, more insults and abuses, and a greater variety of opportunities for gratification than any other set of organs suffers or enjoys. Such disturbances occur in all of us, and in some of us all too frequently."

Such considerations may go some way towards explaining why abdominal pain should be such a widespread phenomenon, but it raises the question as to why some children should be particularly prone to the problem. A number of different 'predisposing' factors may be hypothesised. Naturally there is no reason to expect that any one of these possible influences need provide an exclusive explanation of the problem.

We have seen that the family may be an important influence in this syndrome. Apley has pointed to what he terms 'painful families' - in which there is already a history of pain. It is

easy to see that, in agreement with the previous quotation from Miller, the members of such a family may be particularly responsive to pain symptoms exhibited within the family. Also, imitative learning may be at play.

There are, moreover, a great many other factors which could be at work in the family. For example, in a large family children may need to indulge in special strategies to gain attention (c.f. the work of Gonda 1949 on neurological pain in children). To give another example - 'double-bind situations', as described by Bateson and his co-workers (Bateson, Jackson, Haley and Weakland 1956) and popularised by Laing (e.g. Laing 1962), could put some special stress on children. The relationships within the families of children with abdominal pain are in need of further systematic study beyond the speculation of medical writers such as MacKeith and O'Neill, Apley, and Stone and Barbero.

There may also be factors working at a more individual level - at the level usually referred to as 'individual differences'. For the moment, it is useful to see such differences as falling into two categories - physical and personal.\* At the physical level, one might postulate some innate constitutional factor predisposing children to 'learn' abdominal symptoms. Such a possibility is put forward by Neil Miller (1969) who states that:

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\*Although, of course, this distinction is largely arbitrary. A personality difference may very well be the product of an underlying constitutional difference.

"A person who has a stomach innately susceptible to emotional disturbance might be more likely to the use of the symptom."

Similarly Groen (1957) suspected that:

"Certain inborn properties of the Central Nervous System made 'normal' people less susceptible to psychosomatic conditions." \*

At the personal level, we have seen that Apley reports his clinical impression that 'belly achers' tend to be:

"highly strung, fussy, excitable, anxious, timid, ..... and over-conscientious."

and could be classified as 'indrawn' rather than 'outgoing'. These clinical impressions require further empirical validation. As Apley points out the physician does not have access to the 'specialised techniques' available to the psychologist. It is curious that little or no work has been carried out with these children using psychometric measures of personality. This reflects the neglect of this syndrome by psychologists. Techniques of psychological description seem most appropriate to providing pointers towards answering these questions which are of considerable concern to practising paediatricians. It was with this in mind that Dr R. H. Davies wrote to the U.C.N.W. Psychology Department.

#### 4.2. Design of the Study

The study described in this chapter attempts to apply psychological testing procedures, of a number of different kinds, to the question of why particular children should exhibit the symptoms of

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\* Groen, incidentally, adopted a 'biosociological' approach towards psychosomatic medicine - seeing such syndromes as substitutes for other behaviours which had been frustrated.



of Recurrent Abdominal Pain; paying attention to the children's family environment and their individual characteristics at both a 'behavioural' and a phenomenological level. A total of four measures were employed.

4.2.1. The Junior Eysenck Personality Inventory (1965, S.B.G. Eysenck)

This test was chosen as representing the traditional 'psychometric' approach towards personality measurement. Probably one of the most widely used of all psychological tests, the Junior E.P.I. is a development of the Maudsley Personality Inventory (Eysenck, 1959), and the Eysenck Personality Inventory (Eysenck and Eysenck, 1964) for adults. Like these two earlier inventories, it attempts to give scores on two of the three so-called 'orthogonal personality dimensions' at the heart of H.J. Eysenck's personality theory - 'Introversion - Extraversion' (E) and 'Neuroticism - Stability' (N). It also comprises an 'L' scale - 'for the detection of faking'.

As detailed in the manual, construction of the scales began:

"by carefully selecting, adapting or rewriting some items contained in the adult version of the E.P.I. and adding some others. Originally a total of 124 items were administered to sets of children (a total of 2,777) aged between seven and 16. Results from these surveys were factor analysed, and 60 suitable items were chosen for the final inventory of which 24 measured E, 24 N and 12 constituted the L scale. The choice was based on the loadings of the items for their respective factors, and their lack of loading on other factors."

The inventory hence consists of a series of 60 questions to which the child is required to respond 'yes' or 'no' by putting a cross in the appropriate box. Scoring is carried out with the aid of a cardboard template. Typical of the items of these scales are:

11 "Are you rather lively?"

A child who responds 'Yes' to this item scores 1 on the 'E' scale.

37 "Do you have many frightening dreams?"

A child who responds 'Yes' to this item scores 1 on the 'N' scale.

12 "Have you ever broken any rules at school?"

A child who responds 'No' to this item scores 1 on the 'L' scale.

Norms are provided in the manual based on samples of children, mostly drawn from the Rotherham area, aged between seven and 16 years. The norms are broken down into year groups, and separate figures are given for boys and girls. Figures for split-half and test/re-test reliability are given. Again, these are broken down into age groups, and by sex. The split-half reliabilities for the E factor range between 0.581 (for boys aged eight years) 0.868 (for girls aged 16 years). The manual in fact suggests that Extraversion may be a factor which does not clearly emerge until the age of nine or ten - or that its measurement with young children may present special difficulties. The latent roots for the E factor until 11 years of age are also extremely low, which fits this picture. Split-half reliabilities for the Neuroticism dimension are more consistent, all being above

the figure of 0.8 - the highest being 0.89 (for girls aged 16 years). Split-half reliabilities on the L factor range between 0.409\* (girls aged seven years) and 0.779 (boys aged 15 years). Test/re-test figures for Extraversion range from 0.512 (girls aged seven years) and 0.768 (boys aged 12). For Neuroticism the range is between 0.619 (girls aged seven years) and 0.869 (boys aged 11 years), 'L' score figures vary from 0.411 (girls aged 12) and 0.789 (16 years old girls). Generally, the test/re-test figures are a little lower than the split-half reliabilities.

The inventory was chosen for the study because it gives a description of the individuals's 'personality' or 'temperament' at a fairly general level, is in widespread use, and has generated a large volume of research. It was preferred to the Children Personality Questionnaire and the High School Personality Questionnaire developed by R. B. Cattell (which would seem to be the J.E.P.I.'s nearest 'rivals') for two reasons. Firstly it allows some hypotheses to be tested which would be missed by using Cattell's questionnaires. Eysenck relates his dimensions to the underlying 'conditionability' of the individual's nervous system. Introverts are supposed to be particularly responsive to operant conditioning, whilst Neurotics are specially sensitive to classical

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\* This figure is, in fact, something of a freak - the next lowest figure is 0.607 (boys aged seven years).

conditioning. Given an approach which sees Recurrent Abdominal Pain as a learned behaviour one might hypothesise that 'belly-achers' are particularly responsive to operant conditioning of autonomic functions, and hence expect them to score high on Introversion (i.e. to score low to the E scale). The same argument would of course, hold for classical conditioning of autonomic reactions and one might also expect high N scores. These hypotheses square with Apley's characterisations of children with Abdominal Pain as being 'indrawn' rather than 'outgoing'; and as being anxious, fussy, timid and so on.

Secondly the Junior E.P.I. is quicker to administer than the Cattell questionnaires, taking between 20-25 minutes\* to complete, as opposed to 45 minutes for the Children's Personality Questionnaire and the High School Personality Questionnaire.

It was decided to also ask the children's mothers to complete the adult version of the E.P.I., as it was felt that this might provide interesting data; and might also put the child more at ease, knowing that his mother was carrying out the same task as he was. As the above discussion indicates, this is very similar in both form and content to the J.E.P.I. - except that it comprises only 58 items, as opposed to 60 for the Junior version.

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\*These figures, and those quoted for the Cattell questionnaires, are taken from the N.F.E.R. Handbook (1978)

#### 4.2.2. The Vineland Social Maturity Scale (E.A. Doll, 1964)

It was considered desirable to obtain some data on the level of the children's 'social competence'. The instrument chosen for this purpose was the Vineland Social Maturity Scale.

First formulated in 1935, the scale aims at providing:

"a definite outline of detailed performances in respect to which children show a progressive capacity for looking after themselves and for participating in those activities which lead toward ultimate independence as adults."

The principles involved in the design of the scale seek to closely parallel those employed by Binet and Simon in the measurement of intelligence. In essence, it consists of a number of items of social behaviour arranged in a progressive order supposed to relate to chronological age. The items are divided into eight categories. The method is that the scale is worked through with an 'informant' (usually the subjects' mother) who has sufficient knowledge of the child to allow accurate scoring of the items by the investigator.

There are a total of 117 items. Standardisation data, based on a total of 620 subjects had been used in assigning age norms (referred to as LA's - Life Ages) to each item, and the items are arranged in a progressive order based on these values. The eight categories into which the items are divided are:

SHG - Self Help General	O - Occupation
SHE - Self Help Eating	C - Communication
SHD - Self Help Dressing	L - Locomotion
SD - Self Direction	S - Socialisation

'Detailed' descriptions of the items are supplied in the test manual. Typical items would be:

- Self-Help General
- 2 (LA 25) Balances head  
Holds up head voluntarily (unassisted) with trunk erect for indefinite period (about a minute).
- Locomotion
- 77 (LA 9.43) Goes about home town freely  
Goes about home town alone or with friends, outside own immediate neighbourhood to other than specific points. May be restricted as to area or 'deadlines', but these are more remote than nearby neighbourhood.
- Occupation
- 106 (LA 25+) Performs skilled work  
Is employed at journeyman level of skilled (technical or clerical) or supervisory occupations, such as office clerk, artisan, nurse, farmer, small merchant, foreman, household manager. Or continuing schooling at upper-class college level.

The procedure is that the investigator is expected to 'quiz' the informant in a sympathetic manner about the items on the scale. It is important to avoid asking whether the child can perform the task in question, but rather to ask whether he does usually or habitually do so. The investigator retains the scoring sheet himself and begins by questioning the informant well below the anticipated final score on each item category. An important section in the manual emphasises that:

"The scale is not a rating scale and scores are not to be based on mere opinions. The informant does not make the scoring judgement. This is done by the examiner after obtaining from the informant as much detail as practicable regarding the behaviouristic (sic) facts which reveal the manner and extent of the subjects actual performance on each item."

On each item, the investigator scores plus (+) if:

"it seems clear that the essentials for that item are satisfied and habitually performed without need of undue urging or artificial incentive, or with only occasional assistance in case of special circumstances."

Minus (-) is scored where:

"the person ... has not yet succeeded at all, or only rarely, or under extreme pressure or unusual incentive."

Provision is also made for scoring '+ N.O.' where the subject is clearly capable of performing the given task, but has no opportunity to do so. There is also a 'plus-minus' ( $\pm$ ) category where performance is in a 'transitional' or 'emergent' state; that is, where the task is performed occasionally but not habitually.

The investigator continues asking questions of progressive difficulty on each category, until at least two consecutive minus scores have been obtained for each category.

A 'total score' is then obtained by adding to the basal score (the highest of all the continuous pluses) the additional scattered positive scores and expressing this sum as a total number of items passed. 'Plus-minus' scores are scored as half-passes. 'Age equivalents' are provided in a table for each total score. Thus a total score of 81.5 yields an 'Equivalent Social Age Value' (S.A.) of 11.2 years. Mimicking the Binet-Simon procedure, a 'Sociality Quotient' (S.Q.) may then be obtained using the formula:

$$\text{Sociality Quotient} = \frac{\text{Social Age Equivalent}}{\text{Chronological Age}} \times 100$$

Clearly an 'S.Q.' of 100 will represent an 'average' figure. The manual claims that Vineland S.Q.'s are 'statistically and methodologically' comparable to Terman-Merrill Intelligence Quotients.

The 'Vineland' was chosen for this study for a variety of reasons. Firstly, it gives data close to the level of 'behavioural description'. It was considered that gathering information at the level of - "Does he go to bed without any help?" would assist in 'casting the net' as wide as possible; and that such data would provide a useful 'balance' to the more 'phenomenological' measures employed. In a similar vein, the way in which the scale mimicks the procedures of intelligence testing was considered to be an advantage in the search for eclecticism. A further very important consideration was that advantages were seen, in terms of the testing procedure itself, in employing a procedure in which the mother acted as informant. Firstly it provided a 'break' for the subject. Secondly, it provided an opportunity to involve the mother in the procedure. It is worth noting that Doll claims that his procedure 'provides the parent with insight and awareness of the maturational factors in child training'.

Some problems were foreseen with the use of this scale, however. Firstly, it was felt that there was a lack of normative data (620 subjects spread over an age range for birth to adulthood!), which might lead one to be wary of assigning too much significance to the obtained 'Sociality Quotients'. Secondly there is, inherent to the procedure, a certain amount of 'judgement', on the part of the investigator, in determining scores. The procedure might thus be open to a certain amount of 'experimenter bias'. The test takes 15-20 minutes to administer.



4.2.3. The Family Relations Test (E. Bene and J. Anthony, 1957)

We have seen that the family is thought to play an important part in Recurrent Abdominal Pain in Childhood. It was hence felt desirable to gain some data on the child's family situation, and his involvement within it. The instrument selected for this purpose was the Family Relations Test (F.R.T.).

Subtitled 'An Objective Technique for Exploring Emotional Attitudes in Children', this test attempts to create a 'miniature life situation' in which the child is invited to 'play a game of pretence' where he is asked to let certain cardboard figures 'stand for' the members of his family or his family or household. Presumably because these figures are meant to 'represent' the child's family to him, the F.R.T. is included in Semeonoff's (1976) review of 'Projective Techniques', although he does state that it is 'not strictly speaking a projective technique'. The F.R.T. is also reminiscent of projective techniques in that it is fairly unstructured and is intended primarily for diagnostic work.

In the childhood form\* the test may be applied at two levels - ages 3-7 and 7-15. These differ in the number of item cards (i.e. questions asked) and also in the format of these questions. It is the more senior of these versions which is described here.

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\*There is also an adult form of the test - subtitled 'An Objective Technique for exploring recollected childhood feelings' - in which the subject is asked to recollect himself and his family at a given point in time. The format of this version is almost identical with that of the 'senior childhood version'.

The test material 'was designed to give a concrete representation of the child's family'. It consists of 20 figures, drawn in simple outline, representing people of various ages, shapes and sizes. They are described as being:

"sufficiently stereotyped to stand for members of any child's family yet ambiguous enough to become, under suggestion a specific family."

From these the child is invited to choose one figure to represent himself and others to represent the members of his family. The family is to be interpreted in the sense of 'household', since other people, such as lodgers, living in the family home are meant to be included. Family pets are not to be included. No particular importance is attached to the actual choice of figures. The chosen figures are arranged before the subject, plus a figure known as 'Mr Nobody' who is a large man seen in backview wearing a hat.

The figures are attached to cardboard boxes, each with a slot on the top. Into these boxes are 'posted' item cards consisting of statements describing relationships within a given family. The subject is instructed to read each card (or, where appropriate, they are read for him) and to 'put it into' the person whom it fits best. If a statement applies equally to more than one person in the family the tester notes this. If a statement does not apply to any member of a family the card goes to 'Mr Nobody'.

The 86 item cards are categorised under the following headings:

- Mild Positive Outgoing Feelings
- Strong Positive Outgoing Feelings
- Mild Negative Outgoing Feelings
- Strong Negative Outgoing Feelings
- Mild Positive Incoming Feelings
- Strong Positive Incoming Feelings

Mild Negative Incoming Feelings  
Strong Negative Incoming Feelings  
Paternal Over-Indulgence  
Maternal Over-Indulgence

'Outgoing' feelings are concerned with how the child feels about the members of his family. 'Incoming' items are concerned with the child's perception of how the family members feel about him.

'Positive' refers to liking, kindness and so on; 'Negative' to the opposite.

Sample items might be:

Mild Positive Outgoing

- 09 The person in the family is very kind-hearted

Strong Negative Incoming

- 77 This person in the family does not love me

The parental indulgence and protection items are straightforward enough. Sample items would be:

Maternal Over-Protection

- 86 Mother is afraid to let this person in the family play with rough children

Paternal Over-Indulgence

- 93 This is the person in the family father spends too much time with

Maternal Over-Indulgence

- 97 This is the person in the family mother spoils too much

The cards are presented to the child in a random order - the investigator shuffling the 'pack' before starting the procedure. The manual does advise, however, that the first four, and the last two cards should be of a positive nature; and that the investigator should try to avoid a 'long run' of items from the same area which might establish a response-set in the child. Where necessary, the investigator

reads the cards out for the subject - but children are encouraged to read them for themselves. The administration of the test usually takes between 20 and 25 minutes.

An important index which may be extracted from the results of this test is that of 'Total Involvement'. This refers to the total number of items, of whatever kind, allocated to each person. The authors argue that 'lopsidedness' in any observed distribution may be important - in particular where items are displaced from the parents to other members of the family. Obviously a number of different indexes, taking account of 'quality of feelings' exhibited may also be extracted from the raw data where relevant.

This last point emphasises the essentially clinical nature of the test. As will be apparent, the measures derived from the technique are inevitably at least partially idiographic. Presumably for this reason, Bene and Anthony present very little by way of validity data. Rather, they present individual case histories illustrating 'syndromes' identified by the procedure. The test, in contrast to the J.E.P.I. and the Vineland Social Maturity Scale, seems to be designed primarily to give the clinician some 'way into' studying the individual family problems confronted by the child he is working with. Scope is left for intuition on the part of the clinician in assessing the significance of the results. It was felt, however, that this bias did not necessarily vitiate against the use of this technique in the present investigation.

Bene and Anthony do provide some data relating to the 'split-half' reliability of a slightly modified version of the test. This

assessed reliability in terms of positive feelings to and from Father, Mother and first named sibling; negative feelings to and from Father, Mother and first named sibling; and over-protection scores. The obtained reliabilities, using the Brown-Spearman prophecy formula, ranged between 0.78 and 0.96.

Again, this instrument was included in the test battery for a variety of reasons. Most importantly, it aims to gather data which are theoretically of great relevance to the problem in hand. Secondly, although it is by no means the only technique for investigating the child's perception of his familial environment, it does appear to be easier to administer than alternatives discussed by Semeonoff (q.v.) such as The Family Relations Indicator\* or the Test of Family Attitudes. Further, it was felt that the data derived from the Family Relations Test would be more directly open to numerical manipulation than would be that derived from these T.A.T. derivatives - or indeed than that to be derived from the Two Houses Technique. Another important point was that it was felt that the child might be put at ease by the element of 'play' involved in the technique. It is to be noted that this technique to some extent mirrors the previously mentioned use of puppets by MacKeith and O'Neill (1951).

#### 4.2.4. The Self-Identification Form of the Rep. Test in this Study

It was decided to use the version of the Self-Identification Form of the Rep. Test derived for the study of Dyslexia as the final

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\*Note that it is recommended that the F.R.I. should not be presented during a first interview.

part of this battery. In particular, it was decided to use the format used for the Dyslexic 'control' series (see p.126) where repetition of the descriptions was not allowed. The procedure hence consisted of a series of 11 triads where the child was required to supply his or her own 'constructs', followed by a further ten where the descriptions were provided for him. The same supplied constructs were employed as those listed in Chapter Three. In addition, the same introductory procedure, employing 'dummy runs' with animals as the elements to be construed, was employed.

In view of the possible role of family relationships in the syndrome of Recurrent Abdominal Pain in Childhood, a measure of self-other perception amongst these children might yield interesting data in terms of the children's perceived relations with other people. However, no precise hypotheses were formulated as to what forms these differences might take.

From the viewpoint of trying to assess the usefulness of the Self-Identification Technique this study was considered a valuable opportunity to judge its potential value as a part of a battery of tests. One of the aims of this project was to gain some idea of the extent to which the technique complemented and supplemented the material available from other sources. It was considered important to have some indication as to the extent to which it provided material which could be culled just as easily from more 'conventional techniques' - such as; in particular, the J.E.P.I. In addition, it was felt important to know, given that the technique did provide data not otherwise available, whether this material was useful in delineating the picture which was to emerge from the study, or whether such data

were merely of peripheral importance. Finally, to the extent that data derived from the Repertory procedure employed overlap with other sources, it was felt that a degree of cross-validation would emerge. The idea was that data derived from the Repertory Technique used with these children would provide fruitful comparisons with the children, categorised as 'dyslexic', experiencing difficulties in their school progress. The comparison would, of course, be of mutual interest, and would be valuable in that both groups represent children with some kind of 'problem', although the problems were - superficially at any rate - of very different nature. It provides a check against attributing to stomach-pain pathology any peculiar feature which might be typical in general of 'problem-having' pathology.

#### 4.2.5. General Comments on the Tests Battery

It has previously been emphasised that the approach towards selecting measures for this tests battery was deliberately eclectic.

Firstly, it was borne in mind that this study was of an 'exploratory' nature. Little or no specifically 'psychological' data exists on this problem, such that in designing this study, one was not influenced by the need or desirability to build-on and validate previous findings. Such findings as these are largely of an impressionistic nature such as the clinical intuitions reported by Apley. Hence the need not to exclude artificially what might be important areas of measurement was felt to be particularly important. For this reason it was decided to 'cast the net' as wide as possible whilst still arriving at a 'manageable' selection of tests. Hence

it was felt desirable to employ one - but only one - psychometric measure; and one - but only one - index of family relations.

Secondly, it was considered necessary to compare the Self-Identification technique employed with as wide a variety of different measures as possible. These two desires are, of course, clearly mutually compatible. Nevertheless, it must be admitted that there is a certain amount of compromise in the design of the study; between the considerations involved purely in carrying-out a study of Recurrent Abdominal Pain in Childhood; and those involved in looking into the usefulness of the Self-Identification Form. It might be the case, in fact, that had the problem of attempting to 'profile' children suffering from belly ache been approached purely for its own sake, then the Rep. Test may not have been included in the study at all, and its place taken by, say, the Rorschach Test. Of course, its inclusion may or may not be 'a good thing'. This is an empirical question to be answered by the data derived from the technique.

One feature of the battery employed is the gradation from 'behavioural' measures to 'phenomenological' ones. The Vineland Social Maturity Scale would represent the most clearly 'behavioural' measure whilst the Rep. Test could be regarded as having the largest phenomenological component. (The distinction can, of course, never be toally clear-cut). It is to be emphasised that both kinds of approach were felt to be relevant to this problem. This is to underline the point, implied in Chapter One, that although the author believes that psychology in the past has tended to lose sight of the



importance of 'personal experience', it does not follow that, in redressing the balance, the opposite mistake should be made - that of ignoring the usefulness of data at the level of behavioural description. It should be noted that the J.E.P.I. is felt to be intermediate on this gradation. Although Eysenck lays considerable stress on his claim to be a 'behaviourist' there is a large element of 'self-report' in the Eysenckian questionnaires.

It was stated that it was hoped to arrive at a battery which could be completed in under two hours. It should be apparent that most children should experience little difficulty in completing all elements of the battery within 90 minutes\*. Further, some of this time will be occupied by the child's mother completing the Vineland Social Maturity Scale.

#### 4.2.6. Criteria for Inclusion in the Study

The medical criteria employed in determining the inclusion of a child in the study as a case of Recurrent Abdominal Pain were that the child should have complained of at least three episodes of pain, severe enough to affect his or her activities over a period longer than six months. Children in whom pain had not occurred in the year prior to the study were excluded. The criteria is based on those employed by Apley (and mentioned in Section 4.1.1.) but are stricter in that they stipulate that the pain should have been recurrent over a six-month period. Because of the nature of the study it was also considered necessary to employ a higher minimum age criteria than in the majority of previous studies. The minimum age was set at seven years, to ensure

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\* As will become apparent, not all children were required to complete the 'full battery'.

that the subjects were capable of completing the materials involved in the study.

#### 4.2.7. The Use of Appendicitis Cases as a 'Control' Series

It was also considered necessary to recruit a control series of children not suffering from the syndrome of Recurrent Abdominal Pain. Normative data are available for the J.E.P.I.; and this is also the case, - although perhaps to a less satisfactory extent - with the Vineland Social Maturity Scale. Such data are not available for the Family Relations Test; nor, of course, for the Rep. Test adaptation employed. To enable the significance of findings from the Recurrent Abdominal Pain series to be assessed, it was hence vital to obtain comparison data.

It was decided to recruit a series of children recovering from Appendicitis for this purpose. There were three main reasons for this. Firstly, such children were readily available to the investigators! Secondly, and more importantly, it was considered that this group would go some way towards providing a 'meaningful' basis for a comparison. As we have already seen, the symptoms of Recurrent Abdominal Pain can, on occasion, closely mimic those of Appendicitis - such that appendectomy is quite frequently performed on 'belly achers'. The children in the two series' would hence have somewhat similar medical histories - or at least their recent experiences of pain would be likely to be quite similar, although the Appendicitis cases would usually have experienced their pains over a much shorter period. Again, the testing situation for the two series would be closely comparable in that both groups would be tested in a hospital setting. Had it been decided to recruit a random series of schoolchildren, this

would not have been the case, and unnecessary variability in the testing situation would have been introduced. Thirdly, if the battery employed could distinguish between these two groups, this would go some way towards further eliminating Recurrent Abdominal Pain as a clinical entity distinct from Appendicitis (see Section 4.1.1.).

Of course, an important consideration in deciding upon this comparison is the idea that Appendicitis is a complaint of known organic aetiology. One paper (Eylon, 1967), however, has been published which suggests that there may also be a significant psychosomatic component involved in at least some cases of Appendicitis.

Eylon investigated a series of hypotheses arising from Freud's (1905) description of the case of 'Dora', who when 17-years-old experienced an attack of Appendicitis nine months after receiving an improper suggestion from a married man. She had been caring for the would be seducer's children and had secret hopes that he would marry her. Freud concluded that:

"Her supposed attack of appendicitis has thus enabled the patient .... to realise a fantasy of child-birth."

Eylon also quotes Groddeck (1923) who interpreted an attack of Appendicitis in a male patient as a wish to be a woman in order to be able ultimately to bear a child. Inman (1958, 1962) suggested a possible relationship between appendicitis and 'birth fantasies' - referring to cases in which there was a temporal proximity between Appendicitis and the birth of a baby, or a wedding. From this Eylon derived the hypothesis that:

"The proportion of appendectomies among surgery patients who have a birth event in their personal history will be significantly higher than the proportion of appendectomies among surgery patients who do not have a birth event in their personal history."

Eylon investigated this hypothesis by interviewing a series of 35 patients, all over the age of 15, recovering from appendectomies; and comparing them with a matched sample of 35 patients recovering from operations other than appendectomy. He defined a 'birth-event' as the birth of a baby, a pregnancy, or a wedding at which the patient had been present occurring up to six months or after the operation and involving the patients parents, spouse, siblings, children, aunts, uncles or first cousins. Eylon found that birth events were significantly more common amongst the Appendicitis series than amongst the control series, and that this association was unrelated to sex or age. Further, he found, contrary to expectation, that the proportion of 'normal' appendicies amongst those cases in which a birth event was identified was no greater than in those cases of ppendicitis where no such event could be found. Eylon concludes that:

"Psycho-genic factors appear to operate in the genesis of appendicitis with an organic basis."

If one accepts the validity of Eylon's findings, it would seem reasonable to argue that in a significant proportion of cases (Eylon's data suggest rather less than half) 'psychogenic' factors in the form of birth fantasies may be present as a predisposing factor towards the organic condition of Appendicitis.

It is this author's contention that such a theoretical proposition does not invalidate the proposed comparison in the present study - for a number of reasons.

Firstly, although a psychogenic component may feature in the pathology of certain cases of Appendicitis, there still exists a major distinction between the organically understood aetiology of appendicitis, in which potentially fatal damage occurs to the internal organs of the sufferer following infection, and the syndrome of Recurrent Abdominal Pain in Childhood in which no such damage may be identified. The important distinction between an 'organic' condition and a more purely 'functional' one still exists. Given that we are involved in an essay into psychosomatic medicine, we should hardly be surprised to find psychological factors involved even in a condition of known organic nature - the purely 'physical' illness quite possibly does not exist.

Secondly, a defining characteristic of 'belly ache' in children is its recurrent nature. There is an important contrast between this chronic condition and the acute pathology of Appendicitis. We have argued that it may be useful to look at Recurrent Abdominal Pain as a learned behaviour, sustained by the child's environment over a long period of time. This cannot be the case with Acute Appendicitis - which by its very nature is a 'one-off' event. It would seem reasonable to argue that, although both conditions could, conceivably; be precipitated by 'psychological' factors, the functional significance of the two sets of symptoms is likely to be different.

Finally, a methodological point. It should be emphasised that Eylon restricted his sample to patients aged 15 or over. In fact, his sample had a mean age of 28.34 years (sic). In the present study we are, of course, concerned with Appendicitis in children, not adults.

One practical problem does emerge in using these patients as controls. This is that very often the child's mother was not available at the time that the child was interviewed. This meant that in most cases it was not possible to obtain scores on the Vineland Social Maturity Scale - nor, of course, for the parent herself on the E.P.I.

#### 4.3. Conduct of the Study

All interviews were conducted at St. David's Hospital, Bangor, between February 1977 and May 1978.

##### 4.3.1. Recruitment of Subjects

Recurrent Abdominal Pain children were selected from those cases seen by Dr R.H. Davies in his practice as Consultant Paediatrician to the hospital. In two cases, the children had been admitted to the hospital (one had in fact undergone appendectomy - the appendix was found to be normal). The remaining 18 were seen as out-patients. All met the requirements outlined in Section 4.2.6.

Control subjects were drawn from those children recovering from appendectomy in the hospital, as available. These interviews were conducted between two and six days after the operation. Unfortunately, Dr Davies himself had to undergo surgery during the early part of 1978. Dr A.H. Morris kindly helped in informing the investigator of the

availability of such children during this period.

Parental permission was, of course, sought before all interviews; and the voluntary nature of participation in the study was emphasised at the start of each interview.

#### 4.3.2. Place and Time of Interview

Wherever possible, interviews were conducted in two small rooms separate from the main wards of the hospital (and, in fact, some distance from the children's ward). This was the case for all Recurrent Abdominal Pain children and 12 of the appendectomy patients.

Patients and their families (occasionally mothers would also bring the patients' younger siblings; fathers attended in four cases) were initially met in a small room containing soft chairs, magazines etc., which was used as a waiting room for the hospital's Special Care Unit.\* The Family Relations Test and Rep. Test were administered to the child on his/her own in Dr Davies' adjoining private office. These rooms were quiet and free from interruption.

Eight of the control subjects were interviewed in the children's ward itself, usually because the nursing or medical staff felt that they had not yet sufficiently convalesced to move to the other end of the hospital. This did not seem to affect the performance of these patients; but it was felt to be an unsatisfactory procedure

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\*Occasionally parents would be met in the office, and would be asked to retire to the waiting room during the administration of these tests.

as the ward could sometimes be noisy; and there were occasionally interruptions, particularly for the child to be given tea! It was felt always preferable to interview the subjects 'off the ward', unless the hospital staff advised otherwise.

Interviews took place in the afternoon, usually just after two o'clock. Normally, 'belly achers' were seen on Wednesday, and controls on Tuesdays.

#### 4.3.3. Order of Materials

In all cases the J.E.P.I. was the first instrument presented. At the same time, mothers were asked to complete the adult E.P.I. None refused. This was followed by the Family Relations Test. Mothers were asked to stay in the waiting room whilst this was administered. On completion, the child returned to the waiting room and the Vineland Social Maturity Scale was completed with the assistance of the subject's mother (or, in one case, the subject's father). As mentioned previously, the parents of the Appendicitis series were not usually available. Hence, it was not possible to administer this scale to the control series. Finally, the child returned to Dr Davies office and carried out the Repertory procedure used in this study. It should be noted at this point that it was decided to restrict the age range of the subjects completing the Rep. Test, excluding those below about nine years of age as it was thought that these children might have difficulty. In fact only half (ten) of the subjects in either group completed this instrument.

No difficulty was found in administering these instruments. Very occasionally some of the subjects who spoke Welsh as their first



language experienced difficulty with the English of the J.E.P.I. In these cases the investigator attempted to explain the meaning of the item. If this failed, the child's parent was asked to provide a suitable Welsh translation for the item. This only occurred for a small number of items with a small minority of cases.

The purpose of the study was outlined in general terms at the beginning of the interview. It was explained that the investigators were attempting to see whether children suffering from Recurrent Abdominal Pain 'tended to be the same kind of children'. It was emphasised that the investigators did not feel that there was anything 'abnormal' about these children. It was emphasised that this was a 'group study'. After the materials had been completed, the purpose of the various instruments were briefly explained to the parents. A small number of parents asked how their children had 'done' or 'come-out'. Of course, it was in any case impossible to give scores on the tests as the protocols had not yet been analysed - but it was, naturally, considered to be ethically dubious to give anything but a vague reassuring answer to the question such as 'pretty much as one would expect for a child of her age'. Parents seemed satisfied with this. It was, of course, explained that these were not intelligence or attainment tests and that it made no sense to talk of 'Gareth doing well' or 'Carys doing badly'.

The Family Relations Test and the Repertory procedure were presented to the children as being 'games to play'.\*

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\*Children were asked whether they enjoyed doing them, for the sake of giving them the chance to express any feelings that they had rather than of gaining any more information. They all said "Yes". How honest they were being is not clear!

#### 4.4. Findings

##### 4.4.1. Clinical Findings

The medical examinations of the children involved in this study were undertaken by Dr R.H. Davies. He reports that, for those suffering from Recurrent Abdominal Pain, the clinical findings in the children studied were similar to those described in other series. They had all had intermittent attacks of pain for a period of at least six months (range 6 months to 5 years). In all the attacks were intermittent lasting 1-7 days. The age varied from 7 years to 14 years (Mean age = 9 years 11 months). There were 11 females and 9 males. Fourteen out of the 20 complained of central abdominal pain. Seven out of the 20 complained of right-sided pain (two of right and central pain). Only one complained of left-sided pain. All of those with lateral pain were investigated with I.V.P.s and none showed any abnormality. Vomiting was the most frequent accompanying symptom (in 7 out of 20), along with Headache (7 out of 20). In this series only one of the subjects suffered diarrhoea with the abdominal pain, and none suffered fever with the pain as a regular feature. Physical signs were uniformly absent.

As in other series investigations were uniformly negative. All of the children had urine examined: 18/20 had full blood counts; 11/20 had E.S.R.'s; 9/20 had abdominal X-rays; 7/20 I.V.P.'s; 14/20 had blood ureas; 8/20 had Serum Electrolytes Measures (Sodium, Potassium, and Chloride). A few other investigations such as Barium Meals, Urine Porphyrins, Live Function Tests, and Serum Amylase estimation were done in individual cases where there seemed clinical indication. A standard format for the investigation of

these children did not seem justified. These findings are summarised in tables 4.4.(1) and 4.4.(2).

Table 4.4.(1) Clinical Findings - Individual Subjects

SUBJECT	AGE*	SEX	DURATION**	VOMITING	DIARRHOEA	HEADACHE
1	10.8	M	5	✓	X	X
2	11.10	F	1	X	X	X
3	9.6	M	6/12	X	X	X
4	7.4	F	1	✓	X	✓
5	8.6	F	2	✓	X	X
6	11.0	M	5	X	X	X
7	11.6	F	1	✓	X	✓
8	13.6	F	1	X	X	X
9	13.0	M	6/12	X	X	X
10	12.6	F	5	X	X	X
11	7.7	M	6/12	✓	X	✓
12	9.9	F	8/12	✓	X	X
13	7.7	F	9/12	✓	X	✓
14	13.9	M	3	X	X	X
15	9.1	F	2	X	X	✓
16	12.8	F	2	X	X	X
17	7.0	M	6/12	X	X	✓
18	14.2	M	5	X	✓	X
19	10.9	M	2	X	X	✓
20	7.6	F	6/12	X	X	✓

\* Age in years and months

\*\* Duration in years or fractions of years

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Table 4.4.(2) Clinical Findings - Summary

MEAN AGE	MEAN DURATION	VOMITING	DIARRHOEA	HEADACHE
9 years 11 months	1 year 11 months	7	1	7

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In general the clinical description of these subjects is very similar to that which emerges from Section 4.1.

The control series comprised 20 children recovering from histologically proven Appendicitis. There were ten boys and ten girls. Ages ranged from seven years to 13 years five months (Mean age = 10 years 8 months).

#### 4.4.2. Junior Eysenck Personality Inventory Findings

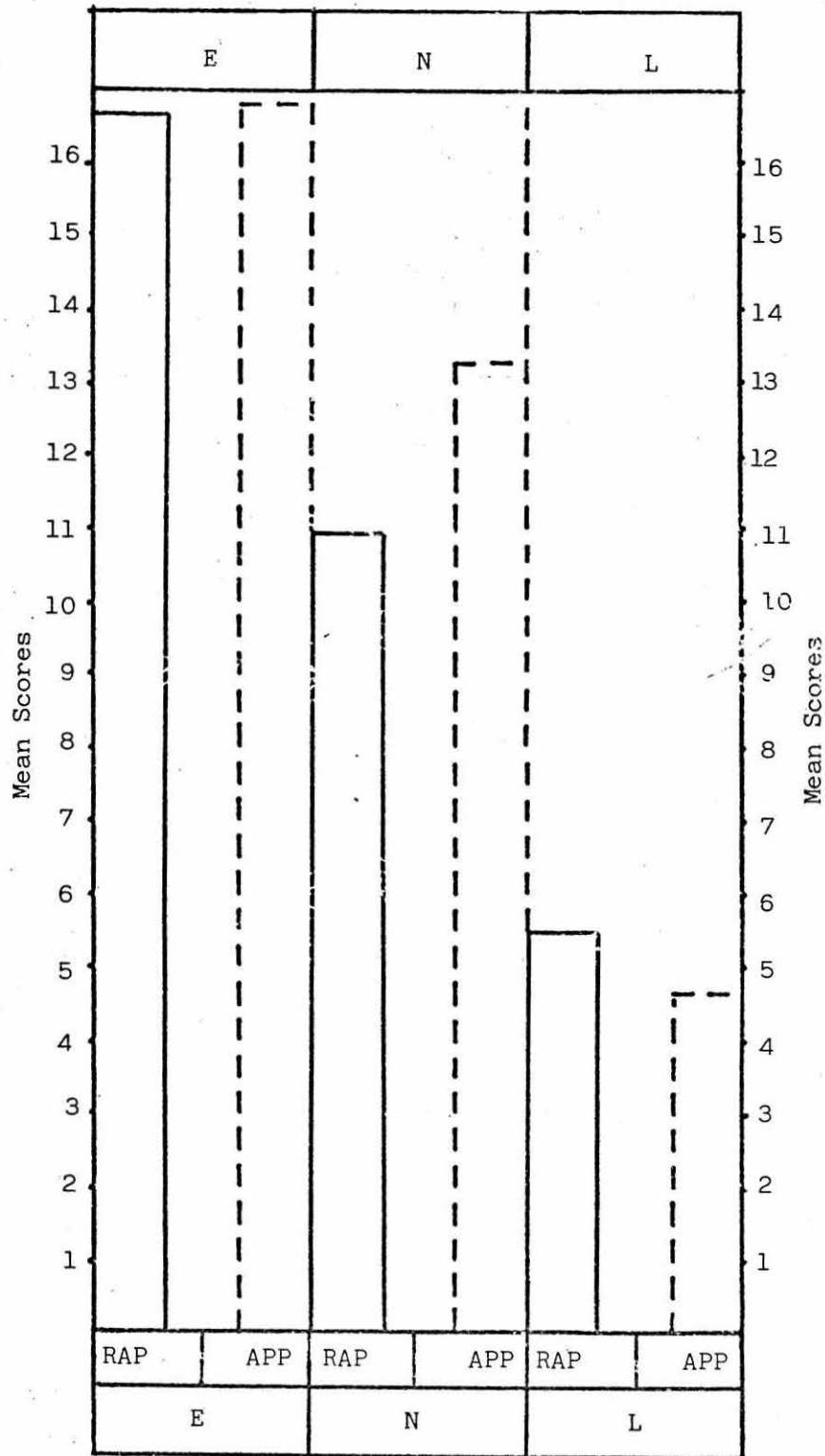
The J.E.P.I. was administered to all 40 children involved in this study, both subjects and controls. Individual scores for each of the three scales are presented in Tables 4.4.(3) and Figure 4.4.(a), together with group means and standard deviations. Mann-Whitney 'U' tests (Siegel, 1956) revealed no significant differences at the 0.05 level between Recurrent Abdominal Pain and Appendicitis patients for any of the three scales. The J.E.P.I. scores obtained in this study fall within the range of the normative data for this instrument presented by Eysenck (1965).

The mothers of 14 of the children suffering from Recurrent Abdominal Pain completed the adult Eysenck Personality Inventory.

Table 4.4.(3) - Junior Eysenck Personality Inventory Data

RECURRENT ABDOMINAL PAIN				APPENDICITIS			
SUBJECT	E	N	L	SUBJECT	E	N	L
S 1	19	10	7	C 1	21	20	1
S 2	18	13	4	C 2	18	9	9
S 3	16	3	7	C 3	9	10	0
S 4	19	13	10	C 4	21	9	7
S 5	17	10	4	C 5	17	11	2
S 6	14	18	1	C 6	17	10	7
S 7	10	4	7	C 7	15	16	1
S 8	16	12	2	C 8	10	17	3
S 9	17	9	4	C 9	17	17	4
S10	14	19	8	C10	16	13	3
S11	20	19	3	C11	16	10	8
S12	10	13	6	C12	18	19	6
S13	16	8	8	C13	16	9	3
S14	21	11	7	C14	15	18	6
S15	17	12	10	C15	23	7	8
S16	18	11	6	C16	13	17	9
S17	19	15	1	C17	17	17	0
S18	17	11	2	C18	18	18	1
S19	20	2	6	C19	20	11	8
S20	15	6	7	C20	19	7	7
TOTALS	330	219	110	TOTALS	336	265	93
MEANS	16.65	10.95	5.5	MEANS	16.8	13.25	4.68
S.D.	2.98	4.82	2.74	S.D.	3.43	4.42	3.17

Figure 4.4.(a): J.E.P.I. Data, Group Means



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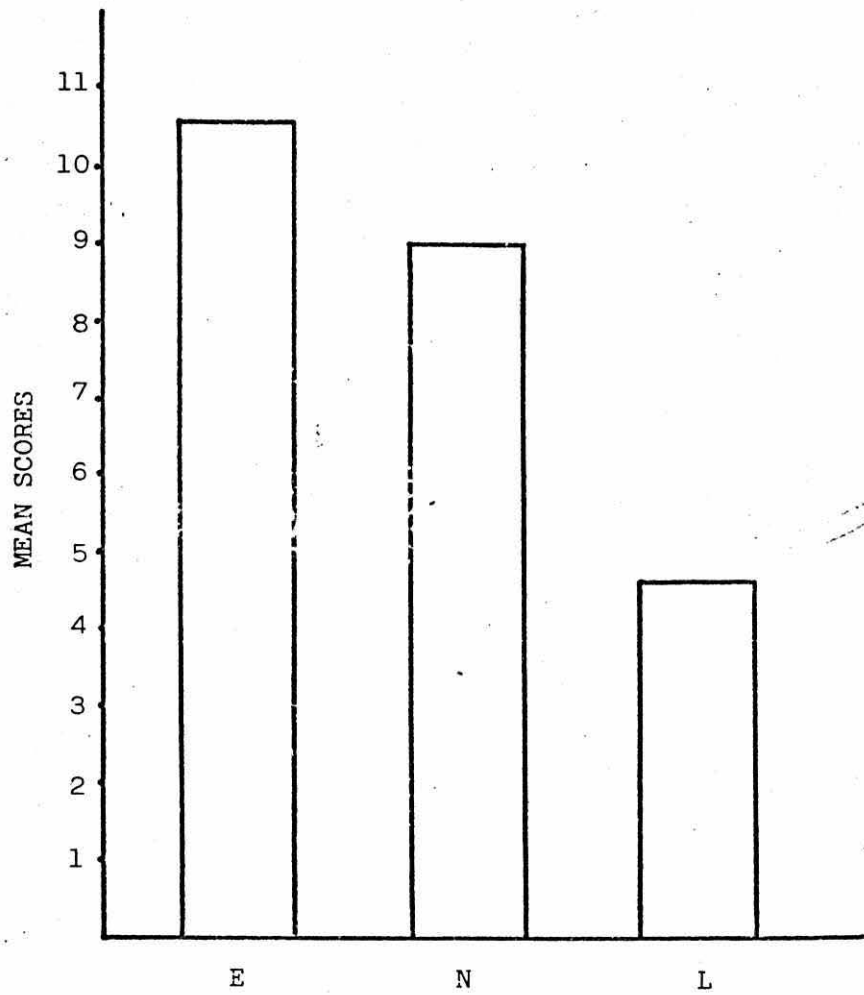
Table 4.4.(4) - Eysenck Personality Inventory Data  
(Subjects' Mothers)

SUBJECT	E	N	L
1	4	8	7
2	3	17	4
3	13	7	3
4	14	11	4
5	13	11	5
6	15	10	1
7	14	3	4
8	8	10	4
11	4	15	7
13	10	11	6
16	19	3	2
17	16	5	2
18	7	11	6
19	8	3	9
TOTALS	148	125	64
MEANS	10.57	8.93	4.57
S.D.	4.81	4.97	1.55

Note: Data were not available for the  
mothers of subjects S9, S10,  
S12, S14, S15 and S20.

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Figure 4.4.(b): E.P.I. Data, Group Means (Subjects' Mothers)





The data obtained are presented in Table 4.4.(4), and Figure 4.4.(b). The group means and standard deviations agree well with the normative data for a group of housewives reported by Eysenck and Eysenck (1964).

Data were not collected for the mothers of Appendicitis patients as they were only rarely present for testing.

#### 4.4.3. Vineland Social Maturity Scale Findings

Data from the Vineland Social Maturity Scale were obtained for 16 of the patients suffering from Recurrent Abdominal Pain, with the patients' mothers acting as informants. Data were not available for subjects S10, S14, S17 and S18 as their mothers were not present at the interview. Insufficient data were collected from the control series to merit reporting because the parents of these Appendicitis patients were only rarely present at testing. The data are presented in the form of 'Social Quotients' in Table 4.4.(5).

#### 4.4.4. Family-Relations Test Findings

As with the J.E.P.I., the Family Relations Test was administered to all 40 of the children involved in this study. 'Total involvement scores' for each of the recurrent abdominal pain subjects are shown in Table 4.4.(6) and in Figure 4.4.(c), and the corresponding data for the Appendicitis control subjects are presented in Table 4.4.(7) and in Figure 4.4.(d). Siblings are classified according to their family position relative to the subject, such that sibling '+1' would indicate an older sibling and sibling '-1' a younger one. Sibling '+2' or sibling '-2' would represent still older or still younger relatives respectively. Falling under the heading of 'others' were cousins and grandparents whom certain subjects wished to include in their families.

Table 4.4.(5) - Vineland Social Maturity Scale Data

SUBJECT	AGE *	S.Q.
S 1	10.8	105
S 2	11.10	119.5
S 3	9.6	94.74
S 4	7.4	122.73
S 5	8.6	114.12
S 6	11.0	109.00
S 7	11.6	95.65
S 8	13.6	93.33
S 9	13.0	110.77
S10	12.6	N/A
S11	7.7	109.5
S12	9.9	N/A
S13	7.7	127.97
S14	13.9	N/A
S15	9.1	110.13
S16	12.8	99.53
S17	7.0	N/A
S18	14.2	N/A
S19	10.9	105.12
S20	7.6	107.10
TOTALS		1624.19
MEANS		108.28
S.D.		10.12

\*Age in years and months

Table 4.4.(8) and Figure 4.4.(e) compare the mean total involvement scores for each figure for the two series. As shown in this table, the statistical significance of the between-group differences for each of these figures were computed using (owing to the ordinal nature of the data) the Mann-Whitney 'U' Test. A difference significant at the 0.05 level (2-tail test) was found for the 'self' figure, with subjects assigning significantly fewer cards to this figure than did controls. No other differences were statistically significant.

More detailed analysis of the FRT data was also carried out, investigating the various sub-scores for 'mild' versus 'strong', 'incoming' versus 'outgoing', and 'positive' versus 'negative' feelings, and for the overprotection and overindulgence items.

Table 4.4.(9) compares the number of 'mild' and 'strong' items assigned to each family figure for the Recurrent Abdominal Pain and Appendicitis groups. Tables 4.4.(10) and 4.4.(11) present the corresponding findings for the 'incoming' versus 'outgoing' and 'positive' versus 'negative' sub-scores respectively.

Figures 4.4.(f); 4.4.(g); and 4.4.(h) represent an attempt to portray graphically the relative balance between these bi-polar indices for each of the family figures. Figure 4.4.(f) illustrates the ratio of 'mild' to 'strong' statements assigned to each figure. In order that each ratio should remain greater than 1.00, in each case it is the ratio of the largest to the smallest sub-score which

Table 4.4.(6) - FRT Total Involvement Scores, Subjects

SUBJECT					SIBLINGS					OTHERS	
	NOBODY	SELF	FATHER	MOTHER	+2	+1	-1	-2	-3	A	B
S 1	53	0	5	9	-	12	6	-	-	1	-
S 2	30	5	7	11	20	7	6	-	-	-	-
S 3	37	9	7	9	7	8	8	1	-	-	-
S 4	12	0	6	24	-	-	22	-	-	-	-
S 5	19	4	9	9	-	24	7	-	-	13	1
S 6	40	5	14	16	-	11	-	-	-	-	-
S 7	45	0	7	11	-	-	13	10	-	-	-
S 8	39	4	12	17	-	14	-	-	-	-	-
S 9	19	6	15	18	-	-	18	1	9	-	-
S 10	37	3	3	7	-	17	3	11	5	-	-
S 11	22	0	31	33	-	-	-	-	-	-	-
S 12	25	6	7	16	16	16	-	-	-	-	-
S 13	33	10	11	14	-	18	-	-	-	-	-
S 14	38	1	2	3	3	9	15	4	11	-	-
S 15	40	5	19	22	-	-	-	-	-	-	-
S 16	56	0	8	8	-	-	9	-	-	-	-
S 17	22	11	27	6	-	14	6	5	-	-	-
S 18	36	6	11	10	-	12	11	-	-	-	-
S 19	51	5	3	12	-	15	-	-	-	-	-
S 20	42	4	15	13	-	-	12	-	-	-	-
TOTALS	696	84	219	268	46	177	136	54	25	14	1
MEANS	34.8	4.2	10.95	13.4	11.5	13.62	10.46	7.71	8.33	7	1
S.D.	12.12	3.37	7.64	7.01	7.85	2.13	5.44	7.43	3.06	9.90	-

Figure 4.4.(c): F.R.T. Total Involvement Scores, Subjects

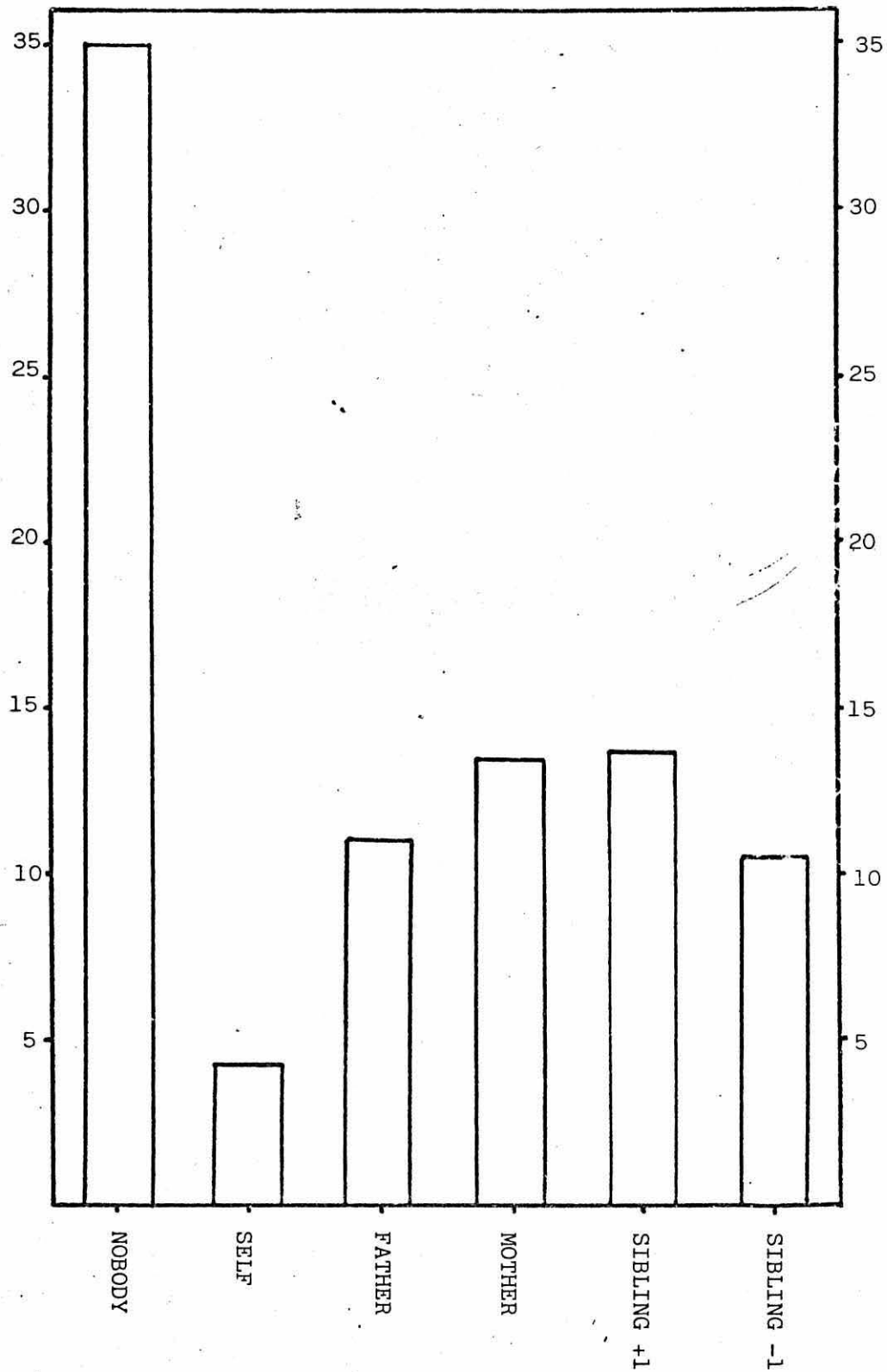
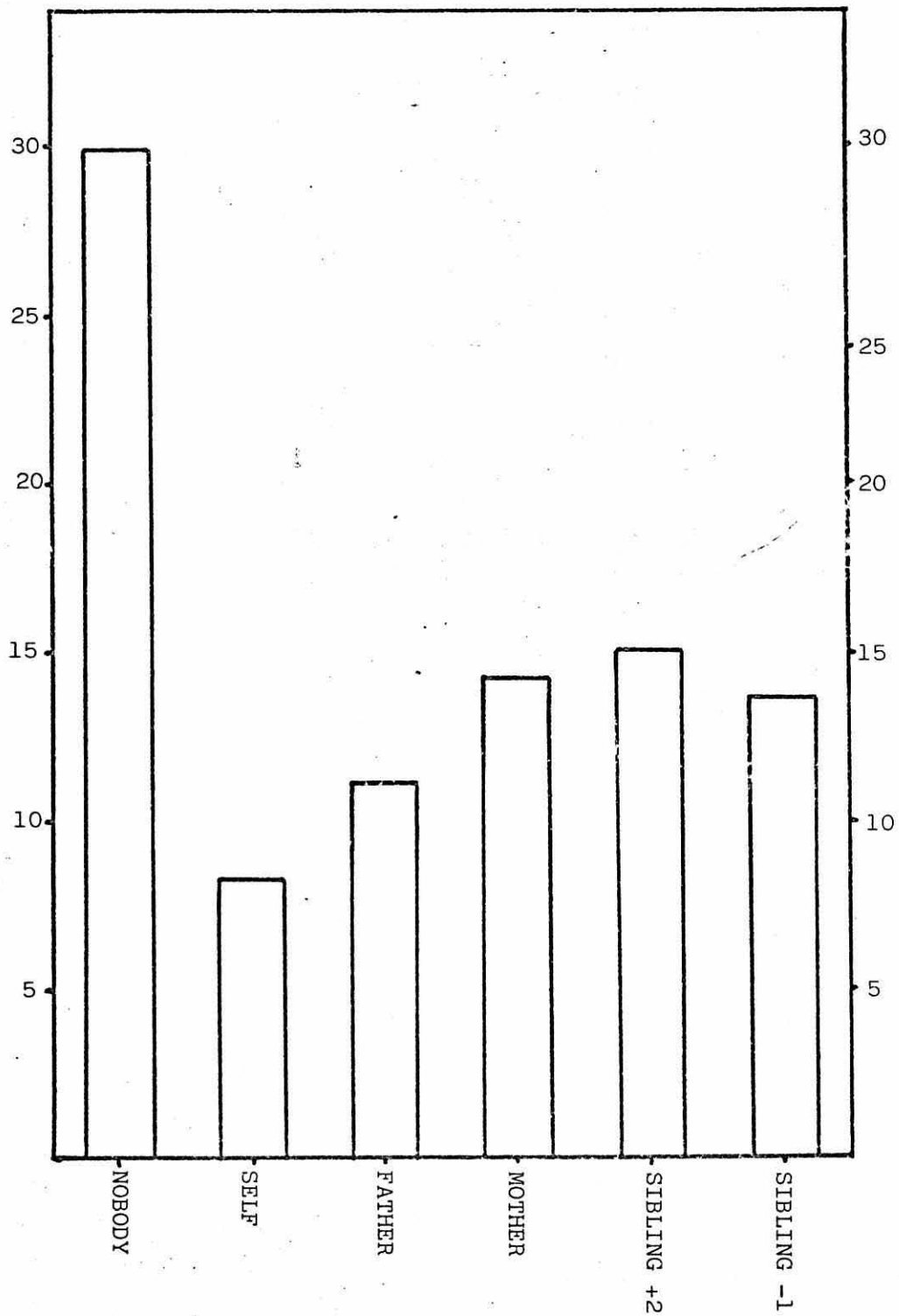


Table 4.4(7) - FRT Total Involvement Scores, Controls

SUBJECT					SIBLINGS							OTHERS	
	NOBODY	SELF	FATHER	MOTHER	+4	+3	+2	+1	-1	-2	-3	A	B
C 1	29	6	23	20	-	-	-	-	8	-	-	-	-
C 2	34	13	13	26	-	-	-	-	-	-	-	-	-
C 3	24	13	13	13	-	-	-	-	-	-	-	-	-
C 4	30	3	12	12	-	-	-	29	-	-	-	-	-
C 5	24	24	20	18	-	-	-	-	-	-	-	-	-
C 6	41	3	19	7	-	-	-	-	16	-	-	-	-
C 7	25	10	4	17	-	-	20	10	-	-	-	-	-
C 8	21	7	6	9	-	-	-	21	22	-	-	-	-
C 9	23	10	18	17	-	-	-	18	-	-	-	-	-
C 10	41	8	5	12	-	-	-	20	-	-	-	-	-
C 11	1	16	5	2	-	6	7	7	5	37	-	0	-
C 12	31	17	11	23	-	-	-	4	-	-	-	-	-
C 13	15	3	8	13	-	-	-	26	22	-	-	0	-
C 14	55	0	10	4	-	-	-	-	10	-	-	3	5
C 15	26	0	0	21	-	-	-	7	13	9	10	-	-
C 16	39	4	14	9	-	-	-	-	10	-	-	5	4
C 17	24	4	14	26	-	-	-	18	-	-	-	-	-
C 18	15	11	6	18	4	6	13	13	-	-	-	-	-
C 19	60	3	7	10	-	-	-	-	6	-	-	-	-
C 20	39	10	5	7	-	-	3	7	15	-	-	-	-
TOTALS	597	165	221	284	4	12	43	180	150	46	10	8	9
MEANS	29.85	8.25	11.05	14.2	4	6	10.75	15	13.64	23	10	2	4.5
S.D.	13.60	6.22	5.35	5.39	-	0	7.19	8.30	6.53	19.80	-	2.45	0.71

Figure 4.4.(d): F.R.T. Total Involvement Scores, Controls



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Table 4.4.(8) - Mean FRT Total Involvement Scores

	SUBJECTS	CONTROLS	
NOBODY	34.80	29.85	N/S
SELF	4.20	8.25	*
FATHER	10.95	11.05	N/S
MOTHER	13.4	14.20	N/S
SIBLING +4	N/A	4.00	N/A
SIBLING +3	N/A	6.00	N/A
SIBLING +2	11.50	10.75	N/S
SIBLING +1	13.62	15.60	N/S
SIBLING -1	10.46	13.64	N/S
SIBLING -2	7.71	23.0	N/S
SIBLING -3	8.33	16.00	N/S
OTHER A	7.00	2.00	N/S
OTHER B	1.00	4.50	N/S

N/A = Not Applicable

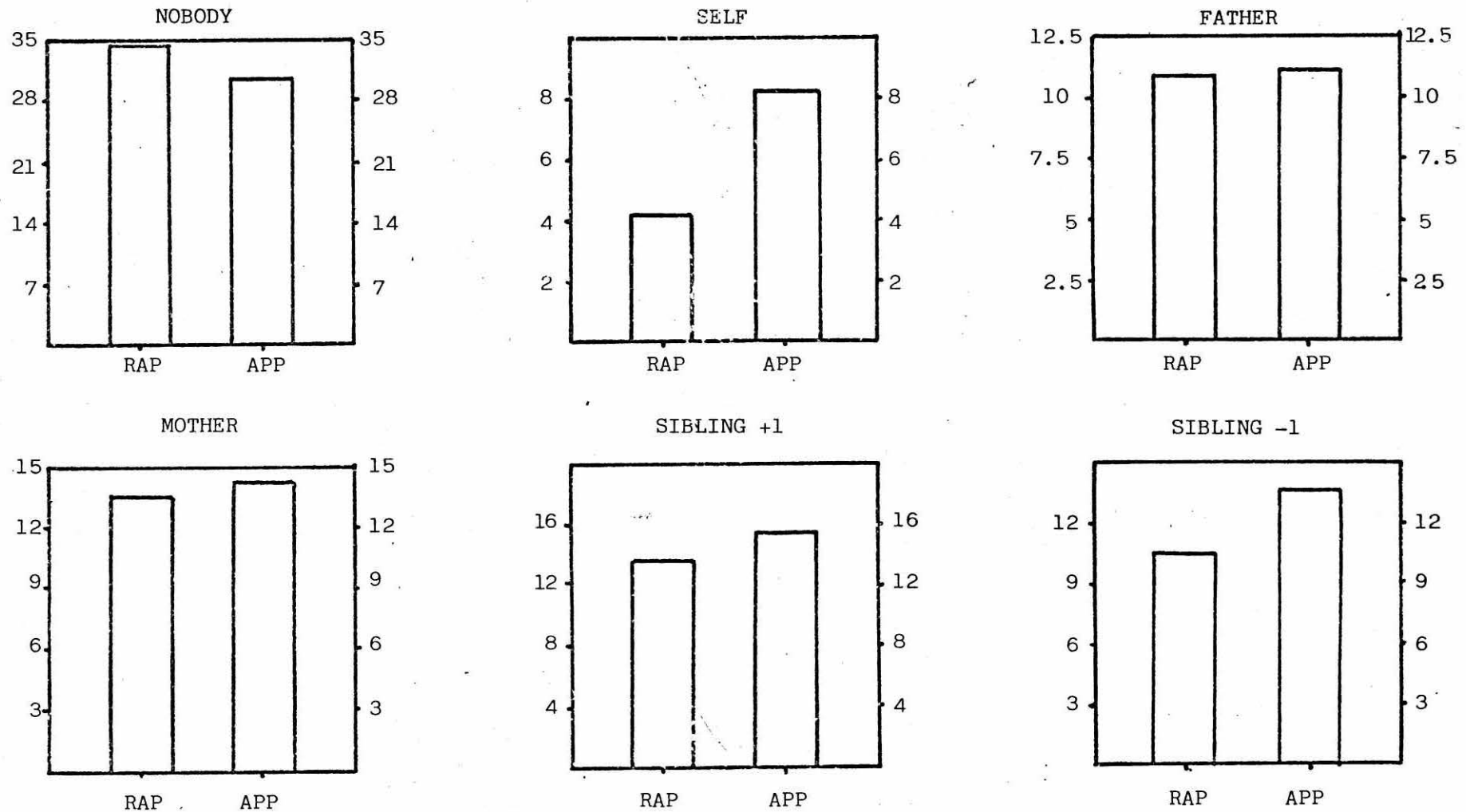
N/S = Not Significant

\* Significant at the 0.05 level (2 tail test)

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Figure 4.4.(e): Mean F.R.T. Total Involvement Scores



RAP = RECURRENT ABDOMINAL PAIN

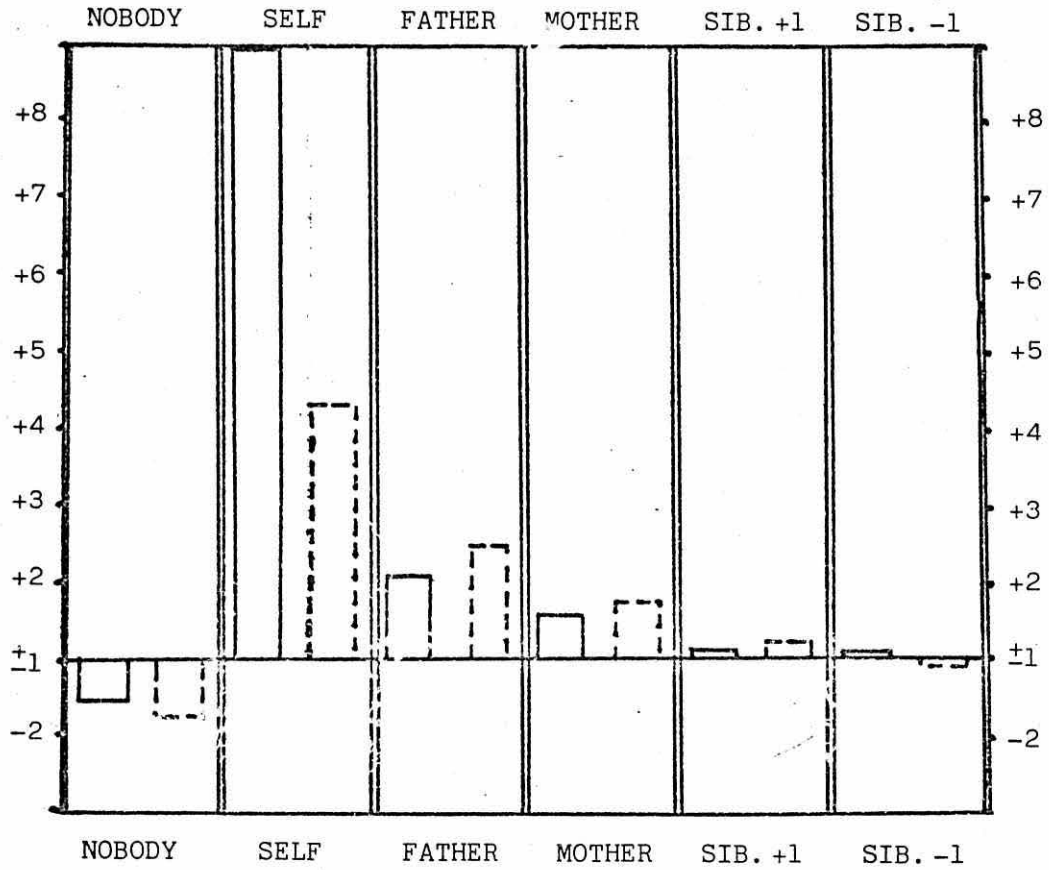
APP = APPENDICITIS

Table 4.4.(9) 'Mild' versus 'Strong' Feelings - Group Sub-scores

	RECURRENT ABDOMINAL PAIN				APPENDICITIS			
	MILD		STRONG		MILD		STRONG	
	TOTALS	MEANS	TOTALS	MEANS	TOTALS	MEANS	TOTALS	MEANS
NOBODY	199	9.95	309	15.45	174	8.70	302	15.10
SELF	18	0.90	2	0.10	43	2.15	10	0.50
FATHER	141	7.05	68	3.40	142	7.10	59	2.95
MOTHER	153	7.65	97	4.85	171	8.55	98	4.9
SIBLING +4	N/A	N/A	N/A	N/A	3	3.00	0	0.00
SIBLING +3	N/A	N/A	N/A	N/A	6	3.00	6	3.00
SIBLING +2	20	5	17	4.25	19	4.75	16	4.00
SIBLING +1	82	6.31	71	5.46	76	6.33	60	5.00
SIBLING -1	56	4.30	49	3.77	52	4.73	53	4.82
SIBLING -2	22	3.14	10	1.43	21	10.50	18	9.00
SIBLING -3	10	3.33	3	1.00	0	0.00	2	2.00
OTHER A	11	5.50	3	1.50	3	0.0	2	0.25
OTHER B	1	1.00	0	0.00	6	3.00	3	1.50

N/A = Not Applicable

Figure 4.4.(f): Data; Ratios Between Mild and Strong Statements Assigned to Particular Significant Others



 = RECURRENT ABDOMINAL PAIN

 = APPENDICITIS

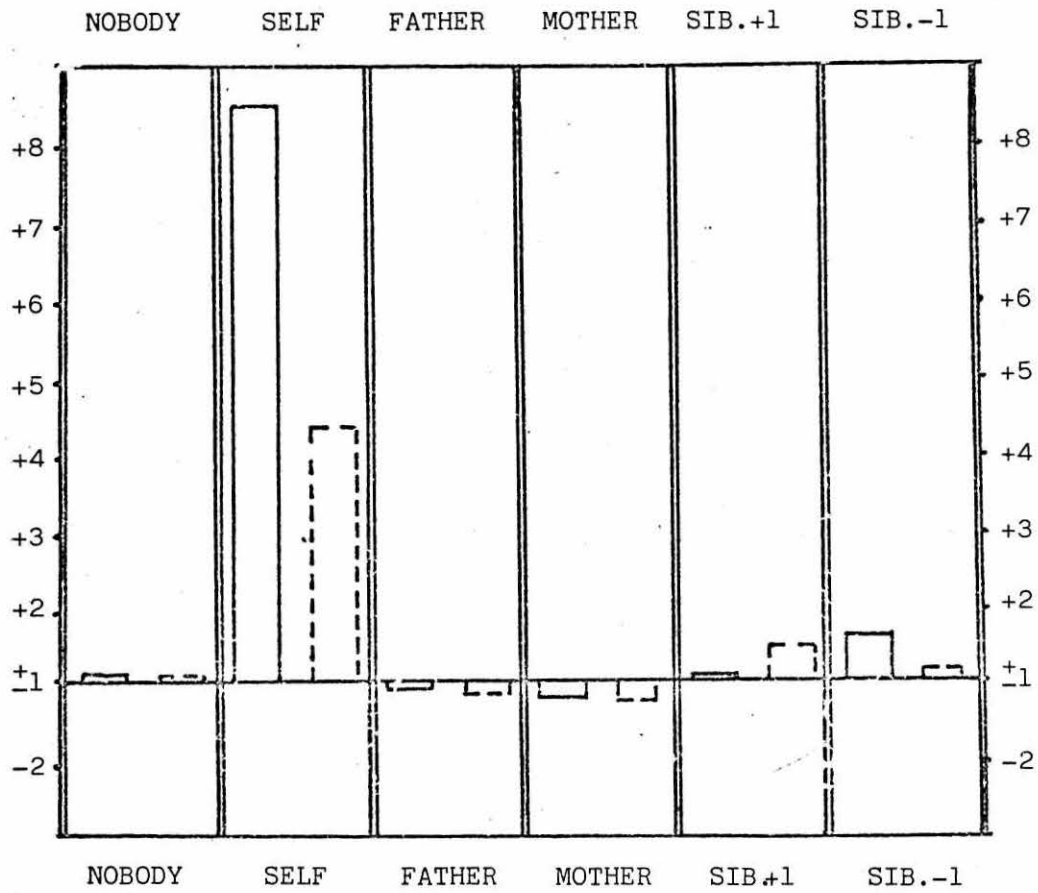
	NOBODY	SELF	FATHER	MOTHER	SIBLING+1	SIBLING-1
SUBJECTS	-1.55	+9.00	+2.07	+1.58	+1.16	+1.14
CONTROLS	-1.74	+4.30	+2.41	+1.74	+1.27	-1.02

Table 4.4.(10) 'Incoming' versus 'Outgoing' Feelings - Group Sub-scores

	RECURRENT ABDOMINAL PAIN				APPENDICITIS			
	INCOMING		OUTGOING		INCOMING		OUTGOING	
	TOTALS	MEANS	TOTALS	MEANS	TOTALS	MEANS	TOTALS	MEANS
NOBODY	272	13.60	236	11.80	239	11.95	237	11.85
SELF	17	0.85	2	0.10	43	2.15	10	0.50
FATHER	102	5.10	107	5.35	108	5.40	93	4.65
MOTHER	115	5.75	135	6.75	123	6.15	146	7.30
SIBLING +4	N/A	N/A	N/A	N/A	2	2.00	1	1.00
SIBLING +3	N/A	N/A	N/A	N/A	7	3.50	5	2.50
SIBLING +2	19	7.75	18	4.50	19	4.75	16	4.00
SIBLING +1	78	6.00	75	5.77	80	6.67	56	4.67
SIBLING -1	65	5.00	40	3.07	56	5.10	41	4.45
SIBLING -2	19	2.71	13	1.86	24	12.00	15	7.50
SIBLING -3	10	3.33	3	1.00	2	2.00	0	0.00
OTHER A	10	5.00	4	2.00	5	1.25	0	0.00
OTHER B	1	1.00	0	0.00	4	2.00	5	2.50

N/A = Not Applicable

Figure 4.4.(g): F.R.T.Data; Ratios Between Incoming and Outgoing Statements Assigned to Particular Significant Others



□ = RECURRENT ABDOMINAL PAIN

□ = APPENDICITIS

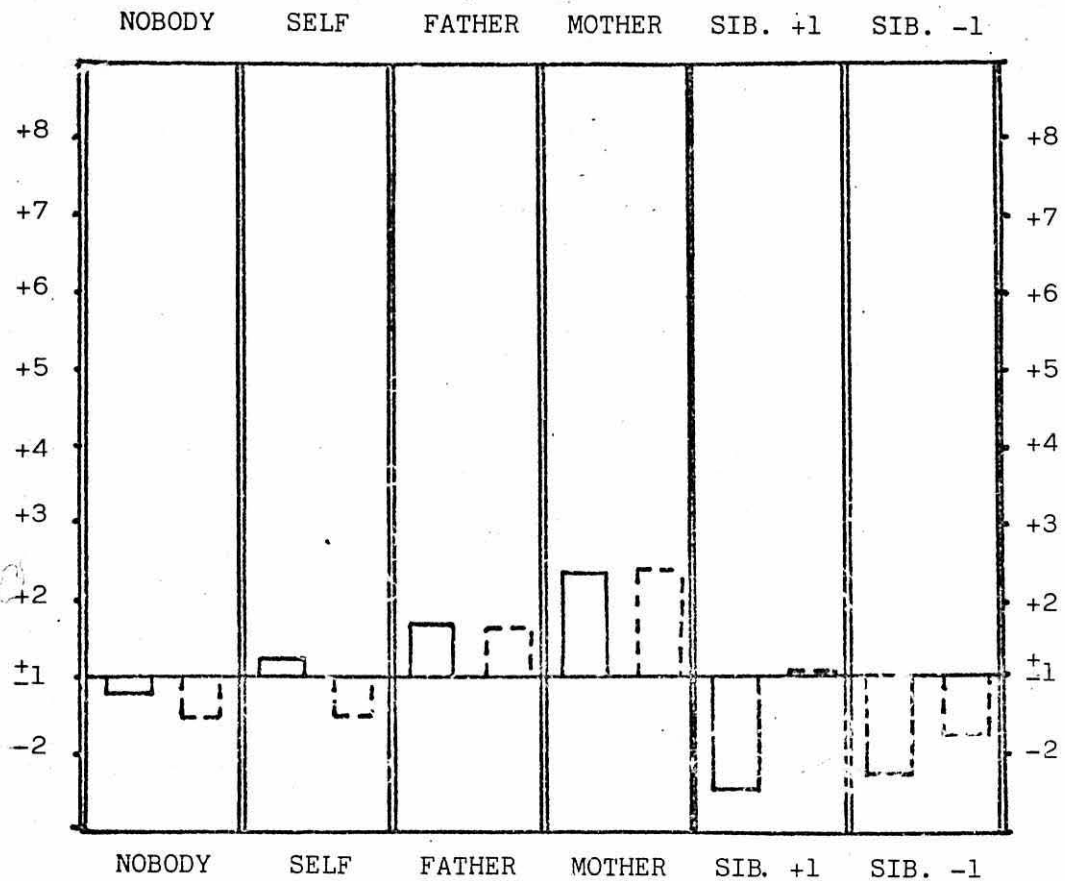
	NOBODY	SELF	FATHER	MOTHER	SIBLING +1	SIBLING -1
SUBJECTS	+1.15	+8.50	-1.05	-1.17	+1.04	+1.63
CONTROLS	+1.01	+4.30	+1.16	-1.19	+1.43	+1.15

Table 4.4.(11) 'Positive' versus 'Negative' Feelings - Group Sub-scores

	RECURRENT ABDOMINAL PAIN				APPENDICITIS			
	POSITIVE		NEGATIVE		POSITIVE		NEGATIVE	
	TOTALS	MEANS	TOTALS	MEANS	TOTALS	MEANS	TOTALS	MEANS
NOBODY	228	11.40	280	14.00	185	9.25	291	14.55
SELF	11	0.55	9	0.45	21	1.05	32	1.60
FATHER	131	6.55	78	3.90	125	6.25	76	3.80
MOTHER	178	8.40	72	3.60	194	9.70	75	3.75
SIBLING +4	N/A	N/A	N/A	N/A	3	3.00	0	0.00
SIBLING +3	N/A	N/A	N/A	N/A	2	1.00	10	5.00
SIBLING +2	9	2.25	28	7.00	7	1.75	28	7.00
SIBLING +1	44	3.28	109	8.38	71	5.92	65	5.42
SIBLING -1	28	2.15	77	5.92	38	3.45	67	6.09
SIBLING -2	29	4.14	3	0.43	23	11.50	16	8.00
SIBLING -3	11	3.67	2	0.67	2	2.00	0	0.00
OTHER A	11	5.50	3	1.50	1	0.25	4	1.00
OTHER B	0	0.00	1	1.00	5	2.50	4	2.00

N/A = Not Applicable

Figure 4.4.(h): F.R.T. Data; Ratios Between Positive and Negative Statements Assigned to Particular Significant Others



□ = RECURRENT ABDOMINAL PAIN

□ = APPENDICITIS

	NOBODY	SELF	FATHER	MOTHER	SIBLING +1	SIBLING -1
SUBJECTS	-1.23	+1.22	+1.68	+2.33	-2.48	-2.75
CONTROLS	-1.57	-1.52	+1.64	+2.39	+1.09	-1.77

is presented - such that in three cases it is actually the ratio of 'strong' to 'mild' statements which is shown. This convention was adopted to ensure direct comparability. Figures 4.4.(g) and 4.4.(h) present the corresponding data for 'incoming' versus 'outgoing', and 'positive' versus 'negative' feelings.

It is clear that these figures reveal a close correspondence between the Recurrent Abdominal Pain and Appendicitis groups. This is particularly apparent in figures 4.4.(f) and 4.4.(h). With regard to figure 4.4.(g) it will be noted that the ratios for 'incoming' versus 'outgoing' sub-scores are relatively low; suggesting that this index accounts for a smaller proportion of variation than do the other two.

This is confirmed by Table 4.4.(12) which, as an index of the amounts of variation apparent for each of these sub-scores presents the total group differences between 'mild' and 'strong', 'incoming' and 'outgoing' and 'positive' and 'negative' statements and presents these total difference scores as percentages of the total number of items employed in this section of the test. It will be noted that in all three cases the percentage difference scores for the Appendicitis subjects are higher than for the Recurrent Abdominal Pain subjects. Mann-Whitney 'U' tests, however, reveal none of these differences to be significant at the 0.05 level.



Table 4.4.(12) - Group Differences for Bi-Polar Sub-scores

	RECURRENT ABDOMINAL PAIN		APPENDICITIS	
	TOTALS	%	TOTALS	%
Mild vs Strong Statements	304	22.35	399	29.34
Incoming vs Outgoing Statements	74	5.44	137	10.07
Positive vs Negative Statements	314	23.09	383	28.16

Table 4.4.(13) presents the group scores for maternal over-protection, paternal over-indulgence and maternal over-indulgence, and figures 4.4.(j); 4.4.(k) and 4.4.(l) compare scores on each of these respective indices for the two groups. Again, these figures show close similarities between the two groups. It is noticeable, however, that for all three indices the Recurrent Abdominal Pain group produces higher scores for 'Mr Nobody' and lower scores for 'self' than do the Appendicitis subjects. Again, however, Mann-Whitney 'U' tests show that none of these differences are significant at the 0.05 level.

Table 4.4.(13) - Parental Overprotection and Overindulgence Group Sub-scores

	RECURRENT ABDOMINAL PAIN						APPENDICITIS					
	MOP		POI		MOI		MOP		POI		MOI	
	TOTALS	MEANS	TOTALS	MEANS	TOTALS	MEANS	TOTALS	MEANS	TOTALS	MEANS	TOTALS	MEANS
NOBODY	64	3.20	55	2.75	58	2.90	33	1.65	46	2.30	44	2.20
SELF	37	1.85	16	0.80	11	0.55	60	3.00	22	1.10	25	1.25
FATHER	8	0.40	1	0.05	5	0.25	9	0.45	2	0.10	2	0.10
MOTHER	2	0.10	13	0.65	0	0.00	1	0.05	11	0.55	1	0.05
SIBLING +4	N/A	N/A	N/A	N/A	N/A	N/A	1	1.00	0	0.00	0	0.00
SIBLING +3	N/A	N/A	N/A	N/A	N/A	N/A	0	0.00	0	0.00	0	0.00
SIBLING +2	3	0.75	4	1.00	1	0.25	1	0.25	6	2.50	1	0.25
SIBLING +1	10	0.77	2	0.15	12	0.92	10	1.50	13	1.10	12	1.00
SIBLING -1	17	1.31	0	0.00	11	0.85	27	2.45	5	0.45	11	1.00
SIBLING -2	10	1.43	8	1.14	4	0.57	4	2.00	1	0.50	2	1.00
SIBLING -3	11	3.67	0	0.00	1	0.33	7	7.00	0	0.00	1	1.00
OTHER A	0	0.00	0	0.00	0	0.00	3	1.50	0	0.00	0	0.00
OTHER B	0	0.00	0	0.00	0	0.00	0	0.00	0	0.00	0	0.00

MOP = Maternal Overprotection

POI = Paternal Overindulgence

MOI = Maternal Overindulgence

N/A = Not Applicable

Figure 4.4.(j): Maternal Overprotection; Group Mean Sub-Scores

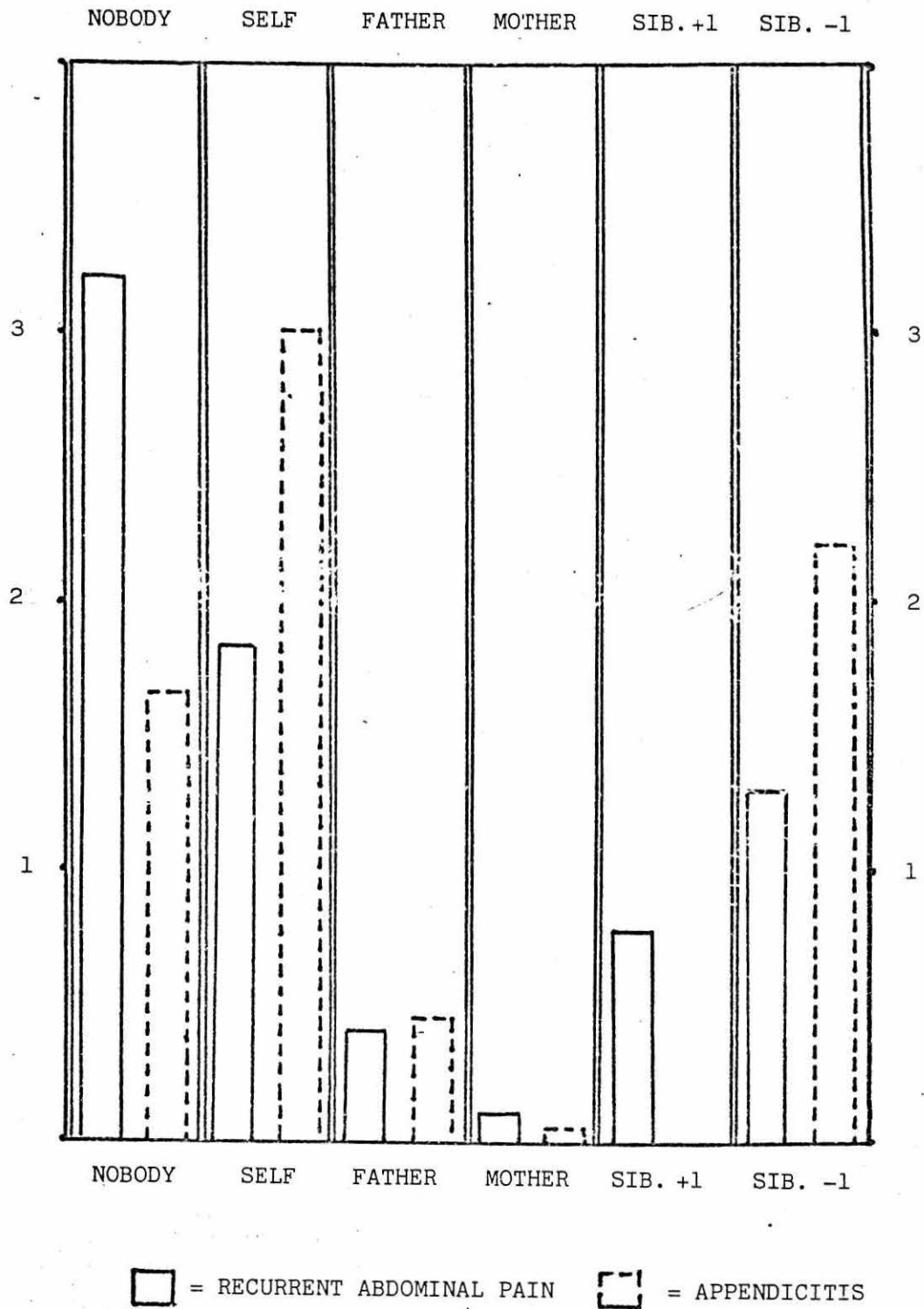


Figure 4.4.(k): Paternal Overindulgence, Group Mean Sub-Scores

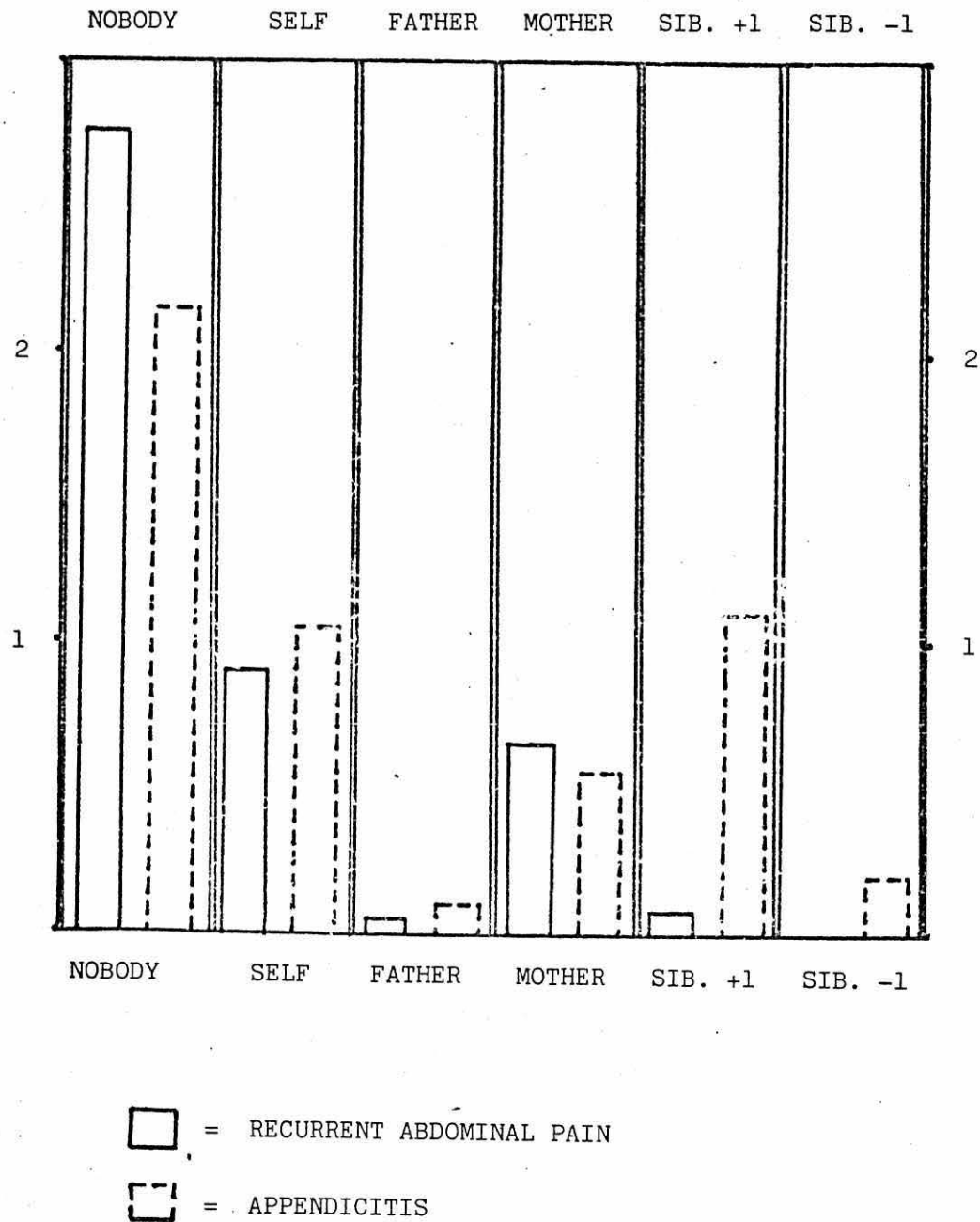
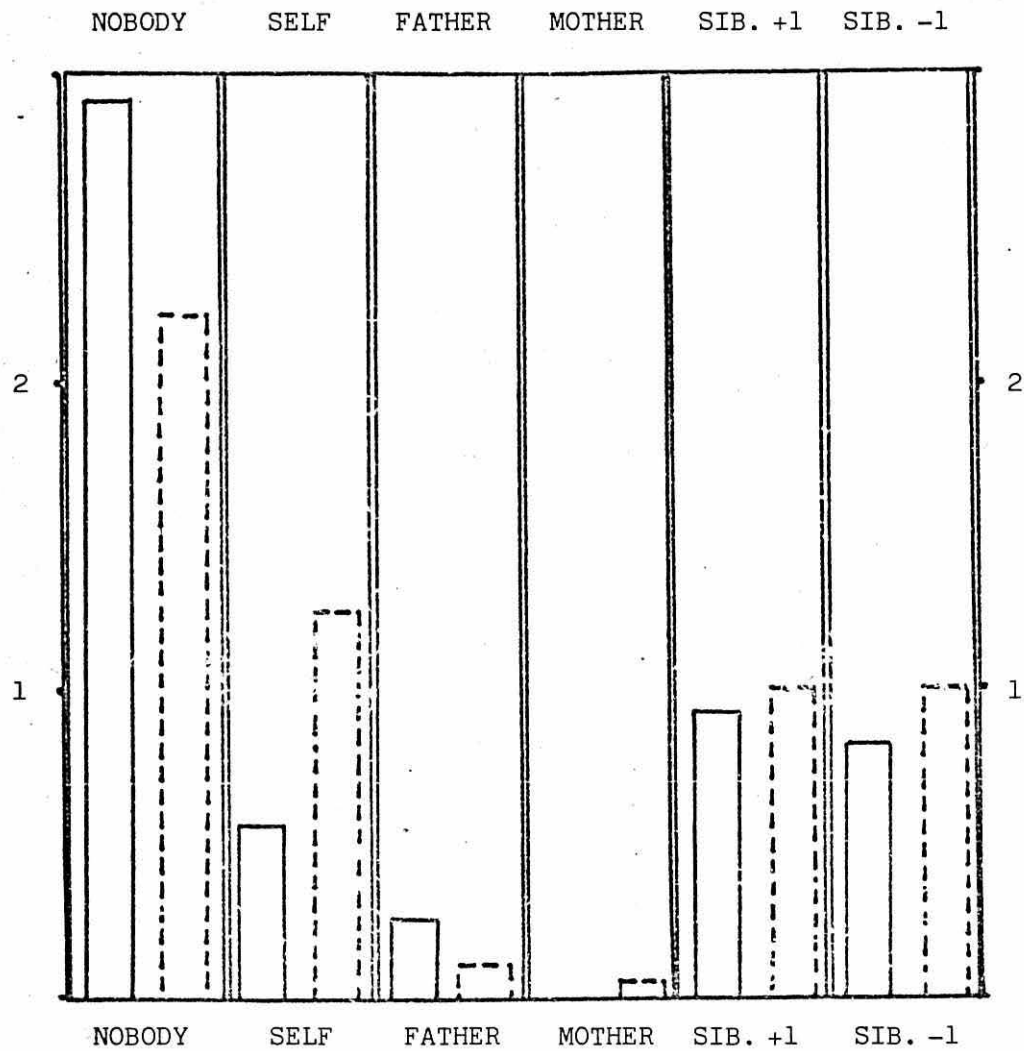


Figure 4.4.(1): Maternal Overindulgence, Group Mean Sub-Scores



□ = RECURRENT ABDOMINAL PAIN

□ = APPENDICITIS

#### 4.4.5. Rep. Test Findings

The variant of the Self-Identification Form of the Rep. Test used in this study was administered to 10 children suffering from Recurrent Abdominal Pain and to 10 who had recently undergone Appendectomy. Table 4.4.(14) presents data derived from the nine supplied constructs\*; recording, in each case, the pole which the children ascribed to themselves. These data were analysed using Fisher's Exact Probability Test, which was used in preference to the Chi-square test in accordance with the criteria suggested by Siegel (Siegel 1956, p. 110). As recorded in table 4.4.(14), differences statistically significant at the 0.05 level emerged for three of these supplied constructs. More Recurrent Abdominal Pain patients described themselves as 'Messy' rather than 'Tidy'; 'Confident' rather than 'Nervous' and as 'Failures' rather than 'Successes' than did appendicitis cases.

Table 4.4.(15) presents data illustrating a rather different kind of distinction between these two groups of children which also emerged from the analysis of their Rep. Test protocols. This table records the number of times that each child assigned him or herself to the 'implied pole' of any of the 21 triads which they were required to construe. The Table shows that the arithmetic means on this index were 5.8 for the Recurrent Abdominal Pain patients and 3.4 for the appendectomy patients respectively. A Mann-Whitney 'U' test revealed this difference to be statistically significant at the 0.05 level.

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\* Data for the final construct 'like Soccer' versus 'prefer Cricket' were not analysed and are not presented here.

Table 4.4.(14) - Rep. Test Supplied Construct Data

CONSTRUCT	SELF-DESCRIPTION	
	CONTROLS	SUBJECTS
INDOOR vs OUTDOOR	6 4	3 7
	N.S.	
TIDY vs MESSY	8 2	3 7
	*	
POPULAR vs ALONE	8 2	8 2
	N.S.	
CONFIDENT vs NERVOUS	1 9	5 5
	*	
LUCKY vs UNLUCKY	6 4	7 3
	N.S.	
LAZY vs HARD-WORKING	4 6	5 5
	N.S.	
CLEVER vs AVERAGE	1 9	2 8
	N.S.	
LIKE SCHOOL vs DON'T LIKE SCHOOL	4 6	7 3
	N.S.	
FAILURE vs SUCCESS	3 7	7 3
	*	

N.S. = Not Significant

\* Significant at the 0.05 level (2 tail test)

Table 4.4.(15) : Rep. Test Data - Self as Implied Pole

SELF AS IMPLIED POLE			
CONTROLS		SUBJECTS	
C 3	7	S 1	4
C 5	3	S 2	4
C 6	3	S 6	7
C 8	3	S 7	7
C12	3	S 9	3
C13	4	S10	3
C14	2	S15	11
C16	6	S18	8
C17	2	S19	7
C20	1	S20	4
	34		58
	3.4		5.8

#### 4.5. Discussion and Conclusions

##### 4.5.1. Clinical Histories

As was stated in Section 4.4.1., the clinical picture of Recurrent Abdominal Pain in Childhood which emerges from the paediatric examinations of the 20 children who took part in the present study, which were undertaken by Dr Davies, agrees well with earlier studies such as those of Moro (1913), Apley and Naish (1958) and Stone and Barbero (1970) summarised in section 4.1.1. (see especially Table 4.1.(1), page 00; and pages 00-00). The descriptions of both



primary and associated symptoms are essentially similar, and we can hence be confident that the 20 children who participated in this study are directly comparable to those who have participated in the earlier studies reviewed in Section 4.1.

#### 4.5.2. Eysenck's Theory of Personality

Given Apley's (1975) clinical impression that 'belly achers' tend to be 'highly strung', 'anxious', and 'over-conscientious'; and 'indrawn' rather than 'outgoing' one might have expected the Recurrent Abdominal Pain subjects in this study to score more highly on the Neuroticism (N) and Extraversion (E) indices of the J.E.P.I. than the Appendicitis 'controls'. As discussed in Section 4.2.1. (p.170) precisely the same hypothesis may be derived directly from Eysenck's theory of personality, which proposed that Introverts are unusually responsive to operant conditioning, whilst Neurotics are correspondingly responsive to classical conditioning, since (see Section 4.1.4.) it has been proposed in this chapter that children suffering from Recurrent Abdominal Pain are likely to be particularly responsive to operant conditioning of <sup>AUTONOMIC</sup> automatic functions. The data reported in Section 4.4.2. reveal, however, that no distinction can be drawn between Recurrent Abdominal Pain and Appendicitis patients in terms of their J.E.P.I. scores and, indeed, that those scores fall within the range of normative data reported by Eysenck (1965).

Three points emerge from the failure to support this hypothesis.

Firstly, one may draw attention to the failure of the J.E.P.I. data to replicate, in psychometric form, the clinical impressions of paediatricians presented by Apley (1975). This may, of course, be

taken as illustrative of the unreliability of such 'incidental' and 'unsystematic observations'. An alternative explanation, however, might lie in a mis-match between the intuitive criteria on which clinicians may base impressions of 'anxiety' and 'outwardgoingness' and the much more formalised and operationalised definition of 'Introversion-Extraversion' and 'Neuroticism-Stability' used by Eysenck and his colleagues in devising the J.E.P.I. and other psychometric measures designed to test his theory of personality. In particular the factor-analytic techniques used in deciding upon the inclusion or rejection of particular items within the different scales intended to measure these hypothesised dimension (see Section 4.2.1.) may be seen as leading to 'operational definitions' of 'Neuroticism' or 'Introversion' which are significantly removed from those employed in ordinary language. It may be, therefore, that the failure, in this instance, of J.E.P.I. data to support clinical impressions need not suggest that such impressions are of no real value, but rather that psychometric measures such as the J.E.P.I. do not necessarily represent suitable instruments for exploring their significance.

A second, but related, point concerns the level of generality at which the J.E.P.I. operates. An instrument which seeks to describe the range of characteristic individual differences in action and behaviour in terms of only two\* underlying 'dimensions' inevitably imposes a very broad focus in describing such person characteristics. Such a 'broad-mesh' may mean that less gross and simplistic distinctions are lost from view; and, for instance, that the

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\*Or three such dimensions in the case of Eysenck's later E.P.Q. instrument. (Eysenck 1972).

characteristics which clinicians refer to as 'fussyness' or 'over-conscientiousness' loose significance in the context of Eysenck's much broader and all-inclusive 'dimensions'. This argument could  
X suggest that instruments based upon Cattell's more 'fine-grained' personality 'factors' could prove more fruitful in the present area of study.

Thirdly, and perhaps most significantly, the data obtained in this study must cast some doubts upon the overall validity of Eysenck's theory of personality. The hypothesis under test in this study, that children exhibiting a somatic disorder held to represent a 'learned' behaviour generated and sustained by their social, and particularly their familial, relationships should display a characteristic pattern of J.E.P.I. scores, is one which may be derived directly from Eysenck's theory, and is certainly one which falls well within the theory's 'focus of convenience'. Logically, given the failure to find support for this hypothesis, we are left with three alternatives. Firstly, we may reject the notion that Recurrent Abdominal Pain does indeed represent a learned, non-organic, 'quasi-<sup>so</sup> psychomatic' syndrome. Given, however, the persistent absence in  
X studies of this syndrome of any organic findings which could explain the children's pain; and, in this study, the presence of psychological findings - derived from other indices - which clearly distinguish children exhibiting this syndrome from a second group who do not, this conclusion does not seem to be justified. Secondly, we might conclude that the validity of Eysenck's hypothesised connections between individual personality characteristics and the 'underlying conditionability' of the individual's nervous system has not been supported by this study. Thirdly, we might conclude that J.E.P.I.

data do not represent an adequate basis on which to test this hypothesis.

Of these last two hypotheses, the latter perhaps represents the most parsimonious resolution, given that the former would represent an outright refutation of Eysenck's theory, and also taking into account the reservations concerning the utility of the J.E.P.I. which have been expressed in this section. It should be stated, however, that to conclude that this hypothesis can not be adequately tested through administration of the J.E.P.I. does pose serious problems for Eysenck's theoretical position. Eysenck (see e.g. Eysenck 1957 or 1965) has consistently argued that, for example, psychodynamic approaches towards understanding the individual are deficient because they are not open to direct empirical verification or refutation, and that psychometric approaches such as his own are clearly superior in this context. The utility of Eysenck's inventories in allowing such empirical examination, represents an important plank in this, in any case very contentious, argument. The results of the present, albeit rather minor, study must significantly undermine this position.

#### 4.5.3. Vineland Social Maturity Data

The lack of any control data in this study, and the rather inadequate nature of the available normative data (see p. 174) limit the significance of the Vineland Social Maturity data presented in Table 4.4.(5); and these findings should hence only be accorded suggestive value. It should be noted, however, that the mean Social

Quotient obtained from the 17 Recurrent Abdominal Pain patients for whom data are available was 108.28 which compares with a hypothesised 'average' of 100. Given the lack of comparative data, this finding is probably best interpreted as illustrating that there is no evidence that this group of children was noticeably deficient in what Doll has called 'Social Maturity'.

#### 4.5.4. Self-Other Perception

In this section we discuss the findings which have emerged from both the Bene-Anthony Family Relations Test and from the Self-Identification modification of the Rep. Test used in this study.

In discussing data obtained from the F.R.T. attention should first be focussed upon the essential similarities between the data obtained from Recurrent Abdominal Pain patients and the scores of the Appendicitis controls. Tables 4.4.(9) - 4.4.(11) and, correspondingly, figures 4.4.(f) - 4.4.(h) reveal no noticeable differences in scores for the two groups in terms of each of the three bi-polar indices of 'mild vs strong', 'incoming vs outgoing' and 'positive vs negative' feelings. Further, Table 4.4.(13) and the corresponding figures 4.4.(j) - 4.4.(l) show that there are no significant differences, indeed that there are close similarities between the data obtained for the two groups on the maternal overprotection, paternal overindulgence and maternal overindulgence sub-scores. Finally, with the important exception of 'self' which is discussed in some detail later in this section, Tables 4.4.(6) - 4.4.(8) and figures 4.4.(c) - 4.4.(e) show that there are no significant differences in total involvement scores for any of the 'significant others' used in this test.

The failure to observe significant differences in family relations scores between these two groups is rather surprising. In Section 4.1.4. considerable attention was paid to the notion that in many cases the origins of Recurrent Abdominal Pain in Childhood may be traced back to the child's family situation. As early as 1913, Moro suggested that the disorder had its roots in the child's 'neuropathogenic domestic surroundings'. Similarly, Apley (1975) has stated that in a considerable proportion of cases the children belong to 'painful families'. Given the results recorded in Section 4.4.4. we can only conclude that the F.R.T. data reported in this study have failed to support, or to 'flesh-out', this picture.

We should notice however, that, rather as with the J.E.P.I. findings discussed earlier, it may be that the F.R.T. does not sample the most appropriate aspects of the child's familial relationships to shed light upon the phenomena to which Moro, Apley and others are alluding. Referring to Section 4.1.4. the suggestion that the origins of the child's pains may lie in his or her familial environment may be restated more specifically in terms of a hypothesis that the 'little belly acher's' parents, and perhaps others relations, are likely to be particularly responsive to complaints and the symptoms of pain. This is, in fact, precisely the view which emerges from the consideration of the ideas concerning autonomic conditioning put forward by Miller (1969)\*. Apley (1975) suggested that other members of the family may themselves be more likely to complain of pain. In a sense therefore, it could be argued that the 'model' considered in Section 4.1.4. rests more on assumptions about

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\* See particularly pages 160-162.

the attitudes of members of the child's family towards symptoms and complaints of pain rather than on assumptions about the quality of the relationships between the child and different family members. The Bene-Anthony test is primarily concerned with the child's emotional life within his or her family, providing indices of the individuals 'involvement' with each family member and the strength, direction and valence of the emotional relationships between the child and the individual family members and, hence, it need not follow that responsiveness to evidence of pain on the part of parents or others would necessarily affect these indices.

Notwithstanding this argument, however, it does remain rather problematic that no significant differences emerge between the two series in terms of the F.R.T.'s parental overprotection and over-indulgence indices. Perhaps intuitively rather than logically, it would seem likely that the kind of phenomena discussed in section 4.1.4. would be reflected in these sub-scores. We should notice however, that these three sub-scales comprise only eight, five and five items respectively. It might hence be unrealistic to expect to obtain statistically significant data from scales of this length which are, after all, primarily intended for clinical rather than statistical inspection.

It will be seen from sections 4.4.4. and 4.4.5., however, that two very interesting statistically significant findings did emerge from the Bene-Anthony test and the Rep. test respectively. These findings are of special relevance to this thesis in that they are concerned with differences between the two series in terms of self-other perceptions.

The significant difference which emerges from the Family Relations Test is that the Appendicitis control patients were almost twice as likely to assign statements to 'self' than were subjects in the Recurrent Abdominal Pain series. This is evidence that 'belly-achers' are either less able or less willing to make statements about themselves when asked to construe themselves amongst a range of other people with whom they are familiar.

An essentially similar finding is revealed by the self-identification modification of the Rep. test. In this case Recurrent Abdominal Pain subjects were significantly more likely to assign themselves to the implied pole of a construct than were the Appendicitis control patients. In comparison with the task presented by the F.R.T., in this case the individual is confronted by a situation where he or she must make an explicit comparison between him or herself and other people. The finding of this study is that in this situation Recurrent Abdominal Pain patients were significantly less likely to stress similarities between themselves and others than were Appendicitis patients.

These two findings are hence illustrative of a relative inability or unwillingness on the part of children suffering from recurrent abdominal pain to compare themselves with other people and to point to similarities between themselves and others. It is argued here that these findings may be interpreted as indicating a relative inability or unwillingness for these children to construe themselves as a person amongst other people. According to this view, it is as if these children can not or will not stand-back from their own experience and self-perceptions and construct a picture of themselves



which they can use in drawing comparisons between themselves and others. To use rather more 'technical' language, it is as if these children either can not or will not de-centre from their own experience, and hence do not have their self available as an object; to be construed in the same terms as other people with whom they are familiar.

Parallels with this view of a relative inability to de-centre, and to construe self as object, can be found in the work of at least two major psychological theorists; Piaget and Rogers.

Of particular relevance is Piaget's notion of 'egocentricity', a construct which he introduces as one distinction between pre-operational and concretely operational intelligence, and which he sees as characteristic of the pre-operational stage typical of children aged, roughly, between two and seven years. In Piaget's view the pre-operational child is 'egocentric' in that he or she cannot de-centre from his or her own perspective and construct an alternative viewpoint. Brown (1965) has characterised 'egocentricity' in this way:

"The pre-operational child is 'egocentric', that is he is captive within his own point of view, unwittingly captive. He is not aware of other points of view and so cannot assume another's point of view. Being unaware of other points of view he is necessarily unaware of his own - as a point of view. It is simply the way things are."\*

Brown, R. (1965) p. 220

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\* My underlining.

Piaget and his co-workers have provided a number of instances of such pre-operational egocentricity. For example, he claims that if a child at this stage of development who is walking away from his home is asked to show where his house is, he will point in the direction behind himself, but, if the same question is repeated to the child on his return journey he will again point behind himself. This is interpreted as illustrating that, for the pre-operational child, objects - in this case the house - are conceived as occupying a fixed position relative to the child's ego or self.

Similarly, Piaget and Inhelder (1948) report an experiment in which one hundred children aged between four and twelve were each seated at a certain position relative to a three-dimensional papier-mache model depicting an Alpine scene. The child was always seated in one particular position. A small wooden doll was placed in a number of other positions. The child's task was to indicate how the scene must look to the doll in each case. Piaget and Inhelder found that whatever the position of the doll, younger children attributed the perspective which they gained from their own position to the doll. In this instance it is as if the child cannot abstract his or her own self from a more generalised construction of reality, and hence is unable to compare his or her own viewpoint with one which is potentially available to another person sitting in a different position.

In Piaget's view, pre-operational children are 'captive to their own experience', and cannot de-centre themselves from this experience to examine their own point of view. It is argued here that the findings under discussion, derived from the Family Relations

Test and the Rep. Test, illustrate a similar relative inability on the part of 'belly-achers' when compared to the 'control' series of children.

Parallels with this view are also apparent in Carl Rogers' description of the stages which he views as characteristic of the development of the person through the process of psychotherapy. Rogers (1961) describes this as a seven-stage process. In his view, clients at an early phase of the psychotherapeutic relationship, particularly during Roger's first and second stages, may be characterised as being unable or unwilling to communicate about the self as an object. He described one aspect of the first stage as follows:

"There is an unwillingness to communicate self.  
Communication is only about externals.

Example: 'Well, I'll tell you, it always seems a little bit nonsensical to talk about one's self except in times of dire necessity'."

Rogers (1961) p. 132

and goes on to describe the general features of this stage in these terms:

"Differentiation of personal meanings in experience is crude or global, (.....). He does not communicate himself\*, but only communicates about externals. He tends to see himself as having no problems, or the problems he recognises are perceived as entirely external to himself.

Rogers (ibid.) p. 133

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\* Italicised is original.

The second stage of the process is also characterised as one where the client does not refer to him or herself as an object, and in Roger's view expression in this area only begins to 'flow more freely' during the third stage of the process. In summarising his overall picture of the psychotherapeutic process, Rogers states that:

"The process involves a change in the manner in which, and the extent to which the individual is able and willing to communicate himself in a receptive climate. The continuum runs from a complete unwillingness to communicate self to the self as a rich and changing awareness of internal experiencing which is readily communicated when the individual desires to do so."

Rogers (ibid.) p. 157

The findings under discussion here are clearly illustrative of a similar distinction to that which Rogers is trying to make in this passage.

This evidence of a relative egocentricity or reluctance to construe 'self as object' represents a clear psychological distinction which can be drawn between the Recurrent Abdominal Pain subjects and Appendicitis controls employed in this study.

A second, qualitative, difference between these two groups in terms of self-other perceptions also emerges from the Rep. Test data reported in this chapter. Table 4.4.(14) reveals three statistically significant differences in terms of the individual's self-ascription of the supplied constructs used in this modification of the technique. Significantly more 'belly-achers' than controls described themselves as 'failures' rather than 'succeses' and as 'messy' rather than 'tidy'. These findings could be interpreted as evidence of more

'negative' self-perceptions on the part of subjects rather than controls.

Conversely, however, 'belly-achers' were significantly less likely to describe themselves as 'nervous' rather than 'confident'. This appears to be a rather paradoxical finding, particularly given the previously discussed clinical impressions of these children. Perhaps it might be appropriate to interpret this finding as illustrating the use by these children of the ego-defence 'denial'.

*See Footnote on p. 237.*

#### 4.5.5. Conclusions: Psychological Correlates of Recurrent Abdominal Pain in Childhood.

As discussed in section 4.1.4. of this chapter, to date the most common paediatric explanation of the syndrome of Recurrent Abdominal Pain in Childhood has been to point to psychological factors within the child's personal make-up and within his or her family. Hitherto the evidence for this explanation of the syndrome as essentially a 'psych<sup>so</sup>matic' disorder has rested more on a lack of any evidence for a organic basis to the children's pains than on the presence of any positive evidence of any psychological correlates to these pains and associated secondary symptoms, beyond unsystematic 'clinical impressions'. Although the work of Miller and his associates on the concept of 'autonomic conditioning' has provided paediatric practitioners with a plausible description of how such a somatic disorder could be generated and maintained by the child's familial environment, there has been little or no previous evidence to illustrate broadly 'psychological' features which could be said to be characteristic of children suffering from this syndrome, and which might therefore help to explain why certain children might be particularly sensitive to such 'conditioning'.

Whilst the present study has been conducted very much on an exploratory basis, and on a very small scale, it could be said to have added significantly to this picture. It has provided positive evidence of the presence of psychological correlates to the disorder. Whilst more 'global' and behavioural indices, such as the Junior Eysenck Personality Inventory and the Vineland Social Maturity Scale have failed to demonstrate any statistically significant differences between a group of children suffering from this syndrome and a comparable series of Appendicitis patients, more 'subtle' phenomenological differences have emerged from the Bene-Anthony Family Relations Test and the Rep. Test. These suggest that such children may have relatively negative self-images, and that such children are less likely to have 'self available as an object', which they can use in making comparisons between themselves and other people. This latter finding is particularly well supported in that it has emerged independently from two quite different indices. Whilst it would almost certainly be unjustifiably bold to point to any frankly causative links between these findings and the children's susceptibility to the disorder, we should perhaps lay some emphasis upon Piaget's description of such 'egocentricity' as being a characteristic of an early or 'immature' stage in the child's intellectual development, and upon Rogers' description of seemingly similar phenomena as characteristic of people in the earliest stages of psychotherapy, and it could well be that further study might reveal these findings to have some genuinely explanatory importance.

In the earliest clinical study of the syndrome, Moro (1913) argued that the explanation for the disorder lay in the children's 'neuropathogenic domestic surroundings' and in their own 'neuropathic

constitution'. Miller's work on autonomic conditioning has provided a mechanism which could validate the importance which Moro placed upon the children's family. The present study has, it is hoped, begun to shed some light upon what Moro termed their 'neuropathic constitution'.

FOOTNOTE to p. 235

There is in the psychodynamic literature a group of hypotheses concerning what is called 'the psychosomatic phenomenon' which includes, in effect, the claim that people suffering from such disorders tend to lack constructs dealing with feelings and emotional experience [c.p. Stephanos 1979]. However, inspection of the constructs elicited from the subjects in this study does not suggest any clear differences between two samples of children along these lines.

Chapter Five: In Conclusion - Snark or Boojum?

"He remarked to me then," said the mildest of men,  
"If your Snark be a Snark, that is right:  
Fetch it home by all means - you may serve it with green's  
And it's handy for striking a light."

Lewis Carroll: The Hunting of the Snark

5.1. Introduction

The three immediately preceding chapters have described studies which have employed derivatives of the Self-Identification Form of Kelly's Repertory Technique. Each of these chapters has presented the particular study which it has detailed as a separate and self-contained undertaking. This final chapter is an attempt to pull-together some of the various recurrent themes of these three chapters to arrive at a position from which to assess the potential of this family of techniques in achieving the kind of science of human action which meets the criteria suggested in Chapter One (see, especially pages 19-22). As Lewis Carroll might have put it, the time has come to face the unpleasant possibility that the Snark which we have pursued over the last two hundred and more pages might, in the end, turn out to be a Boojum.

The chapter falls into two sections. Firstly, there is a consideration of what might be termed the more technical aspects of the three different applications of the Self-Identification Form employed in this study. Essentially, this section focusses around the question of whether the studies presented here have demonstrated that the technique does have potential as a flexible and useful way of collecting quantitative and qualitative data. Secondly we consider whether the data which have been collected, and the uses which



have been made of them, are consistent with the views of a 'genuinely' scientific psychology which will deal with significant aspects of human activity delineated in Chapter One.

## 5.2. A Flexible and Useful Research Tool?

In this section we consider the extent to which the three studies which have been reported here have shown the Self-Identification Form of Repertory Techniques to be a 'flexible' and useful research tool. Does this group of methodologies indeed represent an approach which can be applied to a variety of different research problems and yield illuminative data? The answer proposed here is a definite 'yes'.

Firstly, it should be noticed that each of the three studies has used the basic technique in a distinctly different way. In Chapter Two the Self-Identification Form was used in combination with a more 'conventional' psychometric measure to investigate individual preferences and choices. In this study, alone amongst those presented in this thesis, a 'grid' methodology was used - in such a way that it allowed the data from the two different research measures used to be 'combined' or 'pooled' in building-up a picture of occupational preference. The use of this grid allowed important correlations to be computed between self-perception and occupational preferences, in a way which had not been achieved in earlier studies in this area.

In Chapter Three, a quite simple adaptation of the Self-Identification Form of the Rep. Test was used to investigate individual 'strategies' of activity. An important point in this study was

that the research hypothesis under investigation made use of what might be called the 'test' characteristics of the technique. The - in the event seemingly justified - assumption was made that children would react to the tasks presented by the Self-Identification Form in much the same way as they might react to a school activity - such as perhaps a spelling test or, to cite Holt's own example, a 'game' of twenty questions.

Finally, Chapter Four employed an essentially similar Self-Identification format as part of a 'four-pronged battery' of psychological measures in an exploration of the paediatric syndrome of Recurrent Abdominal Pain. Emphasis should be placed here upon the quite different ways in which basically the same technique was used in this study and in that of 'failure at school'. Research attention did not here focus upon the ways in which the participants approached the Rep. Test as a test, but rather the data which were yielded from the technique were interpreted in such a way as to reinforce and reconfirm conclusions initially drawn from one of the other measures employed - the Bene-Anthony Family Relations Test. It should be noted that the different emphasis of the 'Failure in School' study was affected by a very simple change in the instructions given to the participants, i.e. the removal of the prohibition upon repeating constructs. It is held here that this point in itself represents an important illustration of the technique's inherent flexibility.

In both of the studies in which children and young adolescents acted as participants a series of 'supplied constructs' was presented; in an attempt to gain data relating to the 'qualitative' aspects of

their personal perceptions which would be comparable for all of the participants. Comments have been made about the direct relevance of these data to the topics under investigation in the relevant chapters. In this section it is, however, necessary to draw some comparison across the four groups of participants who each completed the same 'supplied constructs' section of the protocol. These four 'groups' were: 'dyslexics'; the dyslexia study school series; Recurrent Abdominal Pain patients and appendicitis patients. The relevant data are accordingly presented in Table 5.2.(1).

Given that Chapters Three and Four have described how the only statistically significant differences to emerge from these supplied constructs, within either of these studies, have occurred between Appendicitis and Recurrent Abdominal Pain patients (for the constructs 'Tidy vs Messy', 'Confident vs Nervous' and 'Failure vs Success' see pp 222 and 221) one might have anticipated that this table would reveal the Appendicitis group yielding data similar to those of the School and Dyslexia series'. The assumption here would be based on the idea that both the Appendicitis and School series' were intended to represent 'control' groups, and might hence be similar in their alleged 'normality' and different from the 'pathological' Recurrent Abdominal Pain group. In fact an inspection of Table 5.2.(1) reveals a rather more complex picture than this. For example on both the 'Tidy vs Messy' and 'Popular vs Alone' constructs the two 'Failure at School' series were both approximately evenly divided between the two poles. In contrast, on the 'Tidy vs Messy' construct there is a statistically significant difference between the two 'hospital' series with a preponderance of the Appendicitis patients assigning themselves to the 'Tidy' pole and a majority of

Table 5.2.(1) Supplied Construct Data, 'Failure at School' and Recurrent Abdominal Pain Studies

CONSTRUCT	SELF-DESCRIPTION			
	DYSLEXICS	SCHOOL SERIES	APPENDICITIS	RECURRENT PAIN
INDOOR vs OUTDOOR	4 16	0 20	6 4	3 7
TIDY vs MESSY	10 10	11 9	8 2	3 7
POPULAR vs ALONE	9 11	11 9	8 2	8 2
CONFIDENT vs NERVOUS	6 14	7 13	1 9	5 5
LUCKY vs UNLUCKY	10 10	11 9	6 4	7 3
LAZY vs HARD-WORKING	11 9	10 10	4 6	5 5
CLEVER vs AVERAGE	2 16	3 17	1 9	2 8
LIKE SCHOOL vs DON'T LIKE SCHOOL	7 13	10 10	4 6	7 3
FAILURE vs SUCCESS	12 8	6 14	3 7	7 3

See also Tables 3.3.(3), p 124; and 4.4.(14) p 221.

the Recurrent Abdominal Pain patients describing themselves as 'Messy', whilst on the 'Popular vs Alone' construct both hospital series show a clear preference for the 'Popular' pole.

The failure to support the possible assumption of a similarity between the two Control series' may be explained in a number of ways. Firstly, we must place emphasis upon the point that whilst both of these groups were chosen as representing 'control' series' it should be stressed that neither of them represents a statistically random sample of the general population - rather, they were each selected as fulfilling criteria which would allow a clear and valid distinction to be drawn with the two 'subject' groups; viz. - 'dyslexics' and Recurrent Abdominal Pain patients. Hence Appendicitis patients were chosen (see pp. 184-188) on the grounds that they represented a relevant comparison with 'belly achers' in that they had recently experienced a comparable 'traumatic' episode of pain, which differed in that it was of known organic aetiology. This represents a factor which the two hospital series had in common which was not shared by either the school or 'dyslexia' series'. Similarly, the school series were not a random selection of school children, but rather (see pp. 118-119) the investigator specifically sought to exclude children who could be said to be failing to live up to expectations.

Again, one might point to differences in the test procedures undertaken in the two studies. Most notably, participants in the 'Failure at School' study were only asked to complete one test whereas in the Recurrent Abdominal Pain study the Rep. Test was presented as the last of a series of tasks. This could conceivably account for some of these between-study differences. One could also point

to differences in the 'subject characteristics' of the participants in the two studies. In particular, those in the 'Failure at School' study were rather older, on average, than either of the 'hospital' series and they also involved a preponderance of boys over girls.

The differences which have become apparent in drawing direct comparisons between similar data presented in Chapters Three and Four may be taken as emphasising the essentially 'self-contained' nature of the studies reported in these respective chapters. They are concerned with quite different research problems and this has inevitably been reflected in their design. This point also serves to emphasise the basic flexibility of the Rep. Test as a research tool.

In this author's experience there is a tendency for workers with a slight acquaintance with Repertory Procedures to regard them as complicated, indeed perhaps cumbersome, techniques. It is worth making the point, therefore, that each of the studies reported here has employed essentially 'simple' versions of the technique - indeed two out of the three studies have relied solely on 'Rep. Tests' without any grid procedure being used at all. Whilst there is certainly nothing 'unconventional' about this aspect of the research reported here, it is probably the case that the majority of reported studies using Repertory Procedures have employed some of the 'higher-level' mathematical analyses alluded to in Chapter One (see p. 35). This is not seen as representing, in any way, an inadequacy or failure on the part of the investigator. On the contrary, adaptations have been devised which have been sufficient and adequate to suit the investigation and exploration of the topics in hand.

Indeed it may well be argued that there are real advantages in the use of 'simpler forms' - apart from the important fact that they remain comprehensible to the present, not especially numerate, investigator! In particular, the use of rather straightforward variants in Chapters Three and Four has allowed the use of techniques with children - some of whom have been as young as seven-years-of-age.

The amenability of Repertory Techniques to sophisticated mathematical treatment does represent an important factor in favour of the basic procedure. Indeed, it enhances its basic 'flexibility'. The studies reported here have however emphasised that the simpler forms may often have a utility and validity of their own. A rather similar approach has been adopted by Duck (1973) in his work on friendship formation.

In conclusion it does appear that the three studies which have been reported here have suggested that the Self-Identification Form of Repertory Technique does indeed represent an extremely flexible research tool, and one which may be administered in a simple and quick way to a variety of different kinds of people. Importantly, Chapters Two and Four have also pointed to its potential value as part of a 'battery' of techniques, suggesting that its use is compatible with that of more common 'psychometric' or indeed 'projective' approaches and that it may significantly reinforce and extend the data available from such other sources. The most important point, in assessing the utility of this family of techniques on the basis of this series of three small-scale studies, is that they have each been 'successful' in the sense that the variants employed have

yielded clear-cut data which have significantly illuminated the topics under discussion. When we consider the disparate nature of these topics - choice and preferences between different occupational roles; the supposedly negative effects of 'schooling' on a child's potential to learn; and the possible psychogenic origins of children's recurrent abdominal pains - and the point that in none of these studies did the research draw a blank, it is apparent that this thesis has strongly suggested that this family of techniques is appropriate for studies involving a wide range of different research areas and theoretical problems.

### 5.3. Towards a Science of Human Action

The above conclusion could, of itself, be enough to justify the research which has been reported here. However, Chapter One set distinctly more grandiose objectives, those of shedding some light upon the technique's potential as part of a truly 'effective psychology' - a science of human action. We are now in a position to consider whether the studies reported here do indeed point the way toward such an endeavour.

It is relevant at this point to restate the principal conclusions and questions to emerge from the introductory chapter.

On pages 19 and 20 the view of an 'effective science of human action' was summarised in the form of two main conclusions, viz:

- "i) That an effective science of psychology would have as its aim not the restrictive goal of an understanding of the 'behaviour of organisms', but the more inclusive one of an understanding of the experience of being human, which



would take as its starting point our everyday knowledge of human action.

- ii) That such a science would not necessarily be primarily 'experimental' and laboratory-based but would, like other disciplines, permit and encourage 'exploratory' research which would expand and refine our existing common sense knowledge."

From this basis, on page 22 the two fundamental questions to be addressed through this thesis were stated as:

- "i) Is it in fact possible to construct an empirical psychology which is in accord with the views of scientific endeavour which have been exemplified by Harré and Secord?
- ii) Further, can such research methods build upon not only our 'common knowledge' of human action, but also upon the insights which have been gleaned through existing or traditional approaches?"

These 'desiderata' of such an effective psychology may be re-stated in terms of an area of empirical study which takes as its objective the goal of improving our understanding of the experience of being a human agent, accepting that such an understanding should seek to incorporate both 'common knowledge' of human action and also existing 'traditions' within the discipline of psychology. In seeking this goal the psychologist need not slavishly follow the narrow positivist vision of scientific research being restricted to the laboratory 'manipulation' of supposed 'dependent' and 'independent' variables, but should regard him- or her-self as being free,

where appropriate, to adopt what Harré and Secord have described as an 'exploratory' approach.

In the few pages which remain it is argued that the studies reported in Chapters Two, Three and Four do indeed suggest that psychological research may be carried out, using Repertory Procedures, which does conform with this interpretation of scientific activity; and indeed that they have demonstrated that such research may be clearly seen to have extended our empirical knowledge of the topics under consideration. We turn first to consider the extent to which the research which is reported herein may be said to have built-upon common knowledge, whilst remaining clearly within the tradition of academic psychology. Finally, we shall move on to consider the 'exploratory' nature of the work reported here, and particularly that described in Chapter Four.

There are three senses in which the research reported here may be said to be rooted in our existing or 'common' knowledge of human experience. One of these is perhaps slightly trivial but is intrinsic to the nature of repertory procedures. A second sense arises from the criterion adopted in selecting the research problems to be considered - i.e. that they should be of some interest or significance to the 'concerned layman'. The third, and perhaps most significant, sense considered here is the extent to which the studies reported here have been compatible with the 'anthropomorphic' model of man, proposed by Harré and Secord and discussed in Chapter One.

To address the first of these themes, one of the basic justifications which Kellians have put forward for the use of Repertory

Techniques is the point that they are built around the person's own language and perceptions, in short around his or her own personal construct system. In seeking the person's bi-polar constructs the investigator may be seen as recognising the value and validity of the individual's own description of his or her experience. Looked at in this way, Repertory Procedures cast the participant as the 'expert' on his or her experience. Hence, for example, in Chapter Two the participants were asked to apply a series of descriptions which they themselves had provided, to a range of occupational roles, such as 'accountant' or 'journalist'. The 'accuracy' or otherwise of these descriptions was not at issue, the person's own constructions were seen as having a value and a validity of their own and became, in a very real sense, the focus of study.

In this, albeit perhaps rather limited, sense therefore the three studies reported here may be seen as taking common - indeed 'personal'-knowledge as their initial starting point. In this, sense these studies have quite certainly accorded such descriptions a significance and a degree of respect which would not arise from the perspective of a radical behaviourist.

Turning to the second sense in which these studies have been rooted in everyday or lay perceptions of human activity, as early as page 2 of this thesis it was stated that 'the topics which were under investigation; namely a student's choice of a career; a school child's difficulties with spelling and reading, or a child's recurring and unexplained belly-aches, were issues which were of significance not only to the investigator and to the scientific community

to whom he hoped to communicate his results, but they also meant something to the participants themselves', whilst on page 39 it was stated that one criterion adopted in selecting research topics was that 'the broad problem to be investigated should have some significance for the layman'.

In none of these three studies is it difficult to substantiate the case that the topic under investigation had 'meaning' for the participant as well as the investigator. In Chapter Four the child's recurrent pains had 'sufficient meaning' for him or her to have been referred to a hospital paediatric unit and, similarly, in Chapter Three the child's perceived 'failure' to live-up to educational expectations had been accorded sufficient significance by parents and/or teachers for them to be referred to a highly specialised unit attached to a University department! Whilst the issue of occupational choice perhaps had a less dramatic form of significance for the participants in the study reported in Chapter Two than did the issues studied with either of these groups of children, it was certainly one they were well aware of - and indeed one having consequences which, through their choice of a vocationally relevant course of study, they were living with from day-to-day. This also represents a sense in which each of these studies may be seen as paying respect to common notions of human experience.

Perhaps the issue here is clearest when the hypothesis under investigation in Chapter Three is considered. In essence this was derived from the writings not of an academic psychologist but of a practising teacher - indeed John Holt's books grew out of a series of memos which he shared with colleagues at his own school, and they

in fact contain occasional disparaging remarks about 'psychologists' as a profession. This point is an important one in terms of the objectives of this thesis, for this study has clearly used the Self-Identification Form to provide an empirical extension of the essentially 'lay' insights which Holt's books provide. This is a very long way indeed from the scorn which Skinner poured upon the 'flight to laymanship'.

The third sense in which these studies may be said to have built-upon our everyday notions of human experience is probably the most crucial in terms of the concerns of this thesis. This centres around the extent to which these studies may be said to have been congruent with Harre' and Secord's 'anthropomorphic model of man' which was discussed in the introductory chapter and which is itself seen as drawing upon our commonplace notions of human agency. It is argued here that this thesis has in fact shown the Self-Identification Form of Repertory Procedure to be consistent with an approach which accepts this view.

In Section 1.2.1. it was argued that Kelly's Personal Construct Theory, from which the techniques at the core of this thesis have been derived, is of itself essentially compatible with the notion of human agency which was derived, principally, from the work of Harre' and Secord. Further, it was argued that Role Construct Repertory Technique represented a methodological tool which itself incorporates many of the important aspects of the notion of human agency, and in particular that the rather neglected Self-Identification variant of these techniques might hold especial promise in that, in focussing around the individual's perceptions of him- or her-self,

it may allow us to encompass something of the individual's 'sense of personhood'. The argument here is therefore that the methodological technique employed in each of these studies is itself derived from an image of human activity which is closely congruent with the anthropomorphic model of man.

It may also be argued that each of these studies has employed concepts which taken the notion of human agency seriously. Hence, the study reported in Chapter Two hinges around the notion of 'occupational choice' and in the event has also pointed to the possible theoretical significance of recognising a distinction between occupational choice and occupational preference. Whilst it is the case that a traditional behaviourist might in fact refer to a notion of 'choice' - when, for instance, discussing a 'choice point' on an animal maze - it is argued here that the notion of occupational choice does, of itself, tend to stress the concept of human beings as agents - that is as individual's empowered to initiate and change the manner and objectives of their activities.

Similarly, Chapter Three has focussed upon a number of ideas, notably the notion of children inventing 'strategies' for dealing with their school work, and also of them seeking to avoid instances of 'failure', which could be said to be couched in what might be termed a language of human agency.

Finally, Chapter Four deliberately adopted a broadly eclectic approach, taking into account physical, behavioural and phenomenal descriptors of the children under consideration. In a real sense, this study was concerned not simply with the children's pains but

with the child as a person. It is extremely interesting (not to say fortuitous!) therefore, that the significant findings to emerge from this study do in fact emerge from the two broadly 'phenomenal' measures and indeed could be said to focus quite specifically around the young individual's awareness of him- or her-self as a person amongst other people. Viewed in this way, this particular study has stressed that not only does the Self-Identification Form of Repertory Procedure represent a technique which allows us to take the notion of human agency seriously, but it also stresses the potential importance of us doing so.

In addition to these three senses in which the criterion of paying respect to the significance of our commonplace knowledge of human experience, have been met, it is clear that these studies have also met the author's requirement that such research should seek to build-upon existing traditions within academic empirical psychology. The study reported in Chapter Two, for instance, clearly relates to an existing and reasonably extensive body of research literature. This study, and the earlier one carried out by this author (Knasel, 1976) belong squarely within the same family as those of Shiner (1963), Oppenheimer (1966) and Bingham (1966). Similarly, the study reported in Chapter Three also drew upon existing psychological research into dyslexia and also made some use of Skinner's ideas about education. Finally, much the same can be claimed about the study reported in Chapter Four. Whilst little or no previous frankly psychological work has been carried-out into this pressing problem of paediatric practice, the issues have been treated in such a way as to employ some of the existing ideas in psychosomatic medicine and, most notably, the work of N.E. Miller

and his colleagues upon 'autonomic conditioning'.

The claim being made here is rather an ambitious one. It is that the three studies reported here have illustrated that the Self-Identification Form of the Rep. Test is compatible with research which takes seriously both everyday conceptions of human action and also existing traditions within the academic discipline of psychology. This thesis has represented one of an increasingly widespread series of attacks upon the hitherto prevailing supremacy of the positivistic experimental approach to empirical psychology. The main thrust of this attack, as laid down in Chapter One, has been to criticise the prescriptions of many laboratory-based psychologists as being derived from an outmoded, inadequate and seriously inaccurate vision of scientific endeavour. So far in this section, we have addressed the extent to which this interpretation of science has ignored the fundamental basis of 'everyday' knowledge and the associated claim that it has in consequence imposed, to borrow Shotter's phrase, an inadequate 'image of man'. Harré and Secord also make the claim that this positivist tradition have also placed quite unnecessary and potentially debilitating restrictions upon research methodologies. In particular (see pp 8-9 of this thesis) they have pointed-out that workers in other scientific disciplines frequently pursue activities which are non-experimental in the sense that they do not focus around critically checking what is already known but which, on the contrary, may be described as 'exploratory' in the sense that they instead focus around extending what is known.

All three of these Repertory Procedure-based studies could be said to have something of an exploratory flavour. It is Chapter



Four, however, where this is most clearly the case. It is relevant to again quote Harré and Secord's description of 'exploratory' work:

"In exploratory studies a scientist has no very clear idea of what will happen, and aims to find out. He has a feeling for the 'direction' in which to go (increase the pressure and see what happens) but no clear expectations of what to expect."

Harré and Secord (1972) p. 69

This would seem to be a very good description of the study reported in Chapter Four! The author had two rather vague 'hunches' - neither of which, in the event, proved fruitful. Firstly, he expected to find differences between the two groups of children in terms of their J.E.P.I. scores; and, secondly, he anticipated that there might be some differences between the two groups in terms of their 'family dynamics'. He had no clear idea at all as to what the Rep. Test would reveal, except for some expectation that the 'supplied constructs' would suggest some broad differences in terms of 'morale'. The statistically significant findings which did emerge were in no way anticipated.

In a sense the investigator could have been said to have been 'lucky'. Nevertheless, this study has illustrated the potential value of the Self-Identification Form of the Rep. Test as an exploratory tool, and at the same time has underlined the importance which Harré and Secord have placed upon this category of research activity.

In conclusion, in this section we have seen that it does appear, from the studies reported here, as if the Self-Identification Form of Repertory Procedure does permit research which is broadly consistent with the criteria for an 'effective psychology' introduced

in the first chapter. The research reported here has drawn upon our existing 'lay' knowledge of human experience and has also incorporated ideas from existing traditions within psychology. It has also paid attention to what had been referred to as an 'anthropomorphic model of man'. It has, in effect, gone a long way towards demonstrating the techniques' compatibility with what has here been termed a 'truly scientific study of human action'. It is fervently to be hoped that many other research methodologies may also demonstrate such compatibility. In the meantime, this series of studies has suggested that such a science of human action is indeed possible.

In this thesis we have, rather like Lewis Carroll's Bellman and his colleagues, pursued the Snark of a science of human action. Let us hope, therefore, that on no occasion has 'the bowsprit got mixed with the rudder' for, despite the misgivings of the radical behaviourist, the indications are that this Snark may not be a Boojum after all.

POSTSCRIPT

"'Tis a pitiful tale," said the Bellman, whose face  
Had grown longer at every word:

"But now that you've stated the whole of your case  
More debate would be simply absurd"

Lewis Carroll THE HUNTING OF THE SNARK

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