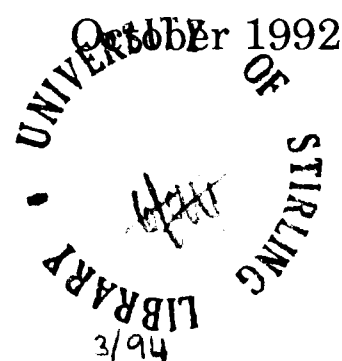University of Stirling

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Thesis
2332.

Origins and Development of

Representational Systems

in Early Childhood

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This dissertation is dedicated

to the memory of Monsieur J. P.,

the 'onlie begetter of these insuing' works

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CONTENTS

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I would also like to acknowledge the encouragement and support of my colleagues here, particularly Professors Ivana Markova, who had the ill luck to be my supervisor and who gave me the kind of supervision I desired, and Roger Watt, who supplied the kind of supervision I needed, and the constant encouragement I have received from former student Dr Julia Dockrell (London School of Economics), from my associates Robert Grieve and David Olson, from my former teacher Margaret Donaldson (University of Edinburgh), from my former colleague Professor Philip Smith (University of Reading), and from my long-suffering wife Victoria Nash, who has had much to put up with during the summer preceding submission.

To all of these persons I owe a debt of gratitude which I shall try to discharge in the years to come.

Abstract

It is argued in Chapters 1 to 4 that in cognitive psychology in general, and in the disciplines of language acquisition and cognitive development in particular, there is substantial benefit to be derived from distinguishing between two representational systems, one system being deployed in long-established or highly-practiced functions, and the second deployed in novel tasks, or where difficulties interrupt the first system. It is also argued that the proper subject of cognitive development is the second of these systems.

Chapters 5 and 6 are concerned in different ways with the origins of language in the individual, in particular with the question of what innate knowledge of language might be justified. It is concluded that many questions regarding innate knowledge remain open, and that a source in human evolution for knowledge of language is no more likely than sources in individual or social development. In Chapter 7 it is argued that representational drawing emerges late in the 4th year of life, and some new techniques are described for studying early representational drawing.

Following these treatments of external systems of representation, Chapter 8 offers a general developmental theory of forms of representation, extending Piaget's insight that mental representation is co-extensive with thought, and that the main axis of cognitive development is the content of thought and representation. Chapters 9 to 12 apply this theory to the representation of belief and desire, and of extrinsic and intrinsic qualities of objects, by $1\frac{1}{2}$ to 4 year-old children. Chapter 13 introduces a new method for analyzing the free classification task, a task sometimes used to assess children's ability to think about intrinsic qualities, and applies this method to various data sets.

Chapter 14 applies these insights and results to the problem of characterizing concepts and concept development and favourably discusses the idea that more precise knowledge of this aspect of development may help to explain certain features of early language acquisition.

Foreword

This dissertation is submitted under the provisions of regulations A7.4 and A7.5 of University Ordinance 23. Regulation A7.4 states that 'Work published, in press, or submitted for publication, may be included in the thesis, provided the status of such work is stated in the candidate's thesis'. Regulation A7.5 states that 'Work carried out in collaboration with others may be included in the thesis, provided that the nature and extent of such collaboration are specifically acknowledged in the candidate's thesis'.

Chapters 1 through 6, and Chapter 8 are based closely on published papers, and the details of publication are specified in each case in a footnote. I have made alterations to the text of these chapters only where it seemed necessary because of passage of time since the original time of writing, or in order to make connections with other chapters within this dissertation. Chapters 1 and 6 contain closing postscripts in which I attempt to identify developments subsequent to the publication of the original papers which affect, or appear to affect, the conclusions drawn.

Chapters 9 and 10 are based closely on papers which have been submitted for publication, and Chapter 14 is based on a paper in press; again, the details are given in footnotes. Depending on the time interval between submission of this dissertation and its examination, papers based on Chapters 7, 11, 12 and 13 may be submitted for publication in the intervening period. I undertake to advise the examiners of any such submissions.

So far as regulation A7.5 is concerned, my Acknowledgements recognise collaborators, and specify the extent of collaboration, in respect of Chapters 5,7,8,10,11 and 12. In addition, I affirm that (except for quoted texts) the text of the dissertation is entirely my own work. In the case of Chapters 5 and 8, where some assistance with writing is acknowledged, this assistance took the form of revision of the original draft papers, which I had produced.

Regulation A1.1 of Ordinance 23 requires a Ph.D. candidate to demonstrate

the ability 'to conduct original investigations, to assess ideas critically, and to relate his (*sic*) investigations to a wider field of knowledge'. I believe that most of the chapters demonstrate ability to assess ideas critically; Chapters 7 and 13 present methods of investigation (of early drawing and object sorting respectively) which I believe to be novel; Chapters 10 to 12 present novel findings relating to the representational capacities of two and three year-old children. Chapters 1 and 5 were included with the third requirement of A1.1 - construed as one of scholarship - particularly in mind; their inclusion perhaps merits this explanation, since Chapter 1 is to some extent dated, and Chapter 5 is somewhat peripheral to the main drift of the dissertation.

In a dissertation based on published work, and completed on a part-time basis, punctuated by leaves of absence, as this one has been, it is difficult to achieve the level of coherence, singleness of purpose and economy of expression normal in a Ph. D. dissertation. During the period of registration, the focus of my research has shifted from the area of language and language acquisition to the more general, and possibly more fundamental, area of early cognitive development as it relates to representational systems; the contents of my dissertation plainly reflect this shift of focus; they also reflect my enthusiasm for a multi-disciplinary approach. I hope, nevertheless, that the residue of continuity and coherence is sufficient to satisfy the reader that this product of my middle years is 'a coat of many colours' rather than 'a thing of shreds and patches'.

Affidavit

Finally, in accordance with Regulation A7.2 of Ordinance 23, I affirm that, subject to the exceptions noted above and in my Acknowledgements, the dissertation is entirely my own work, and has not been included in any other thesis.

<center>Introduction and Overview</center>

Introduction

This introduction will be very brief, since many of the ensuing chapters (particularly Chapters 1 and 8) contain lengthy introductions.

In one of the oldest and best developed branches of psychology - the study of memory - it is now a commonplace that different memory systems must be distinguished (e.g. Tulving, 1985a), and clever methods have been developed to demonstrate the dissociations between memory functions that are implied by these distinctions, for instance those drawn by Tulving between procedural, semantic and episodic memory systems. Tulving (1985b) perhaps goes further than others would go in associating the different memory systems with different types or levels of consciousness, but this willingness to face up to the problem of consciousness has since spread to neighbouring fields of research, such as neuropsychology (Milner and Rugg, 1991).

The common purpose of the reviews, essays, experiments and methodological proposals which compose this dissertation is to make the case for drawing analogous distinctions within developmental psychology, and to argue that the proper subject of cognitive development is the developing system (or systems) of mental representation which allows the consciousness of the child to reach out from the here and the now to the past, the future, the remote and the possible; and to reach in from the goals of actions and the individual function of objects to the inner structures of actions and objects.

There is little that is genuinely original in the theoretical claims that I shall make: many of my proposals find their origins in Piaget's voluminous writings. The same disclaimer might be made, however, by those who have tried to introduce distinctions linked to consciousness into the study of memory or brain injury, and especially by those, such as Byrne and Whiten (1988), who have tried to introduce such distinctions into the comparative

psychology of intelligence. In these other fields, for Piaget we may substitute Bartlett, Luria, Köhler, etc. Perhaps there are cracks in the latest of the succession of monistic monoliths - behaviourism, information-processing, and now 'cognitive science' - but to be sure of this, someone must try to drive in a wedge!

Representation in the broadest sense of the word is found wherever we look in psychology. The residue of experience entailed by memory, imitation, learning or perceptual recognition may be counted as a kind of representation of that experience; computational analyses of perceptual, linguistic and cognitive processes seem to entail representations of the various constructs involved in the computations (in the words of Fodor (1975) - 'no computation without representation'); in traditional areas of cognition - recollecting, planning, reasoning, imagining - the mind is always focussed on some remote entity, and must therefore represent it.

However, - obeying Lloyd Morgan's Canon (1894) - just as we should be wary of extending the last-mentioned model of cognition to animal intelligence, so we should be cautious in extending it from its natural domain of application in <u>human</u> psychology to other psychological functions (cf. the caution of J.J Gibson with respect to perception and of B.F. Skinner with respect to learning and language), and also cautious of extending it to the earliest signs of intelligence in the child (cf. the caution of Piaget).

Accordingly, if the notions of representation and representational system are to have distinct and useful meanings, then criteria must be sought to distinguish cases of genuine representation (where something in the mind stands for something external) from cases where it is true only that matters may be described <u>as if</u> that were true.

I will make no attempt to define an appropriate concept of representation or representational system here, since these concepts will undergo development in the course of the dissertation. However, it is worth observing

that in the various attempts by philosophers to identify criteria needed to define such concepts for the domain of external representations (e.g., Goodman, 1969) only one criterion is at all plausible for the case of mental representation, namely <u>causation</u> (mental representations are caused by the thing represented). <u>Resemblance</u> (mental representations resemble the thing represented) was tried and found wanting by the Gestalt school of perception, and in any case such resemblances could hardly be examined. <u>Intention</u> (mental representations are intended to stand for the thing represented) fails because it begs the question of who intends them to be representations (Perner, 1991, p. 21). Of course, although <u>causation</u> certainly might be satisfied by representations needed to account for memory, imitation and learning, it could hardly be satisfied by those needed to account for imagining. Because of these obvious difficulties, more recent philosophy has explored other possible criteria, e.g. <u>proper function</u> (Millikan, 1984) and/or <u>capacity for misrepresentation</u> (Dretske, 1988). A modest discussion of these suggestions in relation to my own will be offered in the Afterword.

Overview[1]

It is argued in Chapter 1 that in cognitive psychology in general, and in the special disciplines of language acquisition and cognitive development in particular, there is substantial benefit to be derived from distinguishing between two different systems of representation, one system being deployed in long-established or highly-practiced functions, and the second system being deployed in novel tasks, or in other situations where special difficulties interrupt the smooth operation of the first system. Chapters 3 and 4 explore this argument in different contexts, identifying difficulties that are encountered if such a distinction between representational systems is not drawn, and problems that can be solved with the help of two such systems. Chapter 2 is included to support the sketch of an important argument against

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[1] Included since my Abstract is restricted to 300 words by Regulation.

over-interpretation of early speech which is offered in Section 3 of Chapter 1.

Chapters 5 and 6 are concerned in different ways with the origins of language in the individual, in particular with the question of what innate knowledge of language might be justified, either on the basis of linguistic research, comparisons with other species, or psychological investigations. It is concluded that many questions regarding innate knowledge remain open, and that seeking a source in anthropoid evolution for knowledge of language is likely to be no easier than seeking sources in individual or social learning.

In Chapter 7 it is argued that representational drawing emerges late in the fourth year of life, and some new techniques are described for identifying the earliest representational drawings.

Following these discussions of external systems of representation, Chapters 8 to 13 address the origins of systems of mental representation in the early pre-school years, such systems being systems of the second type identified in Chapters 1 to 4.

It is argued in Chapter 8 that it is such systems of mental representation that enable us to think about entities of various kinds, and that attribution of systems of mental representation of this or that kind (i.e., having this or that content) can be done safely only on the basis of evidence of deployment under difficult circumstances; for example, that attributing the ability to think about an individual object (and hence ascribing to the subject a mental representation of it) should wait upon evidence that the child can think about an absent individual object; that attributing the ability to think about a property of an individual object (and hence ascribing to the subject a mental representation of that property) should wait upon evidence that the child can think about such a property when it is not presently manifested by any object, or at least not salient in any present object; that attributing the ability to think about a proposition (and hence ascribing to the subject a mental representation of that proposition) should wait upon evidence that the child

can think about a proposition which she believes to be <u>false</u>.

Chapters 9 to 13 apply the ideas developed in Chapter 8 to different aspects of early cognitive development. Chapter 9 discusses recent work on the representation of beliefs and desires in terms of the content of the beliefs and desires. Chapters 10 and 11 report experimental studies of word knowledge in which the salience of the properties which these words denote is manipulated. Chapter 12 examines the ability of two year-old subjects to match objects while manipulating the manifestness and salience of properties. Chapter 13 reports an attempt to improve a traditional method for investigating representation of properties, the free classification task.

Finally, Chapter 14 returns to the themes of Chapter 1 (particularly to the developmental puzzles identified in Section 4 of Chapter 1) and attempts to apply some of the insights and findings of Chapters 8 through 13 to these themes.

Chapter 1: Language Acquisition and Cognitive Development[1]

1. Introduction

To explore the relationships between cognitive development and language development is to enter a very dark forest indeed! It is not so much a question of not being able to see the wood for the trees: one cannot even see the trees. Accordingly, the best advice one might offer to, say, a graduate student would be 'Danger, keep off'. For those with more leisure and securer positions it is perhaps possible to make an occasional foray without becoming entirely lost, but it should be emphasized that what is both desirable and possible in the study of language development at the present time is more facts, more flower-picking natural history. However, it is sometimes useful to make the attempt at a larger enterprise, if only as a source of ideas about where to look for new flowers.

The large mass of research with a bearing on this relationship has been reviewed by Bowerman (1976) and in a number of publications by Cromer (e.g. 1974, 1976a,b). I think it can fairly be said that one thing missing from these reviews is any sign that a coherent theoretical framework informs current work on the relationship. Indeed, the same might be said of psycholinguistics in general. There is a coherent framework on offer, exposed in Chomsky's (1975, 1980a,b) and Fodor's (1975, 1983) writings and in their various contributions to Piattelli-Palmarini (1980). Whatever the virtues of this framework, it has become increasingly nativist with the passing years and has won few hearts and minds amongst those actively engaged in the study of language acquisition or cognitive development. Why then has so little progress been made towards an alternative framework? It is my firm view that a principal reason has been the failure all round to square up to the task of allocating a distinct role to consciousness, as it is involved in speaking, understanding, thinking and learning. Conscious mental processes are

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[1] Published as Campbell (1986), being a revision of Campbell (1979).

typically not distinguished from other cognitive processes either structurally (e.g. in terms of their temporal properties or the information that they manipulate) or functionally (e.g. in terms of what kinds of purposes of the organism they serve). While the role of consciousness in these activities was a feature of European psychology until the mid-1930s (as may be readily seen in the work of Brunswik, Bühler, Piaget, Stern, Vygotsky and Werner), it failed to survive the flight across the Atlantic. In 1934 Vygotsky wrote in the opening paragraphs of his Thought and Language :

> 'All that is known about psychic development indicates that its very essence lies in the change of the interfunctional structure of consciousness. Psychology must make these relationships and their developmental changes the main problem, the focus of study, instead of merely postulating the general interrelation of all functions. This shift in emphasis is imperative for the study of language and thought.'

In this chapter I have chosen to explore some of the reasons why we find it difficult to satisfy Vygotsky's imperative and to attempt to persuade the reader that the effort is nonetheless worthwhile, in the hope that some genius will come forward to pick up the intellectual burden laid down fifty years ago by Vygotsky and ten years ago by that great European stay-at-home Piaget.

2. Cognitive Development

What is cognitive development and how should it be studied? There is widespread disagreement about this. One can discern two clear positions, one represented in mainstream American psychology, the other in the genetic epistemology of Jean Piaget. In the first approach, cognitive structures and processes are identified with symbolic structures and processes (often called information structures and processes), which mediate the connection of outputs from sensory mechanisms with inputs to motor mechanisms. Thus every action of the organism beyond the simplest reflex is said to involve cognitive processes. In Piaget's system, cognitive structures

and processes are identified with representations and operations upon representations that are tied in an intimate way to explicit knowledge and awareness; thus, only certain functions in certain organisms are said to involve cognitive processes. The use of the qualifier 'explicit' in the previous sentence will seem strange to those who are not accustomed to the peculiarities of the information-processing idiom. In that idiom it is commonplace to speak of 'tacit' knowledge in circumstances where the justification for calling something a cognitive event is noticeably lacking. Thus, Chomsky often speaks of tacit knowledge of rules of language. What is meant by this is of course that the rules in question are <u>not</u> known but merely observed. There are several possibilities here. It might be that the rules of language are represented in the mind of the speaker in a fairly direct way, but are inaccessible to consciousness. This is what Chomsky means by 'tacit knowledge'. On the other hand, although falling apples observe the law of falling bodies they are not accused of having tacit knowledge of that law - but then, apples (falling or not) know nothing explicitly either. More to the point, apples are not capable of independent action; they contribute nothing to their fall beyond certain physical properties. Put another way, it is not <u>necessary</u> for them to know anything, tacit or not, about the laws of motion, in order to fall in the way that they do: they merely have to <u>be</u>. Most organisms, on the other hand, <u>are</u> capable of independent action. They are driven by motives the origins of which are remote and complex. The ape's controlled descent of a tree is mediated by something more than its physical properties. It seems to be necessary that the ape should <u>know</u> something about the shape of the tree, the disposition of its branches, the shape of its own body and the pull of the earth in order to descend in the way that it does. It is conceivable that we could, by considering possible trees, possible apes and actual dynamics, devise a theory of tree descents amounting to a set of rules, which rules are followed by our ape in the course of his descent. Since common experience

and the study of language tell us that (a) there are rules of language and (b) in using language we adhere to them but are not aware of them, it is possible that the ape is similarly unaware of the structures and rules that guide his progress down the tree and hence knows them only tacitly. It is, however, more likely that just as we know <u>something</u> about the rules of our language and exercise this knowledge on <u>some</u> occasions[2], the ape, too, has as a back-up some knowledge of the rules for descending trees and sometimes employs it, for example, when in fog, in unusual trees, broken limbs (ape) or broken limbs (tree) (see, remarkably enough, Chevalier-Skolnikoff, Galdikas and Skolnikoff, 1982).

So we may think of our tree user (or language user) as exploiting two kinds of knowledge, explicit and tacit, and functioning in two modes. Where action is governed by explicit knowledge I shall describe the organism as functioning in a <u>phenic</u>[3] mode (characterized by phenic structures and processes); when not functioning in this way I shall describe the organism as functioning in a <u>cryptic</u> mode (characterized by cryptic structures and processes). Thus, in the phenic mode action is regulated by structures and processes accessible to consciousness; in the cryptic mode it is regulated by structures and processes inaccessible to consciousness. While certain other symptoms are typically clustered with consciousness, such as mental effort (cf. Kahneman, 1973) and reportability (cf. Dennett, 1978: ch. 9), there are good grounds for introducing new terminology. Briefly, (1) the proposed terms have clear interpretations from the point of view of the experiencing subject - what is evident to the subject is <u>phenic</u>, what is hidden is <u>cryptic</u>; (2) it is central to my proposal that the distinction drawn should be <u>theoretical</u> and potentially applicable to infants and to other organisms. Thus, <u>effortful/</u>

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[2] See Chapter 4.

[3] This terminology is adapted from Whorf's <u>phenotype</u> and <u>cryptotype</u>.Whorf used these terms to denote grammatical categories which had an overt and covert formal basis, respectively (see e.g. Whorf, 1945).

effortless and conscious/unconscious fail, since these are simply the usual empirical marks of the distinction; reportable/unreportable is not only empirical but limited to language users; lastly, existing theoretical oppositions such as higher/lower and inner/outer have unfortunate anatomical implications. My strategy in employing this theoretical distinction will be to try to show that it solves problems for us, rather than to show that it has a direct empirical justification - since this latter route has been pursued long and hard without success. I should expect the distinction to turn out to be only conditionally associated with each of its usual empirical symptoms.

It might be wondered why I do not simply use the terms 'explicit' and 'tacit'. It seems to me best to reserve these for describing representations, since that has been their customary use. Moreover, mental acts which psychologists have described as governed by representations or involving computations in a representational system seem to be co-extensive with what philosophers, following Brentano (1874), have called intentional acts - acts directed on some object. For the notion of mental representation - qua semantic entity which is the staple medium of cognitive processes- see Fodor's writings (1975, 1980b, 1981) and for some problems with it see Dennett (1983b) . For Brentano, intentionality was 'the mark of the mental'. Dennett (1978, 1981a,b, 1983a) has pursued this line of attack with great patience and sophistication, seeking to characterize different sorts of intentional system and to explore the grounds for describing various organisms and devices as this or that sort of intentional system. Now, Chomsky's claim about tacit knowledge, echoed by many other psychologists and by Dennett, can be seen as a claim that many of our acts, though cryptic in my terms, are nevertheless intentional in the sense of Brentano, because they involve computations in a tacit system of representations. Chomsky describes such acts as involving 'cognizing' (1980a: 70) intending by that term to denote

processes that appeal to knowledge, whether explicit or tacit, and to exclude processes which do not. This claim of Chomsky's and my linking of it to the notion of intentionality are extremely controversial (see Stabler (1983) and the commentaries by Rosenthal, Searle and Sober on Chomsky (1980b) for discussion), and it may be that consciousness and intentionality will turn out to be more closely linked than they seem to be in the reading of the Chomskyan position just offered. For the moment, however, it seems best to allow that consciousness (my suggestion) and intentionality may define quite different conceptions of 'the mental'.

We have arrived at a point where it is possible to frame what, for me at any rate, is the fundamental question of cognitive psychology. The question is 'What criteria determine the attribution of explicit knowledge?' Or, in the terms of the preceding paragraph, 'How can we know that we are dealing with a phenic process or structure?' For adherents of the information-processing approach to cognitive psychology this question is not fundamental: an answer will be provided, if at all, only when a theory is completed. For what an organism knows explicitly is regarded as epiphenomenal (i.e. devoid of causal significance).[4] However, for Piagetians and their like, the question is fundamental since it affects description of psychological events. Theory cannot begin until a way is found of deciding when to attribute explicit knowledge and what knowledge to attribute. It should be evident that there are no easy answers to this fundamental question. There is available neither a convincing refutation of epiphenomenalism nor a general method for determining criteria for explicitness; on the other hand, information-processing analyses are bedevilled by paradoxes and absurdities. For example, perceptual and inferential processes often receive identical treatments and manipulate

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[4] With respect to the modified position attributed to Chomsky, there is a different fundamental question, i.e. what criteria determine the attribution of 'cognizing'?

structures with identical descriptions.[5] Again, no formal distinction (there is often some lip-service paid) is or can be made between an automatic routine process which carries out a certain function and a deliberate, conscious process which is 'called in' to carry out the same function when the routine process fails. The experimental/ statistical requirements of reliability, replicability and low mean variance have led to widespread adoption of techniques which involve either lengthy periods of practice or many experimental trials per subject, with the result that the functions which are studied, though normally functions carried out - if at all - by phenic processes, are in fact observed only as cryptic routines.

This has had several disastrous consequences: (a) 'cognitive psychology' - which once had a clear meaning, denoting the psychology of phenic structures and processes - has slid through a period of ambivalence into its present appalling state[6], being now almost exclusively concerned with cryptic processes; (b) success in modelling these cryptic processes has given rise to the illusion that we have achieved some understanding of the (normally phenic) functions that they carry out; (c) valid work carried out within older or more peripheral traditions has been mistakenly called in question by robust mountains of data which in fact pertain to quite different processes. The most striking and unfortunate case of this is the persistent and breathtakingly insensitive impeachment of Genevan results in the field of cognitive development by American and, lately, British psychologists;[7] (d) onlookers from neighbouring disciplines encounter insurmountable difficulties in evaluating psychological research. Philosophers of mind, for

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[5] Escape from this paradox is not easy. The suggestion is sometimes offered (see, e.g., Brunswik, 1956) that whereas errors of perception - illusions - are incorrigible, mistakes of reasoning can be corrected. But this is fallacious, since mistakes of reasoning are corrected only by further reasoning.

[6] What is 'appalling' is of course not the content of modern cognitive psychology - which is perfectly sound and scientifically respectable, but the fact that it has lost its connection to remembering, imagining, thinking, planning and so forth.

[7] This is discussed in Chapter 8.

example, who - to my mind correctly - often tend to discount the usefulness of 'images', 'concepts' and 'meanings' in epistemological contexts, are repelled and bewildered by the welter of such notions employed in cognitive psychology In fact, of course, the notions (sometimes just the labels) have usually been misappropriated and applied to cryptic structures which have only some (at best) of the properties of the phenic structures to which these terms were originally applied.

In the case of models of the adult we are constantly reminded by our own nature of the need to be clear about which mode we are describing, and can often determine independently of any empirical criteria what sort of process or structure is being described. Thus, for example, in Forster's (1976) analysis of the lexical decision task as a massive search process we are sure that any such search is a cryptic process and that the role of the phenic mode in such a task (once initial orientation has been achieved) is simply to hold constant the ad hoc links established between perception, memory and action. Hence we learn nothing from such work about cognitive structures and processes (if the term cognitive is restricted to its traditional referential domain, as I am urging that it should)[8]. However, in studying cognitive development we lack such empathetic guidance and are often at a loss. We thus encounter difficulties and paradoxes which, I believe, can only be resolved by adopting a framework of the kind that I am advocating and, ultimately, by discovering methods of investigation and descriptions which acknowledge such a framework. I will describe in detail only one such troublesome phenomenon - space perception in infancy - but similar difficulties arise in many disparate aspects of cognitive development, for example in perception of weight (Mounoud and Bower, 1974) and classification (Campbell, Donaldson and Young, 1976): for relevant reviews

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[8] Henceforth, unless otherwise indicated, the term 'cognitive' will be used in this restricted sense.

see Donaldson (1971) and Bower (1974b).

Brunswik (1956) elucidated a crucial distinction, originally due to Koffka (1935) and later elaborated by Heider (1939), between distal and proximal stimulus variables. To illustrate: the distance of an object from the eye (a distal variable - i.e. a goal of description) is partially specified by the values - strictly speaking, simple functions of these - of various proximal variables: retinal expansion, motion parallax, ocular convergence, lenticular accommodation, etc. Successful discrimination of variation in distance is thus achievable on the basis of sensitivity to variation of these proximal variables. Which proximal variable will vary in the appropriate way will depend upon the circumstances. However, successful discrimination does not tell us anything about how such proximal variation is interpreted by the infant; still less does it tell us that it is interpreted as variation in the distal variable - in this example, distance. Various remedies have been proposed (see Bower, 1974a: 79ff; Yonas and Pick, 1975: *passim*). Bower makes two suggestions. First of all, it is known that infants habituate to any constant and regular stimulation. If their discriminations are based upon variation in distance rather than on proximal variation, then a slight change in the task which shifts the basis of discrimination from one proximal variable to another should not interrupt habituation. Otherwise, it should. Bower claims that methodological difficulties get in the way of attempting this ingenious experiment. His second suggestion is not accompanied by any argument: he states baldly (1974a: 80) that

> 'faced with problems of this sort, one feels that natural response
> methods are a refuge. If an infant reaches out for an object
> intentionally . . . there can be no doubt that the infant sees the object
> in the third dimension. This kind of simple certainty we just cannot
> get from discrimination experiments.'

While one might well agree with this, the problem of determining whether or not a reaching movement is intentional (i.e. distally aimed) is not

conceptually different from the problem (to be solved) of whether a spatial discrimination is distally based. Yonas and Pick likewise offer two suggestions, that on the one hand distally invariant stimulus presentations which have variable proximal outcomes and which elicit a common reaction (stimulus convergence, in their terms) and, on the other hand, proximally diverse response movements with an invariant distal outcome and a common eliciting stimulus (response convergence, in their terms) each indicate that perception is distally oriented, since other explanations of such convergence are grossly unparsimonious.

There are many things that remain unclear here. Is distal orientation a necessary or a sufficient condition for a claim about explicit knowledge? Do all or any of the suggested remedies constitute sufficient conditions for a distal claim? It seems to me that there is still room for doubt. Is there not in the behaviour of a butterfly landing on a flower a co-ordination and integration of sensory information and a flexibility of motor response that equally invites the inference that the butterfly has constructed a representation of space and objects in space with reference to which the sensory data and motor commands are interpreted? However, despite these uncertainties some things are clear. Able practitioners like Bower, Pick and Yonas evidently recognize the need to distinguish at least two qualitatively distinct levels of function of the organism, one which involves direct sensori-motor connections and another which requires an intermediate interpretative structure which gives meaning to the sensory data and purpose to the motor commands. Moreover, there is a laudable reluctance in the field as a whole to fudge the issue by means of either of the two well-worn stratagems (a) 'It's a complicated internal process: that's cognitive enough for me' or (b) 'Any criterion will do, so long as we stick to it'. To return to the first point, a common reaction to the claim that levels or modes of function must be distinguished is simply to deny the possibility of doing so in an

empirically principled way. My retort to this is that for sixty years or so we have tried to create a valid psychology without making such distinctions and in most important human functions we have failed. Likewise, philosophers of mind have attempted for centuries to found epistemology on analyses of different sorts of judgement and varieties of reasoning - all phenic functions - and have also failed. It is time to have a look at the foundations of this building which is always falling down.

   If we had some clear idea of the special value of this higher level of functioning then we would be in better shape to attempt empirical determinations. Not altogether surprisingly, we have to dig fairly deep to find suggestions about its value. One of the more illuminating discussions is by Claparède (1917, 1919),[9] who argued that in microgenesis the higher level - which he identified with awareness - functioned as a catch-all standby procedure. Low-level processes which ran smoothly did not involve awareness, but if some breakdown or exceptional input occurred, the data were 'handed up' to the higher level and the process continued there. But this is surely just one role for awareness: it suggests a normally dormant organism which occasionally 'lights up' when things go wrong. Surely our intuitions tend in exactly the opposite direction - we are normally switched-on organisms which, when things go easily, switch off! So Claparède's 'law of awareness' explains only the movements of what Polanyi (e.g. 1968) has called 'focal' awareness. For example, it does not explain, except by means of trivializing extensions, why we are, on the one hand, constantly aware of our physical surroundings when we move around and, on the other hand, only rarely aware of the muscular adjustments involved in thus moving around. The few modern attempts to describe the function of awareness (e.g. Shallice, 1972) seem to suffer from the same limitation. Alternatively, we might

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[9]See also Kirkpatrick (1908), which is acknowledged by Claparede as a source.

examine the empirical procedures of, say, Piagetians, to see what criteria are actually used in practice when phenic structures or processes are under investigation. If there is a single characteristic procedure, it is surely the employment of an interview method which has as its goal the discovery of the rational basis for judgements and actions, and as its principal technique the elicitation of verbal justification. Thus, this research tradition (and I seriously doubt whether other viable ones presently exist in cognitive development) has one great limitation from the present point of view, namely that crucial evidence for the nature and course of phenic development consists of what children say, so, as Smedslund (1970) has pointed out, facts about the nature of the child's language are assumed in order to derive facts about phenic structure. Without some independent method it would evidently be ludicrous then to use facts about phenic development to explain language development. And yet it is a commonly held view that language development is explicable in terms of cognitive development. In fact, I do not believe that Piagetians have yet discovered a valid method for investigating cognitive development in the age range 1-4 years. Their investigations of sensori-motor intelligence provide a basis for speculation about the phenic framework governing very early language (see Edwards, 1973 and Bates, 1979)[10] and, of course, by 4 years child language is sufficiently adult-like to make it a plausible tool for exploring cognitive development. In the intervening age range, however, there has been little convincing progress. Indeed, Piagetian claims about some supposedly vital limitations of child thought during this period (e.g. inability to reason deductively, egocentricity) seem at best to be dubious now (see Donaldson, 1978). So, even if we can justify causal links between cognitive and linguistic development, this is no panacea, since information about cognitive development in the important growth period of 1-

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[10] Even here the needed basis lacks an immediate empirical justification. Some preliminary work has been carried out by Golinkoff (1975; Golinkoff and Kerr, 1978).

4 years is either completely lacking or unreliable, and there is no obvious methodology available for securing such information.

3. Early Language: the problem of perspicuous description

About twenty years ago, a new approach to the study of early language development emerged in which attempts were made to investigate the properties of child language considered as a system for communication. To begin with (in accordance with the Wernerian developmental principle that new forms (of investigation) first serve old purposes (of theory)) this shift was largely a shift of method, the goal being, as formerly, a specification of the set of sentences the child could produce or understand (in some limited sense of this word, e.g. 'assign a structural description to'). The most remarkable effort of this sort was Lois Bloom's (1970) thesis. The point of investigating the system for communication rather than the language per se was that it seemed plain that syntactic description was an impossibly arbitrary exercise unless some additional constraining data could be found. A natural step was then to examine not just the child's utterances but also the messages which were transmitted by these utterances. Assumptions about correspondences between message forms (i.e. meanings) and utterance forms (i.e. sentences) invoking bi-uniqueness could then be used to constrain grammatical description of the child's language. In fact, Bloom made much less use of these putative correspondences than did other workers such as Schlesinger (1971b) and, later, Brown (1973) and Halliday (1975), preferring instead to follow the line urged by Chomsky (1965, 1968) and supported by McNeill (1970), in which arguments about adult English yield a ready-made apparatus for the description of child language. This may be regarded as an aberration from which Bloom (1973, 1974; Bloom, Rocissano and Hood, 1976)

has made an admirable recovery.[11]

However little use Bloom may have made of the child's messages (as determined by adult interpreters), she established the fundamental methodological point, namely that this sort of analysis is possible. This method became known as the 'method of rich interpretation' - a misnomer, since it is simply the 'method of interpretation' - and its use has been a common feature of most subsequent work on early language. Its use has not gone unquestioned (e.g. Brown, 1973; Campbell, 1976[12] ; Howe, 1976; Edwards, 1978; Macrae, 1979); but the questions have been concerned with details rather than general principles. For a rebuttal of Howe (1976), see Bloom, Capatides and Tackeff (1981) and Golinkoff (1981). However, there remains a difficulty. To me, at any rate, it is an open question whether at any intermediate point in development these two constructions, the system for communication and the language, are to be given identical descriptions or not. Certainly it seems clear that in the adult they are not: attempts to describe the former construct (Grice, 1967; Lewis, 1969; Stalnaker, 1972) have so far failed to make significant contact with attempts to describe the latter (*Linguistic Inquiry: passim*) as has been forcefully pointed out by Chomsky (1975) and Katz (1977). Equally certainly, the two constructs have distinct ontogenies (Vygotsky, 1962). In the case of the adult a good case can be made for pursuing investigation of each system independently; and it may even be the case (as Chomsky, 1975 and Katz, 1977 have argued) that it is better (easier) to begin with description of language. For the case of the child, this

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[11] It seems quite remarkable that Chomsky (1975;1980a,b) persists with the notion that linguistic research can reveal innate constraints governing language acquisition, in the face of (a) profound and growing disagreement about how the most studied language - English - should be described; (b) gross differentiation of the concept 'human language' (see the recent clarification of the nature of pidgins, creoles and dying languages), with consequent weakening of the notion 'empirical linguistic universal'; (c) cogent and very early criticisms of his hypotheses by, for example, Lyons (1966) and Putnam (1967); (d) a stubborn concern amongst students of child language for independent empirical justification of descriptive categories; and (e) the failure of the hypothesis to attract direct empirical support or to inspire interesting research.

[12] See Chapter 2.

possibility does not exist. Gold (1967) has shown that languages of nonfinite cardinality cannot be identified on the basis of a text presentation, i.e. a sequence of sentences belonging to them. Since this is exactly (no more, no less) what is available to the child linguist who eschews study of the system of communication, it is clear that we cannot proceed with child language in the same way as we have with the language of the adult.[13] But here is our difficulty: we have not so far found a way of integrating knowledge about human communication with knowledge about human language. Indeed, in a way they seem to pass each other by. On the one hand, efforts have been made to extend structural linguistic analysis to larger and larger structures - from sound to word to sentence to complete texts - and on the other hand, the rational analysis of communication lately initiated by Grice, Lewis and other philosophers has encouraged a downward extension into grammatical processes by functionally-minded linguists such as Garcia (1975). From the point of view of this chapter, this is a clear absurdity: complementary explanations are being presented as if they were in competition! They are complementary, of course, because the rational processes at the heart of communication are at least potentially phenic, while the grammatical processes at the heart of language are normally cryptic - 'grammar is an underground process' (Seuren, 1978). Naturally, each kind of process functions as a default for the other so that, (a) in contexts that are familiar and habitual, communication can proceed in a routine manner and (b) in contexts where deviant or unusual utterances are received, grammatical analysis can proceed under rational control. Thus, as I see it, the case for autonomy of grammar stands or falls with the case for psychological dualism! Further, Chomsky - though perhaps correct in claiming autonomy

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[13] This seems to be the most effective use for results like Gold's, namely to apply them to the activities of linguists rather than to the activities of children, since these formal results cannot readily be interpreted in the context of language acquisition (see Levelt, 1975 for discussion).

for grammar - errs massively in his view that linguistics is a branch of cognitive psychology; on the contrary, it has nothing to do with cognitive psychology (strictly conceived) but deals exclusively with cryptic structures and processes[14] . On the other hand, communicative processes, such as understanding, involve not just the autonomous language function but the whole being (see Ziff, 1972 - easily the best and clearest presentation of this point of view; among psychologists, Herbert Clark's is perhaps the most congenial treatment - see Clark and Clark, 1977).
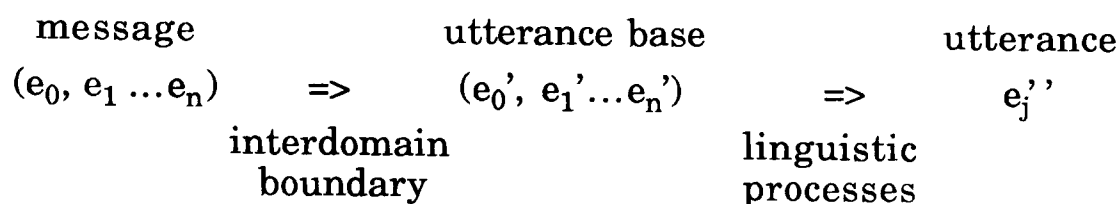
The way in which grammatical and rational processes interlock in deriving a message from a text (i.e. utterance or inscription) is a matter of current speculation and need not be explored here[15] . It is sufficient to note that, from this point of view, messages qua products of understanding are not linguistic objects. Thus, somewhat longwindedly, our difficulty is now clear: certainly it is possible to make defensible guesses about what messages are associated with which utterances in the speech of young children, but without knowledge of the structure of their message representations and the rational processes available to them we still have not radically improved our position as far as linguistic description is concerned. The point may be clearly illustrated with respect to the description of one-word utterances, where at least three distinct positions can be discerned. According to one common view (now less popular - for cogent criticism see Miller, 1976, Barrett, 1982b), such utterances transmit propositional messages which are initially encoded as a (cryptic) isomorphic structure which is converted via grammatical processes of reduction into a single morph (see model 1). A second extreme view (expounded in Chapter 2), which is uncommon but perfectly viable (Klein, 1977), is that such utterances transmit nonpropositional,
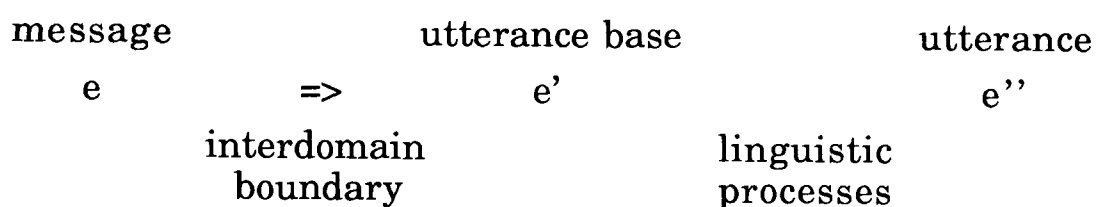
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[14] Of course (see section 2 of this chapter), Chomsky's definition of cognitive psychology is different; 'cognizing' is constitutive in his framework, thus processes involving tacit knowledge are included.
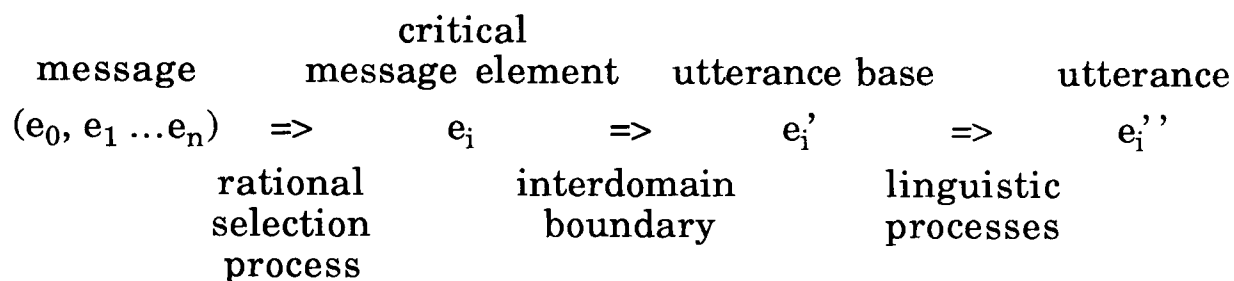
[15] See Chapter 4.

structureless messages which are encoded directly as single morphs (see model 2). A third, intermediate view (Bloom, 1973) - see model 3 - is that such utterances transmit propositional messages by first selecting (by a rational process) a single message element and then proceeding as in model 2.

$$\begin{array}{ccccc}
\text{message} & & \text{utterance base} & & \text{utterance} \\
(e_0, e_1 \ldots e_n) & \Rightarrow & (e_0', e_1' \ldots e_n') & \Rightarrow & e_j'' \\
& \text{interdomain} & & \text{linguistic} & \\
& \text{boundary} & & \text{processes} &
\end{array}$$

<u>Model 1.</u>

$$\begin{array}{ccccc}
\text{message} & & \text{utterance base} & & \text{utterance} \\
e & \Rightarrow & e' & & e'' \\
& \text{interdomain} & & \text{linguistic} & \\
& \text{boundary} & & \text{processes} &
\end{array}$$

<u>Model 2</u>

$$\begin{array}{ccccccc}
& & \text{critical} & & & & \\
\text{message} & & \text{message element} & & \text{utterance base} & & \text{utterance} \\
(e_0, e_1 \ldots e_n) & \Rightarrow & e_i & \Rightarrow & e_i' & \Rightarrow & e_i'' \\
& \text{rational} & & \text{interdomain} & & \text{linguistic} & \\
& \text{selection} & & \text{boundary} & & \text{processes} & \\
& \text{process} & & & & &
\end{array}$$

<u>Model 3</u>

A converse version of this third scheme is explicitly suggested by Sachs and Truswell (1978) as a means of accounting for comprehension of multiword utterances by children whose own utterances are limited to single words. It seems to me that the evidence favours a developmental sequence of model 2 followed by model 3 (see Bowerman, 1976 for a thorough review).

As may be seen by inspecting the models, how the language of a child at this stage is to be described depends on (a) the complexity of the phenic structures that we think are involved, and (b) whether we think that the child can learn that a single message element (suitably encoded) will convey a complex message. Thus, analysis of a corpus consisting of pairings of

messages and utterances must proceed on two fronts - cognitive and linguistic.[16] The discussion of cognitive development in section 2 should make it clear that there are few grounds for optimism amongst students of early child language. An adequate methodology seems more distant than ever.

4. Puzzles and Prospects

In the preceding sections I have illustrated some of the difficulties encountered by the application of a particular theoretical framework - namely one in which consciousness is taken to be more or less constitutive of mental or cognitive activity - to cognitive and linguistic development. In this section I shall try to show some of the benefits that might result from adopting this theoretical scheme.

4.1. Concept Development

One obvious degree of freedom introduced is the possibility of dual representation, for any structure may be represented _differently_ at different levels. I shall argue that this allows resolution of some vexing problems concerning concept development and early lexical development. We can see, though, that it leads straight away to natural descriptions of concept acquisition. Consider first a case involving an _individual concept_. Piaget (1951) describes an encounter (p. 225) between his daughter and a slug. She points at it and says 'There's the slug again!', mistakenly identifying it with a different slug encountered earlier at some distance (perhaps; see Karmiloff-Smith, 1977 for a different account). Presumably, the slug's phenic representation was different for Piaget and his daughter. For Piaget, elementary reasonings about time, space and motion established distinct phenic representations for the two slugs; for Jacqueline the two slugs were assimilated to a single phenic representation. For an individual concept it

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[16] Braine (1976) has made a valiant attempt to do this. However, the requirement of independent analyses of message and utterance corpora calls for new techniques on both fronts. Braine has certainly improved on older ones, but there still seems a long way to go.

seems clear that necessary and sufficient conditions cannot be given in terms of the perceptible features shared by successive appearances of the object denoted. Rather it is a causal criterion something like a path in space-time that determines the extension of such concepts.[17] However, for many objects (if not for slugs) a structure based on perceptible features serves as an indispensable <u>heuristic</u> for identification. In such cases it seems a natural guess that whereas the causal criterion is represented phenically (if at all), the heuristic identification procedure is cryptic.

Interesting cases involving <u>kind</u> concepts are afforded by such skills as plant identification and medical diagnosis: here the phenic representation (of species or syndrome) is determined by its place in a hierarchy, and identification ultimately depends upon serial examination of key characters and the making of inferences based on this hierarchy. However, the skilled botanist or clinician soon acquires a cryptic representation of these plants and diseases which permits rapid and reliable (although unjustified) identification. The representations are obviously quite different in character, since different information is employed at cryptic and phenic levels. That is, in the phenic process, microscopic characters may be employed: obviously these are unavailable for the cryptic process, supposing that this is based on the 'look of the patient or plant'. This example also shows that we must be careful not to confuse the method of acquisition of a skill with its eventual representation. For someone may learn to identify, for example, mushrooms with the aid of a phenic representation and a sequence of diagnostic tests. At the conclusion of this learning, however, he will have acquired an independent method of identifying mushrooms - 'by eye'. Of course, the phenic method is still potentially available as a back-up (and in medical cases

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[17] A particularly interesting example of this principle at work can be seen in Bunuel's film *The Obscure Object of Desire* (the intentional object!), in which a leading part is taken by two quite different actresses. Despite gross differences in physical appearance and behaviour, the viewer has no difficulty in perceiving 'them' as a single individual.

one would hope rather more than just a potential back-up) technique, but if the cryptic method is more efficient, then knowledge of the phenic technique may lapse through disuse. Again, there are surely skills, e.g. swimming, bicycle riding, which do not have a phenic origin. Consequently, although grammar in the adult is an 'underground process', we need not suppose either that (a) all grammatical structures and processes have cryptic origins or that (b) those which do have phenic origins are isomorphic to the phenic structures/ processes from which they derive. For example, syntactic categorization of early vocabulary may reflect simple early phenic distinctions of object, action, etc., although, of course, such distinctions cannot be noncircularly applied to later vocabulary, by which time related, but different, cryptic distinctions have taken their place. Again, if kinds of objects are initially distinguished phenically in terms of what actions can be carried out with them (as Piaget has maintained) then this need not deter us from supposing that a cryptic distinction is simultaneously developing on the basis of low-level perceptual variables. This seems to be the implicit basis of Nelson's (1973b, 1974) suggestion that early concepts have a functional origin despite the fact that identification of instances depends upon registering perceptual similarities.

In the case of both individual and kind concepts, then, there are grounds for distinguishing between what is criterial to the individual and the kind (the phenic content of the concept) and what information might be used heuristically to identify the individual or members of the kind (the cryptic content of the concept). A similar suggestion has been made by Osherson and Smith (1981).

Questions concerning the origins of concepts have proved difficult for epistemologists and psychologists alike. Successful resolution of the longstanding problems in this area would therefore add greatly to the credibility of a dualist approach. An old guess about how concepts are

acquired is the following: a series of denotata of the word in question, say <u>dog</u>, are ostensively presented to the child, who notices what features these denotata have in common. This set of common features, so abstracted, then functions as a dog-identifying-criterion and constitutes the meaning of the word <u>dog</u> and the child's concept of doghood. This is often regarded as an absurd story because in order to 'notice what is common' to the various denotata the child would have to have available a means for identifying each of the relevant criterial attributes, i.e. a system of concepts. According to this model of concept formation, all that the child learns in acquiring a new word is what particular logical function of existing primitive concepts is to be associated with that word. There are two, related, difficulties with this theory of concept formation. Firstly, where do the primitive concepts come from? Evidently they cannot be acquired in the manner just described. One answer (Fodor, 1975) is that they are innate. How happy one is likely to feel about this rather depends on how rich the primitive framework is required to be. Fodor's notion is that it must indeed be very rich, in which case there is an obvious objection, namely that an ontogenetic mystery has been dispelled by postulating a phylogenetic one![18] But if the prior conceptual framework consists of a small set of very general concepts, then the second difficulty arises. As was noted long ago by Brown (1958) and more recently confirmed by Nelson (1973a) and Ninio and Bruner (1978), the earliest predicates are words of moderate generality - denoting kinds of objects (attribute-clusters) rather than qualities or superordinate categories - and even as adults it is concepts of this 'weight' that we naturally employ (Rosch *et al.*, 1976). So evidence from early lexical development suggests that the earliest kind concepts mastered define functionally significant categories of object rather

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[18] See Dennett (1980) for a similar objection to Chomsky's innateness hypothesis. A second consequence of Fodor's analysis is that there is no genuine concept development. I attempt to refute this argument in Chapter 3, using the framework developed here.

than the elementary universal attributes needed to 'bootstrap' concept development.

To some degree these difficulties are avoided by the currently popular notion of stereotype or prototype.[19] Instead of the story given above, we would now say that kinds are identified by comparing each exemplar to a stored stereotypical individual (or, possibly, by comparison with a small set of such individuals). Words are then linked to their denotata via these stereotypes and similarity relations. This theory has many advantages. It relates early vocabulary acquisition in a natural way to the objects that have functional importance for the young child, and the obviously spontaneous quality of these acquisitions is easily explained. The notion that such categories have a nuclear member or members, and that similarity to the nuclear member regulates membership, allows for the possibility that two individuals, sharing no salient properties, can belong to the same category - a frequently noted finding (Vygotsky, 1962; Bowerman, 1978a).[20] Finally, overgeneralization - such a characteristic feature of early use of language - can be accounted for, either by supposing that relative similarity is what is involved (i.e. the 'nearest' stereotype determines what word is used) or that new individuals get added to a pool of stereotypes, each such addition stretching the boundaries of the concept.

This theory has numerous merits. However, the notion of similarity must be unpacked. What does it mean for an individual to be similar to some other individual? Surely, only that they share certain properties. So this theory of concept formation and semantic development, like the older one, makes an appeal to the notion of a primitive conceptual system, albeit in a less

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[19] The earliest formulation of this notion appears in Vygotsky (1934/1962). While the discussion of 'family resemblance' in Wittgenstein (1953) poses the problem (which stereotypes solve), it is not until Putnam (1970) and Ziff (1972) that the notion is once more clearly expressed. It is now, of course, pervasive.

[20] Incidentally, this finding shows that the similarity space is nonmetric (since transitivity fails), which scotches the formal model proposed by Osherson and Smith (1981).

immediate and forthright manner![21] It does not seem likely that there is any escape from such a notion. Even Quine, not noted for ontological generosity, now talks of 'innate similarity standards' (1975: 69ff) as a basis of language learning.

Now what is problematic about this theory? A line of argument leads inescapably to the view that at the onset of language acquisition the child is armed with a primitive system of concepts which groups objects together on the basis of fixed attributes with elementary perceptual consequences. But there is a mass of evidence (Vygotsky, 1962; Inhelder and Piaget, 1964; and see now Kemler and Smith, 1979), derived from children's behaviour in matching and sorting tasks, which shows that young children have very great difficulty in grouping objects together on the basis of an elementary fixed attribute. Certainly, Ricciuti (1965) and Nelson (1973a) have shown that young children can perform well with such tasks when it is members of a kind (i.e. a complex cluster of attributes) that are to be grouped, but use of an elementary attribute as a basis for sorting or matching is a much later acquisition. Moreover, it is also clear (Nelson, 1976) that the first attributive adjectives acquired by young children denote attributes which are <u>transitory</u> (e.g. <u>wet</u>, <u>hot</u>, <u>broken</u>) or <u>context-dependent</u> (e.g. <u>big</u>) rather than fixed and intrinsic.

So, on the one hand, we have evidence for the existence of a certain kind of concept and, on the other hand, evidence for its absence. The way to a resolution, it seems to me, is clear. The elements of the primitive conceptual system, Quine's 'innate similarity standards', are represented only cryptically and participate solely in cryptic processes: such concepts have no phenic representation initially and their establishment at that level is a lengthy developmental process. Thus young children do not learn adjectives denoting fixed intrinsic attributes, since the content of such concepts is

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[21] There is an excellent and conclusive demonstration of this point in Bowerman (1978).

represented only cryptically. For the same reason, they cannot 'hold such attributes in mind' so as to regulate performance in matching or sorting tasks.

4.2. Metalinguistic Development

A second difficult area where a dualist framework should prove helpful is the development of linguistic awareness. Experiments of my own (Campbell and Macdonald, 1982), and of Braun-Lamesch (1972) show that 3-4 year olds have enormous difficulty in making inferences about the meaning of nonsense words occurring in otherwise straightforward utterances which, qua utterances, have been accurately understood by them. An obvious guess about why this should be so is that individual words lack phenic representation at this stage of development (and so cannot participate in inferences). Likewise Karmiloff-Smith (1978) found that children of much the same age 'explained' gender assignment in French in terms of rules that they were plainly not following. Her explanation for this contradictory result, reformulated here in my terms, was that gender concord at this stage is determined by cryptic phonological processes except for a small number of exceptional cases which are decided by a phenic process involving determination of sex. The children's explanations, naturally enough, reflected this phenic process entirely and the cryptic process not at all. Other examples could be cited but the point is already clear enough. However, there is a residual puzzle here. While it seems obviously correct that 3-4 year olds lack phenic representations of words, this is odd in a way, since language learning begins with single words. Indeed, in section 3 I argued that there was a reasonable case for supposing that at certain early stages of acquisition (following Bloom and Sachs) inferences involving words were employed in the production of single-word utterances and in the comprehension of multiword utterances. It seems possible that it is wrong to think of the development of linguistic awareness as a one-way process (from cryptic to

phenic).[22] Surely it is a common pattern of motor skill acquisition that the actions constituting the skill are first carried out in the phenic mode, with selection and coordination of elementary action segments consciously regulated. What practice seems to achieve - and how this happens is an utter mystery - is the gradual replacement of this phenic system by a cryptic system, proceeding from smaller to larger units. Indeed, the ancient study of Bryan and Harter (1897) claimed to show this pattern of acquisition for typing skills: cryptic control (Bryan and Harter spoke of 'habits') is established successively at letter, word and phrase levels, eventually leaving the typist free to concentrate on the incoming text. Now the contemporary wisdom concerning the acquisition of word forms is that children initially use word forms without any awareness of them as semantically and auditorily distinct entities, and that with increasing age they become better able to attend to these forms and thereby attain some metalinguistic understanding. It is less clear whether there is any similar consensus regarding knowledge of word meaning. But what evidence is there for this view, admitting its general plausibility? I can think of nothing that rules out the possibility that (a) in the early stages of acquisition word selection and articulation are deliberate processes guided by the phenic contents of the concept associated with the word and by, say, a phenic sound image respectively; and that (b) in later stages word selection and articulation become automatic processes guided cryptically, leaving the learner free to concentrate on high-level tasks. Suppose this to be true. Why then does the preschooler have such manifest difficulties in bringing words qua semantic or auditory objects to mind? Earlier in this section I mentioned the possibility that phenic processes involved in the early stages of acquisition of concepts might lapse through disuse once mastery had been achieved. This is a familiar and comfortable

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[22] That is, loss of awareness may be as important for acquisition as growth. People who wear inverting spectacles manage to stay on their bicycles just so long as no one asks them whether they see the world the right way up or not!

notion and may possibly apply here also. But there is another possibility, equally familiar and comfortable. A characteristic symptom of cognitive development around 5 years is a difficulty in shifting perspective with regard to some problem or task. Piagetians have offered numerous hypotheses of this sort as explanations for the well-known systematic errors associated with the development of logico-mathematical knowledge. In my terms we might express this idea crudely in the following way: having plugged in one phenic system, children at this stage find it difficult to unplug it and plug in another. So it may be that they lack sufficient flexibility or versatility of consciousness to jump between the levels of sentence, phrase and word. Perhaps the late preschooler's attention in speaking and listening is 'locked' at the higher levels of control, and thus phenic monitoring of word selection and articulation is blocked. Some support for this view is provided by the sustained outbreak of late word-level errors which occurs and lasts throughout this period of development (cf. Bowerman, 1982). At any rate, the notion that children might pass from a stage where words are phenically represented to a stage where they are cryptically represented does not seem incoherent or unintelligible and presents the prospect of interesting explanations for a range of troublesome developmental phenomena.

In conclusion, I would like to emphasize the unoriginality of these proposals: they are to my mind (at least) latent in the recent writings of Bloom, Bowerman, Brown, Karmiloff-Smith and Nelson[23]. It is, of course, difficult for psychologists trained in Britain or America to feel comfortable with psychological dualism, because of the strong empiricist/determinist traditions in these countries. However, in the European literature from the early part of this century until the outbreak of the Second World War there is

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[23] What I have in mind here is the tendency, common to these writers, to resist the easy identification of the conceptual basis of an utterance with its semantic basis (in their terms). Of course, in my terms the former is a phenic structure and the latter is cryptic so that such an identification becomes impossible.

the basis for an alternative approach. Because of the unsolved methodological problems associated with it, such an approach can hardly be said to challenge current scientific practice seriously. However, it seems to me that its considerable theoretical advantages recommend us to reconsider these methodological problems and seek solutions to them. At any rate, no one need be afraid of psychological dualism: our problem is not to distinguish 'the mental' from 'the physical' but to distinguish cognitive from other kinds of psychological process. Whether this distinction is drawn in the way I have urged or in other ways (such as Chomsky's), we need not be unduly nervous about the metaphysical consequences.[24]

5. Postscript, 1992

The original version of this paper was written in 1979, and it was updated in 1986. While it set the agenda for much of my work since (and is included here for that reason), it is in some respects rather dated. For instance, the section on infant development requires reconsideration. The hopeful signs which I identified in some of the work on infant perception have come to nothing. Indeed, infancy work now seems to take less note of the need to assess the significance of infant action. One case in point would be the well-known work on early 'imitation'. I discuss this in Chapter 6, so will not dwell on it here. Another case is the work of Baillargeon (e.g. 1987) on early object permanence. Although this work enjoys great influence and is widely cited, her method involves a long chain of inference in linking the response measured (fixation time) to the thought that prompted it. Roughly, since infants look longer at display A than at display B, it follows that they are surprised by A and not by B: since A contains a disappearing object (or some other misbehaviour) it follows that they are surprised by this misbehaviour. Clearly it is difficult to be comfortable with this sequence of inferences unless the procedures used exclude all less highflown explanations: I would

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[24] Cf. the excellent discussion of this point by Chomsky (1980a: 5ff).

contend that we cannot be at all sure that they do, and that the conclusions drawn by Baillargeon must be considered to be very adventurous!

Elsewhere in Section 2, I castigate 'cognitive psychology' for failing to face up to the issue of awareness and for failing to grapple with functions in which awareness is intimately involved; at the end of the section I lament the absence of post-Piagetian work on 1-4 year olds. I am delighted to say that neither complaint would now hold water. There has been a rapidly burgeoning interest in the nature and function of awareness throughout cognitive psychology (see Weiskrantz, 1988 for a landmark conference), and so far as the latter point goes, the difficult thing now is to find post-Piagetian work that is <u>not</u> concerned with 1-4 year olds! The potent stimulus here has been Heinz Wimmer and Josef Perner's exciting discoveries (see Perner, 1991 for review): it is superfluous to add that it comes as absolutely no surprise to me that this new impetus comes neither from Britain nor from North America but from Austria. I should perhaps add that I don't think any of these developments occurred as a result of anyone reading my words!

So far as the section on early language is concerned, I would change very little. My final words - 'an adequate methodology seems as distant as ever' - seem as true to me today as they did in 1979, and the proof if this is that the ship of syntactic development is more or less abandoned. Certainly, there are an industrious crew of Chomskyans still aboard, but they do not admit any 'problem of perspicuous description': they <u>know</u>, like true believers, that UG must be true. I comment on their work in Chapter 6.

It is the notions and puzzles of Section 4.1 that have particularly directed my own thought and work in the intervening period. Although my view of some of these puzzles, and their possible solution, has changed, I would stand by most of what is said in this section.

Chapter 2: Propositions and Early Utterances

1. Introduction

One of the most common ideas about the early utterances of children is that they express complete thoughts. For this reason these utterances have often been called holophrases or sentence-words. This view of the child's earliest utterances is quite respectable - it has been expressed, for example, by Preyer (1889), Leopold (1939-49), de Laguna (1927) and other pioneers in the field of child language. However, this position has more recently been challenged very strongly by Bloom (1973). What I shall attempt here is to add some of my own arguments to Bloom's and also to raise a general question about methodology in the description of early child language.

First of all, I would like to give some examples of the evil consequences of assuming that one-word utterances express complete thoughts. The thesis takes two forms: (1) that single-word utterances correspond to whole sentences, and (2) that single-word utterances express propositions. Modern treatments of the one-word stage generally appeal to the first form of the thesis - that one-word utterances correspond to whole sentences. In consequence, we have from Ingram (1972) and from Antinucci and Parisi (1974) analyses of one-word utterances in which extremely large trees bear extremely tiny fruits!

Ingram, indeed, is so besotted by this idea that he proposes complex sentential structures for the cries of the neonate! However, the arch-exponent of this thesis is McNeill who argues in the introduction to his book (1966),

> '..not only do children acquire knowledge of sentence structure but virtually everything that occurs in language acquisition depends on prior knowledge of basic aspects of sentence structure. The concept of a sentence may be part of man's innate mental capacity. The argument of the book is designed to justify this assertion. In brief the argument is as follows. The facts of language acquisition could not be as they are unless the concept of a sentence is available to children at the start of

their learning ... children everywhere begin with exactly the same initial hypothesis: sentences consist of single words.'

In deference to McNeill's status as a prominent theorist of child language, I shall call the thesis that one-word utterances correspond to whole sentences -the McNeill Hypothesis.

2. One Word Utterances and Sentences

Now as far as I can see, the principal reason for seeking to analyze one-word utterances in this way is an extrinsic one - it disposes of the problem of explaining how children come to construct sentences by supposing that they know how to construct them before they actually do! However, I believe that the case should be argued on its own merits. Are there intrinsic properties of the one-word stage which require this kind of description or not? If not, then it seems to me we must face the problem of explaining the transition to a sentential language, however inconvenient this might be.

The earliest stage of language acquisition is quite properly called the one-word stage. It lasts normally for about six months in the early part of the second year. It is now generally recognised that the majority of these utterances fall into two distinct types. On the one hand there are utterances by means of which the child names aspects of his ongoing experience - persons, objects, actions, events and so on. Such utterances are sometimes called nominations or comments. On the other hand, there are utterances by means of which the child attempts to obtain something - an object, the attention of a person, an action and so on: these are sometimes called requests. Common to both types of utterance we have the circumstance that the thing named or requested is a participant in an ongoing or familiar sensori-motor scheme (to use Piagetian terminology). It is being looked at, eaten, played with or, in the case of an action requested, it is a familiar and enjoyable event.

The question of interest here is whether or not we have any justification for supposing that the child playing with his ball who says *Ball* is uttering a token of the sentence **This is a ball** or **Ballhood is here**[1] . Similarly, in the case of the child who reaches into his mother's chair and says *Up!*, are we justified in supposing that this is a token of the sentence **I want to be up** or some such? For it is this that those who believe in the McNeill Hypothesis are committed to.

Now of course we have very clear *prima facie* evidence that the claim is false: namely the fact that they utter only one word at a time. However, this in itself is not enough, since we readily identify these utterances as actions with something like the meaning and force of the complete sentences **This is a ball** and **I want to be up**. McNeill and others explain the fact that children at this stage don't say *This is a ball* and *I want to be up* but only *Ball* and *Up!* by appealing to limitations of one kind or another. Obviously, certain words and grammatical devices are lacking so that the best we might hope for would be *This ball* , perhaps, or *Baby up!* , or *Want up!* . We don't even get forms like these, the argument runs, because non-specific limitations of processing capacity preclude the utterance of more than one word at a time. Because the primary object of study is the child's linguistic <u>knowledge</u> rather than his linguistic <u>performance</u> we are therefore justified in speaking of sentences even when no sentences are spoken!

Now what is wrong with this plausible story? Firstly, it is too good a story: it allows us to speak of sentences when the neonate cries, when the dog barks and when the pig grunts, as Ingram appears to want to do. We need to be sure that the utterances really have the meaning or force that they are said to have. I shall argue that they do not. Secondly, Bloom (1973) has obtained evidence that children at this stage are able to produce longer utterances if

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[1] Italics denote utterances or elements of utterance; Boldface denotes sentences or elements of sentences; forms enclosed in single quotes denote propositions or elements of meaning.

the burden of meaning is lifted, so that the limitation in processing capacity - if there is such a limitation - is specific to language. Thirdly, it seems to me that the appeal made to the competence/performance distinction here is utterly *ad hoc* and constitutes a dangerous abuse of an otherwise useful distinction.

I will deal with this last criticism first, since it is the most straightforward. The account which is given by McNeill and others is that one-word utterances constitute predicates and that it is the subjects of these sentences which are unexpressed. But why should this be so? Why should we not rather interpret *Ball* as the subject of a sentence and the accompanying gestures as indicating that it is here. In any case there are no convincing arguments, as the philosopher Quine has often pointed out, for saying that *Ball* is a subject or a predicate since all lexical items may be analyzed as predicates. The competence/performance distinction is being used here simply as a license to invent structures that fit the prejudices of the theorist. To see this, consider the following absurd argument. Children typically do not produce tensed utterances until late in their third year. Let us suppose that this is because their conception of time is extremely primitive and limited. This is, of course, a non-specific cognitive limitation - that is, it affects all of the child's behaviour - not just his language. Therefore it has nothing to do with the child's linguistic competence. So we may describe such untensed utterances as tokens of tensed sentences! Now, this is clearly a dreadful argument but it seems no different in principle from the use of the competence/performance distinction to explain, or rather explain away, the absence of subjects from the one-word utterances of the younger child. It seems evident that appeals to this distinction must be made in a principled way, if the study of child language is not to become devoid of empirical significance.

To turn now to the second objection, evidence is available that children at

this stage are able to produce longer utterances. This strange and unexpected evidence was provided by Bloom's daughter Allison who, around 16 months, began to produce two-word utterances involving an unintelligible word *weeduh*. This word behaved like a classical pivot word: it did not occur alone and it occurred in fixed position at the end of utterances. However, despite strenuous efforts and consultations with others Bloom could make no sense of it. All the episodes involving *weeduh* are listed in her monograph and for what it is worth I can make no sense of it either. Moreover, this turned out to be a false start since Allison then reverted to one-word utterances for a period of 4-8 months before she again began to produce two-word utterances. After its disappearance, *weeduh* never reappeared. Now, clearly one would like to have more evidence of this sort before generalizing. Bloom comments on the lack of such supporting evidence and surmises that it was only because of her elaborate recording arrangements that she managed to identify the phenomenon properly. She supposes that others may have dismissed the phenomenon as baby-talk or jargon. At any rate the significance of the phenomenon, if genuine, is clear enough. If children have this ability but do not make use of it, then the most likely explanation is that they have no use for it. In other words, a one-word utterance is not an incomplete expression of a complete sentence, but a complete expression of an incomplete sentence - or of something quite different from a sentence.

Now let us take my first objection. It is the trickiest of the three but perhaps the most central and certainly the most interesting. How can we be sure that the child's utterances really have the meaning or force of the complete sentences that they supposedly represent? First of all we need some idea of what the meaning and force of the complete sentences are. I am here assuming familiarity with the distinction between meaning and force as outlined originally by, I believe, Frege, and more recently by Austin, Searle and many others. In the cases we are considering with the child our two

sample utterances might be analyzed as follows.

Utterance	Sentence represented	Force	Meaning
			(Proposition expressed)
(a) *Ball*	**This is a ball**	Assertion	'This is a ball'
(b) *Up!*	**I want to be up**	Demand	'I am up'

In (a) the proposition expressed is presented as being true and the listener's responsibility is simply to add this item of information to his model of the world. In (b) the proposition expressed is presented as being false and the listener's responsibility is to try to make it true. So what the McNeill Hypothesis boils down to under this analysis is that the child's utterances, *Ball* and *Up!*, have the force and meaning of the full sentences **This is a ball** and **I want to be up**.

Now I have already indicated the outline of my objection to this. In describing the one-word stage I claimed that the child's utterances, excluding marginal cases, were of two distinct types - acts of naming and requests for objects or services. I believe that one should attribute meaning and force to the child's utterances as follows:

Utterance	Force	Meaning
(c) *Ball*	name	'ball'
(d) *Up!*	request	'lift'

I am suggesting, then, that while the forces of those utterances are roughly similar to those of the sentences that they supposedly represent, they do not express propositions at all. The meaning of those utterances is simply in case (c) the object named and in case (d) the action requested.

The first thing to notice about this account is that it is prima facie reasonable.

(1) It allows force and meaning to the child's utterances, which no one would wish to deny.

(2) No violence is done to our intuitions about the acts of naming or requesting - neither requires _per se_ the expression of a proposition: in the case of naming, a speech act with roughly the same properties - the act of _referring_, is often supposed to be involved in the execution of other speech acts; in the case of requesting, there is no difficulty with the notion that the addressee complies with a request by supplying the requested object or service, rather than by endeavouring to make a proposition true! Quite the contrary, in fact.

(3) We need not worry too much about the grunting of pigs and the barking of dogs. Such bestial utterances may have to be regarded as forceful and not entirely meaningless but at least we are spared the embarrassment of supposing them to express propositions.

3. One Word Utterances and Propositions

Since I have now presented an alternative account of equal face-validity and, I should say, greater plausibility, it only remains to ask whether there are any considerations which make it essential to treat these utterances as expressing propositions, since it is this and this alone which will now justify the McNeill Hypothesis[2]. Two things come immediately to mind. If they express propositions then we should expect to find elements of utterance corresponding to subject and predicate. This circumstance obviously doesn't obtain. Secondly, if they express propositions then there should be some commitment on the part of the child to the truth-value of his utterance. It is this circumstance that distinguishes the ability to lie as an indicator of propositional thought, since in the case of lying we have a statement which is presented as being true whilst it is believed to be false. So what we must look for here is evidence that the child is aware that in saying ball, for example,

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[2] This kind of question - a question about the mental content of communicative acts - is of course unfashionable nowadays, at any rate in Anglo-American psychology. However, there is an older, repressed literature dealing with such questions - some of which contains much good sense and intelligent observation. For example, Romanes (e.g. 1888) and Lloyd Morgan (e.g. 1894) are useful sources and more recently, though in a slightly different context, Köhler (1925) showed a keen interest in such questions. It seems to me likely that the current interest in early language and in pongolinguistics (e.g. Brown, 1970) must restore such questions to their former position of primary significance for general psychological theory.

he is presenting the statement **This is a ball** as true. Now it seems to me that this kind of awareness could only arise from experience with occasions when his utterances turned out to be mistaken. But I shall argue that the properties of the one-word stage are such that this kind of occasion simply does not arise. Take *Ball* first: if it expresses a proposition it is something like 'This object is a ball'. Now, from our point of view we could regard this as true or false depending on whether the object in question was a ball or not. Notice, however, that if you or I examined a stick, say, and said *This is a ball*, an onlooker would surely take this as evidence not that we had made a mistake but that we were either insane or a pathological liar. This observation is due to Wittgenstein (1969) where he makes use of this distinction to examine the epistemological status of certain propositions - as I am trying to do here. Now, of course, the one-word uttering child is in exactly this position, since his naming utterances are always fixed in the here and now. If, while faced with a stick, he calls it *Ball* then our natural conclusion is not that he has made a mistake but that the meaning of **ball** in his language is such that **This object is a ball** is true even when it is a stick. That is, we do not take this as evidence that in looking at the stick the child has mistakenly supposed that it is a ball! It is of course open to us to conclude that the child is insane or a pathological liar. It seems to me then that as far as acts of naming are concerned there are no opportunities for the child to make mistakes about what the named object is, and hence no opportunities to learn about the status of his utterances with respect to truth at this stage and it is only when his conceptual world begins to extend beyond the Here and the Now that we may reasonably raise the question of whether propositions are being expressed. That is, I am suggesting that the design features of

<u>displaced</u> <u>reference</u> and <u>propositionality</u> may be linked in ontogenesis[3] . These arguments apply with equal force to the case of requests, in an exactly parallel fashion. In neither type of utterance, then, do we have any grounds for supposing that they express propositions. To sum up, it seems to me that the notion that children's one-word utterances should be regarded as incompletely expressed complete thoughts lacks any empirical support from, at least, this early stage of language development and, since it also lacks plausibility, that it should be discarded from further consideration in favour of simpler accounts.

4. Consequences for Methodology

I would like to conclude now with a general point concerning methodology. Perhaps in child language analysis we have been too ready to postulate unexpressed elements of utterance. My arguments with respect to the one-word stage amount to a rejection of the stratagem of McNeill whereby unexpressed subjects are postulated.

Now obviously we cannot hope to represent sentences without recourse to unexpressed elements of utterance; the work of Chomsky has shown us that. But equally obviously, the description of child language will inevitably lack a reliable empirical connection if there is to be no limitation to the kinds of element which can fail to survive utterance. Any attempt to write grammar for child language will remain at best a marginally empirical exercise unless we have some consistent criteria for limiting the possible discrepancies between sentences and utterances. As long ago as 1964 Chomsky raised the question of how grammatical descriptions of children's language could be obtained in a case where the usual sources of information

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[3] It is interesting to note that Hockett (1960) in his well-known discussion of design features makes no mention of 'propositionality'. Thorpe (1972) and Lyons (1972) in more recent attacks on much the same problem do give some limited acknowledgement to the importance of this feature. If my arguments are correct and if the value of determining an answer to this question is accepted then the natural way to investigate the presence of propositionality in communicative systems would be to make detailed studies of the ontogenesis of displaced reference.

about grammaticality, ambiguity and structural relationship are entirely lacking. It seems to me that 10^4 years later this question of Chomsky's has still not received a satisfactory answer. We have been building castles in the air. Nor is the problem of discrepancy between sentence and utterance confined to the one-word stage. In the two-word stage, for example, we have the problem of utterances which are analyzed as Noun + Noun.

Some of these involve only a missing copula, e.g. *Sweater chair* (location) or a missing inflection, e.g. *Daddy book* (possession) and cause no serious problems[5]. However, severe difficulties are raised by utterances which seem to be composed of a subject followed by direct object without intervening verb. Here the unexpressed element of utterance is once again lexical. It seems to me that if we have to allow for the possible non-realization of lexical elements in simple sentences in child language, then grammar writing at this stage is a pure mirage. However, there is another possibility. Examples of this kind of utterance are exceedingly rare in the literature. Perhaps they can be explained away. Consider Bloom's famous example of *Mommy sock* (Bloom, 1970). Examination of her data shows that apart from a handful of marginal cases the only Noun + Noun sentences which cannot be analyzed as innocuous instances of location, attribution or possession are those in which the first noun is **Mummy**. Now Eve Clark has recently noted (Clark, 1973) nine different reports of the use of **Mummy** as a request marker. That is, utterances of **Mummy** + Noun occur in contexts where it is clear that the utterance should be glossed as **I want** + Noun. Perhaps the most compelling observations of this sort were made by Piaget (1951). Now, it is noteworthy that some of Kathryn's utterances of the form **Mummy** + Noun occurred in contexts in which her mother was not present. Might it not be that Bloom has failed to notice that here **Mummy**, though superficially a noun, is in fact

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[4] My opinion remains the same, but - alas - for 10 read 28!

[5] These examples are from Kathryn I in Bloom (1970).

functioning in a quite different way? At any rate, I am encouraged to suppose that this is so: after all, Bloom made no special efforts to determine the grammatical category of such lexical items - she merely allocated them to classes in accordance with adult norms. So it may be that there are no subject-object utterances.[6]

At any rate it seems to me that a useful guiding principle in child language research should be that lexical elements, if present in simple sentences, are obligatorily expressed in utterance. And conversely that if lexical elements are absent from utterances, they are absent from the grammatical form which the utterance represents.

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[6] More recently Ewing (1983) has reached a similar conclusion.

Chapter 3: On Fodor on Cognitive Development[1]

Fodor's *Language of Thought* (1975) is an altogether exasperating book. Such prominent features as Fodor's homey fireside style of discourse, his facile and hurried treatment of inconvenient alternative views and his willingness to press on with his central argument through crowds of likely looking *reductio*s are a constant irritation to the reader - or to this reader at any rate. Very likely, to use a Fodorism, it's as full of bad arguments and false assumptions as an egg of meat. However, to use another Fodorism, it is, in a sense, 'the only book we've got'. The solitary-book-owners that I have in mind are those linguists, philosophers and psychologists who feel that an understanding of the general properties of language will result only from living in the debatable lands between these three more or less established countries. Such nomads ought to be grateful to Fodor for providing them with a map, although no doubt they will often have occasion to find fault with it. As one of these nomads - a refugee from psychology - let me therefore record my gratitude before proceeding to find the faults.

The argument of Fodor's that I want to discuss is presented in Chapter 2 of his book, and contains the following assumptions:[2]

(1) The only psychological models of cognitive processes that seem even remotely plausible represent such processes as computational.
(2) Computation presupposes a medium of computation: a representational system.

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[1] Published as Campbell (1982)

[2] Assumptions 1 and 2 are from p. 27 of Fodor's (1975) text; 3 is slightly tendentious in that I have discounted Fodor's caveat on p. 60, to the effect that this particular view of how to cash the notion of learning semantic properties is just one among an equivalent many. My arguments against 3 depend very much on the way this notion is cashed. Assumption 4 appears on p. 65 and elsewhere; 5 is a reading of pp. 89-90. Irritatingly, Fodor gives no explicit indication of how he sees the relation between concepts and predicates of a representational system. I have supposed (perhaps wrongly) that they are to be taken as related in the usual way. Assumptions 6, 7 and 8 are conclusions.

(3) Learning a language involves learning the semantic properties of its predicates, where this is construed as learning a truth definition that delivers, in some cases directly, in others via recursion, a translation of the predicates into predicates of the internal representational system.

(4) In order to avoid a nasty regress, some predicates of the internal representational system must be innate or, at best, not learned.

(5) Concepts (*qua* senses of expressions of the public language or of expressions of the internal representation system) cannot be learned except by a process of hypothesis formation and confirmation .

(6) For such a process to work, hypotheses must be expressed in a form suitable for the necessary computations to be carried out namely as expressions of the internal representation system.

(7) Thus no concept can be learned whose extension we are not already able to represent by the sense of an expression in the internal representational system.

(8) It thus appears that, whatever is involved in cognitive development, it cannot be that children acquire a conceptual system or a representational system of greater expressive power than the one they are born with.

Now this conclusion is in flat contradiction with, to adopt Fodor's idiom once more, the only theories of cognitive development we've got. A common feature of the writings of Piaget, Vygotsky, Werner and Bruner is the claim, supported by many observations, that cognitive development involves a steady increase of representational power. To take Piaget's views (e.g. 1970, Ch. 1), after an initial stage where no representational powers are assumed, there is a subsequent stage where only objects in or adjacent to the immediate surroundings are representable, a third stage where objects remote in time and space can be represented, and a final stage where possible or virtual objects may be represented. Moreover, the study of animals other than man also suggests gross differences in representational power. Köhler (1925), for example, showed what appeared to be very serious limitations in the representational capacities of chimpanzees, our closest surviving relatives. These two positions complement each other and together relieve evolutionary

biology of what would be an insupportable burden, since the enormous intellectual gulf between man and ape is not matched by a corresponding evolutionary gulf. The view that ontogenesis in man results in an increase in representational power allows us to continue to suppose that man and ape do not differ very much in their inheritance. I take it as a basic responsibility of developmental psychology that it must not get rid of ontogenetic mysteries by postulating phylogenetic ones. Accordingly, we should be very sure of our ground before we attribute to human infants representational powers over and above those that we would be happy to attribute to an infant chimpanzee. All this is aside from and in addition to the difficulty and general undesirability of casting aside so many of the hard-won insights of comparative and developmental psychology.

These considerations place important constraints on Fodor's theory; in particular, those predicates of the internal representation system that are supposed to be innate must be quite sharply restricted. To disregard these constraints leads only to absurdity. I should now make my own views clear: while I am sympathetic to some elements of Fodor's general approach, I find the conclusion of his argument about cognitive development extremely disagreeable, for the reasons just stated. Let us see what can be done to resist this conclusion.

Now of course it would be possible to reject some or all of the steps in Fodor's argument. Even step (1) is really not much more than a declaration of faith in determinism and in the possibility of a complete analysis of cognition. While I share this faith (at least strategically), I do not see that it is forced upon us. Assumptions (3) and (5) are, I believe, the most vulnerable to criticism. Although I cannot attempt a full examination of them here, I shall try to indicate the bases from which rejection could proceed. In the case of the third assumption, although Fodor speaks of learning a truth definition rather than of learning independent translations for each predicate, it is

clear (see, for example, Hacking, 1975, Ch. 12) that for very many predicates direct (i.e. non-recursive) translation will be given by such a definition. It may thus be said that Fodor is committed to the view that, for many pairs of predicates, learning the semantic properties of one predicate does not interact with learning the properties of the other. Against this, it is a commonplace of linguistic thought, since Saussure, that the value of any sign is determined by the relations of opposition and contrast that bind it to the other signs - by its place in the language-system. In particular, for very many pairs of predicates, learning the semantic properties of one assists and is assisted by learning the semantic properties of the other (cf. *odd-even*, *young-old*, etc.). For discussion and background, see Lyons, 1977, Sections 7.3-7.6 and 8. At this point, it may be objected that, since learning the semantic properties of a predicate *à la* Fodor means learning (*via* a truth definition) a T-sentence which gives a translation of that predicate in the language of thought, these translations are only superficially independent, since the predicates of the language of thought are already related to each other by the required Saussurean oppositions and contrasts. But how could this be so? Surely it could be so only if the semantic properties of the predicates of the language of thought were not learned as independent biconditionals relating them to primitive innate predicates (as Fodor would have it, 1975, pp. 89-90). There remains the possibility that the innate predicates themselves form a structured system of the required sort. Before unloading this problem on to evolutionary biology, we should be sure that the regress will work. It seems to me rather dubious, in fact. On the contrary, empirical semantic research may be said to show that as well as 'inheriting' oppositions vertically, complex predicates show emergent oppositions at the

new level.[3] It seems to me, then, that there are *a priori* grounds for rejecting assumption (3). (For a different argument with a similar conclusion - though not directed against Fodor - see Harrison (1973, Part II) and 1978).

Perhaps more cogent than these theoretical objections are certain empirical ones. Diachronic studies of various lexical fields in German by Trier, Weisgerber and others (see again Lyons, 1977, Section 8.4-8.6 for review) show that it is pointless to attempt an explanation of historical changes in meaning of isolated words. Research on language acquisition is beginning to yield similarly persuasive results. For example, in the acquisition of the field of size-adjectives in English, the opposed pair of words *big-little* undergoes a restriction of meaning leading to deviance after several years of correct usage (cf. Campbell and Wales, 1970, Section 5, and Maratsos, 1973; see, Carey (1978) for a thorough review). A natural explanation of this phenomenon is that the deviant restriction in the meaning of *big* is the result of pre-emption of part of its range of application by the other size adjectives - *tall*, *long*, etc. - which denote special dimensions of variation in size. Without some systemic assumptions, such an explanation would not go through. From my own research on the field of English colour adjectives (Campbell, Macdonald & Dockrell, 1982) I offer the example in Figure 1. The change in meaning of the adjective *purple*, for example, seems incomprehensible in isolation from the other changes in the field, for example, the expansion of the range of application of the term *red*.

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[3] See, for example, the discussion of the semantics of English causative constructions in Shibatani (1976), where it is argued - to my mind convincingly - that there is a semantic distinction to be drawn between sentences like (a) and (b):

(a) The manager moved Smith from menswear to children's toys.
(b) The manager made Smith move from menswear to children's toys.

Unlike Shibatani (whose characterization of the distinction is that sentences of type (a) involve direct physical manipulation) it seems to me that the distinction involves cancellation of agentivity in the embedded subject in type (a) constructions. However the point remains that (a) is 'vertically' related to (b) and yet distinct from it. Another way of putting my claim would be to say that true paraphrases are possible only in a metalanguage: otherwise, a distinction is induced.
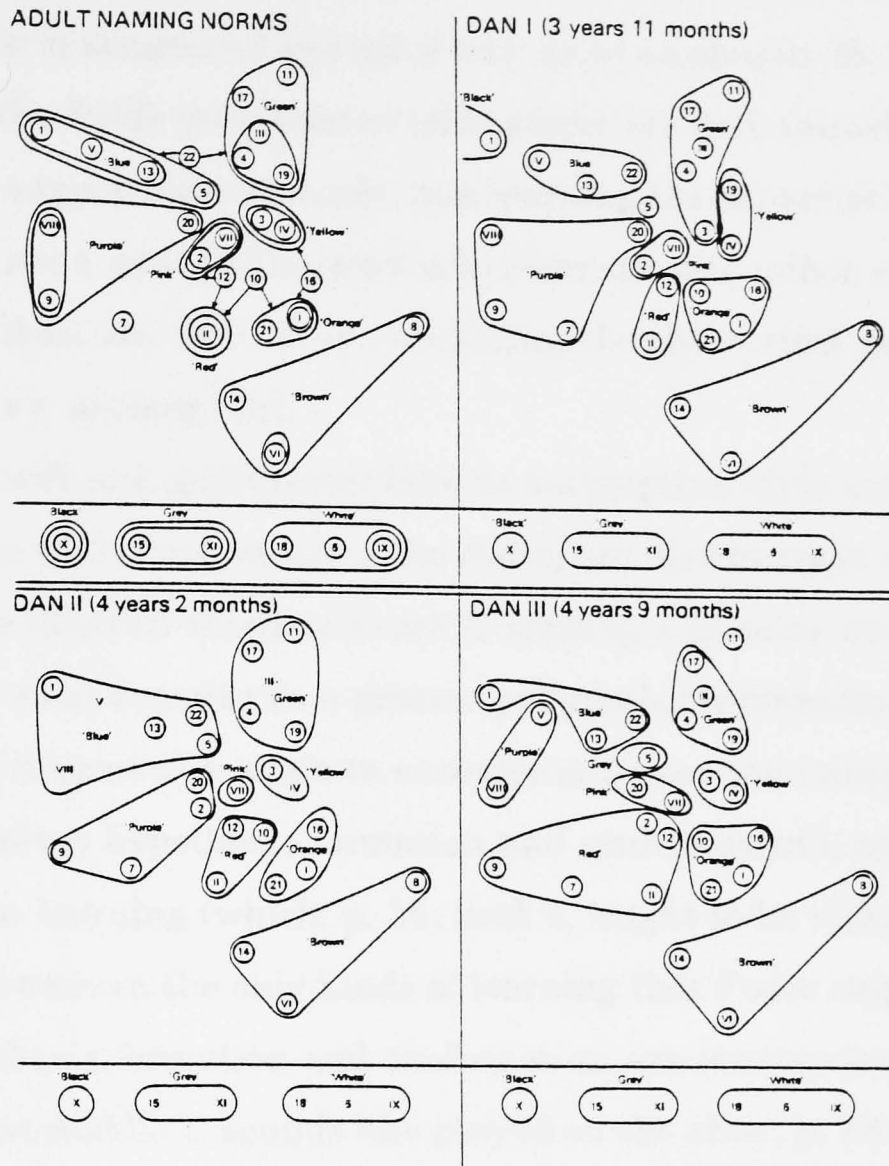
Figure 1: The north-west quadrant contains adult colour-naming norms for a
collection of thirty-three colours. The remaining quadrants show a
series of three maps of the colour-vocabulary (obtained from
naming-tasks) of a male subject at the ages shown, for the same
collection of colours. In these diagrams a numbered circle
represents a particular solid colour. They are disposed on the page
so as to satisfy (a) adult judges' decisions about which basic name
applies, and (b) brightness relationships - the darkest colours being
at the periphery. Thus (1) is a dark navy blue, (20) a lilac shade, etc.
In the diagram showing adult norms, colours enclosed by two
encircling lines were unanimously described and colours enclosed
within a single encircling line were described by the same adjective
by two-thirds or more of the sample. The remaining colours are
allocated by arrows to those names which were most often applied.

Moreover, Rogers (1978) has shown in a study of mothers' talk to children that their talk is structured in such a way as to emphasize the oppositional structure of the fields investigated (size-adjectives and animal-names). These observations suggest very strongly that learning the semantic properties of predicates is not a series of isolated achievements but rather a systemic construction from the first, thus reinforcing the theoretical arguments against Fodor's assumption.

To deal a swift and satisfactory blow to assumption (5) is somewhat harder, partly because of the vagueness of the claim, but also because it may reasonably be doubted whether Fodor is making a genuine empirical claim. His notion of what constitutes a process of hypothesis formation and confirmation is general enough to encompass perception (which, p. 44, 'typically involves hypothesis formation and confirmation'), and discrimination learning (which, p. 35, note 6, 'ought to be reduced to concept learning'). Moreover, the only kinds of learning that Fodor suggests do not involve hypothesis formation and confirmation are sensory learning ('learning what middle C sounds like played on the oboe', p. 34) and rote learning of lists. My conclusion is that Fodor considers every adjustment of the organism that is ampliative - that, in Bruner's phrase, 'goes beyond the information given' - to be mediated by a process of hypothesis formation and confirmation. Thus, assumption (5) is a simple corollary of this much more general claim. I strongly suspect that the only thing that would persuade Fodor to abandon assumption (5) would be a demonstration that concept learning was non-ampliative! It would be quite absurd for me to attempt a refutation here of the view that 'going beyond the information given' necessarily involves a process of hypothesis formation and confirmation. Perhaps it is sufficient to observe that theoretical psychology, past and present, is littered with more or less plausible alternative views. It is surely just the spider in Fodor's web of belief.

However, there is a way of construing assumption (5) so that it does have genuine empirical content: namely, where we (i) understand by 'concept learning' something distinguishable from other kinds of learning and from perception and (ii) understand by 'hypothesis formation and confirmation' some particular variety of non-deductive process that results in the strengthening of existing beliefs or in the acquisition of new ones. Now, there is a kind of learning task (discussed by Fodor, 1975, pp. 35-42) with respect to which such a version of assumption (5) is, at worst, tenable. In its simplest form the task involves the presentation, at short intervals, of a series of complex objects each of which is accompanied (usually followed) by an indication that it is or is not an object of a certain designated *ad hoc* type. The subject may be required to interpolate decisions about whether each object is or is not of the type, or he may be required to stop the presentation when he can identify the type by means of a description. In the former case, the task is ended when the subject achieves some arbitrary criterion of successive correct anticipations: in the latter case, the task is continued until the subject 'correctly'[4] identifies the type. Provided that the designated type is neither the only nor the most plausible/natural basis for partitioning the set of presented objects, it seems reasonable to speak of concept learning. Moreover, both subjective experience of such tasks and the details of performance (see Bourne (1966) and especially Pikas (1966) for review) are consistent with the view that, in solving these problems, subjects typically make guesses about the nature of the designated type and revise these guesses in a more or less principled way in the light of each newly presented object. However, even with respect to this tendentious operationalization of the notion of concept learning, it is by no means clear that such inferential procedures are either necessary or effective, since it often happens that subjects satisfy quite

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[4] Scare quotes, since here (keeping an open mind about Nature) what is designated as a correct description of the type is certainly arbitrary.

substantial-looking criteria of successful performance before they are able to give a 'correct' description of the type, and since under certain circumstances subjects will offer (with iron certainty) absurd and unjustified descriptions (Wason and Johnson-Laird, 1972). Accordingly, there seems to be little justification for supposing that these inferential procedures must be employed in every variety of this concept learning task, and none at all for supposing them to be essential to concept learning in general.

In considering language learning, however, we are not interested in concept learning in general but in a very particular kind - a kind, moreover, that departs in substantial ways from the task just discussed. In the first place, if my arguments against assumption (3) are accepted, the task involves simultaneous learning of several designated types (as in, for example, the case of the colour lexicon), rather than just one type. In the second place, the time-scale is grossly different, the intervals between relevant objects being large[5] (days rather than seconds) and irregular, and the duration of the 'task' being measured in years (Carey, 1978), rather than minutes. The validity of the usual laboratory task (which provides the only and meagre empirical warrant for Fodor's assumption) as a representative of the real task of learning lexical concepts is thus seriously open to question. Of course, the depressing aspect of this discussion is that these trivial observations have not been recognized before as a reason for working with laboratory tasks of greater *prima facie* validity.

Despite these criticisms of Fodor's assumptions, for the rest of this chapter I want to concede all the steps of Fodor's argument and to argue that we can nevertheless resist its conclusion by supposing that we have to deal not with one internal representational system but with two. How does this help us to avoid the conclusion? I will suppose that there is a primary representational

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[5] The duration of this inter-trial interval should not, however be exaggerated. Since, by hypothesis, several types are being learned simultaneously, any object relevant to the learning of one type will be relevant to all.

system, at least some of whose predicates are innate and a secondary system, none of whose predicates are innate. Further, I will suppose that the notion of representational power found in theories of cognitive development such as Piaget's applies only to the development of the secondary system. Finally, I will suppose that concepts associated with the predicates of the secondary system are learned as possibly quite complex functions of the concepts of the primary system. Informally, we may think of new predicates being built up in a Boolean way within each level of representation and copied between levels in either direction (see Figure 2). Representations required for cognitive operations in the sense of Piaget, say, would have to be established in the secondary system before they could be so employed. This provides, incidentally, an interpretation for the Piagetian notion of 'internalization'. Learning the properties of natural language predicates may then involve either direct translations into the primary system or indirect ones via the secondary system, or both. This does not seem to affect the main point, that by this strategem of reserving representational power (of the required sort) to the predicates of the secondary system, we can escape Fodor's horrible conclusion, while staying more or less within the framework that he adopts.

Primary System: p_1 p_2 → $p_1\&p_2$ p_4

                    ↓   ↓   ↓   ↑

Secondary System: P_1 P_2 P_3 → $P_1\&P_3$
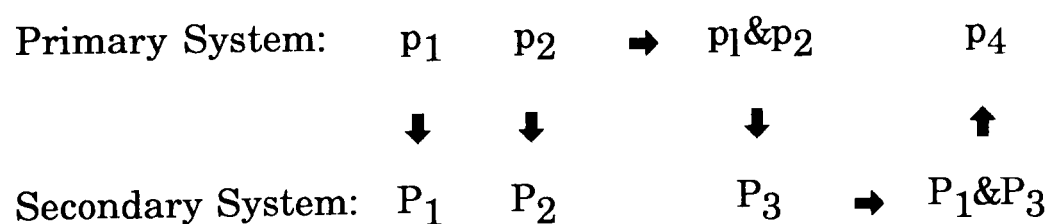
Figure 2: Combination and transfer between representational systems.

In fact, Fodor flirts with a notion almost exactly like this one at various points in his book, for example, p. 85:

> 'Essentially similar considerations suggest how it might after all be
> the case that there are thoughts that only someone who speaks a
> language can think. True, for every predicate in the natural
> language it must be possible to express a coextensive predicate in the

internal code. It does not follow that for every natural language predicate <u>that</u> <u>can</u> <u>be</u> <u>entertained</u> there is an <u>entertainable</u>[6] predicate of the internal code. It is no news that single items in the vocabulary of a natural language may encode concepts of extreme sophistication and complexity. If terms of the natural language can become incorporated into the computational system by something like a process of abbreviatory definition, then it is quite conceivable that learning a natural language may increase the complexity of thoughts that we can think. To believe this, it is only necessary to assume that the complexity of thinkable thoughts is determined (*inter alia*) by some mechanism whose capacities are sensitive to the form in which the thoughts are couched. As we remarked above, memory mechanisms are quite plausibly supposed to have this property. So, I am not committed to asserting that an articulate organism has no cognitive advantage over an inarticulate one.'

But when he turns a few pages later to attack cognitive development with his razor, this is more or less what he does commit himself to. It seems, in fact, that he thinks of 'entertainability' not as an absolute property of certain predicates - as I am proposing for the predicates of the secondary system - but as a <u>property of predicates relative to the particular memory system to which they are summoned</u>:- in this case, working memory or short term memory. Fodor's proposal here seems very strange. His proposed internal representation system will contain innate primitive predicates, complex predicates formed by Boolean recursion from the primitives, and terms of natural language. It is hard to see exactly what he might have in mind. For example, are the natural language terms to 'baptize' certain complex predicates with a compact tag so that they may fit into the limited accommodation of working memory? Surely there is no need for such contortions. Does a predicate need a name as well as an address? However, for what it is worth - Fodor exegesis is something of a mug's game - I have, again informally, tried to give a picture of what I think Fodor thinks might be

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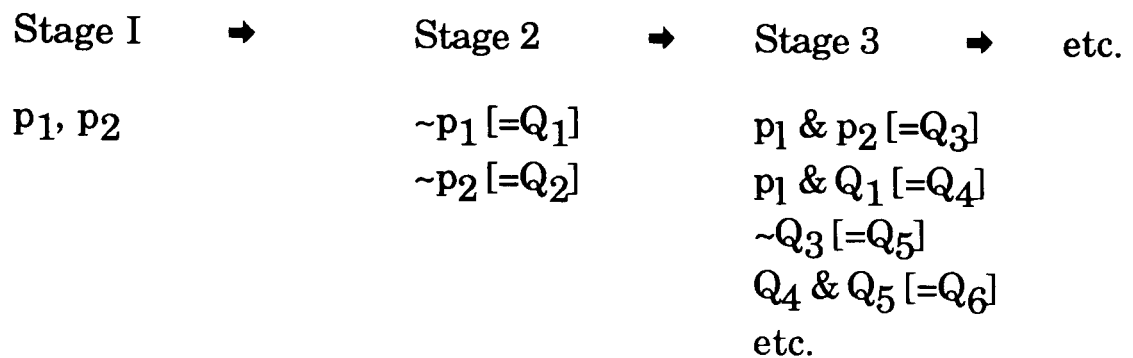
[6] Fodor's emphasis.

happening in Figure 3.

Stage I ➡	Stage 2 ➡	Stage 3 ➡ etc.
p_1, p_2	$\sim p_1\ [=Q_1]$	$p_1\ \&\ p_2\ [=Q_3]$
	$\sim p_2\ [=Q_2]$	$p_1\ \&\ Q_1\ [=Q_4]$
		$\sim Q_3\ [=Q_5]$
		$Q_4\ \&\ Q_5\ [=Q_6]$
		etc.

Figure 3: Predicates entertainable at successive stages of development of working memory capacity. p_1, p_2 are 'language of thought' predicates; Q_1, Q_2 are natural language predicates; Stage <n> is the stage in which the length of the longest entertainable combination of predicates is <n>.

For Fodor, then, development involves some expansion of the capacity of this memory system, thus allowing more complex expressions to be entertained, a rather modest cognitive advantage. If, on the other hand, we take 'entertainability' as a defining property of the predicates of the secondary system, then the cognitive advantages of developing such a system become rather more substantial, like being able to understand what is said and to figure out what to say next, for example! It should be made clear that, for myself if not for Fodor, I see no future for the notion of 'entertainability' unless it means something like 'accessible to consciousness'. Accordingly, it seems to be appropriate and indeed justifiable to identify the notion of representation found in Piaget's writings and interpreted here by my proposed secondary system of representation with Fodor's notion of 'entertainability' and other such cautious flirtations with psychological dualism.

I would like now to forget Fodor for a moment and to ask whether - quite apart from this technical question of dissolving an argument - there is independent motivation for the idea that two levels of representation and two

kinds of representational system are necessary in theoretical psychology, with just one system being involved in (roughly) conscious cognitive activity. This set of notions is not, of course, a new one. It has been aired, in one form or another, for as long as there has been a philosophy of mind. Even in the context of experimental psychology, we can find a similar set of notions in the theoretical writings of the founders of the subject, for example Wilhelm Wundt. I have recently (see Chapter 1) struggled very hard to make a case for resuscitating these distinctions and to give some kind of account of their role in the history of psychological ideas. I will confine myself here to a few remarks. In the first place, recognition of particular individual objects is an ability that we share with many other species. This ability seems to presuppose a representation of the object in question, and yet in general we have no explicit knowledge of the bases of this act of recognition. Aside from the negative results of introspection, the process is normally too fast and too reliable to make it plausible to suppose that any reflective entertainment of hypotheses is involved. On the other hand, we can reflect about these same individual objects. 'It seems to be Fodor all right,' I say to myself when I meet the stranger in the forest, 'but it can't be, because I know for a fact that he's a thousand miles away, in quite another forest!' It seems to me that in thinking of Fodor in that other forest I must be employing a system of representation quite different from the one that led to my initial, faulty identification. It was, I suppose, a difficulty in coordinating representations of this second kind that led to the celebrated misidentification by Piaget's daughter of the slug some distance along the track with the slug her father had pointed out to her on leaving their house!

As a second example, very young children use their few early words in a way that suggests a rather liberal process of comparison between the objects they encounter and representations of certain privileged objects that have become associated with the word. When the bases of similarity that are

implicated in this process are extracted by examining children's usage - as Clark (1973) and Bowerman (1978) have done - what often tend to turn up are elementary perceptible features of shape, colour and texture. So these very young children appear to be equipped with representations of these simple perceptual properties. On the other hand, it seems clear that what these children are talking about in such cases are objects - *qua* members of kinds, rather than simple perceptual properties. Furthermore, a study by Nelson (1976) shows that, when young children do begin to talk about properties, it tends to be transitory or extrinsic properties like 'hot', 'wet', 'broken', 'dirty', rather than fixed intrinsic ones, like 'round', 'red', or 'smooth', that they talk about in the first place. Second, as is very well known, it is not until much later that children can indicate, by their ability to solve certain sorts of matching and sorting problems, that they can hold such fixed intrinsic properties in mind; that is to say, can entertain such concepts (e.g., Campbell et al., 1976). It seems to me reasonable to try to resolve this puzzle by supposing that different levels of representation of the properties are involved: that, in the case of early word use, only the primary system is used and that, in the case of the matching and sorting problems, the secondary system is necessary for solution. A final point about this example that is worth making is that the nature of the properties represented is very primitive. If any predicates are to be innate then I would expect these to be. Even Quine (1975) allows such parameters their place as 'innate similarity standards' in his discussions of language learning. The point is that, if 'entertainability' were just a relation of 'containability' between a predicate and a memory-system, as Fodor would have it, then surely such predicates (being so primitive) would be among the first to be entertained instead of practically the last guests at the party. It should be made clear that Fodor is at pains at an early point in the book to deny the psychological validity of precisely the distinction I have been trying to draw, or at least to illustrate (cf.

Fodor, 1975, pp. 52, 53):

> 'While I have argued for a language of thought, what I have really
> shown is at best that there is a language of computation for thinking
> is something that organisms do. But the sorts of data processes I
> have been discussing, though they may well go on in the nervous
> systems of organisms, are presumably not, in the most direct sense,
> attributable to the organisms themselves. There is, obviously, a
> horribly difficult problem about what determines what a person (as
> distinct from his body, or parts of his body) did. Many philosophers
> care terrifically about drawing this distinction, and so they should. It
> can be crucial in such contexts as the assessment of legal or moral
> responsibility. It can also be crucial where the goal is
> phenomenology: i.e., the systematic characterization of the conscious
> states of the organism. But whatever relevance the distinction
> between states of the organism and states of its nervous system may
> have for some purposes, there is no particular reason to suppose that
> it is relevant to the purposes of cognitive psychology.'

Well, of course, there doesn't seem to me to be any intelligent way of
distinguishing levels of representation that doesn't treat the second notion of
representation as a mental act, i.e. as something organisms do rather than
as something that happens to them.

Finally, it would be idle to pretend that I am proposing a strategy that
would be easy for experimental psychologists to implement, or that the
theoretical problems are as straightforward as these carefully chosen
examples might suggest. I have made a personal commitment to this
strategy only after much heart-searching and brain-racking, since I can see
no other possibility of getting to grips with central problems of cognitive
psychology such as language acquisition. To use the Fodorism for the last
time, 'It's the only hope we've got.'

Chapter 4: Language Acquisition and the Definition of Pragmatics[1]

I will begin by identifying two tendencies - 'results' is too strong a word - of work in my own field which I will use in subsequent argument. The first of these tendencies is that it has begun to seem (paradoxically enough) in child language research that there is no such thing as a child <u>language</u>. Rather there are only <u>idiolects</u> - at any rate in the early years. This is of course hardly surprising since, in the cultures mostly studied, it is unusual for young children to have much contact with their peers before age 3 or so. However, even within triplets, as Schaerlaekens (1973) has shown, there are marked divergences in the course of language acquisition. Here, of course, the important point is that the input or target idiolect is the same for each triplet. These divergences are found at all linguistic levels: they make the construction of notional proto-languages such as, say, the 3-word stage in the acquisition of English, quite impossible in my view. This has led me to doubt whether it is reasonable at all to speak - even for adults - of knowing a language where a language is thought of as a transcendental and socially-determined institution. If we know one language then we know several, it seems. Despite enormous divergences of idiolect I - a speaker of an obscure British English dialect - succeed in communicating with inexpert speakers of English as a second language, with 2 year old children and even with North Americans! It may be possible, I suppose, to argue that what makes this communicative virtuosity possible is the possession of a large variety of linguistic competences, but that seems to me to be a doomed hypothesis. Rather, I prefer the idea that we each have our idiolects and use our intelligence and common sense to make them fit! Surely one of the great advantages of the notion of 'negotiation of meaning' - which has been so common in pragmatic writings - is that it frees us from this old idea of a

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[1] Published as Campbell, 1981

language being an institution shared by a community of speakers. Of course, for certain purposes it may be useful to stick - as dictionary makers must stick, for example - to the derivative notion of a language as a union of overlapping idiolects of members of a culturally-defined community, but inasmuch as a science of language is possible the goals of that science should be concerned with idiolect.

The second bias which I will import from work in psychology is more personal. In my recent work (see Chapter 1) I have argued, largely on the basis of certain puzzles in early language acquisition, that it is essential to revive the old psychological distinction between, roughly, <u>conscious</u> and <u>unconscious</u> structures and processes. While there has been a continuing acknowledgement of the importance of this distinction in mainland Europe, in Anglo-American psychology it has practically faded from view outside of psychoanalytic writings. Instead, the distinction has been either ignored, as irrelevant, or worse, conscious activity has been treated as epiphenomenal - being regarded as uninvolved with the causation of behaviour.

Even if that were true - which I do not for one moment believe - it is quite astonishing that such a large group of psychologists should have turned away from the task of describing and perhaps explaining this form of mental life. I have preferred to identify these two levels of cognitive function by means of theoretical terms, referring to effortful, reportable cognition as <u>phenic</u> and to effortless, unreportable cognition as <u>cryptic</u>. A good example of the need to distinguish them comes from work done by Annette Karmiloff-Smith at Geneva on acquisition of the French determiner system (cf. Karmiloff-Smith, 1978, 1979). Her work showed that 3-4 year old children assigned gender to new nouns routinely on the basis of the phonological form of the noun ending, regardless of the sex of the denoted object or of the actual grammatical gender of the noun. However, when faced with new nouns whose ending favoured neither gender, these children would typically assign

gender on the basis of sex, if possible, or invented sex, if not. So, semantically-based gender assignment <u>was</u> used but only as a default procedure when phonologically-based gender failed. Of course with increasing age Karmiloff-Smith's subjects began to use semantic and syntactic information in assigning gender. However, the remarkable aspect of her results was that phonological categories - which were the first to be utilised in the actual assignment of gender - were the last thing to be offered as an explanation of why they had used this article or that, or this form of an adjective rather than that. On the other hand, at the time when phonological categories were plainly controlling gender assignment her children typically justified their gender assignments semantically, in terms of the real or imagined sex of the denoted objects. It seemed to me that a natural way to resolve this contradictory tangle is to suppose that gender-related morphological decisions are normally made by a <u>cryptic</u> process but are defaulted to a <u>phenic</u> process whenever the noun ending cannot be assimilated to one of the key phonological categories. Children's explanations would, naturally enough, reflect this phenic process entirely and the phonological process not at all. Now it might be thought that the distinction between these two levels of cognition could be granted but that phenic structures and processes would have a role to play only in metalinguistic communication. I think this is false, for two reasons. In the first place, ordinary communication involves understanding what is said, which I take to mean something more - a lot more - than just figuring out what proposition is being expressed and knowing what would make it true. It seems to me that the pre-theoretical notion that gives the key to what is additionally involved here is <u>making sense of what is said</u> - making adjustments to a mental representation of the universe of discourse in such a way that what is said becomes sensible. A good example of this sort of thing is what Herb Clark has called <u>bridging</u> inferences (Clark and Clark, 1977, Chapter 3). Suppose, reporting a late-

night gathering someone says 'And then the police arrived and so we all swallowed our cigarettes'. To make sense of what was said we need a bridging inference. For example, that the cigarettes contained an illegal substance. I think it is fairly clear that in general such inferences involve real cognitive effort and hence phenic structures and processes -a conclusion which is supported by Clark's experimental work. Perhaps when communication occurs between ideal speaker-hearers obeying impeccable conversational principles it may proceed effortlessly. However this would be extraordinary communication. Ordinary communication, as Schegloff, Sacks and Jefferson (1977) and Jefferson (1974) have shown, is littered with all sorts of repair-sequences showing, or so it seems to me, effortful cognition at work. My second reason for believing that such processes play an essential role in language is the following. What a speaker thinks about the meaning of linguistic elements - words, syntactic formations or whatever - may be described as prescriptions. But they can hardly be <u>dismissed</u> as prescriptions unless it can be shown that they do not affect his choice of these elements. But surely on occasion they do. Karmiloff-Smith's children entertained a false theory of their own system of assigning gender but did employ this theory in assigning gender to a small set of difficult cases. In English at the moment we are experiencing a wave of idiolect change where the remnant of a system of grammatical gender is being transformed into a strictly semantic system, resulting in several changes, the introduction of new pronouns, for example, <u>he-or-she</u>, the abolition of <u>it</u> as a means of referring to children and other domestic animals, and the employment of pronoun-avoiding syntactic stratagems. In most idiolects this new system is quite unstable, the old cryptic system breaking through whenever the effort of keeping selection of these elements under phenic control proves too much.

As a last example, let us assume the truth of the popular theory, due to Horn (1972) that expressions which can be considered as intermediate on a

scale, such as *some*, *possible*, the number-names, the equative comparative and *or* are for many idiolects semantically compatible with expressions denoting more extreme values on these scales so that *some* is compatible with *all*, *possible* with *necessary*, etc. Of course they may be used and often are used to denote values incompatible with the value denoted by the appropriate extreme. The fact that they may be used in this way is then explained by appealing to implicatures based on the maxim of quantity. Briefly, if the extreme value were applicable, the maxim generally requires the use of the extreme expression. Since the extreme expression has not been used, by implicature the extreme value is not applicable. It seems to me that investigation would show that in many idiolects, mine for example, these cryptic structures and their associated processes are in unstable conflict with phenic structures - prescriptions, if you like - which specify these intermediate expressions as ambiguous, resulting in the redundant and erratic addition of what Horn has called the implicature-cancelling suspenders *only some, some or all, at least some, at least as big as, two or more, and / or*, etc.

A much more awkward question will occupy the rest of this chapter. Suppose that we do acknowledge the centrality of the idiolect and the complementary, if occasionally conflicting, roles of phenic and cryptic activity in language use. What consequences does this have for the traditional division of linguistics into syntax, semantics and pragmatics and for what we understand by pragmatics in particular? What makes this question such an awkward one is that there is no really satisfactory pre-theoretical notion of what pragmatics is, nor is there any obvious consensus amongst linguists or philosophers of language about what phenomena should be counted as pragmatic phenomena. For some it has been little more than a handy label for certain phenomena, such as thematic structure, deixis or reference. Others have employed the notion systematically, but in quite diverse ways.

For some, the principles governing the process of <u>making sense of what is said</u> - of making sense of utterance meaning - are pragmatic principles. For others, such principles are considered to be external to linguistics proper - belonging instead to a theory of rational cooperation. Again, for some, in relating sentence-meaning to utterance-meaning a sharp distinction is made between <u>syntactic and semantic principles</u> which together determine a propositional function for each sentence and <u>pragmatic principles</u> which specify what actual proposition is determined by the function in context: for others pragmatic principles combine first with syntactic and semantic principles to yield a literal utterance meaning and are then employed in a second cycle to yield an actual utterance meaning. Finally, pragmatic principles such as Grice's conversational maxims have been applied at quite distinct levels in the functional hierarchy; Horn, for example in his treatment of the scalars appeals to the maxim of quantity as controlling the move from sentence meaning to utterance meaning; others have chosen to apply the maxims to the determination of the <u>act-content</u> of an utterance already fully specified as to meaning - to what I have been calling <u>making sense of what is said</u>.

Now, it is appealing at first sight to employ the distinction between phenic and cryptic activity in a direct and simple manner, associating knowledge of the language (of one's own idiolect, that is) with the cryptic automatic level and knowledge of how to employ this system in speaking and understanding with the phenic level, thus retaining an analogue of the original radical distinction of Morris (1938) between syntax and semantics on the one hand and, pragmatics on the other. A proposal along these lines has been made by Seuren (1978). However, the confusing picture of pragmatics revealed by recent work suggests fairly strongly that this view is wrong. Why? Consider once again Horn's treatment of the scalars. If A asks B, *How many children do you have?* and B answers *Two*, then, since B may be presumed to know

exactly how many children he has and to be obeying the maxim of quantity, A may reasonably infer that B does not have three children or any larger number since he should then have said so. However, it is hard to imagine A in this situation deliberating *Does B mean exactly two or at least two?* and resolving the equivocality of the scalar by the inference just described. It is even harder to imagine A actually initiating a process of repair by saying to B *Do you mean exactly two or at least two?* Such inferences, if Horn is indeed correct to analyse them in this way, must surely be automatic and unreportable - in my terms cryptic.

On the other hand, if A asks B *Do you have two children?*, it is not at all hard to imagine B thinking *Does A mean exactly two or at least two?* and either resolving the equivocality in favour of *exactly two* by an inference involving the maxim of quantity or in favour of *at least two* by a different inference involving the larger context - say, that A is trying to establish thresholds for tax purposes or the allotment of housing or the like. Of course, the maxim of quantity is involved here as well, since - assuming mutual knowledge of the purposes of exchange - for A to have asked *Do you have at least two children?* would have been redundant. Again, in this example it is not at all hard to imagine B initiating a repair by saying to A *Do you mean exactly two or at least two?* or, perhaps more likely, deciding to treat the question as a indirect request and answering *I have four*. So my claim here is that under different circumstances a listener may be aware or unaware of the equivocality of a scalar and may resolve the equivocality by effortless or by effortful inference. So it would be wrong to make a strong direct connection between phenic and pragmatic processes on the one hand and cryptic and grammatical processes on the other. However, I do agree with Seuren (and practically everyone else) that it is important to distinguish linguistic from extra-linguistic elements in the activities of saying and understanding and if we have to make this distinction with respect to idiolects - as I am urging -

then Seuren's notion that what we ought to count as <u>linguistic</u> processes are, to use Seuren's term, <u>underground</u> - fluent, effortless and unreportable - seems to me to give much the best hope of success. Accordingly, as a first step it seems necessary to distinguish between what might be called <u>micro-pragmatic</u> and <u>macropragmatic</u> levels. Macropragmatic processes would be analyzed in terms of explicit inferences guided by principles of rational cooperation while micropragmatic processes would be analyzed <u>as if</u> they involved such inferences. However, it may also be possible to go a little further and indeed, it is desirable to do so if one dislikes the notion of unconscious inference - as I do. It seems to me that the ontogenesis of many types of skilful performance follows a pattern of the following sort: initially, the performance - whatever it is - is guided phenically; the results of many component parts of the performance are monitored and controlled in a deliberate and thoughtful way. However, with time and practice automatic cryptic processes are acquired - goodness knows how - which enable a smoother and more fluent performance. However, it is typically the case that these cryptic processes are merely heuristic; they deal adequately with the majority of circumstances but when they break down the control of the performance is returned, by default, to deliberate phenic guidance. Clinical diagnosis provides an interesting case in point. It is a truism of the art of diagnosis that experienced physicians can make, in many cases, an accurate diagnosis based on gross aspects of the patient's appearance - the characteristic facial appearance and gait associated with Parkinson's disease is perhaps the best-known example. Of course ethical standards require that such intuitive diagnoses be backed up by a deliberate eliminative procedure based on the results of laboratory tests and so forth. And in some cases, the 'look of the patient' gives no clue. Similar heuristics are employed by those skilled in plant identification: with practice, what began as slow deliberate fumbling through a botanical key gives way to smooth effortless

identification. The important point for my argument is that these intuitive cryptic procedures are based upon criteria - presumably complex and subtle perceptual properties of faces, leaf-shapes, etc. - quite different from the explicit phenic procedures which generally involve close examination of highly local properties. A slightly different example is the process of attribution of art objects, such as paintings. There, one may know a Piero della Francesca painting when one sees one without having any idea why. Such intuitive attributions can be contrasted in the same sort of way with the deliberate feature-by-feature attributions of a professional art historian whose judgments are subject to verification by examination of brushwork, composition, iconography and historical data. It seems to me that in all these cases there is little reason to suppose that the intuitive procedure is homomorphic with the explicit one - that it is not a sequence of inferences driven underground by practice but rather an independent heuristic (i.e. operating within a more limited domain) with a possibly quite different dynamical structure. What I want to suggest, then, is that the pattern of development just discussed occurs also in the learning of a language: that success in achieving communicative goals in speaking and understanding can lead in some mysterious way to the acquisition of short-cut cryptic heuristics which satisfactorily replace intelligent and deliberate processes of guidance and control through a large part of their domain. When these processes fail to yield a result, then control may be returned by default to the phenic level. Accordingly, it may be possible to relate macro and micro-pragmatic processes in roughly the following way: a macropragmatic process is one constituted by a sequence of explicit inferences governed by principles of rational cooperation. A micropragmatic process develops as a cryptic and heuristic procedure which <u>partially</u> replaces some macro-pragmatic process and which defaults to it in the event of breakdown.

Chapter 5: Royal Investigations of the Origins of Language[1]

Introduction

Students of language development in children are familiar with accounts of three isolation experiments, that is, experiments in which infants are isolated in order to discover whether they have a natural tendency to create language and, if so, what sort of language. These experiments are alleged to have been carried out (in chronological order) by Psamtik I of Egypt. Frederick II (Hohenstaufen) of Sicily, and James IV of Scotland (cf. Marx, 1967, p. 443, 450-51; Blumenthal, 1970, p. 100).

In view of the present revival of interest in the origins of language, it has seemed to us timely to examine these ancient investigations from two points of view. First of all, there is the question of authentication: did they indeed take place and, if so, how were they conducted and what were their outcomes? In the second place, what light do they shed on the genetic question to which they were addressed, viz., what is the nature of spontaneously created language? In the course of our research we stumbled upon a fourth royal investigation, that of Akbar the Great, 16th-century ruler of Moghul India. This isolation study is not well-known (see sect. 4 below) and deserves to be better known. In addition, although accounts of the three earlier experiments are commonplace, they are seldom accurate and never accompanied by full texts and bibliographic details of source materials. For these reasons we feel that it is worthwhile to provide contemporary students of child language with a convenient and comprehensive guide to the origins, weird and wonderful though they be, of their subject.

We will proceed in the following manner. For each experiment we will give the primary source-texts (in translation) and refer to any principal secondary texts. After commenting on these texts we will discuss the question of authenticity. A natural further step is to consider the plausibility of these

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[1] Published as Campbell and Grieve,1982

investigations. That is, to ask whether it is plausible that the individual in question should have carried out such a study. As we show below, it turns out that plausibility is a highly unreliable guide to authenticity. There are two grounds for this claim. The first is an *a priori* argument:[2] an individual who is known for empirical inquisitiveness and an interest in genetic or linguistic questions is not only more likely to carry out isolation experiments than other individuals, he is also more likely to have such experiments falsely attributed to him by garrulous and imaginative chroniclers (whether motivated by malice, a desire to 'gild the lily' or a simple wish to make their books more interesting).

The second ground is empirical: plausibility and authenticity are simply not correlated in the cases discussed.

Finally, we will consider whether these experiments illuminate the genetic question in any way and briefly review the possibility of investigating this question by less barbarous methods.

1. The experiment of Psamtik (Psammetichus) I of Egypt
1.1 Sources

We know of this experiment only through Herodotus' account[3]. Psamtik lived from 663 to 610 B.C., and Herodotus from 485 to 425. His account is based upon what he was told by the priests of Hephaestus at Memphis. It runs as follows:

> 'Now until Psammetichus reigned over them, the Egyptians believed that they were the eldest of all men. But ever since Psammetichus became king and resolved to learn who were indeed the eldest, they have believed that the Phrygians were before them, but they themselves before the rest. For when Psammetichus was not able by enquiring to learn the answer from any man, he conceived this device. He gave two new-born babes of

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[2] We are indebted to Professor G. Barrow of St. Andrews University for making this point clear to us.

[3] Herodotus' well known *Histories* are of course available in a number of editions with a range of alternative translations and commentaries. The text presented below is from the English translation by Powell (1949:109-10) of Book II, section 2.

ordinary men to a shepherd, to nurture among his flocks after this manner. He charged him that none should utter any speech before them, but they should live by themselves in a solitary habitation; and at the due hours the shepherd should bring goats to them, and give them their fill of milk, and perform the other things needful. Thus Psammetichus did and commanded because he desired, when the babes should be past meaningless whimperings, to hear what tongue they would utter first. And these things came to pass; for after the shepherd had wrought thus for a space of two years, when he opened the door and entered in, both the babes fell down before him, and cried *becos*, and stretched out their hands. Now when the shepherd heard it the first time, he held his peace; but when this word was oftentimes spoken as he came to care for them, then he told his lord, and brought the children into his presence when he commanded. And when Psammetichus had also heard it, he enquired which nation called anything *becos*; and enquiring, he found that the Phrygians call bread by this name. Thus the Egyptians, guided by this sign, confessed that the Phrygians were elder than they. That so it came to pass I heard of the priests of Hephaestus in Memphis.'

Psamtik is thus thought to have believed that isolated children would speak the language of their aboriginal ancestors. This belief is presented as an assumption to be used in interpreting the experimental outcome, not as an hypothesis itself requiring empirical confirmation. It is characteristic of early writing on the subject of language origins that some <u>particular</u> language (perhaps 'dead' but in some way identifiable) should be regarded as the original. A particularly clear example of this assumption is provided by the following passage from the *Wibhanga Atuwaba* (cf. Hardy, 1966, p. 23), an ancient Singhalese Buddhist text:

'Parents place their children when young either on a cot or a chair, and speak different things, or perform different actions. Their words are thus distinctly fixed by their children (on their minds) thinking that such was said by him and such by the other; and in process of time they learn the entire language. If a child, born of a Damila mother and an Andhaka father, should hear his mother speak first, he would learn the Damila language. but if he should hear his father first, he would speak the

Andhaka. If, however, he should not hear either of them, he would speak the Magadhi. If, again, a person in an uninhabited forest in which no speech is heard, should intuitively attempt to articulate words, he would speak the very Magadhi.'

Psamtik is credited with having found it necessary to isolate <u>two</u> children (contra the *gedanken*-experiments in the *Wibhanga Atuwaba*), suggesting perhaps that he thought that language would not be developed without some society. However, it is also possible that the use of two subjects may have been merely a precaution against inadvertent mortality. In any event, the outcome of the experiment, as reported, was hardly sufficient to bear the weight of the inference that ensued. Some have argued that *becos* might have been modelled upon the bleating of the goats (cf. Lloyd, 1976, p. 5; Sennert, 1643, sect.VI; Farrar, 1865, p. 13), but this seems equally tendentious and futile. It is interesting that Psamtik is supposed to have asked in what tongue is there <u>anything</u> called *becos* rather than in what tongue is there something called *becos* which would fit the occasion of utterance. What would he have said if his advisers had informed him that *becos* was Egyptian for the tusk of an elephant? Could it be that semantic emptiness or lability in the Original Language was considered possible but that phonological change was not? At any rate it is clear that a latent *Ursprache* (perhaps consisting only of a collection of meaningless phonological forms) was assumed to be present in the minds of the children by Psamtik (or by the fabricators of the tale, if it was indeed only legendary).

1.2 Authenticity

Herodotus' report of this experiment has several features that might incline one to accept it as genuine. It bristles with impressive detail, the design shows remarkable sophistication (secure isolation, newborn children, minimal caregiving, awareness of babbling, etc.) and the outcome was presumably unwelcome to the Egyptians (they knew perfectly well who were

the oldest people!). However, accepting it as genuine is quite unjustified. A recent commentary on Book II (Lloyd, 1976) makes this very clear.

Firstly, the sophisticated nature of the design suggests an Ionian origin -- Greek doctors were already employing such controlled methods in Ionia (cf. Farrington, 1944; 1969). Secondly, goats' milk was not used for suckling infants in Egypt (indeed, its only known application was as a remedy for anal dysfunction) but was so used in Ionia (cows' milk was not). Thirdly, there is a remark, at the end of the account, to the effect that the story was known to Greek scholars in a slightly different form. Lloyd argues convincingly that a version of this experiment had appeared earlier in Hecateus of Miletus' *Genealogies* (only fragments of this survive) and it is possible that Herodotus may have simply 'lifted' the story from there (for exhaustive discussion see Heidel, 1935).

There are still various possible alternative explanations. For example, it might have been a fictional satire aimed at foolish Egyptians or it might have actually happened in Ionia and somehow 'migrated' to Egypt. However, whatever its provenance, any claim that the experiment took place in this way or in that way is seriously weakened by this analysis of Lloyd's, since, at best, it greatly increases the remoteness of Herodotus' report from the actual event.

1.3 Plausibility

Herodotus describes an experiment of Psamtik's which might be thought of as an investigation of a genetic question, namely an attempt to fathom the depth of the springs then regarded as the source of the Nile, but since this is no better authenticated than the language experiment it does not help much. In the case of Psamtik, then, we know neither that such an experiment was consistent nor that it was inconsistent with his personal characteristics, since so little is known about him.

2. The Experiment of Frederick II (Hohenstaufen) of Sicily
2.1 Sources

Once again, we know of this experiment through only one source, the chronicle of Brother Salimbene, a Franciscan friar. The story occurs in the Chronicle as one of a long list of strange experiments recounted by the friar and runs as follows:

> 'Like Psammetichus, in Herodotus, he made linguistic experiments on the vile bodies of hapless infants, bidding foster-mothers and nurses to suckle and bathe and wash the children, but in no wise to prattle or speak with them; for he would have learnt whether they would speak the Hebrew language (which had been the first), or Greek, or Latin, or Arabic, or perchance the tongue of their parents of whom they had been born. But he laboured in vain, for the children could not live without clappings of the hands, and gestures, and gladness of countenance, and blandishments.'[4]

2.2 Authenticity

Although, unlike the previous case, Salimbene (1221-88) was a contemporary of Frederick (1194-1250), he had no connection with his court, and his personal knowledge of Frederick was limited to a glimpse of him passing through the streets of Parma in 1235. It would be unwise, therefore, to suppose that Salimbene's sources of information were reliable. This is particularly the case since Frederick, though Holy Roman Emperor and active in Crusades, was excommunicated by Pope Gregory IX in 1227 and thereafter lived in almost continual conflict with the Church. It is therefore likely that Salimbene would have included in his Chronicle any discreditable tale that reached his ears (or his imagination).

As for the text itself, once again we have as a background assumption that some identifiable language should be the basis of the *Ursprache*. However, in this case, as well as the usual classical languages, it is thought possible that

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[4] The text given is from Coulton's (1907) translation of part of the Chronicle. An edition of the original chronicle, *Cronica Fratis Salimbene de Adam Ordinus Minorum*, appears in *Monumenta Germaniae Historiae*, XXXII, Scriptores, ed. by O. Holger-Egger, p. 350.

the language of the parents might be produced, in this case probably a vernacular, Sicilian. At this time the classical languages and the vernaculars were regarded as being quite different, only the former involving 'necessary' forms. There is an illuminating contemporary discussion of this point by Dante,[5] who argues that the outcome of experiments such as this would inevitably favour the classical languages.

The explanation of the outcome of Frederick's experiment is equally striking. In Western educational thought it is not until the early 18th century that such weight begins to be placed on the social and affective aspects of early childrearing. As we shall show below, Frederick was an extremely original thinker who cared nothing for received opinion and whose grasp of the elements of scientific investigation was not to be equalled for several centuries. These unusual aspects of Salimbene's report add to its credibility, since they are unlikely inventions.

2.3 Plausibility

Frederick was possibly the most unusual monarch in European history. In his time he was called *stupor mundi et immutator mirabilis*, 'wondrous transformer of the world', and with good reason. Two excellent biographies, by Kantorowicz (1928) and by Van Cleve (1972), and the specialist investigations of Haskins (1924, 1929) are the principal sources for the sketch which follows. Frederick's strongest personal intellectual interests were biological. He assembled an immense menagerie which accompanied him whenever he travelled. It included camels, lynxes, leopards, lions, panthers, apes, bears, a giraffe and an elephant; hawks, owls, eagles, buzzards, falcons, peacocks, parrots and an ostrich. In addition, his court was always accompanied by a collection of human curiosities: concubines, slave-girls, eunuchs, acrobats, conjurors, rope-dancers, etc. He maintained animal

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[5] In *De Vulgare Eloquentia*, translated into English, with notes, by A.C. Ferrers Howell. (London: Kegan Paul, Trench, Trubner & Co. London, 1890).

reservations in various parts of his kingdom, the most remarkable of which was a collection of waterbirds housed in 'natural' (i.e., not in a zoo) conditions in a marsh near Foggia. Like many mediaeval rulers he was passionately fond of falconry but here again his interest was scientific as well, so much so that he wrote a treatise, *De Arte Venandi cum Avibus*, which is the basis of all subsequent writing on the subject.[6] This treatise is a large work and it contains an introduction of about 100 pages dealing with avian biology in general and describing many simple investigations of anatomy and behaviour carried out by Frederick. Aristotle is taken to task on more than one occasion for mistakes in his natural histories. Frederick is never in any doubt which should be given greater weight, the wisdom of the great philosophers and teachers or the evidence of observation and experiment. Of course, in other quarters, such as the mediaeval universities, such empirical questioning of received knowledge would have been regarded as obscene and heretical. There is little doubt that Frederick carried out other sorts of experiments as well. In the list provided by Salimbene some are undeniably dubious, but others have some substantiation - such as the 'ringing' experiment to discover the longevity of fish (cf. Hauber, 1912) - and others, like the isolation experiment, are sufficiently unusual in design or conception to make us doubt whether they are fabrications. For example, to compare the effects of activity and inactivity on digestion he had two men eat the same meal, sleep and go hunting respectively, whereupon they were simultaneously disembowelled and their stomach contents examined!

Of Frederick's other intellectual interests we should mention at this point his interests in language. He is reputed to have spoken nine languages and to have been literate in seven. He anticipated the vernacular poetry of Dante by composing many Sicilian odes and lyrics and, most remarkably, inspired the

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[6] Bibliographical details and a review from the standpoint of the history of science can be found in Haskins (1921). A modern edition in English with copious illustration and accompanying background material is available (Wood and Fyfe, 1948).

officials of his court to write vernacular poetry as well! It is clear that the
imputed isolation experiments is entirely consistent with what is known
about Frederick's interests and resources.

3. The Experiment of James IV of Scotland
3.1 Sources

This experiment is reported in the History of Robert Lindesay of Pitscottie
as follows:

> 'The king gart tak ane dum woman and pat hir in Inchekeytht and gaif
> hir tua zoung bairnes in companie witht hir and gart furnische them of
> all necessar thingis pertening to their nurischment that is to say, meit,
> drink, fyre and candell, claithis, witht all wther kynds of necessaris
> quhilk (is) requyrit to man or woman desyrand the effect hierof to come to
> knaw quhat langage thir bairnes walk speik quhene they come to
> lauchful aige. Sum sayis they spak goode hebrew bot as to myself I knaw
> not bot be the authoris reherse. (Thir actis foirsaid was done in the zeir of
> god I^{m} iiij^{c} lxxxxiij zeiris.)'[7]

3.2 Authenticity

Like the previous case, Pitscottie (1500?-1565) had no personal acquaintance
with the monarch, nor personal knowledge of the experiment, since James
died in 1513. 'Lauchful aige' is 'lawful age'. Exactly what is meant by this is
unclear: it may have been that age at which children were thought capable of
sin, that is, according to the Church, 7, or it may have been the age at which
children ceased to be 'pupils' - 12 for girls, 14 for boys.[8] Pitscottie's final
remark is clearly a signal to the reader that the reported outcome is to be
regarded as a humorous invention, since his entire history up to that point
and for a decade or so beyond is based upon 'authoris reherse' - hearsay. The
experiment is, unlike the previous two, characterised as open-ended, no

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[7] Robert Lindesay of Pitscottie's *Historie and Cronicles of Scotland* exists in several
manuscripts. The (1899-1931) edition by A.J.G. Mackay (Edinburgh: Wm. Blackwood for
the Scottish Text Society) is based on two of the oldest MSS. It is well-known as a colourful and
unreliable history (cf. the remarks in Nicholson 1974:328, 627). The text reproduced below is
from volume 1, p. 237 of Mackay's edition.

[8] This latter suggestion was offered by Professor G. Donaldson of Edinburgh University.

particular possible outcomes being specified. It is plainly a less satisfactory account evidentially, than either of the previous texts. Herodotus described Psamtik's experiment in an impressively detailed way and Salimbene's account, as we have noted, had certain features which make it an unlikely fabrication. But Pitscottie's lacks details of procedure and outcome and, indeed, has all the characteristics one would expect of a colourful fabrication - an uninhabited island as the venue (i.e. no-one could bear witness that it had not taken place) and a ludicrous outcome. It does, however, have two features that the others lack, a date and a place. The island of Inchkeith lies in the Firth of Forth, readily accessible from James' palaces in Edinburgh and Linlithgow. It was certainly visited by James during the 1490s for the purposes of falconry and he maintained a mews there (cf. Treasurer's Accounts[9]). However, there are no entries in the Accounts which relate to the experiment (such as, e.g., payments to the dumb woman). Moreover, Inchkeith was used from 1497 onwards as a repository for the victims of venereal disease, introduced to Edinburgh by the followers of Perkin Warbeck - a pretender to the English throne.[10] It is a small island and it seems likely that the experiment would have had to be concluded before this use for purposes of quarantine. Of course, it could be that the date given, 1493, refers to the conclusion of the experiment. But if this is so, then the experiment is even less credible since James was only 20 in 1493. On the whole, then, we have no very good grounds for taking Pitscottie's story as genuine. Despite this, we have made very extensive inquiries in search of additional historical

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[9] (Treasurer's Accounts) *Compota Thesaurariorum Regum Scotorum: Accounts of the Lord High Treasurer of Scotland*, ed. by T. Dickson and Sir J. Balfour Paul (1877-1916). These accounts, though incomplete for certain years (e.g., 1499), together with the *Exchequer Rolls of Scotland*, ed. by J. Stuart and others (1878-1908) provide a fair record of the official expenditure of the Crown. As such they are perhaps the most important primary documents for the royal history of the period. Indeed, there are few other reliable records from the mediaeval period in Scotland that survive.

[10] Mentioned in Edinburgh Burgh records (cf. Marwick 1869-82, 1871). Inchkeith was used in 1475 for a similar purpose - quarantine of plague victims (cf. *Exchequer Rolls*, VIII, 364). Apart from these incidents, Inchkeith's principal function was as an important naval station.

evidence, but without any substantial results. There are no records surviving from either the Parish of Kinghorn (in which Inchkeith is located) or the Abbey of Inchcolm, a neighbouring island in the Firth. As footnote 9 shows, the surviving state papers are not complete, but contain no mention of the experiment. Neither do the unofficial contemporary writings of De Ayala" or the court poet Dunbar (Mackenzie, 1932). As well as that of Pitscottie there are three other 16th-century histories, Buchanan (ed. Ruddiman, 1715), Drummond of Hawthornden (1655), and Lesley (1830). None of them mentions the experiment. The solitary ray of hope that there may be more to the story than frivolous invention comes from Pinkerton's 18th-century history (1797, p. 25) in which there is an account of the experiment (later embellished by Thomson,1893), which differs from that given by Pitscottie. Pinkerton was a normally reliable professional historian and we can be sure that he must have had good reason for giving a different account. But what was his reason? He cites, in addition to Pitscottie, a location in Buchanan (Chap.XIII, sect.7) but in Ruddiman's edition of Buchanan there is no mention of the experiment, at that place or at any other. Pinkerton's account runs as follows:

> 'To make some discovery on the origin of language, two infants under the charge of a dumb woman were sent into the isle of Inchkeith: but the self-taught speech has not been explained: and it is needless to add that it must have been original, and perhaps though there was some society, little superior to the brutish babble of those unfortunate beings, lost during infancy in extensive forests.'

So, according to this account, there <u>was</u> (undeciphered) spontaneous speech. Pinkerton's own observations - 'it is needless . . .' show that he was acquainted with the great debate amongst continental philosophers and educators which took place at this time and which had as one of its foci the

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" Ambassador to Scotland from the Court of Spain. His letters are collected in volume 1 of Bergenroth (1862-68), and a translation of the material relating to James IV can be found in P. Hume Brown ,1891.

nature of feral children (cf. Lane, 1977, for a thorough account of this). It seems to us a distinct possibility that somewhere in that massive literature there is material relating to this and other isolation experiments but so far we have been unable to find it. It may be, then, that the last word has not been said about this putative experiment of James. However, it must be conceded that, on available evidence, it seems unlikely that the experiment was performed.

3.3 Plausibility

There is a mass of evidence which shows that James was interested in experiment and in the development of the sciences, particularly medicine. Moreover, like Frederick he was a collector of curiosities. To take the last point first, he gave instructions to his merchant seamen (all of whom engaged in occasional piracy) that he would reward them for any human or animal oddity (cf. Chambers and Thomson, 1855). It is also fairly certain that he kept and maintained a joined twin. There are accounts of this in Pitscottie (Vol.I, p. 233) and in Drummond of Hawthornden (1655, p. 69). We shall give the account which appears in Aikman's translations from Buchanan's Latin (for purposes of clarity):

'About this time a strange kind of monster was born in Scotland. In the lower part of the body it resembled a male child, differing in nothing from the ordinary shape of the human body, but above the navel, the trunk, and all the other members became double, and were distinct, both in their use and appearance. They caused it to be carefully brought up, and educated, particularly in music, in which it wonderfully excelled. It also learned different languages, and in their various inclinations, the two bodies appeared to disagree between themselves, sometimes disputing, each preferring different objects, and sometimes consulting, as if for the common pleasure of both, and, what was remarkable, when the lower limbs, or loins were struck, both felt the blow in common, but when pricked or otherwise hurt above, only one of the bodies was sensible of pain, which distinction was most conspicuous in death; for, when the one body had died several days before the other, that which remained,

when the dead half became putrescent, wasted away by degrees. I write this the more confidently, because there are many persons of undoubted veracity still alive, who saw the prodigy.'

Buchanan's dating of the monster's birth is 1490 and Pitscottie states that it lived for 28 years and, during this time was maintained by the court. (For a more recent case, see Gedda, 1951.) James' interests in medicine and science have been amply substantiated by Guthrie (1497) and Read (1938a, 1938b & 1947, Chap. 2). His new university at Aberdeen had a separate medical faculty (albeit one devoted to the customary doctrinaire and theoretical methods of teaching) and he founded the more practically-minded college of surgeons and barbers in Edinburgh, whose graduates had to satisfy their examiners that they 'knew anotamell [anatomy], nature and complexion of every member humanis bodie, and . . . the vaynis of the samyn, that he may mak flewbothomell [phlebotomy] in dew tyme . . . (for) every man aucht to knaw the nature and the substance of every thing that he werkis, or ellis he is negligent'. Moreover, each member was entitled to 'anis in yeir ane condempnit man efter he be deid to mak anatomell of, quhairthrow we may haif experience' (cited in Mackie, 1958).

James himself was an enthusiastic surgeon: the Treasurer's Accounts show payments to individuals who had their teeth extracted by him, and suchlike. In science, James founded what Read (1938b) has described as Scotland's first research laboratory in Stirling Castle around 1500. The director of this establishment was a mysterious Italian, John Damian de Falcusis (later appointed Abbot of Tongland - a sinecure) who came to Scotland around that time from Paris. Damian was an alchemist who became a close friend of James. He was provided with excellent facilities for his work, as the Treasurer's Accounts show, in addition to a position at the Court and luxurious garments of office (cf. Read, 1938b). There is no evidence either that he succeeded in multiplying his seed gold or that he was ever

seriously called to account. However, another of Damian's experiments apparently was subjected to public test - human flight! The experiment is described in William Dunbar's hilarious and savage satirical poem, *The Fenyeit Frier of Tungland* (ed. by Mackenzie, 1932) and also by John Lesley, Bishop of Ross (see Lesley,1830, p. 76).

Like Frederick, we can reasonably suppose that James had an interest in language and poetry. According to the Spanish ambassador to the Scottish court, Don Pedro de Ayala, James was a wonderful linguist, speaking Latin, French, German, Flemish, Italian, Spanish and Gaelic, but Mackie (1958, p.118) regards this as a diplomatic embellishment. It is certain, however, that James fostered vernacular poetry, which flourished as never before (nor since) during his reign. Dunbar, whom we have already mentioned, was the finest of these poets and was attached to James' court.

It should now be evident that it is wholly consistent with what is known about James that he should have carried out the isolation experiment. However, the direct evidence, as we have noted, is very weak.

4. The Experiment of Akbar the Great, Moghul Emperor of India, 1542-1605
4.1 Background

Akbar's experiment is by far the most interesting of these royal investigations. In the first place, it is the only one that is adequately vouched by contemporary documents. There are three primary sources for the experiment: Abu'l-Fazl, Badauni, and Xavier (see below) - and a large number of secondary sources some of which, though not contemporary, are still of interest. Some of these are referred to below. In the second place, Akbar's experiment was superior to the other putative experiments in conception and design. Lastly, Akbar's experiment has received hardly any attention from psychologists or linguists this century. It is described as a matter of course, along with the other experiments in some 19th-century texts, e.g., Farrar (1865), Muller (1861, pp. 480-82) and Tylor (1878, pp.79-81),

but after Tylor does not appear to have been mentioned in linguistic or psychological texts until Panconcelli-Calzia (1937, 1955: reference only to Catrou, 1705 - a poor secondary source) and Borst (1957-63 IV, p. 2050: reference to the same source). Marx (1967) and Blumenthal (1970), though including surveys of early investigations, fail to mention Akbar's experiment. More recently, Hewes (1977, p. 98) and Blakemore (1976) have mentioned it, though once again without adequate detail or references. We 'discovered' it by reading Panconcelli-Calzia (op.cit.) as a result of reading Marx, who evidently had not read it himself.[12] Muller, as well as being a prominent linguist, specialized in Indian history and hence was thoroughly familiar with the Akbar experiment. However, he made little reference to it in his copious writings, possibly because of his rather exotic views on the subject, to wit,

> 'It is useless to inquire whether infants, left to themselves, would invent a language. It would be impossible, unnatural and illegal to try to experiment, and without repeated experiments, the assertion of those who believe and those who disbelieve the possibility of children inventing a language of their own are equally valueless!' (1861, p. 480)

Thus Muller takes the possibility of replication as an essential criterion of 'genuine' experiments and, presumably, empirical observations in general. For a student of history this is a fairly extraordinary point of view!

We shall begin by giving some relevant details of Akbar's life and character, since we assume that neither Akbar himself, nor the mode of life of a Moghul Emperor, is familiar to most readers of these pages.

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[12] One may reasonably speculate that other investigations may come to light. For example, there is a tantalizing reference by M. Robert Réboul in Hervé (1909) to an attempt to carry out an isolation experiment by Louis-François Jauffret in the early 19th century. Jauffret was the founder of the famous *Société des Observateurs de l'Homme*. Hewes (1978) contains further details of this possible 'lost' experiment. Although we have described this method of investigation as 'barbarous', this last example shows that the method's appeal is not limited to the mediaeval mind. Indeed, at the time of writing this note we have received news of an attempt by a Californian group to obtain funds (and presumably permission) for such an experiment in the remote southern seas . .

The standard biographical source for Akbar for many years has been Smith (1917), which contains a useful, but biased bibliography of contemporary material. Smith, like most biographers, concentrates on economic and political aspects of Akbar's reign and, in fact, fails to mention the experiment. Smith's biography has now been superseded by Srivastava (1962, 1967), which is much superior. Good sources for the religious and intellectual history of the period are MacLagan (1932) and Aziz (1969).

Akbar became Moghul Emperor on the death of his father in 1556. His early career showed no particular unusual features, except for a notable degree of religious tolerance (for instance he abolished a poll-tax on non-Muslims and took non-Muslim wives) which may well have been adopted as a political expedient, and a taste for exotic sports, such as pigeon-flying and polo (played in the dark with slow-burning balls of lignite) as well as the traditional royal pursuits of falconry and hunting. Hunting of large game in Moghul India was a particularly bloody business, involving the slow encroachment over several days of a 60 mile diameter circle of beaters on the hunters located at the centre. In one such *qamargha* during Akbar's reign 15,000 animals were slaughtered.

The first sign of eccentricity appears in 1575, when Akbar built the 'Ibadat-Khana (Hall of Worship) in his palace at Fatehpur Sikri, just south of Agra. This was, despite its name, a debating hall, built in the form of a cross. Akbar was illiterate and, naturally, greatly valued oral debate, although he possessed, and was said to be thoroughly acqainted with, a library of 24,000 books and manuscripts. The four wings of the cross were occupied during debates by the Ularna (Islamic jurists and professors), the Sayyids (descendents of Muhammad), the Shaikhs (wizards and prophets) and finally, dilettante members of Akbar's court. Some details of the subjects of

debate are provided in The Dabistan[13] and Badauni[14]. It is 1578, however, which marks the intellectual watershed of Akbar's life. At this time he experienced a kind of vision and, as a result, adopted a thoroughly free-thinking, though still spiritualised, mode of life. He introduced various non-Muslim regimes, e.g., special fasts, into his personal habits, abandoned the murderous *qamarghas* (described above) and introduced into the debates in the 'Ibadat-Khana Christians, Hindus, Jain Buddhists, Jews, Zoroastrians and Sabeans. He established himself in partial authority over the faith by means of the so-called Infallibility Decree of 1579 and invited the first of three Jesuit missions from Goa to his court. These missions failed in their object of conversion, Akbar having deep rooted objection to (1) obedience and submission to the Church and (2) dismissal of all but one wife, the latter action being, of course, politically impossible. In fact, it is clear that Akbar's interest in Christianity was almost entirely academic. He wished to introduce diversity into the debates in the 'Ibadat-Khana and also to obtain fresh ideas about possible faiths. In 1582 he promulgated, with the connivance of Shaikh Abu'l Fazl, his closest advisor, a new faith - the Din-i-Ilahi - which was eclectic and ecumenistic in nature, bringing together what Akbar perceived as being the most valid doctrines of the religions that he had studied. This faith did not survive him. The language experiment, which we now describe, was carried out during the period 1578-82 and thus coincides with this great intellectual and spiritual crisis in Akbar's life.

4.2 Sources

We give below, in full, the three contemporary accounts of the experiment.

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[13] The *Dabistan* is a Persian manuscript thought to have been written by one Muhsin Fani around 1648. It deals with various religions and, in particular, with the origins of Akbar's own faith, the Din-i-Ilahi The text given below is extracted from section Vlll of David Shea's English translation (Paris, 1843).

[14] Mulla Abdul Qadir Badauni published a three-volume history in Persian (the second volume being devoted to the reign of Akbar) shortly after Akbar's death, the *Muntakhab-ut-Tawarikh*. The whole work has now been translated into English and published in the Bibiliotheca Indica series by the Asiatic Society of Bengal. The text given below is from the translation of volume II by W. H. Lowe (1884), p. 296

The first of these is from the Akbarnama of Abu'l-Fazl.[15] The Shaikh was a close personal confidant and advisor and, like Akbar himself, a partisan of free-thinking attitudes to Islam. Although Abu'l-Fazl's history is given a low valuation by Smith (1917) and also by other Europeans such as Beveridge (cf. footnote 16) and Haig (1937), this is called in question by Srivastava (1962) and by Aneer (1973). The work is often described as tedious, repetitious, full of pointless flattery and obscure allusions. That may be, but nothing factual in the Akbarnama has been faulted and the work was based closely upon state papers and other court records including *aide-memoires* recounting Akbar's every action and word. Akbar (like other Moghul emperors) kept a close eye upon posterity and employed scribes to follow him and compile these *aide-memoires* for the benefit of his historian, Abu'l-Fazl. For these reasons we feel that the greatest weight should be given to his account of the experiment, which now follows:

> 'One of the occurrences was the testing of the silent of speech. There was a great meeting, and every kind of enlightenment was discussed. In the 24th Divine year (1578) H.M. said that speech came to every tribe from hearing, and that each remembered from another from the beginning of existence. If they arranged that human speech did not reach them, they certainly would not have the power of speech. If the fountain of speech bubbled over in one of them, he would regard this as Divine speech, and accept it as such. As some who heard this appeared to deny it, he, in order to convince them, had a *serai* built in a place which civilized sounds dit not reach. The newly born were put into that place of experience, and honest and active guards were put over them. For a time tongue-tied wet-nurses were admitted there. As they had closed the door of speech, the place was commonly called the Gang Mahal (the dumb-house). On the 9th August 1582 he went out to hunt. That night he stayed in Faizabad, and next day he went with a few special attendants to the house of experiment. No cry came from that house of silence, nor was

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[15] The *Akbarnama*, once again a Persian manuscript, was translated into English by H. Beveridge and published in 3 volumes by the Asiatic Society of Bengal in their Bibiotheca Indica series (1897-1910). The text which follows appears on pp. 581-2, sect. 393, volume III of that edition.

any speech hear there. In spite of their four years they had no part of the talisman of speech, and nothing came out except the noise of the dumb. What the wise Sovereign had understood several years before was on this day impressed on the hearts of the formalists and the superficial. This became a source of instruction to crowds of men. H.M. said, 'Though my words were proved, they still are saying the same things with a tongueless tongue. The world is a miserable abode of sceptics. To shut the lips is really to indulge in garrulity. They have hamstrung the camel of the Why and Wherefore, and have closed the gate of speech with iron walls.

<div align="center">

Verse

Enough, Nizami, be silent of discourse,
Why speak to a world with cotton in its ears,
Shut your demonstrations into a narrow phial,
Put them all in a phial and place a stone thereon.'

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We may note, for further reference, two points of ambiguity. First, it is not clear whether the speech, whatsoever it might be, is to be regarded as Divine or whether the type of speech that it is (e.g., Persian, Hebrew, etc.) is to be counted as Divine. That is we do not know whether Akbar entertained two hypotheses (no speech, some particular known speech) or three (no speech, some unknown speech, some particular known speech). Second, something 'came out', *viz*. 'the noise of the dumb'. It is unclear to us exactly what is meant by this. Beveridge makes no additional comment here. Although a scholar of 16th century Persian might be able to make this more precise, it seems likely that the outcome will remain essentially ambiguous: were they simply 'noises' or perhaps a kind of language, however primitive?

The second contemporary account is by Badauni. Badauni was an orthodox Sunni Muslim and detested Akbar's religious vagaries. His history contains much derogation of these tendencies and, accordingly, was not published until after Akbar's death. Srivastava (1962) regards it as bigoted and less reliable than the Akbarnama and observes that the translation of the relevant

volume by Lowe is inaccurate.

'At this time they brought a man to Court, who had no ears nor any trace of the orifices of the ear. In spite of this he heard everything that was said to him, though the place of the ears was quite level. And in this year, in order to verify the circumstances of this case, an order was issued that several suckling infants should be kept in a secluded place far from habitations, where they should not hear a word spoken. Well-disciplined nurses were to be placed over them, who were to refrain from giving them any instructions in speaking, so as to test the accuracy of the tradition which says: 'Every one that is born is born with a natural tendency', by ascertaining what religion and sect these infants would incline to and above all what creed they would repeat. To carry out this order about twenty sucklings were taken from their mothers, for a consideration in money, and were placed in an empty house, which got the name of 'Dumb-house'. After three or four years they all turned out dumb and the appellation of the place turned out prophetic. Many of these sucklings became the nurselings of mother earth:

> My mother is earth, and I am a suckling,
> The propensity of children for their mother is strange,
> Soon will it be that resting from trouble
> I shall fall drunk with sleep on my mother's bosom.'

We may note that the connection between the earless man and the experiment is, to say the least, obscure! The 'natural tendency' referred to is, of course, to Islam. The final metaphor is clearly for death.

The third and last truly contemporary account is by Hieronymus Xavier, who had charge of the third Jesuit mission to Akbar in 1598. The account appears in a letter of Xavier's, which was published in a number of different locations[16] . We give the translation by Beveridge (1888). A convenient source for this translation is MacLagan (1932). Xavier is reporting a conversation with Akbar himself:

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[16] Xavier's letter (in Latin) originally appeared in a pamphlet by John Oranus of Liege (1601), *Japonica, Sinensia, Mogorana, hoc est, de rebus apud eas gentes a Patribus Societas Jesu anno 1598 et 99 gestis*. The original Latin and an English translation appear in Beveridge (1888).

'He told me that nearly 20 years ago he had 30 children shut up before they could speak, and put guards over them so that the nurses might not teach them their language. His object was to see what language they would talk when they grew older. He was resolved to follow the laws and customs of the country whose language was that spoken by the children. But his endeavours were a failure, for none of the children came to speak distinctly. Wherefore at this time he allowed no law but his own.'

Although there are some discrepancies with the account in the Akbarnama, the details are very similar and there is a perfect correspondence of dates. Xavier, like Badauni, attributes a purely religious motive to Akbar. Abu'l-Fazl is, as we have seen, much less clear about this. After all, according to Abu'l-Fazl, Akbar's own belief (in advance of the experiment) was that there would be <u>no power of speech</u>. It is interesting that the motive, even if religious, is still different from the motives of Psammetichus and James IV, which were ethnological.

Turning now to secondary sources, we have managed to discover five that are not far removed in time from the date of the experiment. These are Purchas (1626, p. 516), Sennert (1643), the Dabistan (footnote 13), Manucci (1653ff.) and Catrou (1705). Purchas' and Sennert's accounts are clearly based on Xavier's and add nothing to it. The others differ, in different ways, from the primary accounts and are given in full.

From the Dabistan (ca. 1648):

'In like manner, a number of children were put in a place called Gangmahel, where everything necessary was furnished to them; but none could articulate a letter; having remained there to their fourteenth year, they were found to be dumb, which made it evident, that letters and language are not natural to man, that is, cannot be used unless they have been acquired by instruction, and it is then only that the use of conversation becomes possible. From this the conclusion was drawn that the world is very ancient and language of long date.'

From Manucci (1653ff.):

'Akbar had been anxious for a long time to satisfy two subjects of
curiosity, which he kept in his heart. The frst was to know what
language a child would speak who had not the use of speech or any
master to teach it. The second was to find the source of the famous river
Ganges. For the first of these inquiries he ordered the erection of a house
with many rooms at a distance of six leagues from the city of Agrah, and
directed them to place in it twelve children, who should be retained there
till the age of twelve years. An injunction was laid on everyone that,
under pain of death, no one should speak a word to them or allow them to
communicate with each other. This was done, because one set of men
asserted that they would speak the natural language, that which was the
language of our first parents. Others held that they would speak the
Hebrew language; others that they would not speak anything but
Chaldean; while the Hindu philosophas and mathamaticians asserted
that they must infallibly speak the Samscript (Sanskrit) language, which
is their Latin. However, the twelve years having passed, they produced
the twelve children before the king. Interpreters for the various
languages were called in to help. Each one put questions to the children,
and they answered just nothing at all. On the contrary, they were timid,
frightened, and fearful, and such they continued to be for the rest of their
lives.'

From Catrou (1705):

'It may be said, that curiosity and a thirst for knowledge were the ruling
passions of Akebar. His indulgence of these propensities prompted him to
a very singular experiment. He was desirous to ascertain the language
in which children would express themselves, who had been kept in
ignorance of the articulate sounds of any known language. The emperor
had been informed, that the Hebrew was the original language of the
human race, and the one, which all, who had not been taught any other,
would naturally speak. In order to secure a conviction on this point, he
ordered twelve children to be taken from the breast, and to be closely
confined in a castle, which was situated six leagues from Agra. They
had given to them, for nurses, twelve women, who were dumb, with the
addition of a man, who was also dumb, to save as porter. The porter was
forbidden, on pain of death, ever to open the gates of the castle. When the

children had attained the age of twelve years, Akebar commanded that they should be brought into his presence. He then assembled in his palace persons skilled in various languages. A Jew, who was at Agra, was appointed to the office of deciding, whether the language to which they might give utterance, was Hebrew. The capital furnished Arabians and Chaldeans in abundance. The Indian philosophers, on their side, contended, that the children would speak the Sanscrit, which is the dialect of the learned of the country, and holds among them the same place, as does the Latin among the learned in Europe. The ancient books of philosophy and the Indian theology are written in this language. When these children appeared before the emperor, to the surprise of every one, they were found incapable of expressing themselves in any language, or even of uttering any articulate sounds. They had learnt, from the example of their nurses, to substitute signs for articulate sounds. They used only certain gestures to express their thoughts, and these were all the means which they possessed of conveying their ideas, or a sense of their wants. They were, indeed, so extremely shy, and, at the same time, of an aspect and manners so uncouth and uncultivated, that it required great labour and perseverance to bring them under any discipline, and to enable them to acquire the proper use of their tongues, of which they had previously almost entirely denied themselves the exercise.'

These secondary accounts are of dubious value. To varying extents they differ from the contemporary accounts and, naturally, from each other. This is true even of Catrou and Manucci, and in that case we know that the former is directly based upon the latter. In the case of discrepancy we must (other things being equal) prefer the contemporary accounts. So we may reasonably discount the claim in Manucci that the children were not allowed to communicate with one another, which makes a nonsense of the whole experiment. Why undertake the cost and trouble of isolating a large group if isolating one child would be sufficient? The *Dabistan* is interesting on account of its remarkably perceptive conclusion. Catrou's is undoubtedly the most provocative account, since it comes closest to what we would suppose to

be the most likely outcome of such an experiment (cf. discussion below). However, without arduous and costly bibliographic research (which would very likely be fruitless) it is impossible to evaluate Catrou's claims about the outcome of the experiment. As is commonplace in historical research, the state papers and other fundamental documents available to Catrou are now lost (Srivastava, 1962).

At present, then, we are justified (or so it seems to us) in assuming that Akbar's experiment tells us only that a system of communication based upon speech cannot be created in a single generation. This is the strongest claim compatible with Akbar's data. It is possible that, had the period of isolation been longer, such a system would have been developed. Equally, it is possible that there was a rudimentary system of this sort but that its presence went unnoticed. The difficulty of distinguishing language from gibberish (cf. glossolalia) is well-known.

4.3 Plausibility

We would like to begin by drawing attention to the scientific isolation of this experiment of Akbar's. As far as we know there is no general treatment of the development of scientific thought in India (as there is, for example, in the case of China, cf. Needham, 1954-). Islamic influence on the development of astronomy, chemistry and medicine in the West has been considerable, and Frederick II's anachronistic empiricism can perhaps be traced to the Islamic faction within his court. Yet, a recent survey of the intellectual influence of Islam in India (Aziz, 1969) fails to mention any scientific involvement.

Although in the case of Akbar it is superfluous to investigate the plausibility of his experiment, since the direct evidence is so strong, it is worth doing so since it shows that plausibility (obviously not a sufficient condition for attributing such an experiment) is not a necessary condition either. There is no convincing evidence that empirical procedures were

common at the time of Akbar in India. So Akbar's experiment must be regarded as inconsistent with what other information we have about his background and scientific interests. Indeed, the only grounds on which we might claim that the experiment was a plausible one are that Akbar, as shown by his activities in the 'Ibadat Khana, had a strong interest in fundamental religious questions, many of which are genetic in nature. But the tremendous costs of his fantastic many-sided debating forum surely suggest that Akbar regarded scholarly argument, rather than experiment, as the proper way of resolving such questions! Accordingly, we ought to conclude that it was not in character for Akbar to carry out this experiment. And yet he surely did, so it may be that, although it seems to be an entirely natural procedure, our practice of assessing plausibility may be quite valueless as far as the question of authenticity is concerned.

5. General Discussion

Plainly there is little to be gleaned from these experiments which bears on the genetic question. The only experiment with a relevant outcome is Akbar's and there, as we have already remarked, the outcome is ambiguous. The 'noise of the dumb' reported by Abu'l Fazl and corroborated by Xavier could have been practically anything from silence to fully-fledged language. Moreover, in the absence of speech we would expect there to have been some gestural communication (see below for substantiation) and yet this was not reported in the contemporary accounts.

The second issue to be dealt with here is more troublesome: can we, accepting the impossibility of contemporary 'royal investigations', do anything to answer the genetic question today? Our discussion of this issue will be brief and rather dense

First of all, it seems to us that the classical question, 'What language will be spoken by children isolated from birth?' could and should be made more precise. A number of relatively free parameters can be recognised, as follows:

(1) the presence of one or more <u>caretakers</u>;

(2) the <u>number of children</u> isolated;

(3) the <u>ages of the children</u> at the start of the experiment;

(4) the <u>period of isolation</u>;

(5) the set of <u>communicative resources</u> available to each ordered pair of individuals (channel) in the group. By this is meant the repertoires of detectable (i.e., visible, audible, tactile) gestures <u>able</u> to serve as signs;

(6) the <u>system for communication</u> that is (by virtue of experience and/or heredity) <u>natural</u> to each individual;

(7) the <u>system for communication</u> which has been established (by intra-group contact) for each ordered pair of individuals (channel) in the group;

(8) the <u>communicative load</u> during the period of isolation--that is, the interests, wishes, intentions, needs, etc., of the ordered pairs of members of the group insofar as they (a) are novel and (b) relate to joint action and interaction, and

(9) the physical circumstances of isolation.

For any particular experiment, some of these parameters will be constant ((1), (2), (3), (4), the natural system of the caretakers, and (9)) during the period of isolation. The remainder will vary with ongoing sensory and motor development (5), affective and cognitive development (8) or with all parameters (the natural systems of the children, and (7)). When different possible experiments are considered, however, all of the constant parameters must be regarded as variables. In addition, some of the variable parameters may be varied between experiments: most obviously, selection of deaf and/or dumb caretakers and/or children will interfere radically with parameter (5). Although parameter (6) has a clear interpretation for caretakers and will in general be a natural language, it is less clear for the case of the children. What we intend here is the minimal system of meaningful audible and visible gesture which is given by inheritance together with (for points later in the course of an experiment or for later starting points) the modification and augmentation of this system which results from development of the systems

of variable (7). Roughly, for each child, the system mentioned in (6) will be revised so as to correspond with the union of the systems of variable (7) for each channel involving that child. Now, since the values of all parameters other than (7) are either (a) available for direct manipulation or (b) dependent on relatively autonomous psychological development, we may think of the array of values mentioned in (7) as the output of a very general time-function of the other eight parameters.

The classical question may then be seen as a question about the <u>value</u> of this function (let us call it the language acquisition function, LAF) for a limiting[17] set of values of its arguments, namely where <u>no</u> resources are available on any caretaker-originating channel and where <u>no</u> system of communication has been established on any child-originating channel. In fact, it is evident that this is not only a limiting case of the general function but also an unattainably ideal one. In practice, no caretaker could be 'pruned' of communicative resources and still be able to take care. Equally, children who were isolated <u>without a caretaker</u> before their communicative resources had developed to the point where systems of communication could be established would simply not survive. It is therefore quite plain that the classical question (construed as this ideal case) cannot be answered directly, i.e., empirically. However, if we could discover the nature of the general function LAF (by determining its value for other empirically and ethically feasible values of its arguments) then we could give a theoretical answer to

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[17] We shall not discuss the most extreme limiting case, where a single infant is isolated. As is well-known, there have been a number of celebrated 'natural' experiments of this sort (cf. for reviews, Panconcelli-Calzia 1935 & 1937; Zingg 1940, and Brown 1958 (Chap.V); for the most interesting such cases, see Lane 1977, Curtiss 1977, and the strange and possibly fanciful Armen 1971). It is sufficient to note that in no such case has the isolate shown signs of being able to communicate in a systematic way and to record our own view that it is inconceivable that any should, except for the sort of communication system that might be established with an animal caretaker. From the point of view of this paper, a more interesting 'natural' experiment would be one in which two or more infants were accidentally isolated. However, apart from the dubious case of the wolf-children of Midnapore (Singh and Zingg 1942; Ogburn and Bose 1959; MacLean 1977), who showed no reported signs of ability to communicate, there are no usable reports of such cases.

the classical question. In fact, we know a little about LAF already. For example, Feldman *et al.* (1977) have shown that in the case of only children who are profoundly deaf and who are raised by parents who deliberately inhibit conventional visible gesture, the output of LAF after 3 years or so is a system of visible gesture substantially more complex than the residue of natural gesture (pointing, etc.) employed by these caretakers. Further, it has often been noted (cf., e.g., Stokoe, 1974; Tervoort, 1961) that groups of young deaf children will develop (with minimal assistance or, indeed, despite active opposition) quite complicated gestural systems well-adapted to their communicative load. So it seems as if LAF has some limited powers of augmentation, a position which is also implicit in the hypothesis that the 'creole continuum' cf. Bickerton, 1974) is the synchronic consequence of rapid development of an original pidgin by successive generations (Hall, 1966).

Another kind of 'natural' experiment which bears on LAF is the case of twins. It is well known that a high proportion of twins (identical or otherwise - develop 'secret languages' for communication within the twin (cf. Zazzo, 1960 II, chap.4). Although these are often marginal and simple this is not always the case. In addition to the more or less inscrutable cases cited by Hale (1888) and Jesperson (1922) there is the spectacular modern case of Poto and Cabengo (Orange, 1977)[18]. Whether such cases provide evidence for the position that LAF is an augmentative function is unclear but it seems certain at least that quite radical alterations are possible. Finally, there is a natural source of evidence which has only recently begun to be tapped, viz., cases where children with normal hearing are reared by deaf parents. Potentially, such cases are promising since they come so close to the conditions of the classical isolation experiment. However, modern urban cases are complicated by the factors of television and hearing relatives, both readily

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[18] According to Edward Klima (personal communication), the 'secret language' of Poto and Cabengo appears to be a version of English.

available speaking caretakers. Some systematic work has recently been reported by Schiff and Ventry (1976) and Schiff (1979). The latter paper gives a comprehensive bibliography of previous, unsystematic case reports. While such children typically show eventual deficits which may persist through middle childhood (Schiff and Ventry, 1976), they swiftly surpass the oral language of their principal caretakers and, at any rate in the first year and a half of language acquisition, their oral language develops in much the same fashion as normal-caretaker controls. In Schiff's words (1979, p. 581) "children, when cognitively ready, need little exposure to the normal model language to learn to speak during the early stages of development". Certainly, there is nothing in this research which speaks against the notion that LAF is augmentative and much that speaks for it.

However much information such natural experiments may yield, it remains unlikely that it will be sufficient to determine the function LAF. Moreover, the timespan of such investigations is perforce long and their moral character (given the alternative of intervention) dubious at best. A more practical, if less gaudy, method for obtaining further information about LAF would involve parents' voluntarily adhering to speciific communicative regimes for short periods during development whilst closely monitoring the systems for communication employed by the children during this period. Natural experiments which approach this characterisation have now been quite widely investigated (cf. the studies collected in Snow and Ferguson, 1977). All that would additionally be involved is the element of control over caretaker-originating channels and control or measurement of the communicative load.

As a final point, we would like to mention the old argument due to Hale (1886, 1888) that the geographical distribution of unrelated language isolates is correlated with the probability of abandoned groups of children managing to survive. Thus, in hostile environments, such as the Arctic regions or the

great deserts, no unrelated languages are to be found, while in regions where the climate is favourable and food is plentiful (Hale's favourite example was California) many unrelated languages are found. Hale used this presumed correlation as evidence supporting his view that abandoned groups of children would routinely re-invent language. This now seems rather hopeless, since the number of apparently unrelatable languages has shrunk so dramatically since Hale's time. The question which now arises is whether the lack of language isolates in favourable areas constitutes evidence against Hale's view that rapid language invention is a routine consequence of isolation? That is, granting the possibility of rapid language re-invention under conditions of isolation, should there not be many more unrelated languages than there appear to be? We think not: the processes of pidginisation and creolisation show how massive and rapid the effects of contact can be. It seems more reasonable to suppose that unrelated language isolates are a consequence of geographical, political or economic isolation, rather than of natural isolation experiments![19]

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[19] An additional ground for not discarding Hale's hypothesis is that it is highly likely that such natural isolation experiments have occurred and will continue to occur in politically turbulent areas such as Central Africa and SouthEast Asia. The displacement of the Californian Indian peoples resulted in one known case (the Lone woman of San Nicolas Island, see Heizer and Elsasser 1963) which might well have been a case of the type Hale envisaged (but was not). Also, Moffat (1842, p. 10-11) in a discussion of the varieties of dialect found among the Balala and Sechuana Bushmen of Southern Africa observes that '[on hunting and scavenging expeditions] fathers and mothers and all who can bear a burden, often set out for weeks at a time, and leave their children to the care of two or more infirm old people. The infant progeny, some of whom are beginning to lisp, while others can just master a whole sentence, become habituated to a language of their own .... In the course of a generation the entire character of the language is changed.'

Chapter 6: On Innateness: *Nec rasa est, nec omnia tenet*[1]

This chapter presents a sceptical view of innateness claims made by linguists, notably Chomsky (e.g., 1980), and by some psychologists. Although the topic of innateness is much discussed, there is in linguistic circles a clear consensus that linguistic research can help us to discover aspects of language-relevant biological endowment and, for some linguists too, there is the notion that supposed biological/psychological constraints on what might be learned should have a shaping influence on the form of linguistic theory. The suggestion is made that as little as possible should be specified by the grammars of particular languages and as much as possible should be specified within a theory of universal grammar, the latter being a representation of innate endowment.

So long as this tendency was confined to linguists, a sceptical psychologist could tolerate it as an aberration of outsiders, in the same sympathetic way that allowances are made for foreigners who are ignorant of local custom. However, in recent years the disease has spread to the formerly healthy branches of developmental psychology. Innateness hypotheses, supported by cognate arguments, are now commonplace in the literature of cognitive development. So it is time for the sceptic to give utterance! Part of my discussion has an autobiographical flavour and I will therefore depart occasionally from the usual impersonal approach.

1. Preamble

I am old enough to remember the fag-end of the behaviourist hegemony in the early 1960s. In those days the mind of the infant was wrongly regarded as a *tabula rasa* - a blank passive slate on which experience inscribed associations amongst stimuli and responses. My own enthusiasms were for the rather different views of Piaget, and of the group centred around Bruner and Miller at Harvard and my own teacher, Margaret Donaldson's group in

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[1] Published as Campbell, 1988.

Edinburgh, who shared the view that the developing infant constructed

representations of the world around him in an active, eventually thoughtful

way and that behaviour could be explained only in reference to these cognitive

representations and the intentional connections they support. While that sort

of approach to developmental psychology enjoyed a brief period of popularity,

it now shows signs of being overtaken by a new hegemony: in this new

cognitive psychology, which describes itself hubristically as a branch of

'cognitive science', the infant is represented as a *tabula omnifera* - a slate on

which all necessary systems of representation are already inscribed.

However, the model of mind is once again a passive one - the child plays only

a trivial role in shaping the course of its development. In my view this new

hegemony is just as repulsive as the previous, behaviourist paradigm. Why?

Because it denies the existence of the 'struggle to understand' which is so

obvious a feature of cognitive development to anyone who cares to look at

children with unprejudiced eyes. Further, it replaces the previous wrong

view of our relationship with animals with a new equally wrong one. Under

behaviourism, differences between animal species were ignored because they

were thought to be superficial and uninteresting. Under cognitivism,

differences between animal species are emphasized and exaggerated. In

neither paradigm is an effective comparative psychology of mental

development possible. To clarify my own position, I think that there are no

interesting systems of representation inscribed on the slate; I think that the

representations that matter are actively constructed by the child in the

manner adumbrated by Piaget and Bruner and I think that developmental

psychology must look constantly at animal models if it is to avoid serious

error. What I want to argue here is that this new tendency is not by any

means forced on us, despite the urgings of Chomsky and Fodor. I will discuss

two examples from infant psychology briefly and devote the remainder of the

chapter to the many manifestations of the tendency in the field of language

acquisition.

2. Infant Development

The first example is the well-known phenomenon of categorical perception of phonetic contrasts in infants of a few months old. It was demonstrated originally by Peter Eimas (Eimas *et al.*, 1971) that such infants noticed changes in voice onset time of synthetic stop consonants only when these changes crossed the boundary of around +35ms which distinguishes voiced from voiceless stops for most English speakers. Since a good many other languages exploit the same boundary in the same manner, this was quickly seized upon as evidence of innate preparation for speech perception. Infants in cultures whose language exploits a different boundary showed categorical perception with discrimination occurring at the same +35 ms boundary, so the discrimination is clearly unlearned. However, a subsequent experiment by Kuhl and Miller (1975) showed that the <u>chinchilla</u> - a rodent with similar auditory function to man - also discriminated VOT categorically at +35 ms. Since it is clear that chinchillas are not innately prepared for speech perception, the claim for infants should therefore (pending further evidence) lapse as well. However, it did not. Eimas, and even Kuhl herself, now systematically ignore the chinchilla result and maintain the fiction that such experiments demonstrate an innate capacity for speech perception. Of course, it is perfectly possible to represent the categorical perception result in this way, but it makes as much sense as to say that we are innately prepared to wear shoes, or to operate scissors. Certainly our shoes fit our feet and scissors fit our hands, but it is the feet and hands that have influenced the design of the shoes and scissors, and not vice-versa. In order to justify a non-trivial claim about innate capacity for speech perception it is necessary to demonstrate that the functional requirements of speech perception (or of some closely-related auditory process) somehow shaped the development of the capacity. Unless we can establish a causal linkage of this kind as a

plausible event in human evolution it makes little sense to speak of innate capacity. Some may say that this is all very well, but the work still shows that speech perception in some languages exploits biologically determined structures, but I don't think the empiricist position in relation to language ever amounted to less than this anyway. That position might be stated in the terms that languages exploit pre-adapted structures. Just as speaking involves the cooperation of organs all of which have other prominent natural functions, so the perception of speech exploits the pre-adapted structures of the auditory system.

The second example I will discuss is the claim for neonatal imitation made by Meltzoff and Moore, 1977. Meltzoff and Moore claimed that neonates would imitate mouth-opening and tongue-protrusion and that the precision of imitation was sufficient to justify the further claim that these imitations depended on the presence of a system of intermodal representation in the infant. The neonate's mouth movements cannot be monitored visually, but only kinesthetically (using the muscle sense). So Meltzoff and Moore argued that these movements must be represented in <u>both</u> sensory modalities, implying an innately-specified system of intermodal representation. Meltzoff and Moore's result has been enthusiastically received and is now routinely included in introductory textbooks as evidence of innate imitative skill and representational ability. It is also widely taken as evidence that Piaget (1951) was quite wrong to locate the emergence of imitation of unseen movements at around 12 months.

Now, the data supporting Meltzoff and Moore's claims are muddy and controversial; the experiment has a history of subsequent instability, some workers finding effects, some failing to find them: those who do find effects have similarly weak data that may be just as readily impeached. However, even if clear evidence of neonatal imitation of these mouth-movements and some hand-movements were obtained, there would still be much to complain

about. First of all, the mouth and hand movements constitute elements of the baby's suckling repertoire. Mouth and hand movements stimulate the breast to deliver milk. This repertoire is found throughout the mammalian order and is undoubtedly fixed by heredity. These fixed actions are released by the stimulus of the breast and can surely be released by a wide range of other stimuli, too. In fact they are all performed regularly, whenever the baby is moderately aroused. So what has to be explained in neonatal imitation is a simple mapping of modelled actions onto a tiny repertoire. There is clearly no need for anything more in the way of intermodal representation than a crudely-specified stimulus-response connection! The neonatal cortex shows only rudimentary activity and so these behaviours are very likely under brainstem control. With the exception of Meltzoff, recent comment in the technical literature (see, e.g., Vinter, 1986) has ascribed the observed reactions to structures in the <u>superior colliculus</u> and has pointed out that (a) the imitations disappear subsequently when cortical mechanisms take control and (b) do not reappear until much later, around the time suggested by Piaget. Neonatal imitation then, such as it is, does not bear on the Piagetian account and certainly does not force the assumption of a system of intermodal representations.

3. On Disparaging Apes

To introduce the topic of innateness claims in language acquisition I will begin with a personal reminiscence. Some years ago I was enjoying lunch in a Paris restaurant just opposite the *Maison des Sciences de l'Homme*. My table-companions were several noted linguists and psychologists, to varying degrees committed to what is sometimes called 'the generative enterprise'. The subject of Terrace's botched chimpanzee language experiment (Terrace, 1979) was being discussed, together with Seidenberg and Pettito's (1979) disparaging dismissal of the Gardners' well-known experiments. The discussants, or some of them, drew what seemed to me quite unwarranted

satisfaction from these two efforts, which I had regarded as rather crude, if commonplace, demonstrations of academic incompetence and malice, respectively. They went on to disparage the chimpanzee in general, mocking its efforts to master a system devised by another species and making the observation that if chimpanzees had any language-learning capacity, why hadn't they developed such a system in the wild state? I will return to this argument later. For now, let me say that it was at that moment that I decided I wanted no further part in an enterprise that sought to distance us from the apes in this way. It seemed to me, and still seems, anti-Darwinian and anti-biological to attribute to ourselves much in the way of mental endowment beyond what we share with the great apes. We share a great deal: to mention a few shared achievements which are typically not found in other species, there is object permanence in the sense of Piaget (1954), productive imitation of novel models, insightful solutions of novel problems, culturally-transmitted tool-use, and self-recognition in mirrors. Comparison of the course of acquisition of these achievements in man and ape is often instructive. In the case of sensori-motor development, research by Chevalier-Skolnikoff (1983) gives us just such an illuminating result (see her Tables 6 and 7). Treating the sequence of sensorimotor stages as a rough scale, Great Ape acquisition proceeds more rapidly up to stage IV (manual search for a screened object) but then proceeds much more slowly. The ape's rapid early progress fits the well-known neotenous relationship between ape and man for physical characters (e.g., hair covering and dentition are also more rapid) but she is then overtaken. Since most other mammals progress rapidly to stage IV and then go no further (this is the case for Old World Monkeys, for instance), a plausible hypothesis here is that stages I to IV are due primarily to genetically determined developments common to many mammals while stages V to VI may require the construction *via* some learning process of efficient mental representations of objects and space, as Piaget suggested.

I would like to return now to the argument that spoiled my lunch ten years ago in Paris - the argument that since chimpanzees had developed no language of their own, it was hardly surprising that they failed to develop one under instruction. This is a stupid argument and these clever men would not have made such a stupid argument were they not blinded by their prejudice that we possess, to use Chomsky's unhappy phrase, 'a mental organ' which the chimpanzees lack and which enables language acquisition. Chomsky himself has offered the same stupid argument, with his customary hedges and *caveats* (e.g. Chomsky, Huybregts and van Riemsdijk, 1982), and it has been much aped by fellow 'entrepreneurs' such as Lightfoot, e.g. 1982, p. 166:-

> '..the important question concerns not what an animal can learn, but what an animal does in fact learn under natural conditions. It would be an incredible evolutionary accident if chimpanzees had a usable capacity for humanlike language but in fact did not use it except when subject to laboratory training programs.'

Why is this a stupid argument? Consider a Bushman and a native of Toronto. The Bushman has an extremely limited upper Paleolithic wood and stone technology and wears animal skins; the Torontonian on the other hand is thoroughly familiar with many technologies and has a complex cognitive culture - reading and writing skills, arithmetic skills and so forth. Why has the Bushman not developed these Torontonian technologies? Whatever the answer, it is most certainly not that the Torontonian has some 'mental organs' that the Bushman lacks. A rudimentary examination of what the Bushman learns 'under natural conditions' reveals a veritable host of Lightfoot's 'incredible evolutionary accidents'. Equally, it is stupid to conclude from the apparent absence of language in the wild chimpanzee that she is incapable of learning one. On the other hand, compare the Bushman and the chimpanzee. They differ genetically rather less than two arbitrary species of frog or mouse. Mouse zoologists would probably have heated

debates between lumpers and splitters about whether Bushman and

chimpanzee should be classed as separate species or as different subspecies

or races. So we have to look for some very tiny difference that will explain how

it is that Bushmen twitter at each other in a purposive way, that they use

portable tool-kits and decorate their bodies and so forth. Why should it not be a

reasonable research strategy to attempt to account for the several small

differences between Bushman and chimpanzee culture in terms of a general

difference in cognitive capacity?

That strategy is rejected by Chomsky and his followers. Instead it is

proposed that the Bushman is endowed with a 'mental organ' which the

chimpanzee lacks. This 'mental organ' is characterized in roughly the

following way:

Chomsky's Innateness Hypothesis: The Core

1) Children inherit, as part of their biological endowment, a structure which
   specifies a sharply restricted class of grammars. This structure has
   various free parameters, some initially set to default values. When all
   parameters have been set or adjusted, we have a grammar for a possible
   human language.
2) This structure is the mental embodiment of a theory of universal
   grammar.
3) In the course of development, the linguistic experience of the child sets the
   parameters so as to correspond with the grammar of the language being
   learned.

Ancillary Hypotheses and Devices

4) *Modularity*: The mind is modular, a set of cognitive systems which have
   their own principles of development and functioning. The mind is thought
   of as an assemblage of 'mental organs', analogously with the body and
   physical organs.
5) *Poverty of the Stimulus*: How do we discover what is in the system of
   universal grammar? Grammatical analysis suggests principles of
   grammar. If a principle is not obvious 'in the stimulus', then it is
   presumed to be a part of the system of universal grammar.

6) *Psychological Reality*: Hypotheses about innate structure or about attained structures (i.e., grammars) do not depend upon special evidence of 'psychological reality'. They may be attributed to the mental equipment of the speaker/learner without further ado. This is the result of Chomsky's attitude towards psychological evidence from experiment or field study - such evidence is not regarded as privileged over evidence arising from (mainly) analysis of informant judgments.

7) *General Learning Mechanisms*: These are disparaged as useless and/or incoherent notions of negligible inductive power.

The Corollary

8) By studying our judgments about any language, say English, we can discover properties of every child's innate mental equipment (by means of the argument from Poverty of the Stimulus). Since these are properties of every child's equipment, they will be properties of every other language as well.

In the main, it has to be conceded that Chomsky presents this argument as a hypothesis about the child's innate endowment, which may have to be rejected. However, it is difficult for an outsider not to see the ancillary hypotheses as assumed primarily for the purpose of forcing the inference to the conclusions of the core. For example, (a) that modularity is invoked to justify the encapsulation of grammatical principles from involvement with semantic, pragmatic or conceptual criteria and to impoverish the data that can be used to discover them by the child; (b) that the poverty of the stimulus together with the disparagement of learning capacity is invoked to justify the currently favoured form for principles of universal grammar, namely as parametrized output conditions which require simple triggering; (c) that the no psychological reality requirement is similarly invoked because no psychological reality for such principles is ever likely to be demonstrated.

It is worth demonstrating that Chomsky's presentation of his views on innateness, despite his *caveats*, is an exceedingly arrogant one, though a model of civility compared to the position adopted by epigones (e.g. Lightfoot,

1982). Consider the following remarks (Chomsky, 1981, p.71):-

> 'A factor that impedes the study of language and, more generally, cognitive development, in my view, is the persistence of certain curious doctrines that entirely lack empirical support or inherent plausibility, for example, the Piagetian dogma that language development must reflect sensori-motor constructions'

To see that this is a pejorative assessment, let us make three simple substitutions:-

> 'A factor that impedes the study of language and, more generally, cognitive development, in my view, is the persistence of certain curious doctrines that entirely lack empirical support or inherent plausibility, for example, the *Chomskyan* dogma that *the child's mind* must reflect *principles of universal grammar.*'

In the remainder of the chapter I will attempt to justify this assessment of these 'curious doctrines'.

4. Chomsky's doctrines 'entirely lack empirical support'

As Chomsky himself admits, the argument from the poverty of the stimulus is 'non-demonstrative'. This usage recalls other American innovations, such as 'misspoke myself' and 'inoperative statement'. The argument is, more simply, invalid. This was pointed out elegantly by Bever (1982), as follows:-

Principle X is in UG	Principle X is in UG
Principle X cannot be learned	Principle X cannot be inherited
Therefore, it must be innate	Therefore, it must be learned

While Chomsky's argument conforms to the left-hand schema, the right-hand argument is equally good, or bad, as you prefer. This reversal of the usual pattern of inference is justified since in no case are we offered an evolutionary account of how these putative principles arose. Vague claims about selective pressure for powerful grammars exactly balance vague claims about general learning mechanisms. Moreover, the inductive power

of natural selection is no greater than that of the despised Skinnerian theory of learning (cf.,e.g. Dennett, 1978, Chapter 5).

So if we are to find empirical support, where are we to find it? It seems useless to search for direct genetic evidence for the hypothesis. In the first place, by hypothesis UG is a uniform trait, possessed by all speakers. Like all other mental achievements of man, language mastery has zero heritability in the technical sense. If there were substantial variation in the genotype (as Lightfoot apparently wishes to argue (1982, p. 99, Note 1) before hastily idealizing away from such variations), then surely selection would favour speakers inheriting favourable combinations of the relevant varying genes producing at best, pronounced variation in language mastery and at worst loss of equipotentiality for language learning. Of course, a trait may be biologically-determined even if it has zero or minimal heritability. This is what is claimed for general learning mechanisms, after all (cf. recent discussions of heritability of IQ). However, this is likely only if the trait has been subject to very strong selective pressure. Even such pressure is not always sufficient, cf. visual colour defect and infertility. Language, whatever else it is, is a recent achievement. Is it plausible that a few thousand generations could have eliminated the original variation in the presumed 'mental organ'?

At any rate in taking this view - that if there is an inherited UG it is approximately uniform - I have the support of Mary-Louise Kean, Chomsky's own student, who now works in aphasia research. Reviewing Chomsky, Huybregts and van Riemsdijk, 1982, Kean (1984) writes:-

'More often than not, the typical linguist reveals a serious lack of education and an unthinking attachment to catechism when succumbing to the temptation to discourse on philosophy, psychology and neuroscience..

I found myself wincing mightily over [Chomsky's] discussion of individual differences and developmental neuropsychology. Nothing that

he said was exactly wrong; but it is equally true that one could never appreciate from Chomsky's discussion that a, if not <u>the</u> central issue in the ontogeny of linguistic capacity is the fact that a surprisingly uniform capacity is supported by a physical substrate which shows a remarkable range of individual differences. This should be a serious issue for anyone that holds the view that human linguistic capacity is a biological endowment of the species in any non- trivial sense.'

So 'genes for language', even if there is a UG, are not more likely to be isolated and identified than 'genes for intelligence'. There is, of course, a substantial association between the speech areas and certain brain areas around the fissure of Sylvius in the left cerebral cortex. There are two things to be considered here: (a) the degree of specialization of these brain areas during embryogenesis. What kinds and degrees of asymmetrical development occur? (b) the actual localization of function in the developed adult brain. Regarding asymmetry, studies of skull casts of early men suggest a progressive increase in the vascular supply to the Sylvian areas. However, the supply is increased <u>bilaterally</u> and, moreover, asymmetry is apparently <u>reduced</u> in modern man (Saban, 1983). Asymmetry has also been noted in many primate and songbird species (Galaburda, 1984). Regarding lateralization, a particular function has to be located <u>somewhere</u>. The available data (see Lecours *et al.*, 1984) suggest variation in localization depending on age, handedness, illiteracy, polylingualism, employment of lexical tone and logographic script. Lecours' estimate is that only around 25% of the world's population conforms to the classical Broca/Wernicke theory of localization.

We are left with the despised psychological reality, then, as a possible source of empirical support. The history of attempts to find evidence of this sort for the systems described by Chomsky is a dismal one of failure. I can think of very few psychologists who have pursued that route for long. We might hazard a little inductive generalisation that the future will resemble

the past in the context of finding evidence for his innateness hypothesis, too. I will discuss two examples of attempts to demonstrate the psychological reality of principles of UG.

Matthei (1981) investigated 4-6 year old children's understanding of the notorious *each other* sentences discussed at length by Chomsky in the late 1970's, for example,

(1) *The cows wanted the lambs to kiss each other*

According to Chomsky's discussions such sentences should be correctly understood as soon as *each other* is treated as a reciprocal anaphor, since the other principles determining interpretation are part of UG with appropriately set defaults, or so Matthei argued. So children who treat *each other* as a reciprocal anaphor should select the clause-mate NP as antecedent. Matthei found instead that in 65% of cases his subjects took the main sentence subject NP as antecedent. This result is typical of the success of the new enterprise. I include the example mainly to point out that Matthei's subjects did not understand *each other* as a free pronoun (as is claimed by White, 1984 and also by Lightfoot, 1982. Matthei was careful to determine (so far as this is possible) whether or not his subjects treated *each other* as a reciprocal anaphor beforehand, and to discard those subjects who did not. What Matthei actually says is, reasonably, that in their faulty interpretations of the multi-clause sentences, they treat *each other* <u>as if</u> it were a pronoun like *them* (for which word a referent outside the clause must be found).

Of course, there is no real leverage on the innateness hypothesis from this and other negative evidence. The supposed elements of UG can be supposed to be not yet triggered, subject to maturation and so forth and there is always the last resort of the 'no psychological reality requirement' principle.

As a second example, Deutsch and Koster (1982) have investigated knowledge of binding principles for reflexives and free pronouns in sentences

of the form (equivalent sentences in Dutch, with identical structure).

(2) *Peter's father washed himself.*

(3) *Peter's father washed him.*

Again, given knowledge that *himself* is reflexive and *him* an ordinary pronoun, knowledge of binding principles A and B should ensure that the two sorts of sentence are correctly understood. Deutsch and Koster's 6-year-old subjects produced the pattern of error shown below, despite clearly demonstrating the relevant lexical knowledge. As in Matthei's experiment, the highest level NP - *father* in this case - is uniformly taken as antecedent.

Error rate with Reflexive Pronouns :   2%

Error rate with Non-Reflexive Pronouns: 98%

In a second experiment, groups of children aged 6, 7 and 15 were compared in a task using mixed reflexives and ordinary pronouns. Deutsch, Koster and Koster (1986) thought that mixing the types together might induce the children to separate them by drawing their attention to the formal contrast. Certainly the crudeness of the original result disappears. Six year old children now make errors on both types of sentence in around 33% of cases; 7 year old children make roughly the same amount of errors but align with the findings from the previous experiments more closely. Their errors with reflexives are only 9%, but with ordinary pronouns 48%. Interestingly, even their adult group contained one subject who misinterpreted the free pronouns. In passing, these results of Deutsch and the Kosters are reviewed by Helen Goodluck - a participant in the generative enterprise - in the following favourable way, completely neglecting the import of the high error rate with pronouns (1986, p. 60):-

'Using a picture-identification task Deutsch and Koster demonstrated that 6-year-olds are aware of the reference possibilities for the reflexive in sentences such as *John's friend washed himself* and *The friend of John washed himself*, correctly preferring the friend as referent of the reflexive in both cases.'

I prefer the description of Deutsch, given in the later paper, which reports
these slightly more interesting results as follows:

> 'Children make errors not only in the resolution of coreference relations
> between nonreflexive pronouns and their antecedents, but also in the
> resolution of bound anaphora, with reflexive or reciprocal pronouns.
> The appearance of these errors is not consistent with a position that
> holds that the resolution of reflexive (or reciprocal) anaphors is a self-
> contained part of the repertoire of linguistic knowledge which, at some
> juncture in the acquisition process, simply springs into being, like
> Pallas Athena from the head of Zeus.'

A number of points might be made here, of a cautionary nature:

1. Many of the experiments attempting to establish the psychological reality of
   principles of UG are not well conducted. There are obvious unnatural
   aspects of their design. If naturalised, more interesting results might have
   been obtained.

2. For this defect Matthei and Deutsch and Koster can hardly be blamed. No
   one has found a really satisfactory method of studying the comprehension
   of any kind of sentence with any kind of subject. 'Naturalisation' normally
   supplies extra context which allows correct interpretation to proceed
   without need of close grammatical control.

3. On the other hand, there are strong indications in the language acquisition
   literature that children in the range 5 - 10 employ pronouns in restricted
   ways. Karmiloff-Smith's studies (e.g. 1981) show a strong preference for
   pronouns to be treated as anaphoric to the theme of a narrative or subject of
   a sentence, with referentially-equivalent lexical phrases used to make
   other exophoric links.

4. A positive aspect of this research is that it reinforces the view,
   strengthening with each issue of the *Journal of Child Language*, that
   grammatical development continues well beyond the optimistic estimates
   of age 5 or 6 suggested in early days of research on grammatical
   development.

5. Adherents to the generative enterprise should at least have their
   confidence shaken that direct evidence of knowledge of binding principles
   will be easily revealed. It is also manifest that young children make quite
   gross errors of comprehension in these tasks inconsistent with any simple-

minded hypothesis of innate knowledge of the principles.

5. Chomsky's doctrines have 'no inherent plausibility'

What could make the innateness hypothesis inherently plausible would be a demonstration of how the structure UG might come to be established from a presumed initial state in which it is absent by means of some evolutionary process. But the principles of UG, as parametrised, non-semantic output conditions seem quite inaccessible to any such selectional process. What pre-adapted structure could be moulded into these forms by evolutionary forces? The generative enterprise provides no answer. I suspect the answer here is that <u>if it is inconceivable that a cognitive structure should be learned, it is likely to be just as inconceivable that it should be the product of natural selection</u>.

Notice, too, that while the recondite nature of the principles currently formulated makes the 'argument from the poverty of the stimulus' hard to resist, it has the same effect for someone wishing to follow the other Bever syllogism: - the evolutionary 'stimulus' could not possibly provide selection pressures adequate to engender the binding principles (say); therefore, they must be learned. So current developments in 'the theory of syntax' serve increasingly to purge this school of linguistics of any conceivable biological connection and will force it, in my view inevitably, into scientific irrelevance and scholastic obscurity. *Sic transit....!*

6. Postscript, 1992

In the years since the present chapter was published, there have been a few interesting developments so far as this debate is concerned. Research of the type represented by Matthei's and Deutsch's experiments has increased in quantity, so much so that the exponents - finding difficulty in getting their work published in existing organs - have established one of their own, the *Journal of Language Acquisition*. Such research may have increased in

quality, too, if the recent survey by Stephen Crain (Crain, 1991) is a reliable guide, along the lines suggested at the conclusion of the preceding section. However, for my purposes it is sufficient to note that Crain freely concedes the failure of such research (to date) to provide convincing evidence of innate knowledge of presumed principles of UG. Crain promises that better methods reveal such knowledge (at least in certain cases), but his own work is not yet adequately published and I have therefore been unable to evaluate that promise. Crain's *Behavioral and Brain Sciences* review is followed by the customary long series of commentaries, mostly approving, but the only acknowledged sceptic invited to comment was James McCawley, who saw nothing to get excited about.

Finally, it is worth drawing attention to the recent volume of collected essays by the doyen of British linguistics, John Lyons (Lyons, 1991). Lyons' fervent enthusiasm for Chomsky's work in the late 60s has now decayed, perhaps not so far as mine, but his discussions of language acquisition and language origins in Chapter 5 of that collection make strong connections with the criticisms and reservations of the present chapter.

Chapter 7: The Emergence of Representational Drawing

1. Introduction

Luquet (1913, 1927) described the earliest drawings of children as exhibiting fortuitous realism, meaning by this that although such drawings occasionally seem to resemble objects, such resemblance is only accidental. Kellogg (e.g. 1970) agreed that the earliest drawings lack content, and limited her description of the earliest stages to aspects of form, such as the elements involved in a scribble (principally varieties of line) and their placement on the drawing surface (top, bottom, middle, baseline fan, etc.). Although some apes have been successfully encouraged to make marks on paper with pencils, crayons or paint (see Morris,1967, for review), their drawings also lack content and so far as formal features are concerned seem to go no further than the sort of placement patterns noted by Kellogg.

More recent work has not altered this picture much: Matthews (1984) suggested that some two year-old scribbles might be attempts to represent dynamic aspects of an object, such as its path of motion, and Freeman (1980) showed that when asked to draw a nose, say, on an incomplete face, two year-olds would often begin their scribbling in an appropriate place. Of course, these are modest claims and as yet only poorly supported. By three years, however, most children are producing new formal elements, such as closed figures, intersecting lines and points. Evidently they are equipped to produce representational drawings, yet it is by no means obvious that they are capable of doing so. Nor is it obvious how we might go about determining whether such drawings have a content and what that content might be. This chapter is an attempt to suggest a partial solution to these descriptive problems.

1.1 Van Sommers' findings

Van Sommers (1984) is a remarkable monograph dealing with drawing. In the chapters dealing with children's drawing, he puts forward two claims:

that children's drawings show strong <u>idiosyncrasy</u> and strong <u>conservatism</u>. These claims are based on a range of ingenious studies and demonstrations carried out with children of early school age, his youngest subjects being just over five. So far as idiosyncrasy is concerned, his basic demonstration is to ask a class of schoolchildren to draw some common object such as a tree or a nose. Under these circumstances a very diverse range of drawings results, with different children adopting radically different strategies of representation (see Figure 1[1]). Figure 2 shows the attempts of a Scottish P1 class to copy the drawing shown in the top left corner. The idiosyncrasy is evident and, as this example shows, unaffected by the shift to drawing an object represented in a particular way. Van Sommers claim is amply supported by many other examples in his monograph (bicycles, light-bulbs, tennis shoes, tape-dispensers, etc.)

Turning to his claim about conservatism, this is supported by his studies of repeated drawings of the same object. When children draw the same object on successive occasions, there is strong resemblance between the repeated drawings. Van Sommers argues that this resemblance lies in the finished products, rather than in the details of execution, suggesting that children have a particular graphic goal in mind when constructing a drawing and are not simply repeating a stored sequence of drawing movements. In order to hinder this sort of motor memory from playing a part, Van Sommers' usual technique is to allow some considerable time, as much as a week, to elapse between drawings in such repeated-drawing studies. Figure 3 shows representative data from Van Sommers.

In addition to persuasive demonstration, Van Sommers offers several experiments from his laboratory. For example, Jones (1972) had one group of drawers draw a complex object such as a segmented fan before seeing it disassembled and re-assembled. They then drew the fan again. A second

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[1] Because of the large numbers of figures in this chapter, these are grouped together at the end of the chapter.

group made only one drawing, following the demonstration of the fan's structure. In terms of faithfulness to the structure of the fan, the (only) drawings of the second group were far superior to the second drawings of the first group, which rigidly repeated the naiveties of their first drawings. This result is perhaps surprising, since one might have expected the extra practice in fan-drawing to have helped the first group to produce better drawings. Although he does not acknowledge Kellogg's work, nor refer to it, Van Sommers' claims regarding conservatism are strongly consistent with Kellogg's view that children's drawings have a strong evolutionary character. In fact Kellogg and Van Sommers may be thought of as offering an account of the development of drawing which differs sharply from the standard stage theory reaching back through Piaget (1956) to Luquet and ultimately to Kerchensteiner (1905), namely, that a stage of intellectual realism - in which drawings represent what children know about the object drawn - precedes a stage of visual realism - in which drawings represent the visual appearance of the object drawn. Instead, Kellogg and particularly Van Sommers are saying that at any stage in development the way in which a child will draw an object depends more than anything on the way she has drawn that kind of object before. Again, this recalls Gombrich's claim for art history (e.g. 1972) - Art depends on Art, not on Nature.

1.3 Identifying scribbles with content

The findings of Van Sommers just described may or may not be replicable with younger drawers. However, if they can be replicated, this provides us with a method for deciding when the scribbles of younger children should be ascribed a content. If a child makes illegible but similar drawings of some object over several repetitions, but different illegible drawings of different objects, then it seems unreasonable to deny that he has produced a drawing of that object: we simply do not understand or share his artistic conventions. This point about conventions or iconography is a standard one in art-

historical discussions, perhaps even over-emphasised by some - such as Goodman (1969). There is no doubt, however, that there is an iconography of child drawing. After all, it is practically inevitable that drawers whose repertoire of graphic elements is restricted to lines, closed figures and points will have to adopt a more abstract style of representation than the adult norm. This abstract quality then leads to difficulties of interpretation. Such abstract strategies of representation occur with adult drawers, too, if the characteristics of the medium are sufficiently limiting. Munn's (1973) study of Walbiri iconography makes this point very clearly. Aboriginal skin-paintings such as the ones shown in Figure 4 were at one time thought to be purely decorative, but Munn discovered that Walbiri narratives were commonly illuminated by drawings made in the sand with a stick. With such a medium, and the requirement that construction of the drawings should not detain the telling of the tale unduly, an abstract style was adopted: curved or straight lines, closed figures, points, etc., denoted a very wide range of objects (see Figure 5). In the light of this iconography it becomes evident that the skin-paintings represent traditional scenes or stories.

The literature of child drawing is replete with similar examples. Two well-known cases are Arnheim's (1954) famous drawing of a Man with a Saw (Figure 6), which represents the teeth of the saw and all parts of the man by means of closed curves, and Willatt's (1980) drawing of a tennis ball in which the closed curve represents not the projection of the ball on the picture plane but the outer surface of the ball (Figure 7). In this latter case, the representation of a surface by means of a line is quite alien to adult practice, of course. So we have good reasons for believing that what may be quite illegible to us might nevertheless constitute an attempt to produce a representation with definite content. Van Sommers' findings then offer up the possibility of a method to decide whether apparent scribbles might be representational drawings.

2. Preliminary Observations

I have collected repeated drawings, in the context of class experiments, from three and four year-old children attending the Department Playgroup in every year since 1987. In all cases, three drawings were made of each object, usually on successive days, although on some occasions an extra day intervened. The objects drawn were presented to subjects in a fixed order and with constant orientation. Subjects drew with extra-thick pencils of triangular cross-section on A5 paper. A variety of objects were used: scissors, comb, eggcup, dark spectacles, toothbrush and comforter (see Figure 8). These objects were chosen with the notion that they would all be familiar, and comparatively simple to draw, but that subjects would have had no prior drawing experience with such objects. Drawings were later hand-copied to a standard size and weight of line, and annotated for stroke order and direction. Inspection of these repeated drawings provided ample evidence of an informal sort that these pre-school drawers often produced very conservative series. Figure 9 shows the drawings of one very conservative child, Hazel. It is also evident that there is considerable variation in the order and direction of Hazel's stroke-making, despite the strong similarities of the final products and the circumstance that drawings were made only one day apart.

However, not all drawing sets were as well-behaved as those of Hazel. It was often the case that the drawings resembled neither each other nor the target object, particularly with younger drawers. Figure 10 gives some idea of the range of resemblance found, using scissor-drawings to illustrate this. As is observed in the figure legend, sources of the obvious idiosyncrasy are not hard to discover, even in the case of a comparatively simple object like this. When a subject takes the trouble to attempt a representation of something like a scissor- screw, or to differentiate between the thick and thin teeth of a comb (as occasionally happened) this makes it very likely that the subject is not

'drawing what s/he knows' but rather making a definite attempt to draw what s/he sees. There were other clear signs that the Luquet/Piaget view is an inadequate account. For example, some subjects do take only one swift look at the object and then make their drawing without further visual reference to it, but this is by no means common: most subjects looked back and forth while drawing. Perhaps this object-oriented approach to drawing was encouraged by the fact that they were mainly drawing objects which they had never drawn before. However, even in the case of the eggcup, which is after all simply a small cup (which they might have drawn before), there were many attempts to represent the car-picture and sometimes even the personal name 'Nigel'. The latter finding was a most compelling piece of evidence, since the children naturally knew nothing of writing. Some striking cases of this sort are shown in Figure 11. In such cases there is no possible doubt that the drawing is caused by the object seen.

When the child's response is a complex object such as a sentence, or a free classification or other arrangement of objects, or - as in the present case - a drawing, tried and trusted methods of response measurement fail. It is possible to count things and to make physical measurements, but they seldom yield useful measures and always seem to fall short of answering in a valid way to the form and content of the response. In consequence, it is usual to resort to the use of independent judges to determine characteristics of the response. As a first step towards applying such a method here, judges were invited to sort, say, all the scissor drawings from a group of children into groups of 3, according to artist. While this method often produced results that accorded with intuition, it suffered from the following difficulty. It was quite often the case that a younger drawer would produce three similar drawings of an object, although the resemblance between each drawing and the object was negligible. However, such a subject was apt to make drawings of other objects which could hardly be distinguished from those of the first! In such a

case we are clearly not dealing with <u>drawings of the object</u> but rather with varieties of a *leitmotiv* or signature. So the use of judges must proceed in two stages: a first stage in which all the drawings of one child are sorted according to object drawn, and a second stage in which those drawings sets that survive Stage I are put together (according to object drawn) and sorted a second time according to artist. Thus Stage I picks out cases where a child has drawn an object in a distinctive way, so that we have some confidence that he has indeed drawn the object. Notice, however, that there is no implication of conservatism here. The three drawings might be very different from each other, but share some feature which allows them to be picked out from drawings of other objects; this shared feature might (but not necessarily) be a common point of resemblance to the target object. In Stage II only those drawing sets which exhibit some combination of idiosyncrasy and conservatism (lots of one or the other, or a modicum of both) are liable to be sorted together. While it is tempting to call this combination of properties <u>artistic style</u>, it should be borne in mind that this <u>style</u> need not be constant across all objects. That is, a child could make repeated drawings of one object in one 'style', and of the next object in a different 'style', etc., so that it is not quite the ordinary concept of artistic style which is captured by this method.

In view of these informal observations and decisions regarding method, a more systematic investigation was conducted, using a longer interval between repeated drawings, and with 5 drawings obtained of each object. The first modification was introduced to lessen the risk of 'motor memory'-induced conservatism, and the second to deal with a statistical problem, namely that the independent probability of grouping three particular drawings in an arbitrary sorting of, say, 18 drawings is already quite high (around .01). Of course, this probability becomes higher if the groupings are not independent, as is the case in a sorting task (once 9 sets have been sorted, the 10th is determined, etc.). In consequence, if a set of drawings contains

those of a few very conservative drawers, the chance of sorting the remaining drawings correctly increases sharply.

3. Method

Subjects :Twenty-four children took part in the study, 12 girls and 12 boys, of mean age 3 years 8 months (range 3;1 to 4;7). They were studied while attending a Bridge of Allan playgroup, and tested individually in an adjoining room.

Materials :The objects selected for drawing were a plastic comb, a pair of metal scissors, a tennis racket, a light bulb, a man's woollen glove and a small metal cooking pot. The objects were presented in a standard orientation on all drawing trials, namely: comb - teeth pointing to subject; scissors - open, blades to left; racket - handle to right; bulb - fitting pointing to subject; glove - fingers pointing left; pot - handle on right. These objects were chosen in the expectation that no subject would have drawn any of these objects before, but that subjects would be familiar with them and would find them reasonably straightforward to draw. Drawings were made on A4 paper with thick pencils. All sessions were videorecorded to permit later addition of stroke order and direction, although no analyses of these features will be reported here.

Procedure :Subjects were seen five times at weekly intervals. In each session they made one drawing of each of the six objects. A different random ordering of the six drawing tasks was used in each session. Each object was handed to the child for examination, then removed and laid on the table in the predetermined orientation. The child was supplied with paper and pencil and asked to draw the object. No time limit was applied. When a child stopped and looked up, s/he was asked if the drawing was finished. The time lapse of 7 days was adhered to rigidly where possible. However, due to illness there were one or two occasions when there was a longer followed by a shorter lapse before the weekly tempo resumed. According to Stanton (1973)

this long time interval does not affect the conservative nature of older children's drawing sequences.

Judging: The 720 drawings (24 children x 6 objects x 5 drawings) were handcopied onto 9x7cm. cards to eliminate gross differences in size, weight of stroke, etc. No information regarding stroke order or direction was marked on these hand-copies at this stage.These drawings were sorted by two sets of judges. In Stage I, 12 judges were used. Each judge carried out four sorting tasks - sorting the entire corpus of 30 drawings from each of four children. Children were assigned to judges so that each child's drawings were sorted twice, by different judges of course. The judges were not told what objects had been drawn, but were told that there were 5 drawings of 6 different objects, and that their task was to try to sort the drawings according to object drawn. A relatively strict criterion was adopted to decide (for a given object and child) whether the child had drawn the object distinctively (when compared with other objects). Both judges had to have included at least four of the five drawings in a sorted group. If 2 judges sort 30 drawings randomly into 6 sets of 5, the probability of this criterion being satisfied is about .00003 (see Section 6). The reason for choosing such a tough criterion was not simply the desire to stay well on the right side of chance, but also to make sure that the huge set of drawings was substantially reduced so that sorting according to artist in Stage II would not be too burdensome. For example, if all drawing sets had survived Stage I, judges in the second Stage would have had to sort sets of 120 drawings each! In the event, 220 drawings, comprising 44 drawing-sets produced by 13 children, survived Stage I.

Those drawing sets which survived Stage I were combined according to object, so that all the scissors-drawings, all the comb-drawings, etc. were grouped together. These assemblages of drawings - which varied in size from 20 to 55, depending on the outcome of the judging process in Stage I - were then sorted by a second group of 12 judges. Each judge sorted one such

assemblage. Since there were 6 objects and 12 judges, this was arranged so that each assemblage was sorted by two different judges. Once again, the judges were told nothing about the object depicted, but only that each drawer had made 5 drawings of the object and that their task was to sort the drawings according to artist. To determine (for a given object and child) whether the object had been drawn with consistent and distinctive 'style' (when compared with other children), a more liberal criterion was adopted, since there was now no need for further reduction of material, of both judges having to have included three of the five drawings of the object in a sorted group. The probability of 2 judges sorting N drawings into groups of 5 (19 < N < 55) lies between .084 and .0016 (see Section 6).

The judges greatly enjoyed these sorting tasks and would not relinquish the material until they were fully satisfied that they had done the best possible job. They were sometimes quite sceptical when informed about the nature of the objects drawn at the conclusion of the judging process, and were always eager to see the correct solutions to the task. Judges varied greatly in time taken, but some idea can be got of the zeal with which they tackled this aesthetic task by observing that times exceeding two hours were recorded on two occasions!

4. Results

Stage I: As noted above, only 44 drawing-sets, from 13 subjects, survived this stage of judging. This reduction was more severe than experience with children in the Department Playgroup had suggested. Perhaps differences in population were responsible: it is also the case that the subjects used in this study were slightly younger (they were tested in October, whereas the class experiments referred to in section 2 always took place in mid- November. Intake to playgroups is of course standard, at third birthday or later. The drawings of two subjects whose drawings were all eliminated at this stage are shown in Figures 12 and 13. John (3;1) employed a simple closed curve or

spiral, with the occasional addition of a line or smudge, for most of his drawings. Louise (3;4) produced more effortful drawings certainly, and sometimes achieved a tolerable resemblance, but her habit of including the kind of all-purpose sun-figure drawing much discussed by Rhoda Kellogg derailed the sorting process in every case. The drawings of two subjects, Angela (4;7) and William (3;10), whose drawings all survived this stage are shown in Figures 14 and 15.

Table 1 shows the drawing sets and children who survived this stage of judging.

Table 1: Subjects who produced drawings sets judged as distinctive

Child	Age	Comb	Glove	Scissors	Pot	Bulb	Racket	Total
Angela	4;7	+	+	+	+	+	+	6
William	3;10	+	+	+	+	+	+	6
John	4;7	+	+	+	-	-	+	4
Martin	4;7	+	+	+	+	-	-	4
Laura	4;0	+	+	+	-	-	+	4
Jamie	3;10	+	+	+	-	+	-	4
Duncan	3;6	+	+	+	-	+	-	4
Shonagh	3;8	+	+	-	+	-	-	3
Claire	4;4	+	-	+	-	-	-	2
Kara	3;9	+	-	+	-	-	-	2
Joe	3;6	+	-	-	+	-	-	2
Graeme	3;4	-	+	-	+	-	-	2
Stewart	3;11	-	+	-	-	-	-	1
Total		11	10	9	6	4	4	44

It is evident that there is a strong link with age: in fact the correlation between age and number of sets surviving is $r=.627$ ($N=24$, $p<.01$). There was no effect of subject sex on number of sets surviving ($t=.992$, $df=22$, $p=.35$). Table 1 shows substantial differences between objects. Presumably these are due to differences in distinctiveness among the objects which are 'inherited' by the drawings.

Stage II: Twenty-five drawing sets from 10 children survived this second stage of judging. Clearly distinctiveness within a child's output does not guarantee distinctiveness between children.

Table 2: Numbers of drawing sets judged as consistent in 'style'

Child	Age	Comb	Glov	Scis	Pot	Rack	Bulb	N	N-Stage I	Prop
Jamie	3;10	+	+	+	-	-	+	4	4	1.00
Angela	4;7	+	+	-	-	+	+	4	6	0.67
Shonagh	3;8	+	+	-	+	-	-	3	3	1.00
Martin	4;7	+	-	+	+	-	-	3	4	0.75
Laura	4;0	-	+	+	-	+	-	3	4	0.75
William	3;10	-	+	-	+	+	-	3	6	0.50
John	4;7	-	-	+	-	+	-	2	4	0.50
Kara	3;9	+	-	-	-	-	-	1	2	0.50
Joe	3;6	-	-	-	+	-	-	1	2	0.50
Duncan	3;6	-	-	-	-	-	+	1	4	0.25
Claire	4;4	-	-	-	-	-	-	0	2	0.00
Stewart	3;11	-	-	-	-	-	-	0	1	0.00
Graeme	3;4	-	-	-	-	-	-	0	2	0.00
Total		5	5	4	4	4	3	25		
Total-Stage I		11	10	9	6	4	4		44	

When assessing the relationship between age of drawer and survival of drawings in this stage, two measures seem worth examining: the number of surviving drawing sets, and the proportion of surviving drawing sets. The latter measure is clearly more appropriate, since it is independent of the number surviving Stage I. However, proportions are bound to be very unreliable with bases as low as 1 (Stewart) and no higher than 6 (Angela, William). Fortunately, both correlations are low and non-significant: for number of drawing sets surviving, r=.377 (N=13, p=.2); for proportion of drawing sets surviving, r=.176 (N=13, p=.5). Table 2 shows the patterns of survival through this second stage of judging.

Figures 16-21 show all 44 sets surviving Stage I, with those surviving Stage

II marked with a 'II'. Inspection of these drawings sets reveals the sources of style very plainly. Some comments are offered below:-

Combs: Most distinctive are Jamie's, with a unique sawtooth representation of the teeth and a variety of strange representations of the crosspiece. Kara omits the crosspiece from all her drawings. Only Laura attempts to distinguish thick and thin teeth, but she does not do so consistently. It seems a fairly remarkable feat of judging to distinguish Angela's and Martin's drawings!

Gloves: Jamie's sawtooth fingers are again distinctive, as are William's linear representations for both fingers and hand. Only Shonagh draws a pure outline. Laura draws a square hand and invariably 3 fingers.

Scissors: John succeeds in crossing the blades, a feat which lies beyond the reach of most young scissor-drawers, and all others here save Angela, whose struggles with this difficult problem lead her in different directions and confuse the judges. Kara, Jamie and Laura, though they do not achieve a satisfactory crossing, make different sorts of failed attempts.

Pots: Shonagh's pots have distinctive solid handles, and Joe's wavy linear handles are also distinctive. The source of Martin's distinctiveness might be the oval shape of his pots.

Rackets: With only 20 drawings to be sorted, this set (and the set of 20 bulb drawings) should provide a relatively straightforward task for the judges. However, the 4 sets of racket-drawings are so distinctive that they should be sortable regardless of the size of the group to be sorted. Angela's first racket is quite unlike later drawings; rather more puzzling is William's abandonment of a distinctive style on the 5th drawing.

Bulbs: Only Jamie, who consistently represents the interior parts of the lightbulb, seems to achieve the level of distinctiveness that would be likely to survive in a larger set.

5. Discussion

The judging method adopted seems to work well in Stage I, since the size of set to be sorted is constant, and can be made sufficiently large to meet criteria of arbitrary severity. Application of the method shows that many drawings which are inadequate (in the sense that they do not resemble the object to any great degree and do not permit identification of the object drawn) are nevertheless <u>drawings of the object</u>. Survival of drawings in Stage I is related to age of drawer at a satisfactory level (comparable or better than that found for intelligence test items, for example). Considering the ages of the sample studied here, it looks as if most children begin to make drawings which vary in a stable way with the object drawn between 3;6 and 4;0.

The judging method worked less well in Stage II, because of the considerable variation in the sizes of set to be sorted. However, there are indications that the combinations of idiosyncrasy and conservatism that constitute 'style' are not particularly age-dependent, and are not constant either within drawers across objects or within objects across drawers. So far as the former kind of constancy is concerned, it was observed here only with Jamie. As for the latter kind, although racket and perhaps bulb drawings were well sorted, there were such small numbers of these that it would be unwise to attach any significance to the observation. It is doubtful, therefore, whether the strong consistency of 'style' observed by van Sommers with older drawers is exhibited by these beginners. However, it should be noted that the diagnosis of content in early drawings does not depend on this factor of 'style': Stage I is sufficient to establish whether or not the child is drawing the object.

The technique of repeated drawing of a presented object, when combined with the judging method of Stage I, therefore provides the drawing researcher with a tool to answer this important question. It also reveals particular problems encountered by the beginning drawer in a compelling

way. The scissor drawings, for instance, make it clear that representation of the crossing of the scissor blades is not just <u>difficult</u> for most children: rather, they respond to it with systematic error.

Perhaps the most difficult question to answer about early drawings is whether the drawing is in any real sense a <u>representation of the object drawn</u>. Our technique allows us only to say whether drawings vary with the object drawn, which is not the same thing. Some drawings collected, such as those shown in Figure 9, are undoubtedly <u>caused by the object drawn</u>, but again, causation is not a sufficient condition for representation. There is no functional link between drawing and object in these tasks (as there is between a map and the space mapped, for instance). Perhaps tasks can be contrived in which such a functional link is established, so that the child's ability to regard the drawing as standing for the object can be assessed. For the moment, however, all that can be said on this difficult question is that <u>if these earliest drawings have real representational content, then that would be a finding consistent with other work</u>. De Loache (1987) suggested that children around 2;6 were able to grasp the representational link between a model room and the real room modelled. However, later research by the Sheffield group (e.g. Blades and Spencer, in press) and by my own students (Rutland, Custance and Campbell, submitted) has cast doubt on De Loache's result. The crucial insight is due to Blades and Spencer, namely that in De Loache's task objects are hidden under <u>unique</u> objects (there is just one couch, one chair, etc.). If the hiding place is not unique (more than one chair, etc.) then children do not begin to succeed in these tasks either with models (Blades and Spencer) or maps (Rutland *et al*) until late in their third year. So it seems as if children begin to use spatial representations, such as maps and models, at approximately the same stage of development that they begin to make drawings which vary in a stable fashion with the object drawn. It is therefore plausible that the key development in all three cases might be the ability to

think about (that is, mentally represent) the relative positions of parts of an object extended in space.

6. Appendix: Calculation of probabilities

Let S(N,G,K) be the number of distinct ways of sorting N objects into G groups of size K. Suppose the N objects belong to G types, with K objects of each type. The initial problem is to calculate the number of distinct ways of sorting the N objects into G groups of size K, such that at least r (where r ≤ K) objects belonging to some specified type are grouped together. Let this number be S(N,G,K; ≥r) and let S(N,G,K; r) be the number of distinct ways of sorting the N objects into G groups of size K, such that <u>exactly</u> r (r ≤ K) objects belonging to some specified type are grouped together. Let C(X Y) represent the number of distinct ways of selecting Y objects from a set of X objects. Of course, by a well-known counting rule:-

1) $$C(X\ Y) = \frac{X!}{(X-Y)!\ Y!}$$

N objects may be arranged in sequence in N! ways, and placed in sorting bins, K at a time, working along the sequence. There are therefore N! ways of sorting N objects into G groups of size K. However, not all of these sorts are distinct: the objects in each bin may be rearranged in K! ways, and the bins themselves may be rearranged in G! ways, without affecting the identity of a sort. It follows that:-

2) $$S(N,G,K) = \frac{N!}{(K!)^G\ G!}$$

S(N,G,K; ≥r) will be calculated cumulatively, since direct calculation involves double-counting problems. Thus:-

3) $$S(N,G,K;\ \ge r) = \sum_{i=r\ to\ K} S(N,G,K;\ i)$$

To count $S(N,G,K; r)$, notice that:-

(i)   there are $C(K\ r)$ ways of selecting r objects of the specified type,

(ii) there are $C((N-K)\ (K-r))$ ways of selecting objects of other types so as to complete the group of size K,

(iii) the remaining N-K objects must be sorted into G-1 groups of size K, and this can be done in $S(N-K,G-1,K)$ distinct ways.

(iv) provided $r \geq K/2$, no other group will contain as many as r objects of the specified type. The results below are valid only for $r \geq K/2$.

It follows from (i) to (iv) that:-

4) $S(N,G,K; r) = C(K\ r)\ C((N-K)\ (K-r))\ S(N-K,G-1,K)$

Let $P(N,G,K; \geq r)$ be the probability that in a random sort of N objects into G groups of size K at least r objects of some specified type are in the same sorted group. Then:-

5) $$P(N,G,K; \geq r) = \frac{S(N,G,K; \geq r)}{S(N,G,K)}$$

$$= \frac{\sum_{i=r\ to\ K} C(K\ i)\ C((N-K)\ (K-i))\ S(N-K,G-1,K)}{S(N,G,K)}$$

$$= \sum_{i=r\ to\ K} C(K\ i)\ C((N-K)\ (K-i))\ P(N,G,K; K)$$

The required probabilities may now be calculated as follows:-

$$P(30,6,5; 5) = \frac{S(30-5,6-1,5)}{S(30,6,5)} = \frac{25!\ (5!)^6\ 6!}{(5!)^4\ 5!\ 30!} = .0000421$$

$$P(30,6,5; \geq 4) = (C(5\ 4)\ C(25\ 1) +1)\ P(30,6,5; 5) = 126 \times .0000421 = .005305$$

In the first cycle of judging, the criterion adopted was that, for a given object and child, each judge had to have included at least 4 of the 5 objects in a sorted group. The probability of this criterion being satisfied when sorting is

random (i.e. when the judges have no basis for sorting) is clearly the square of $P(30,6,5; \geq 4)$, namely .000028

In the second cycle of judging the sets to be sorted ranged in size from 20 to 55. Since once again two judges were used, the squares of the associated probabilities are the relevant values, as follows:-

$P^2(20,4,5; \geq 3)$ = .0844   (Bulbs and Rackets)

$P^2(30,6,5; \geq 3)$ = .0173   (Pots)

$P^2(45,9,5, \geq 3)$ = .0035   (Scissors)

$P^2(50,10,5, \geq 3)$ = .0023   (Gloves)

$P^2(55,11,5, \geq 3)$ = .0016   (Combs)
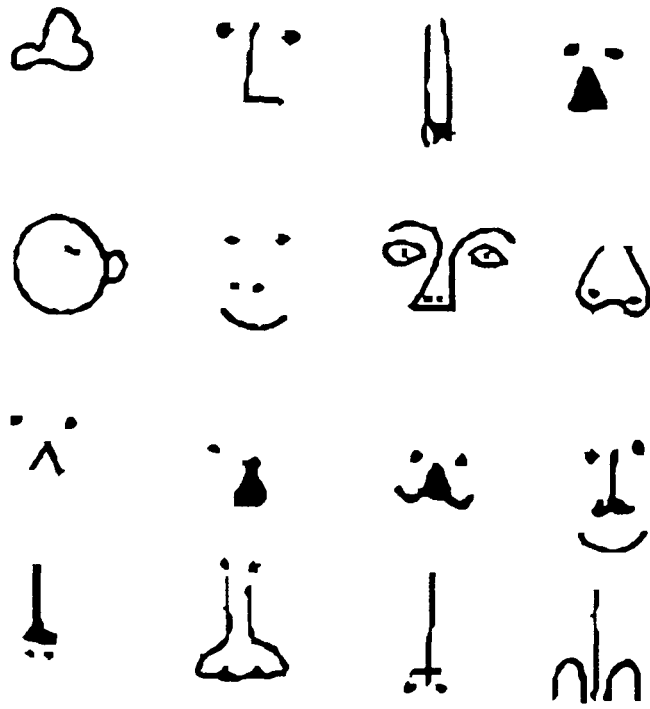
Figure 1: Noses (above) and Trees drawn by a single class, age 5;6 to 7;0, from an Australian primary school (Van Sommers, 1984, pp. 162-3).
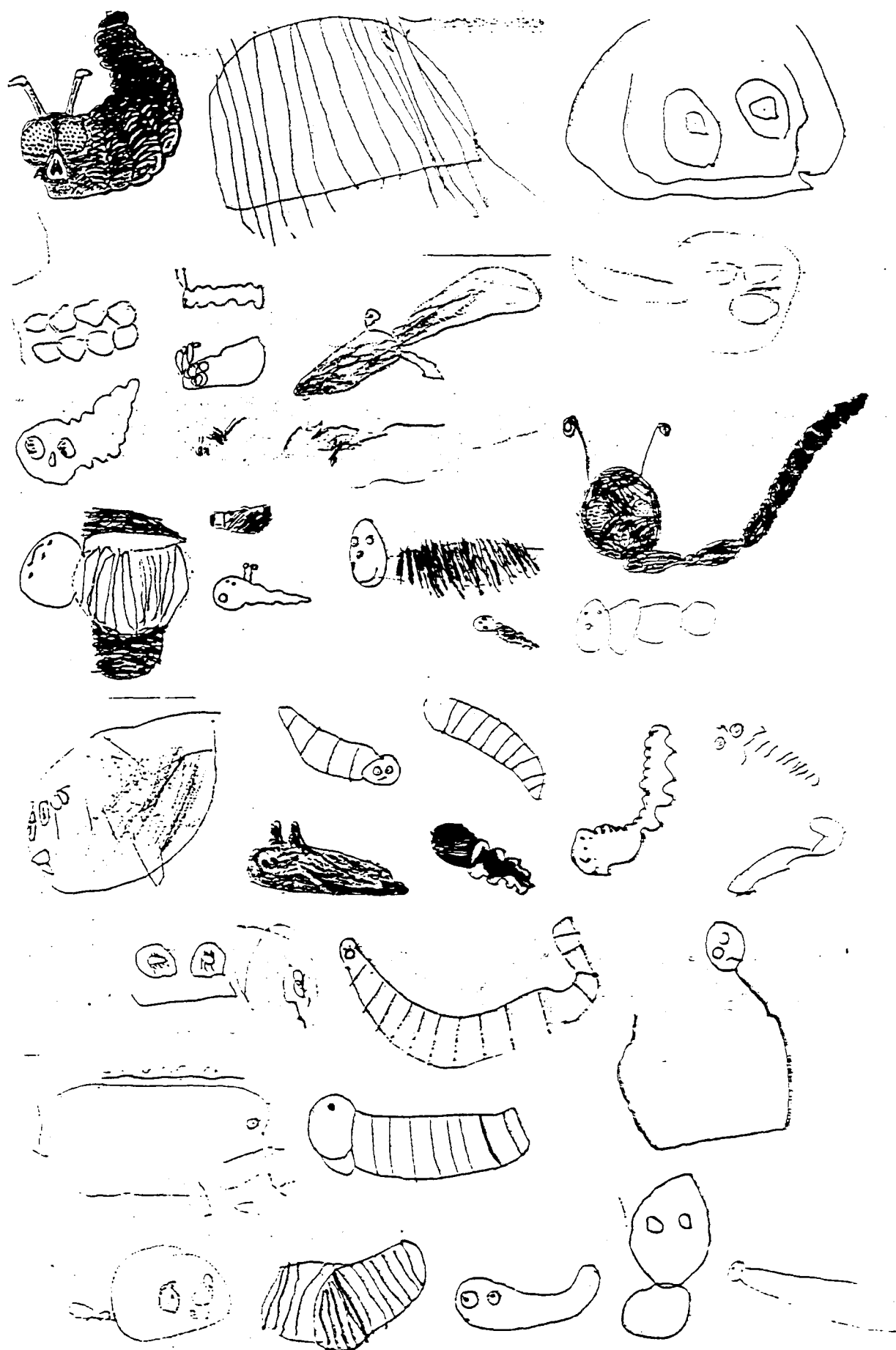
Figure 2: Copies of the 'caterbee' shown in the top left corner, produced by a single P1 class, age approx.5 to 6, from a Scottish Primary school.
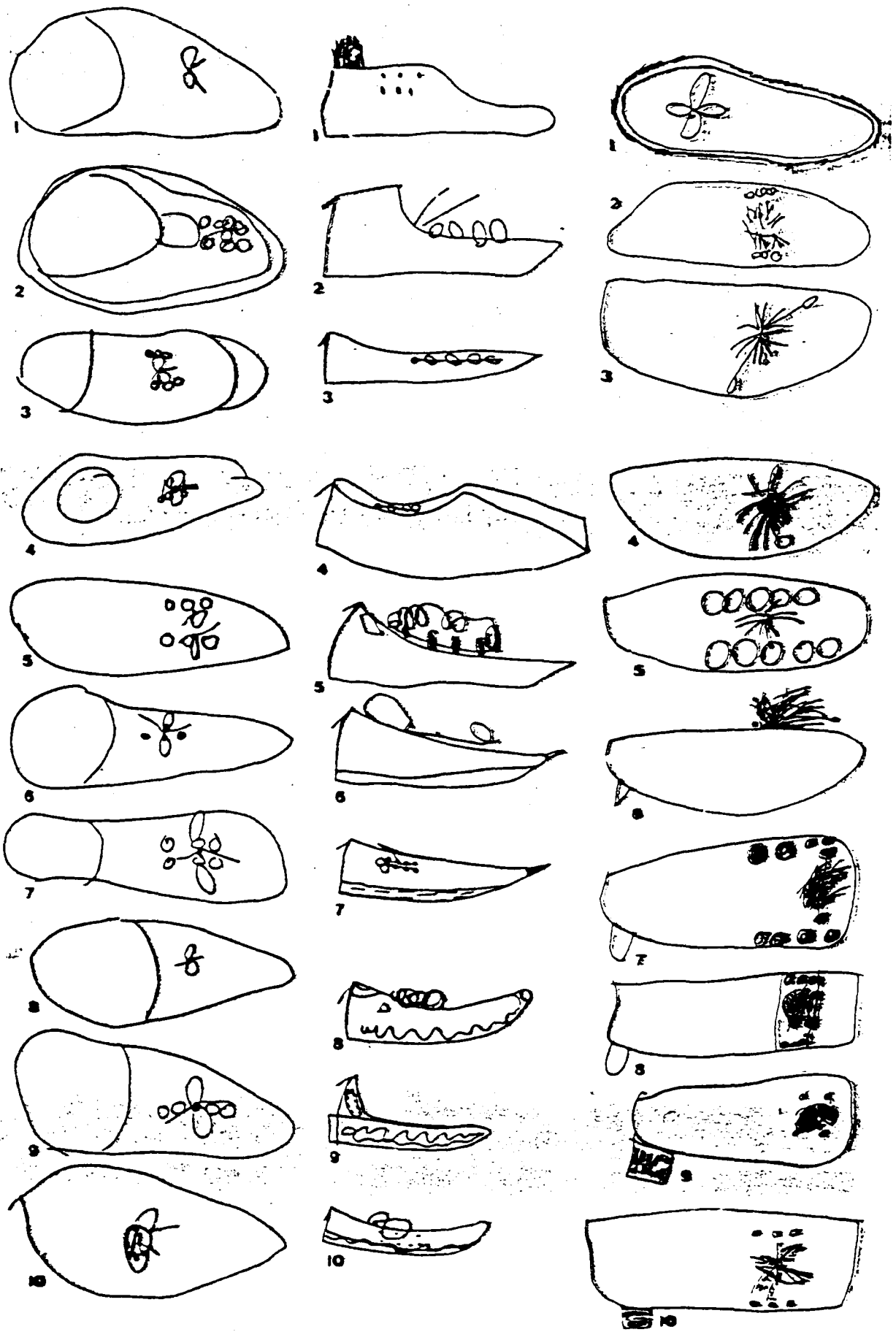
Figure 3: Ten successive drawings from life of a canvas tennis shoe by three children, from Van Sommers, 1984, p.206

A. Yawalyu. After pencil drawing (1); after body paintings (2, 3).

1. Wallaby   2. Opossum   3. Fighting stick
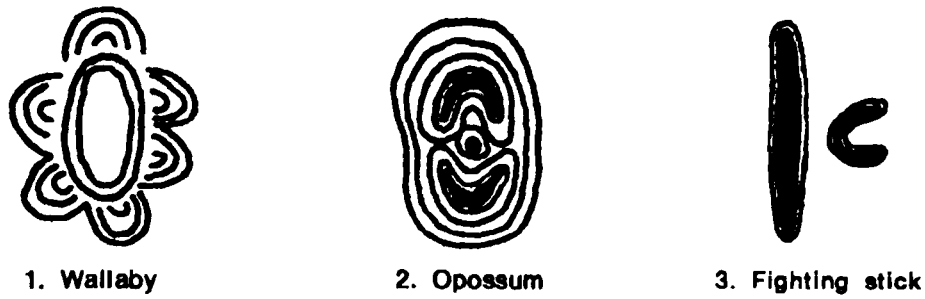
1. Thigh design. O = rock hole. UU = Wallabies drinking.
2. Thigh design. UU = two opossums sitting. According to the associated story, the elements specify a male and female opossum. In the story, the dreamer—a man—and his wife are identified with the opossums. O = meat. The two opossums sit eating meat. This is probably also an allusion to sexual intercourse for which "eating" is a standard metaphor. No explanation was given for the surrounding ovals.
3. U = woman sitting at a *yawalyu* ceremony. /= fighting stick lying down behind her.

Figure 4: *Yawalyu* (Walbiri women's decorative thigh paintings), from Munn, 1973, p. 83.

Figure 1 (continued)

Element	Range of meanings	Category description
8.	Grove of trees	Place marker used for trees only, usually for depicting shade in which a kangaroo is lying
9.	Hut	Enclosure for living
10.	Actor sitting / Actor sitting	Actor in static position, i.e., in contrast to 2, 3, 4, but not prone (in contrast with 1)
11.	Creek bed / Blanket or "bed"	A striated or somewhat crumpled space on which actor may rest
12.	Food or water scoop / Baby carrier / Shield / Spear thrower / Oval "bed" (*ngura*), hollow in ground for sleeping	Oval, hollow containers, and related forms

Figure 5: 'Sand Story' graphic elements on which designs like those in Figure 4 are based.
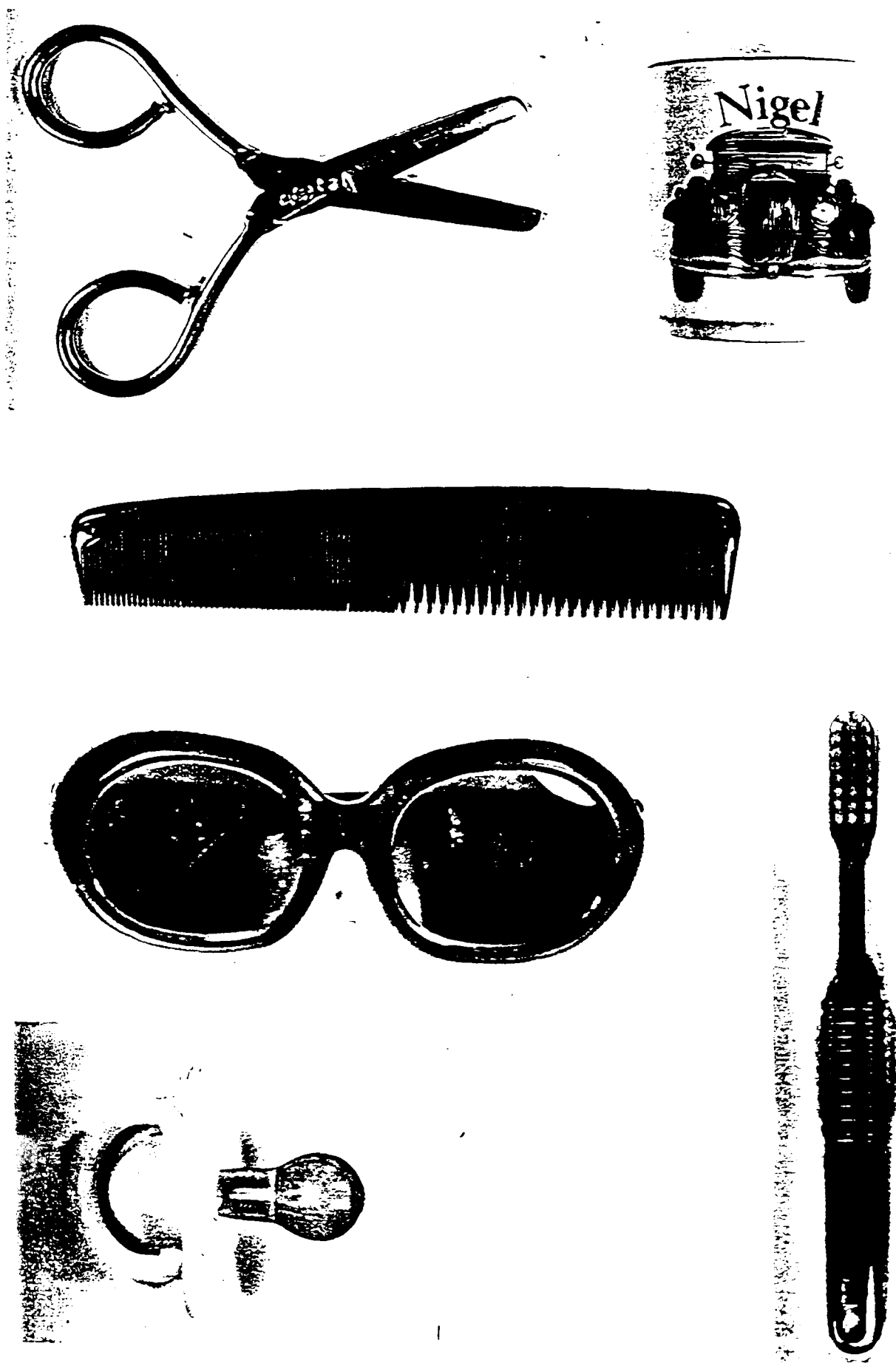
Figure 6: Objects used for repeated life drawing by pre-school children, aged 3 to 5 years.

Scissors
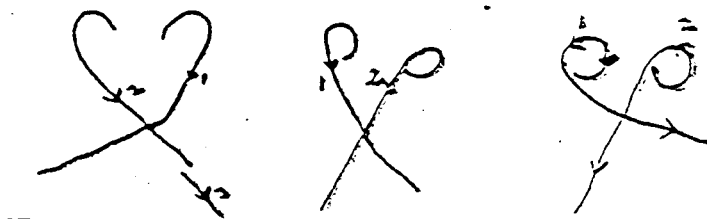
Comb
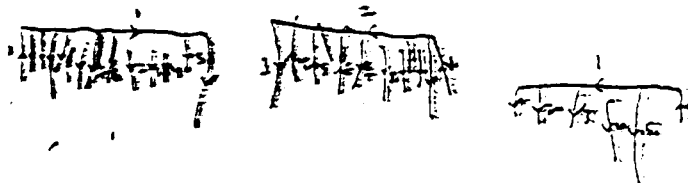
Eggcup
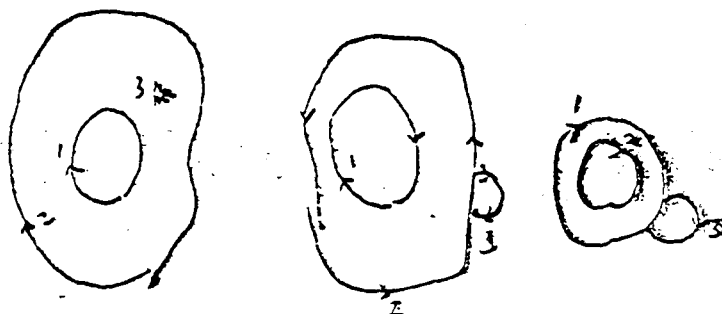
Spectacles
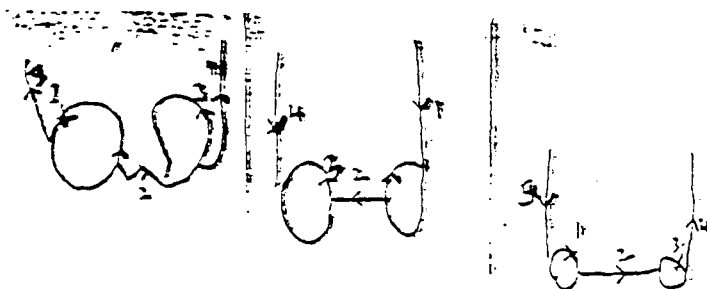
Toothbrush
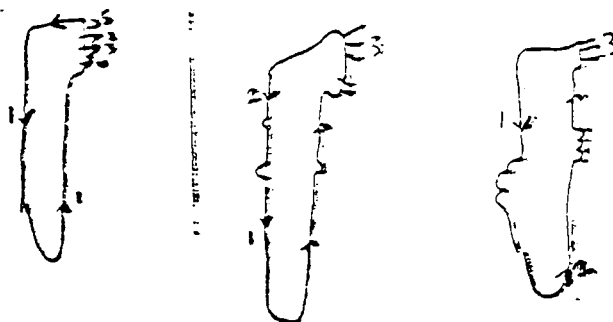
Comforter
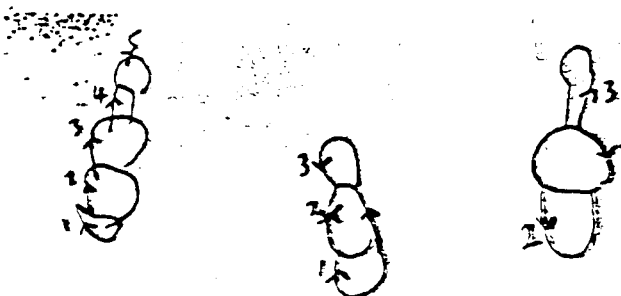
Figure 7: Hazel (4;6) makes repeated drawings which show a considerable degree of conservatism.

Alastair
3;5
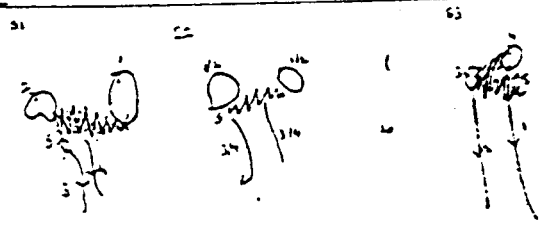
Calum
4;4

Lindsey
4;5

Luke
3;8

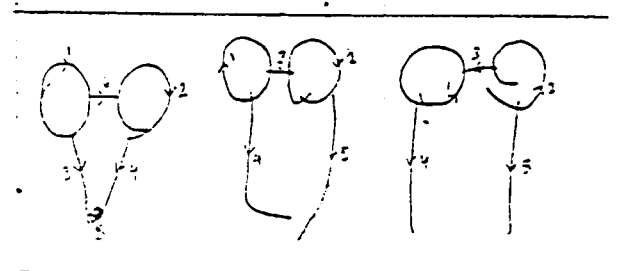Blair
3;8

Sima
4;7

Rosa
4;7

Paola
4;0

Stefano
3;4

Heidi
4;5

Hazel
4;6

Ewen
3;3

Angus
3;9
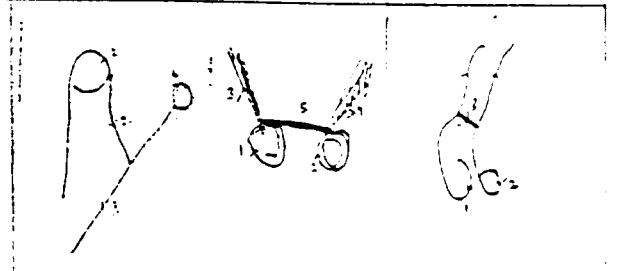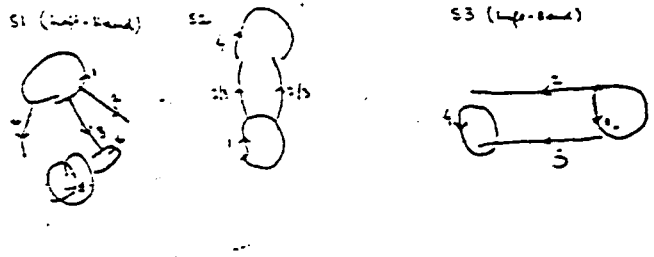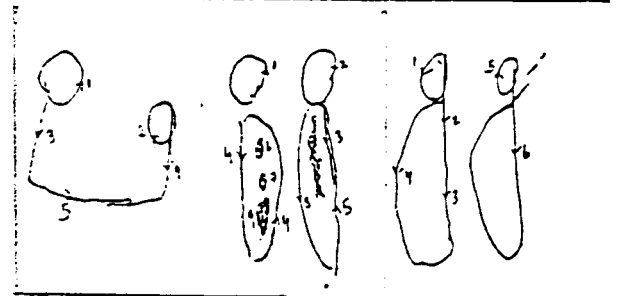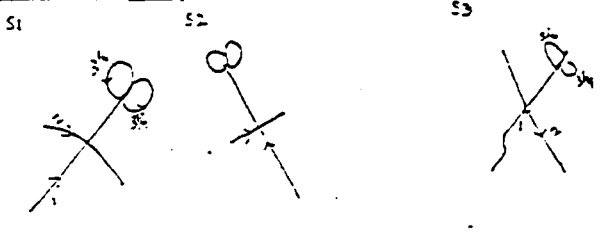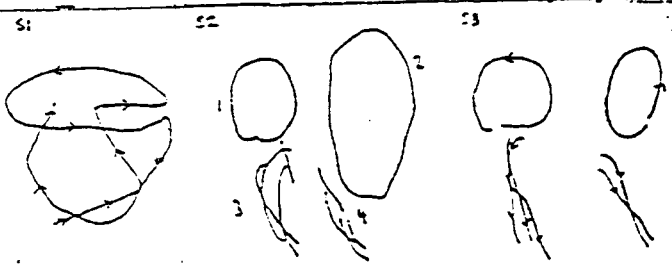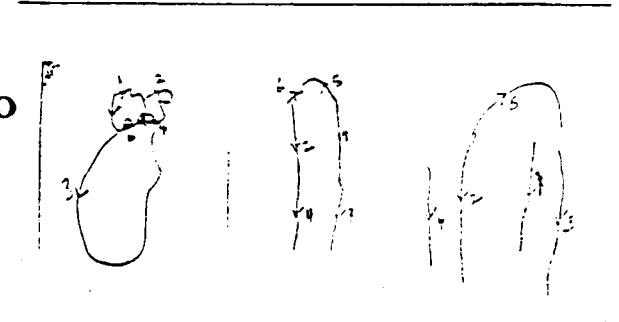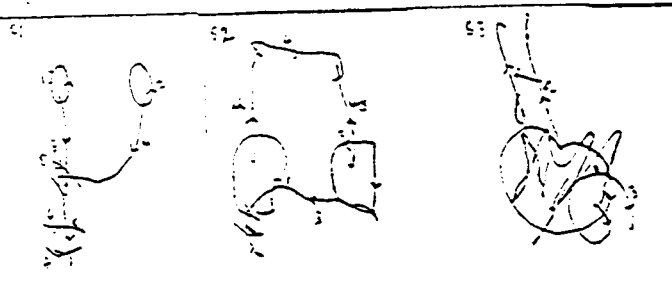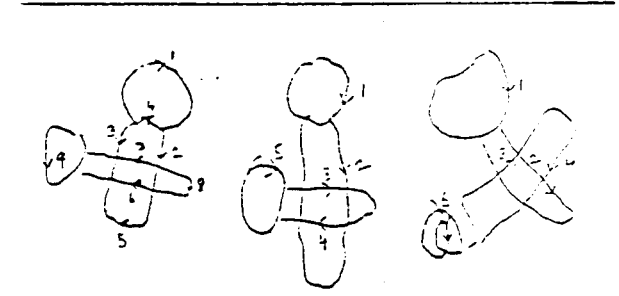
DRAWINGS OF SCISSORS MADE ON THREE SUCCESSIVE DAYS
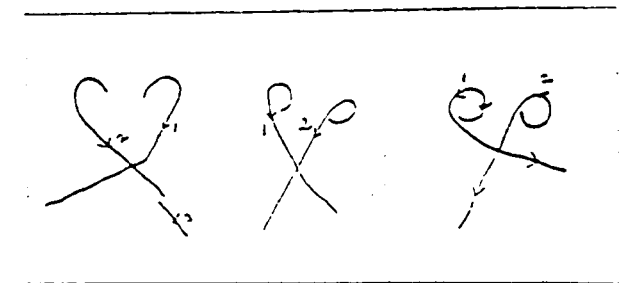
Idiosyncrasy arises because of:-

(1) Variation in solutions to the problem of representing the joining of the scissor blades

(2) Variation in representation of minor object features
e.g. the screw (Rosa, Stefano)
the cutting movement? (Alastair)
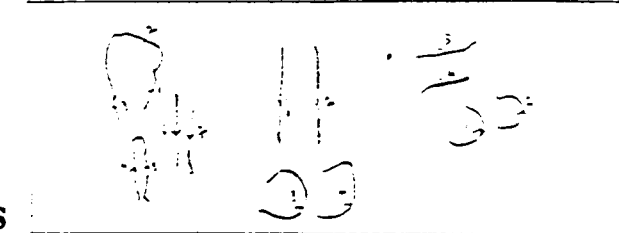
Figure 8

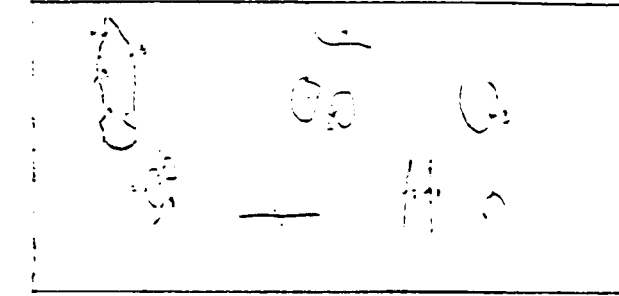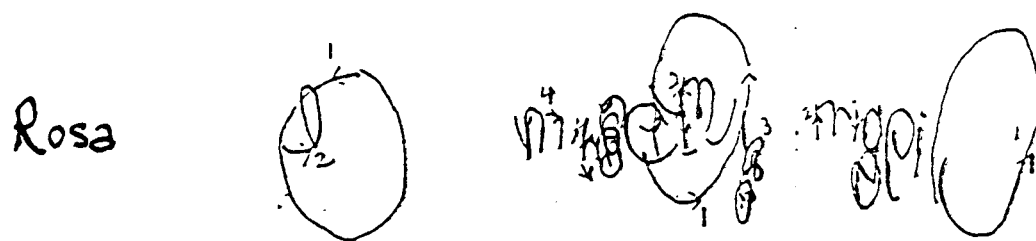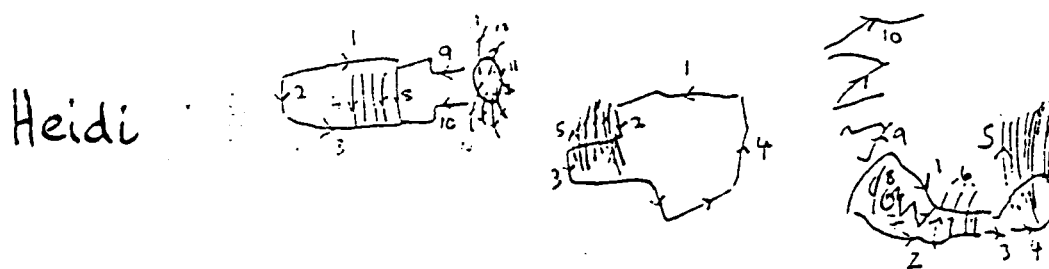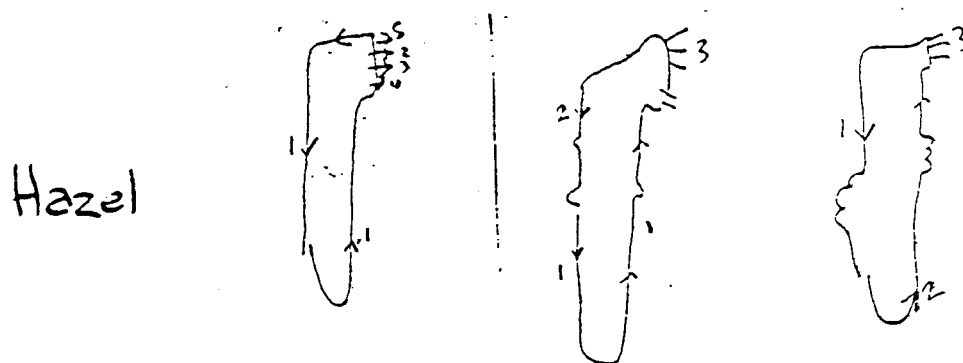## EGGCUP DRAWINGS BY ROSA (4;6) & HEIDI (4;5)

TOOTHBRUSH DRAWINGS BY HAZEL (4;7) & HEIDI (4;5)

Note the attempts to draw "Nigel", the car, the ribs of the toothbrush and the legend "WISDOM".

Figure 9: Repeated drawings showing attempts to reproduced written material on the objects.

JOHN - SCISSORS

JOHN - TENNIS RACKET

JOHN - COMB

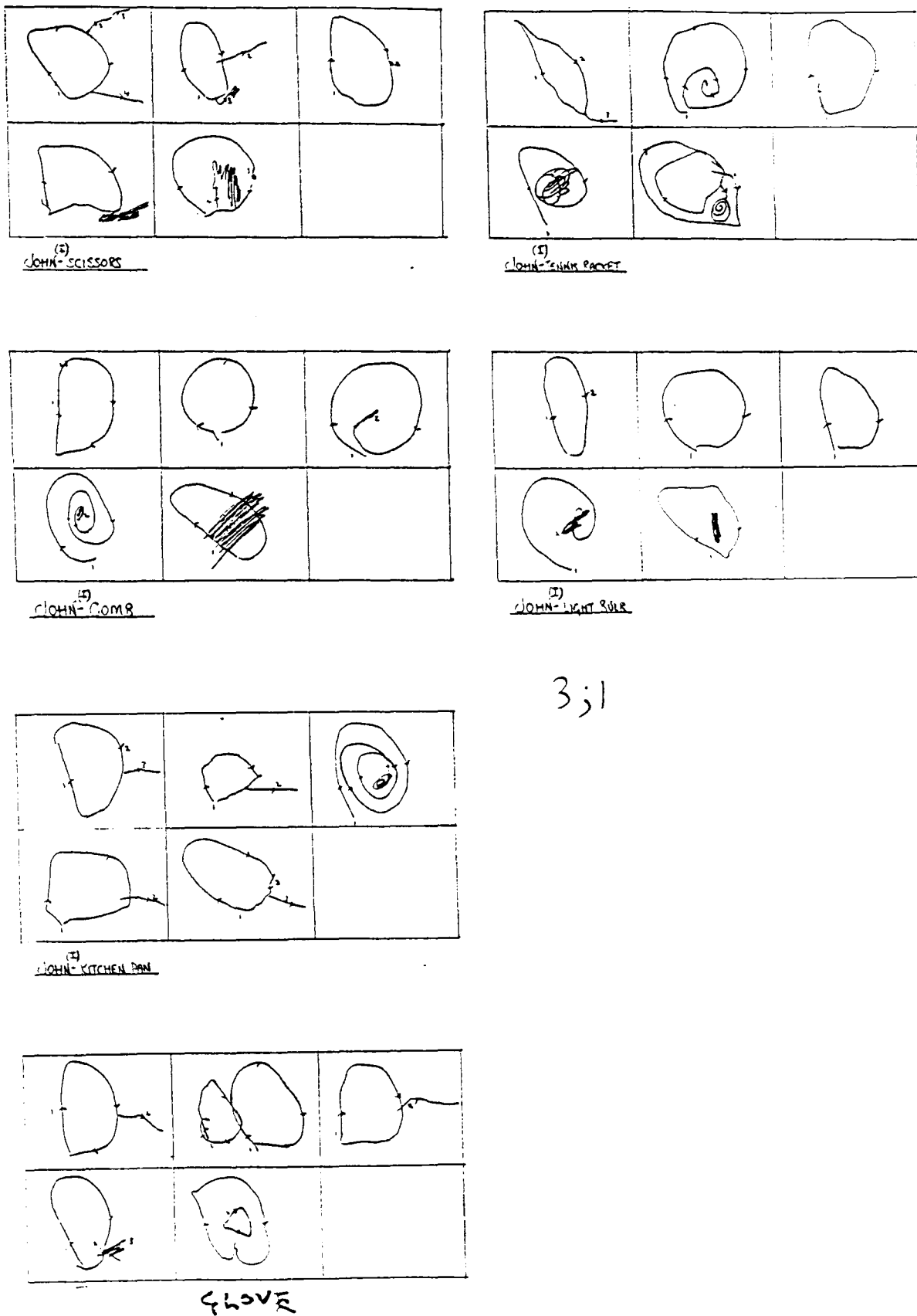JOHN - LIGHT BULB

3;1

JOHN - KITCHEN PAN

GLOVE

Figure 10: Repeated drawings by John (3;1). These drawing sets were all eliminated in the first stage of judging.
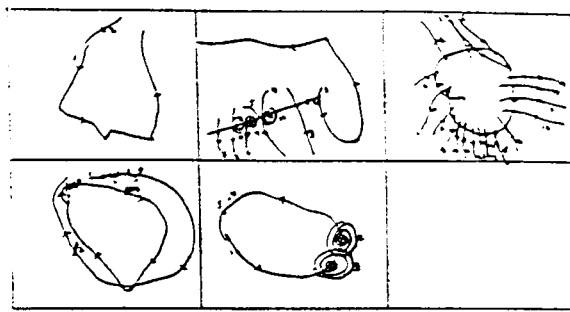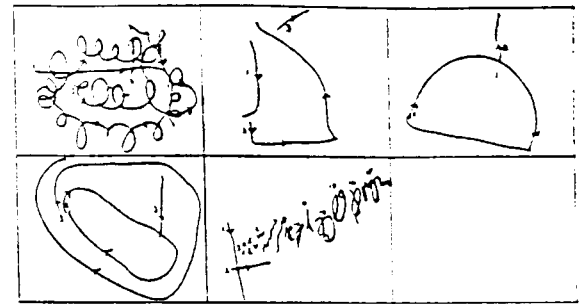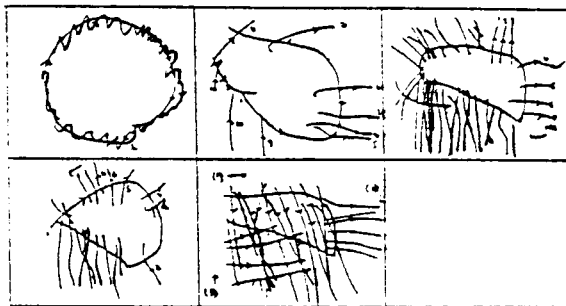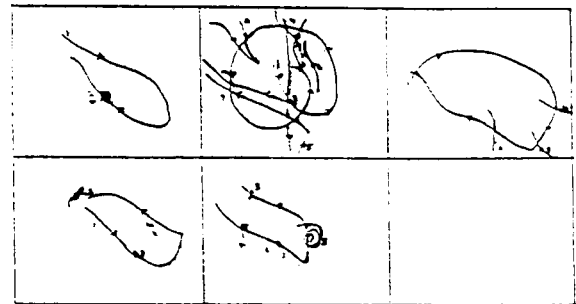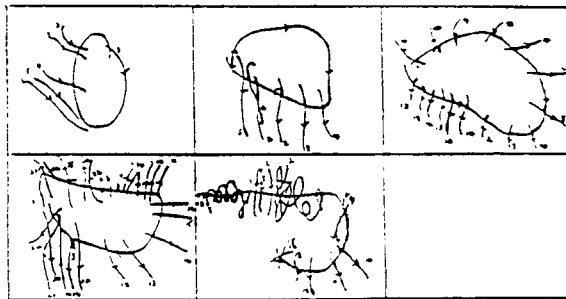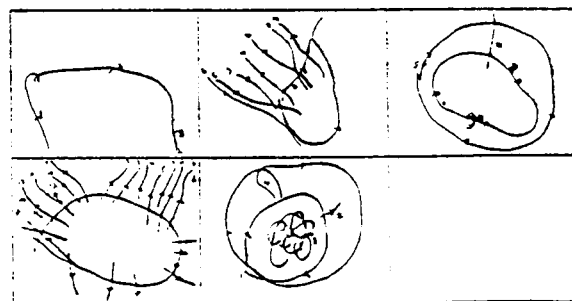
Figure 11: Repeated drawings by Louise (3;4). These drawing sets were all eliminated in the first stage of judging.
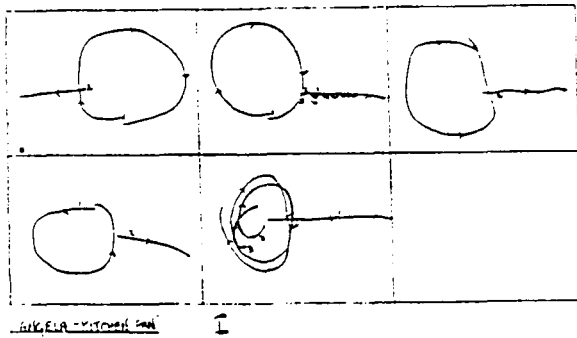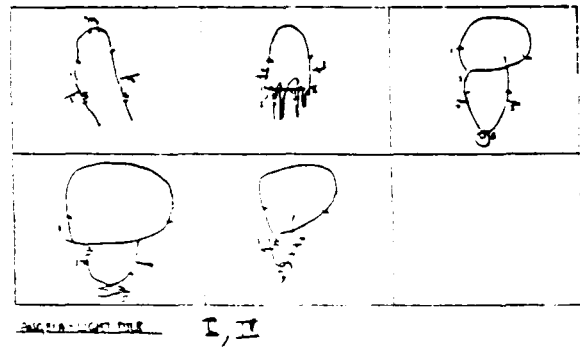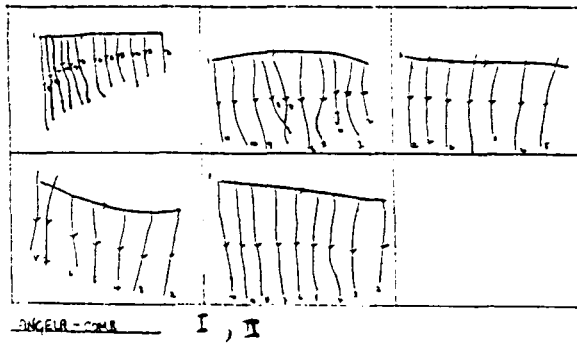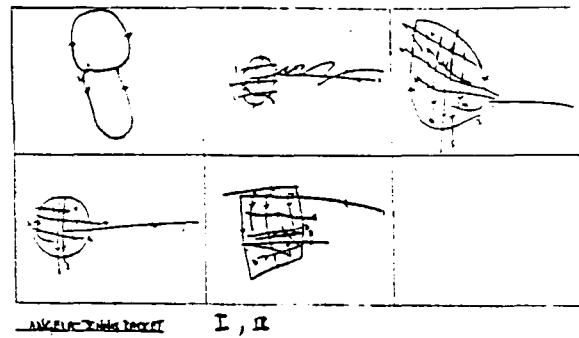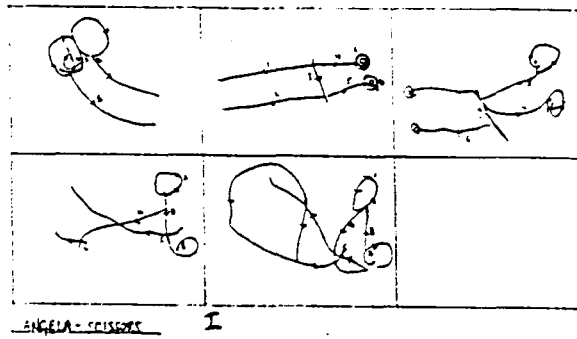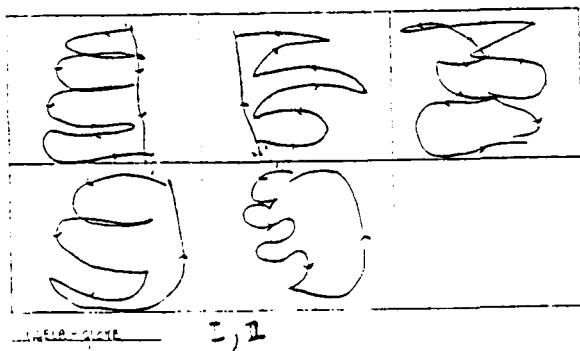
Figure 12: Repeated drawings by Angela (4;7). These drawing sets all survived the first stage of judging.
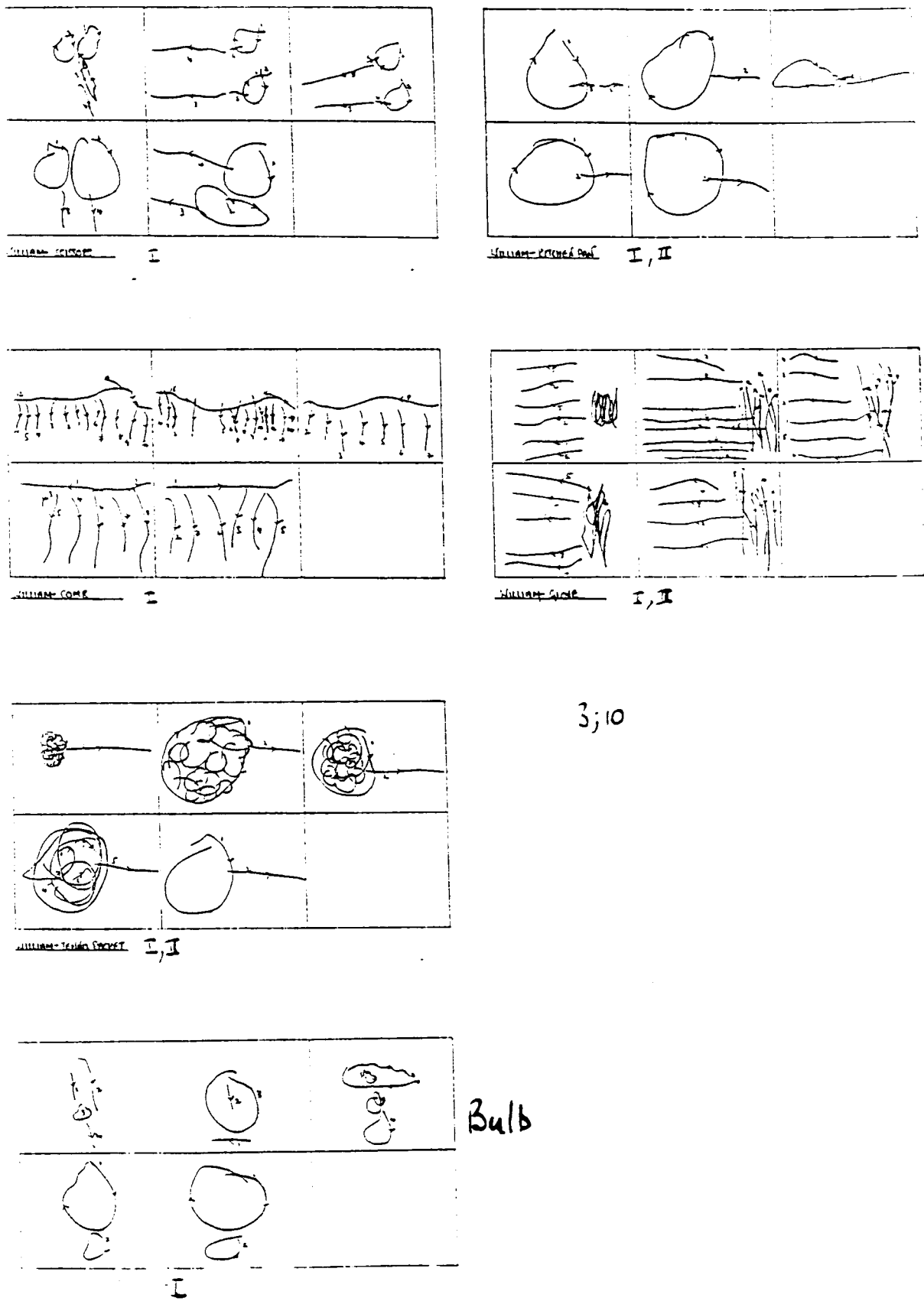
Figure 13: Repeated drawings by William (3;10). These drawing sets all survived the first stage of judging.
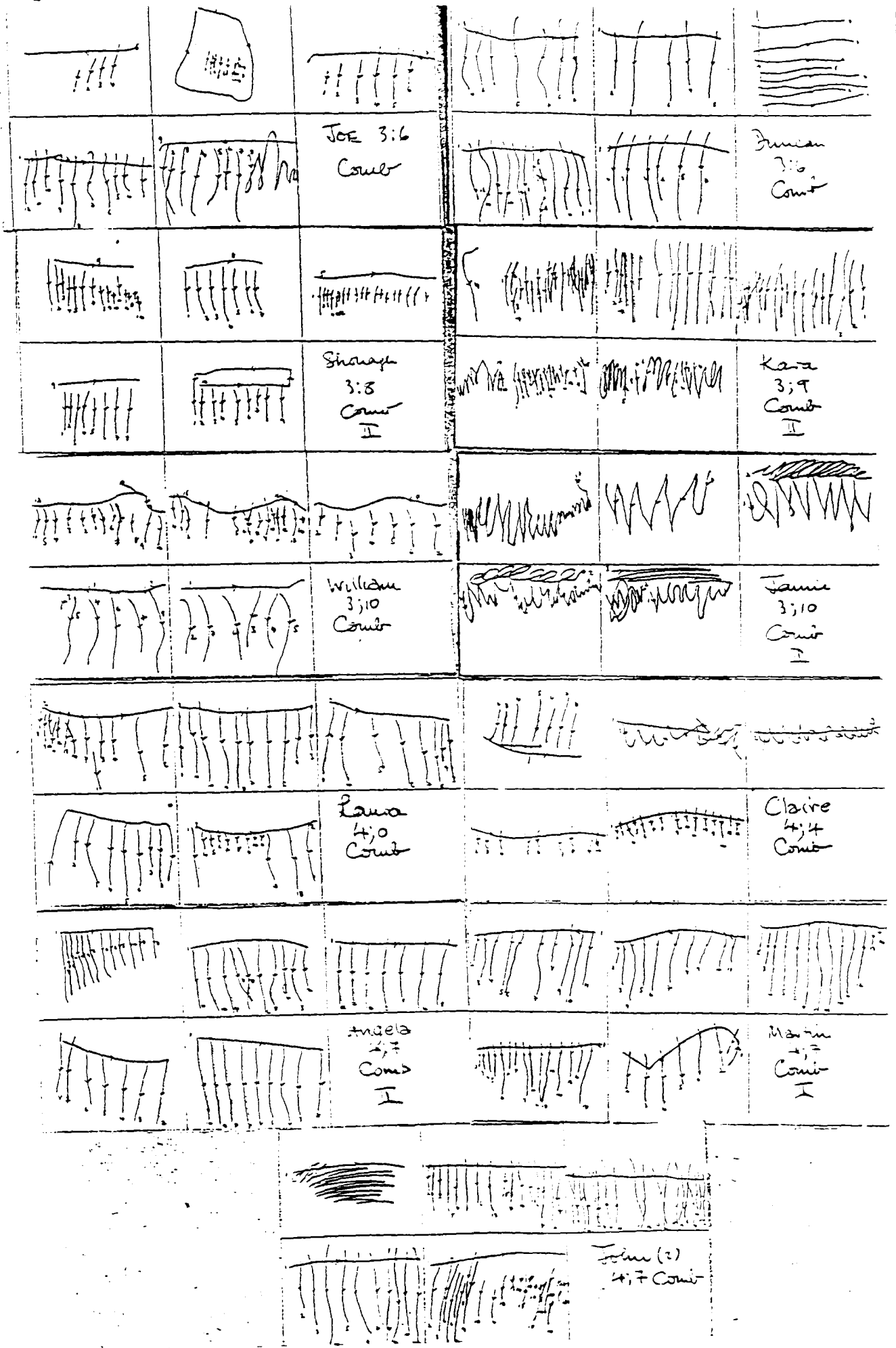
Figure 14: Repeated- drawing-sets of *Combs* which survived Stage I of judging. Those which also survived Stage II are marked 'II'.

Figure 15: Repeated- drawing-sets of *Gloves* which survived Stage I of judging. Those which also survived Stage II are marked 'II'.
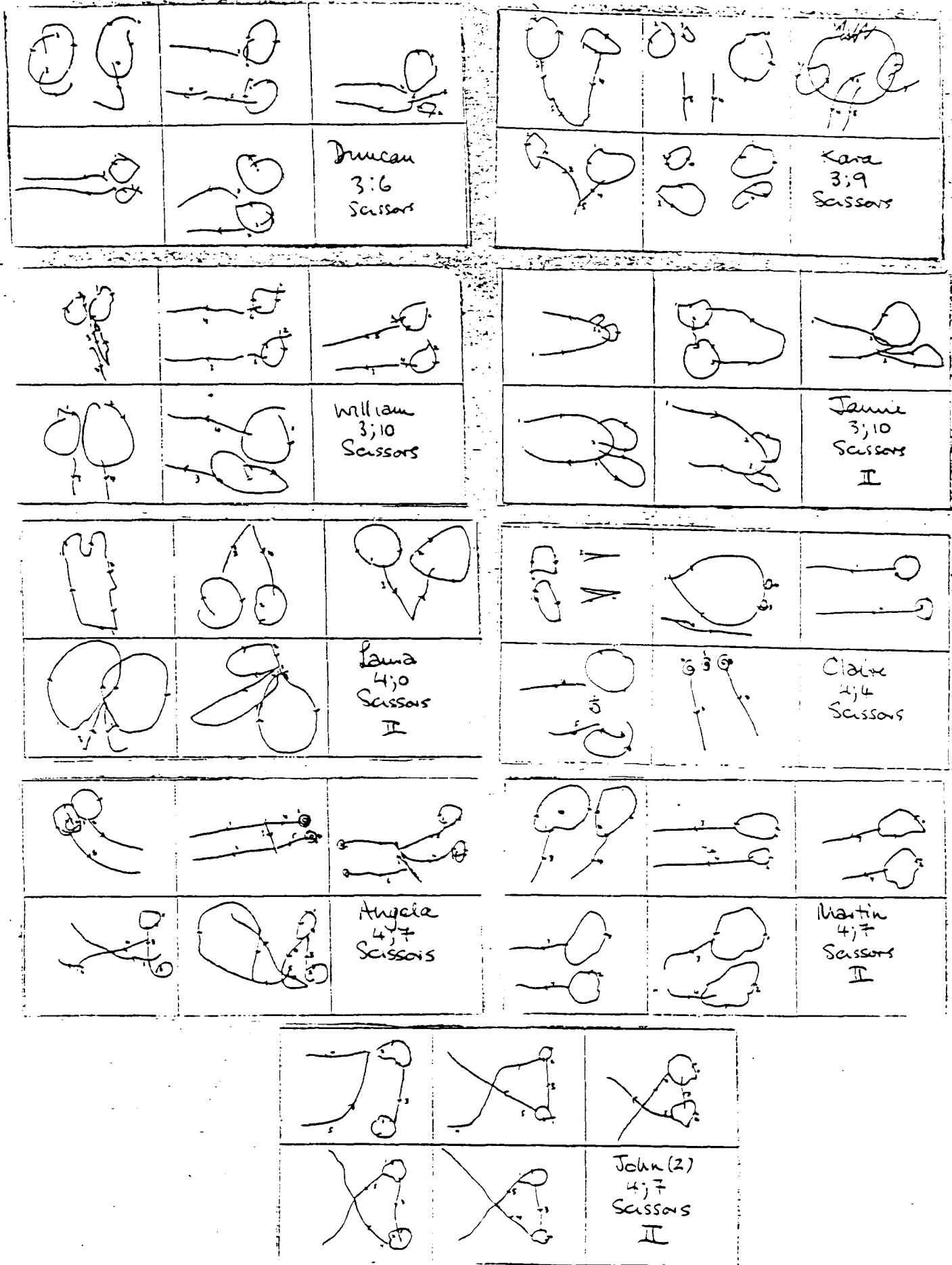
Figure 16: Repeated- drawing-sets of *Scissors* which survived Stage I of judging. Those which also survived Stage II are marked 'II'.
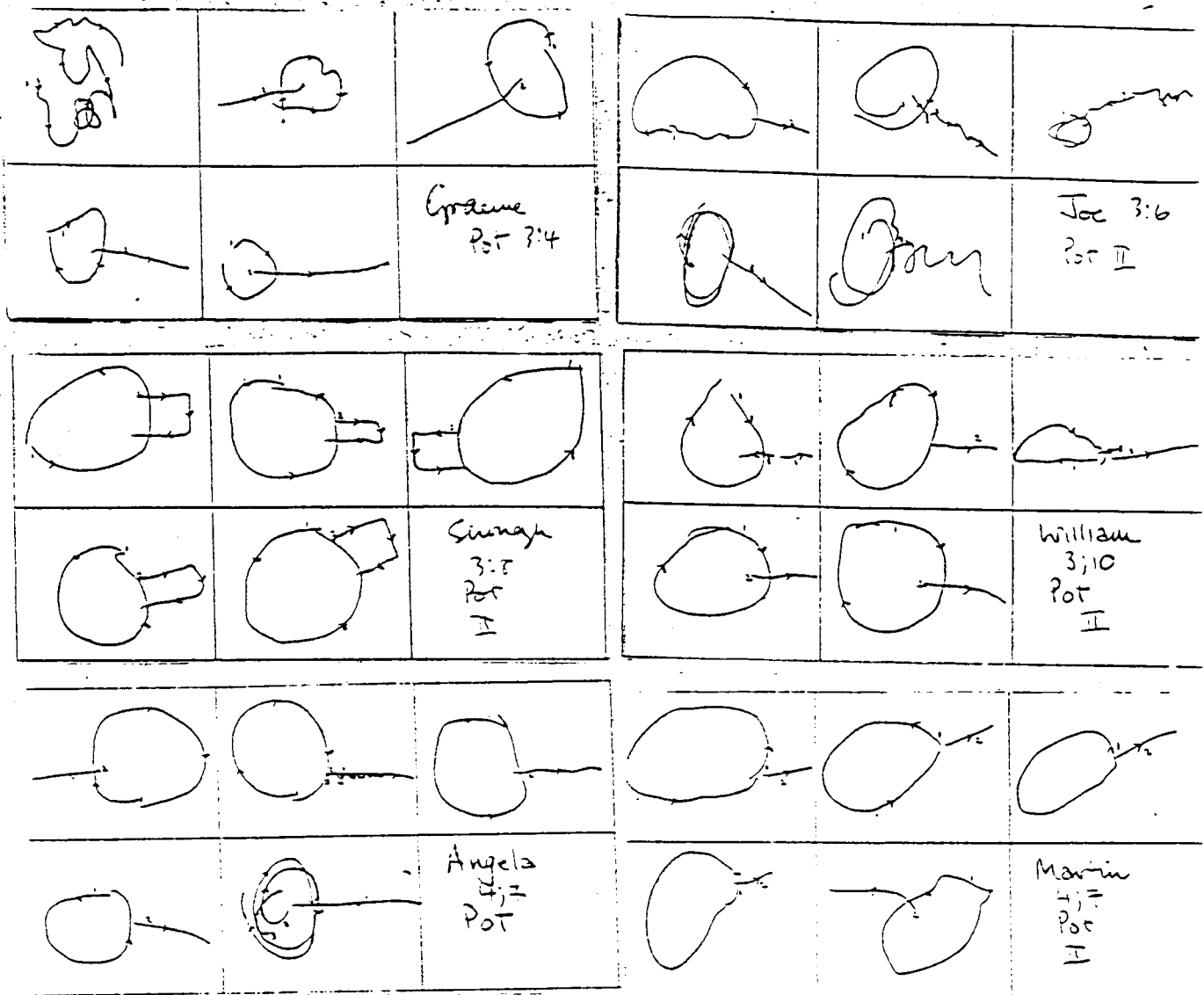
Figure 17: Repeated- drawing-sets of *Pots* which survived Stage I of judging. Those which also survived Stage II are marked 'II'.
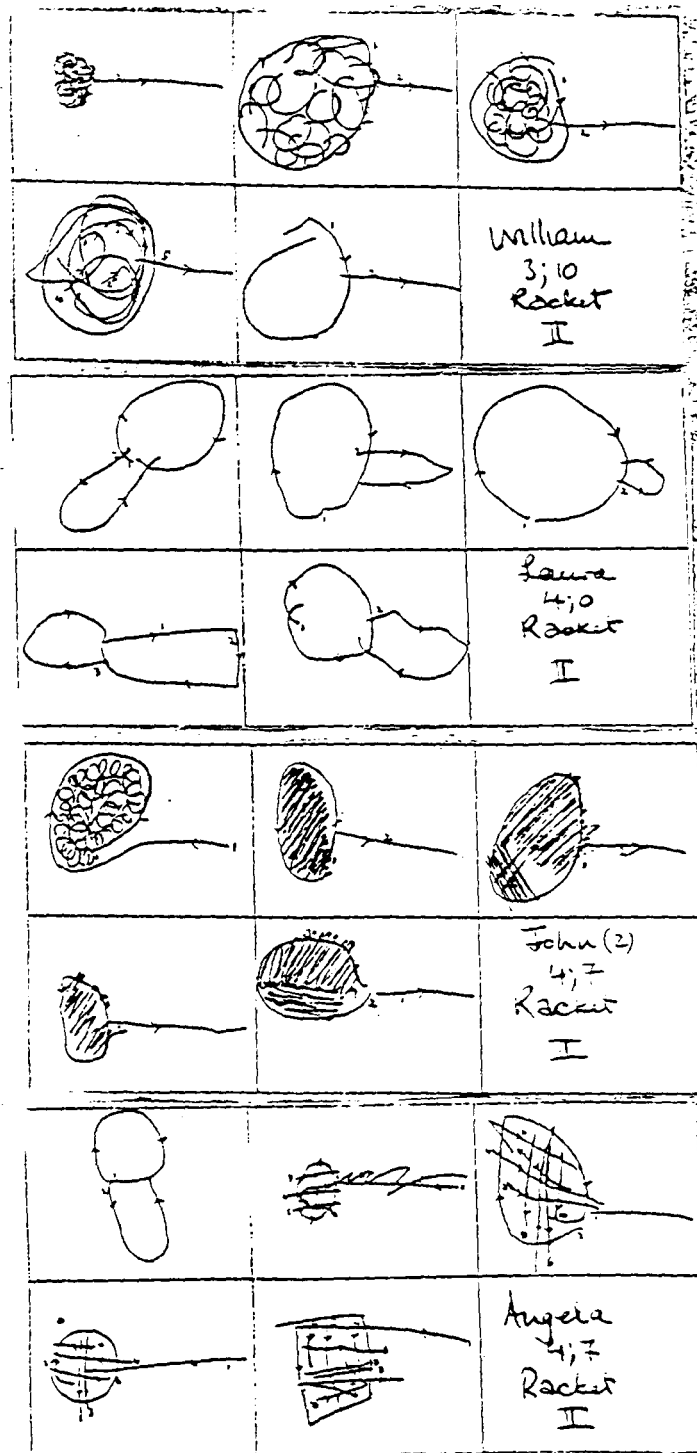
Figure 18: Repeated- drawing-sets of *Rackets* which survived Stage I of judging. Those which also survived Stage II are marked 'II'.
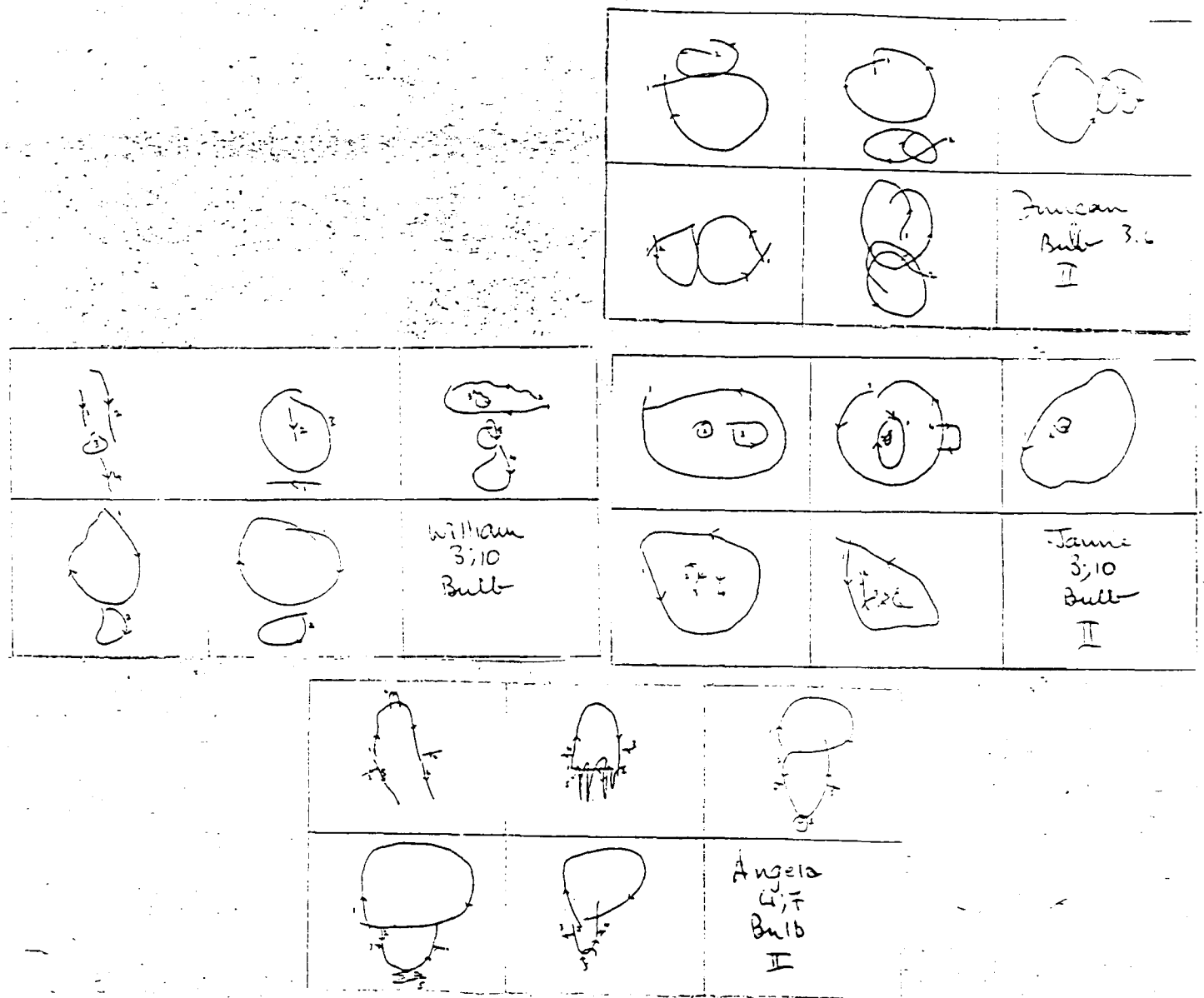
Figure 19: Repeated- drawing-sets of *Light-bulbs* which survived Stage I of judging. Those which also survived Stage II are marked 'II'.

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Chapter 8: Children's Thinking[1]

1. Piaget's Approach to Children's Thinking

The study of children's thinking has been dominated for a very long time now by a concern to explain, or to explain away, the phenomena first explored by Jean Piaget (1896-1980). These phenomena - the slow and faltering approach which children make towards a grasp of the central notions of quantification, measurement and logic - have a typical and special character. In almost every case, the endpoint of development is commitment to a proposition, or set of propositions, which is neither clearly dependent on experience (i.e. not an inductive generalization) nor attainable by deduction from other propositions which are dependent on experience. Principles such as the Piagetian conservations are <u>abductions</u>: like the conservation principles of classical physics and chemistry (see Meyerson, 1930 - an undoubted source of Piaget's interest and approach - for decisive historical analysis) they make the best possible sense of the phenomena in their domain; they are interpretations or construals of the world. This formal property of the phenomena studied was perhaps not Piaget's reason for studying them: his interest in the origins of scientific notions may have been the determining motive. However, it is a property that ensures that what the child is <u>not</u> doing in these tasks is detecting some manifest property of the material presented. Rather, the child's achievement is to invest the presented material with a property of his or her own devising. We shall argue that selection of tasks that can be characterized in this fashion is an essential methodological step in the study of thinking (of child or adult), if thinking is to be effectively distinguished from perception or attention.

Why has there been this long effort after explanation of these phenomena studied by Piaget? What is wrong with Piaget's own explanations? To begin

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[1] Published as Campbell and Olson, 1990.

with, his explanations look like descriptions, albeit perspicuous ones, with only unsatisfactory gestures in the direction of a causal account. The pattern of Piagetian analysis is a fairly constant one: in any specific domain of children's thought he sought first of all to establish a stage-like progression towards adult understanding - an essentially descriptive enterprise, and then, by way of explanation, he sought to relate the observed progression of stages to his general account of stages of cognitive development. Thirdly, to preserve this explanatory move from circularity, he would suppose that the transitions between stages were to be accounted for by domain-independent processes of functional reorganisation, involving coordination of established structures and somewhat mysterious regulatory processes known as equilibration and reflective abstraction. Explanation by assimilation to stages is often thought of as unsatisfactory, for much the same reasons as explanation in terms of powers (e.g. the dormitive power of opium explains its sleep-inducing property) is thought unsatisfactory.[2] Equilibration, too, insofar as the process is described in a comprehensible way, has been criticized as logically unfounded (Fodor, 1975) and indistinguishable from nativism (Haroutounian, 1983).

The pattern of Piagetian explanation is thus often seen as intrinsically weak. In the second place it fails to fit comfortably into the dominant modes of explanation in Anglo-American psychology. There have been two such dominant modes during Piaget's working life. Until around 1960, the infant

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[2] This objection is hardly cogent. If a general sequence of stages can be characterized in such a way that individual developmental patterns from different domains can be related to it, and therefore to one another, then such a general sequence has clear explanatory force. The ordering of stages in the different patterns has thereby been attributed to a common cause. The most successful application of this techniques in Piaget's works is perhaps the sequence of sensori-motor stages, which has been applied to developmental patterns in such diverse domains as imitation, object representation, and tool use, with undeniable success, particularly in the field of comparative primate cognition (see Piaget, 1954 and Chevalier-Skolnikoff, 1983). Of course, although a common cause is identified by this means, there remains obvious scope for further illumination! The situation may be compared with genetic explanation: patterns of inheritance of characters in diverse animal and plant species were seen to follow a common, Mendelian pattern long before the explanation of that pattern in terms of recombination of genes was formulated.

was generally seen as a *tabula rasa* and development was characterized as (a) the acquisition of new response functions and (b) the gradual elimination of error. The sole mechanism of development was the learning of new stimulus and response connections by variants of the conditioning processes isolated in experimental animals. When viewed against this reductive and empiricist backcloth Piaget was seen as a rationalist, since he attributed unwarranted mental structures to children, represented these structures as constructions and represented children as imposing their thought on the world. He was also accounted a nativist, since the emergence of these structures was either not explained at all, or explained by means of an appeal to complicated and mysterious inherited mechanisms. Thus, Wohlwill (1962, p.95) - by no means a firm adherent to the behaviourist hegemony - worried that:

> 'the non-operational, and at times frankly mentalistic, terms used by Piaget. . .may seem to leave his analysis devoid of empirical, and perhaps even of theoretical, significance. . .his whole conceptual apparatus of schemata, operations, centrations, and so forth appears to lack direct empirical reference.'

Since 1960 the behaviourist hegemony has gradually been displaced by a nativist hegemony. The infant is seen as a *tabula omnifera*, development is characterized as the unfolding of inherited flexible structures which are fixed by the phenomena which happen to be present in the child's world. The main mechanism of development is simply endogenous growth triggered or channelled by exposure to relevant phenomena.[3] This spectacular intellectual convulsion will be of strong interest to future historians of science, no doubt. It must have amused Piaget, if it did not drive him to despair. Branded rationalist and nativist through the 1950s and 1960s, when Anglo-American psychology first took serious note of his work, he found the

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[3] We exaggerate slightly: this process of revolution is still very much in progress. While pockets of 'resistance' survive, the main academic citadels have clearly fallen.

same set of ideas - he was not given to hasty changes of mind - condemned in the late 70s by the high priests of the new cognitive science as hopelessly empiricist (see Piattelli-Palmirini, 1980).

Although the two extreme positions which we have just characterized could hardly be more different, there remains a unifying thread of similarity. In both accounts the child is pictured as <u>passive</u> : the course of his development is fixed by external forces. To be sure, under the present dominant view innate structure plays a paramount role in shaping development, but such structure is inherent and its establishment calls for no effort on the child's part. In contrast, Piaget's picture of the child was as an active agent of development, fabricating mental structures from the thin yarn of experience by the slow and difficult application of the instruments of understanding. Facile 'improved' performance, uncoupled from understanding and the 'grasp of consciousness' led nowhere. In this respect, as in many others, Piaget represented the ontogenesis of thinking as a process homologous to the history of thinking (see Kuhn, 1962 for a compelling account of this homology).

Between the demise of the behaviourist picture of development and the establishment of the present innatism, cognitive development in Britain and North America enjoyed a brief period of harmony with Piagetian ideas, under the respective intellectual leadership of Margaret Donaldson at Edinburgh and Jerome Bruner at Harvard. In our view, the case for such harmony remains a strong one.[4] 'Cognitive development' is a bland label for a desperate struggle and institutions such as schools and 20 years of parental care exist neither to install new response functions nor to provide appropriately-timed triggering experiences but to assist and encourage children in that struggle. In this present chapter, we put (part of) the case for

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[4] For a review of research in children's thinking which is strongly sympathetic to Piaget's approach, see Mandler, 1983. This is an excellent and lengthy review, which covers some of the ground reviewed here.

restoration of that harmony.

We will proceed in the following manner. We first discuss the nature of thinking in general and children's thinking in particular, emphasising the rarity of the phenomenon and the variousness of the forms of thought. Some problems of method and proposed solutions to them are discussed and compared with innatist remedies. Finally, we identify some differences between our view of children's thinking and Piaget's.

2. What is Thinking?

In our view, thinking is an effortful activity, involving mental 'work', in which the organism forsakes its normal outward orientation on the presented world and struggles instead with a world indexed only imperfectly by a shadowy inner structure of mental symbols. While we regard this form of thinking as central, we shall refer to it as <u>internally-mediated</u> <u>thinking</u> (conceding the possibility of other forms of thinking), for reasons which will become clear below.

Representations of thinking by artists reflect this view. Rodin's thinker crouches in a withdrawn fetal pose, with closed eyes and lowered head. Conan Doyle's detective Sherlock Holmes, when faced with some knotty problem, retires to consume a pound of shag tobacco, his mind sharpened by the drug and his senses dulled by the resulting fog. Rex Stout's Nero Wolfe leans back, closes his eyes and pushes his blubbery lips in and out in infantile fashion until the imagination delivers. For Stout's detective, as for most of us, thinking is a repugnant and - or so this cameo suggests - regressive activity, to be undertaken only as a last resort. He would rather groom his 10,000 orchids, experiment with new sauces, or read, just as lesser mortals would rather attend some spectacle or entertainment.

Of course there are doubtless individuals for whom thinking is a pleasurable activity. Fortunate universities and publishers have them on their payrolls. Similarly, it may be that from time to time all of us can

undertake this difficult activity with equanimity at worst, if the circumstances are sufficiently propitious. Perhaps these circumstances are most often propitious when we are young! At any rate, once the novelty of internally-mediated thinking has worn off, it seems patent that adults will take almost any pains to avoid it.

A gripping presentation of this pessimistic view of consciousness and thinking is Eugène Marais' extraordinary essay (1969), in which the taking of such steps to avoid thinking is seen as diagnostic of consciousness in the animal world. Marais argued that internally-mediated thinking, or rather the aversion felt for it, led to a variety of depression which he called Hesperian depression, because of its association with the setting of the sun and subsequent attenuation of the presented world. He pointed to certain universal aspects of human culture which in his view constituted avoidance of internally-mediated thinking. There were two basic remedies for Hesperian depression: maintenance of the presented world by illumination of the dwelling or other devices (thus providing the active mind with its customary aliment), and rendering the mind inactive by the use of alcohol or narcotics.

While the first of these remedies makes thinking of this sort unnecessary and the second remedy makes it impossible, it seems to us that many other human devices constitute a third remedy: they make thinking of this sort easier and less repugnant. What we have in mind are of course the innumerable means that we have developed to represent the world externally by means of maps, diagrams, tokens, linguistic and mathematical systems. This is the reading Vygotsky presumably favoured of Francis Bacon's aphorism, which he often quoted (cited in Wertsch, 1985):

'Neither the bare hand nor the understanding left to itself can effect much: it is by means of instruments and helps that the work is done,

which are as much needed for the understanding as for the hand."[5] Of course, Bacon's reading was certainly different: what he had in mind was a method of scientific discovery.

A similarly restricted view of thinking was espoused by Edouard Claparède (1919), who proposed a Law of Awareness, often cited approvingly by Piaget, which took thinking to be a standby activity, invoked only when existing habits failed to deliver an adequate response. There are certain difficulties with this notion of Claparède's. For instance, one undeniable result of laboratory studies of human 'problem-solving' has been that problem-directed thinking is itself governed by habit, like almost every other human activity - witness the many demonstrations of the potent effects of *Einstellung* or <u>Set</u> on problem-solving. However, this difficulty may well have been viewed as a virtue by Piaget, whose accounts of cognitive development, even in the sensori-motor stage, stressed the importance for diagnostic purposes of genuinely novel responses; that is, of thinking not governed by habit (e.g. 1953, Conclusions, Section 3). Claparède's Law may be saved by one of two additional assumptions: either habitual forms of thinking are not guided by awareness or habitual thinking is not really a species of thinking. We prefer the second assumption, on the grounds that the processes governing <u>all</u> highly-practiced and skilled activities differ radically from the consciously-guided processes in which they originate (see Dreyfus and Dreyfus, 1983 for some interesting discussion in the context of the claimed educational value of early programming experience). So a problem initially solved by internally-mediated conscious activity may be solved on future occasions by some stored heuristic, requiring little or no reflection.

If these assumptions are correct, then true thinking is an activity undertaken <u>rarely</u>. Since by hypothesis solutions achieved by true thinking

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[5] from the *Novum Organum Book I*, paragraph 2:'Nec manus nuda, nec intellectus sibi permissus, multum valet; instrumentis et auxiliis res perficitur; quibus opus est, non minus ad intellectum, quam ad manum'.

are rapidly replaced by stored routines, it is then evident that we are

concerned with an ephemeral and evanescent phenomenon and that the

researcher will be faced constantly with the difficulty of distinguishing true

thinking from a routine which has replaced it ontogenetically.[6] Perhaps it is

for these reasons that the study of children's thinking is of particular and

special interest. For there are many sorts of problem which children fail to

solve. By surveying these problems we may be able to build up a picture of the

thinking powers that adults possess as the union of the various powers that

children of different ages lack. This is perhaps one relatively straightforward

sense in which the study of children's thinking constitutes a vital element in

developing an adequate theory of epistemology, a project cherished by Piaget.

The characterisation of thinking we have offered in the preceding sections

is a particularly strict and conservative one, emphasising the central

importance of internally-mediated conscious activity proceeding without the

aliment of an outwardly present world or of external representations of such

a world. Probably it is unduly conservative. Certainly it is common intuition

that we often think about the presented world directly, or indirectly through

the medium of external representations. However, there seems little reason

to doubt that there is at least a gradient of difficulty applying to these three

sorts of thinking. Moreover, we agree with Gibson (e.g. 1972) that there is no

good reason to regard normal perceptual processes as involving inference or

any other type of thinking and we suspect that the routine extensions of the

presented world (a) in space - seeing parts as signs of wholes, automatic

location and tracking of hidden objects - and (b) in time - limited automatic

projection of the present to the past or future - should likewise be excluded. It

may be, though, that the focusing of attention on some aspect of the presented

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[6] Ryle (1949) argued forcefully for the rarity of thinking (in the sense of our definition). Unfortunately, he took the view that this rarity implied epiphenomenal irrelevance. In our view, the rarity of thought merely reflects the rarity of occasions when we are confronted by a genuinely novel set of circumstances (see Dennett, 1983 for useful discussion of these views of Ryle).

world (extended in this Gibsonian fashion) constitutes a kind of thinking, particularly when our attention is <u>directed</u> there rather than simply <u>attracted</u> as a natural consequence of the salience of the aspect in question. This kind of thinking about aspects of the presented world we shall call <u>immediate thinking</u>. So far as the case of thinking guided by external representations is concerned (henceforth <u>externally-mediated thinking</u>), it is at least the case that once grasp of the relation between sign and referent has been mastered, no mental effort is required to maintain that relation. We shall return to the question of generalizing our notion of thinking to these two less clear cases in a later section.

3. Forms of Thinking

Following the assumptions of the previous section, the study of children's thinking should provide us with information, of an indirect sort, about the varieties of adult thinking. Children solve different sorts of problems at different ages. These developmental separations suggest that different forms of thinking are required for the solution of these different sorts of problem. This was Piaget's fundamental assumption - that thought undergoes <u>qualitative</u> development - and if it falls then the whole structure of Piaget's theory collapses.

Piaget's own ontology of forms of thought is very theory-dependent and makes only weak connections with lay intuitions. We shall adopt instead a provisional ontology which does make such strong connections and we attempt to relate it to Piaget's findings and to some contemporary research, modifying it where necessary in the course of the chapter. Our ontology is based on the idea that internally-mediated conscious connection with some entity is correlated with immediate conscious connection with that entity and that the essential step in thinking is to represent external subjects of consciousness internally. Our regulating metaphor is of thought as a <u>grasping or holding in mind of some entity</u> (we shall sometimes use the

locution 'being intentionally connected to some entity' with this sense, roughly following the notion of intentionality due to Brentano (1973) and recently much discussed (see, e.g. Dennett,1987; Searle,1983). These entities are arbitrarily various, but we assume tentatively that at least four different kinds of entity need to be distinguished: objects (individuals), properties (including relations), propositions and worlds. Some objects can be actually grasped, held, viewed, etc.; some properties are immanent in such objects; some propositions are facts, and some worlds are real. The power of thought is of course that we may think about non-existent or abstract objects and properties, about propositions which are false or of unknown value, and about imaginary worlds. It follows from this truism that the devices adopted for constructing mental symbols must be combinatorially productive, since actual entities constitute only a tiny subset of those which are conceivable.

It might be objected at this point that the contemplation of an object is impossible without the simultaneous contemplation of some at least of its properties; that object and properties together determine certain facts and that these facts specify a world or class of worlds. So what sense does it make to speak of four kinds of thinking here, since no kind may be practised in isolation from the others? But if this objection were cogent, then a child who had attained the stage of object permanence - who could therefore 'hold an object in mind' - must also be able to 'hold its location (a relation) in mind', to grasp the fact that the object is located there (a proposition) and to locate that fact with respect to other facts known about the world confronting it. We will make the assumption - consistent with developmental findings - that this aprioristic argument is unsound, and that how these four kinds of thinking are ordered and related is an empirical question. After all, there is ample evidence that we can contemplate objects attentively without grasping (in the relevant manner) innumerable 'obvious' properties. For example, it is a familiar and diminishing experience to be at a loss to specify the eye-colour of

a close friend. Likewise, Gombrich (1982) points out that those who know a cow when they see one are unable to say how the horns are situated in relation to the ears.

Since it is at least possible (see previous section) that the grasping or holding in mind of some entity is sometimes carried out in the presence of that entity or in the presence of some external representation of that entity, these two generalizations enlarge our ontology of forms of thinking to twelve basic kinds: immediate, externally- mediated and internally-mediated thinking about each of the four sorts of entity.

4. Evidence of Thinking

In this section we make extensive use of the notion of representation, intending by that term the symbol, mental or external, that enables the thinker to hold some entity in mind. An arbitrary entity may be externally present, internally represented by a mental symbol, or externally represented by a picture, drawing, imitation, check mark, referring expression or other public symbol. A fundamental developmental question, then, concerns the order of mastery of these two kinds of representation. Does the capacity to represent an entity by means of an external device depend upon the capacity to represent it by means of an internal device, or is the converse proposition true? As we will show, there is a reasonable case to be made for both sides of this argument, whether we are considering ontogenesis, phylogenesis or human cultural prehistory.

To investigate this question it is necessary to have independent means of assessing each capacity. For externally-mediated representation we require evidence that the symbol is used as an aid in calling to mind the absent (possibly virtual or non-existent) entity. For internally-mediated representation we require exactly the same kind of evidence! On the face of it, it seems that evidence for externally-mediated representation should be easier to come by, since the resemblance between symbol and entity may be so

strong that the intentional connection cannot reasonably be denied. But if the entity is absent and resemblance is strong, what is the source of that resemblance? Surely it is likely that the production of the symbol has been guided by an internal representation of the absent entity! Considering the three developmental domains in reverse order, in human prehistory the capacity for external representation is first clearly signalled by the carvings and paintings of the Magdalenian culture, around 20,000 years ago. And since these highly naturalistic paintings were often produced to 'illuminate' the walls of caves, they can hardly have been drawn from life. Although we cannot examine these early men directly for evidence of use of internal representations, their tool-making is a reasonably secure indirect sign, since the separation of site of tool-manufacture from site of tool-use is also clearly established in this culture, implying contemplation of the use in its absence.[7] Considering the second case, the great apes show no clear or compelling sign of capacity to use external representations, but likewise tool-manufacture takes place only at the site of use and neither are tools retained for future use.

These examples, and the preceding discussion, make it seem likely that the two capacities are rather closely bound up with one another. Vygotsky was probably the most forthright advocate of the priority of externally-mediated representation in ontogenesis, with subsequent internalization. He argued in many places that thought was internalized social speech, for instance (1981,p.162),

> 'It is necessary that everything internal in higher forms was external, that is, for others it was what it is now for oneself. Any higher mental function necessarily goes through an external stage in its development because it is initially a social function'

Vygotsky's presentation of this idea in the context of the origins of thinking is

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[7] In fact, neither of these arguments is particularly compelling, although the weakness that they exhibit is the same. Cave drawings may have been drawn 'from death' if not from life, or worked up from sketches made from life (Sandars, 1968, p.56-58); equivalently, a tool may be made at the site of use and then used remotely as a guide for the manufacture of further tools.

well-known, but he applied the notion more widely. For instance, the infant's unsuccessful attempts to reach for an object are first taken by adults as a sign that he desires the object and requires help, later become attenuated as the infant comes to recognize this function, and finally come to be deliberately produced signs. However, even in Vygotsky's discussions, uncertainty intrudes. When discussing memory he writes (1978,p.51)

> 'When a human being ties a knot in a handkerchief as a reminder, he is, in essence, constructing the process of memorizing by forcing an external object to remind him of something; *he transforms remembering into an external activity....*It has been remarked that the very essence of civilization consists of purposely building monuments so as not to forget. In both the knot and the monument we have manifestations of the most fundamental and characteristic feature distinguishing human from animal memory.'

The phrase we have italicized shows that not even Vygotsky could resist the obvious move of taking the capacity for external representation as a sign of pre-existent internal representation, rather than the cause and source of it. But there are difficulties with this move, as noted above. In some cases the problem of deciding whether an external symbol truly represents an entity may be just as hard as deciding, independently of such evidence, whether the child who produced the debatable external symbol might be capable of mentally representing such an entity. This is notoriously the case with early child speech. Whether a simple one or two word utterance represents (i.e. expresses) a proposition is a problem which cannot be solved by examination of the form of the utterance alone (see Chapter 2). More fundamentally, to the degree that such speech is firmly rooted in the here and now, it is unnecessary to count the elements of this speech as representations mediating an intentional connection, since symbol and entity symbolized are present simultaneously.

Given these difficulties and uncertainties in using evidence of externally-

mediated representation as a criterion for attributing capacity for internally-mediated representation, there are excellent reasons to search for independent criteria, such as remote tool-making, for use of internal representations. We may identify in Piaget's methods the use of the following criterion. Children are only credited with representational capacity if such capacity can be demonstrated, roughly, 'under difficult conditions': that is, where the actions or judgments produced are novel and where the entity represented is absent or non-manifest in some other way. We propose this vague criterion as definitive, providing necessary and sufficient conditions and will proceed to sharpen it. Notice that Piaget's use of the notion of representation (like ours) is thereby tied to thinking and to intentionality, and therefore different from the more common and broader notion in which anything that can be recognized or produced is said to be represented (see Dennett, 1983 for interesting, if inconclusive, discussion). This broader notion of representation is pandemic in Fodor's philosophy of mind and psychology (e.g. Fodor, 1981) and has been endorsed by many psychologists (e.g. Karmiloff-Smith, 1986; Leslie, 1987). However, it leads inevitably to premature talk of meta-representations, to the lethal confusion of thought and perception abhorred by Gibson and to needless disagreements (e.g. Piatelli-Palmirini, 1980). According to our usage (and, we argue, according to Piaget's too), crediting a child with the capacity to represent some entity of given type is equivalent to crediting him or her with the capacity for (internally-mediated) thought about that type of entity, and conversely.

5. Thinking about Objects

Although the criterion we have identified is logically impeccable, its application is hardly straightforward. While it seems plain enough what is meant by the question - Can a child represent an object when it is absent? - and while familiar tests of object permanence provide a diagnostic method, it is less clear what must be done to establish corresponding diagnoses for

mental symbols for properties, propositions and worlds, the other elements in our provisional ontology of mentally represented entities. However, a little reflection makes it evident that even for the case of objects, and assuming a simplified perceptual definition of object based on, say, spatio-temporal contiguity, what counts as being present or absent is open to argument. As noted above, there are grounds for regarding the immediately-presented world as going somewhat beyond those entities indexed directly through the senses. Surely the footbrake of a car is present (in all relevant senses) even if we cannot see it (we are looking at the road, of course), nor feel it with our foot. Certainly no mental effort is required to call it to mind: if such effort were required, then cars would be designed with the footbrake prominently displayed! Piaget's own method for assessing Stage 6 of object permanence (1954, Chapter 1, Section 5) requires the child to make a systematic search of the possible locations of the hidden object, rather than simply to keep track of an invisibly-displaced object. So perhaps the notion implied by Piaget's choice of diagnostic is that, lacking representational capacity, intentional connection with an object can be maintained by the Stage 5 child only so long as he or she knows where it is located in the presented world (appropriately extended). It is an interesting question, not much investigated (Huttenlocher, 1974; Acredolo, 1979) whether the Stage 5 child maintains such connections with objects in permanent locations in familiar worlds other than the presented world, for instance in other rooms of the house, etc. Allowing for some developmental separation here, we can distinguish the following hierarchy of notions of 'presence' for objects:

(1) indexed in the sensory array
(2) in the 'field of immediate action'(cf. the footbrake)
(3) in a known location in the presented (extended) world
(4) in a known location in a familiar world
(5) real, but of unknown or unspecified location

Each of these notions of presence specifies a different corresponding sense of

absence. For Piaget, the objects which are present to the mind of the child who lacks representational capacity are at best those objects which are present in the sense of level 3. Once this new capacity is attained, at around 18-24 months, objects at level 5 may be present to the mind, even if not physically present in any conventional sense. The ability to find such objects by means of an efficient search undergoes further development throughout the preschool period (Babska, 1965; Sophian, 1986). Of course, thinking about non-existent, abstract or otherwise unreal objects may depend on later evolutions of representational capacity.

Turning now to externally-mediated representation, at around the same period of development, children begin to use one object as a substitute or surrogate for another in the context of pretend play (Piaget, 1951; Nicolich, 1977). However, it is by no means clear that in such play episodes the child takes the play object as representing some definite absent (level 5) other. Rather, it may be that the play object is assimilated to the schemas associated with the represented <u>kind</u>. For discussion, see Piaget (1951), Huttenlocher and Higgins (1978); for the distinction drawn, see Goodman(1969). Whether a two-year-old's words constitute cases of externally-mediated representation is similarly unclear: the problem here is to be sure that reference is being made to (level 5) absent others. Recently DeLoache (1987) has reported remarkable discoveries in this field, namely that 2-year-olds can use a photograph and 2 1/2-year-olds a scale model as a 'map' of a room in which objects are hidden and retrieved. Again, there is room for doubt about the status of these discoveries. Since DeLoache's successful subjects take the 'real thing' as representing the model just as readily as they take the model to represent the real thing, it may be that the capacity shown here is the same capacity needed to follow the famous story of the three bears with understanding; namely, the construction of parallel analogies - since this is the biggest bed, it must belong to the biggest bear, etc. It may be, then, that the emergence of

externally-mediated representation lags a little behind internally-mediated representation.

6. Thinking about Properties

We have argued that holding X in mind under difficult circumstances - where this is a novel task - provides a necessary and sufficient (albeit vague) condition for ascribing the general capacity to form mental representations of X. In addition we have presented this condition as central to Piaget's notion of mental representation. In the preceding section we applied the condition to object representation. Of course, it is hardly surprising that it fits well here, since Piaget's discovery and analysis of object permanence strongly influenced the development of his theoretical views. What is perhaps slightly surprising is that in this hackneyed field of research so little attention has been paid to gradations of presence/absence or to familiarity and salience, factors which spring immediately to mind as soon as one thinks about what constitute 'difficult circumstances' in this context.[8] In this section we shall see that applying our condition to mental representation of properties is much harder work. This may be because of some difficulty in construing the idea of an 'absent property': however, we suspect it is because past developmental research has not pursued the natural construals of this notion.

So what might make it difficult to hold a property in mind? We may begin by noting that if the property in question is <u>manifested by some present object and salient</u>, then there should be no difficulty and therefore no call for mental representation. Neither of these notions is easy to apply, however. The famous Paradox of the Ravens (Hempel, 1965) provides a clear illustration of the difficulties involved. Seeking evidence for the proposition P that 'all ravens

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[8] But see now DeLoache and Brown (1983) and other work reviewed there. Although this work - a development of techniques first explored by Babska (1965) - is couched in terms of 'memory', 'search' and 'delayed-response' and somewhat studiously ignores its connection and debt to Piaget's investigations, it may be assimilated without undue difficulty to the scheme proposed here.

are black', we stumble on a red wheelbarrow. Since this object is not black, we make a further inspection and discover that it is not a raven either. Our confidence in P should then be boosted by an amount equivalent to the boost provided by spotting a black raven, since 'all non-black objects are non-ravens' is an equivalent proposition, logically speaking. So much the worse for logic! Yes, but the psychological lesson here is that it is almost inconceivable that we should notice that the wheelbarrow was not black and not a raven under these circumstances - such evidential propositions would hardly spring to mind! So deciding what properties are salient will in general depend on assumptions about the interests or expectations of the perceiver. Nor can this difficulty be resolved by accounting all negative properties non-salient (they are certainly non-manifest). So-called 'inalienably-possessed' attributes provide a critical antidote to this view. Surely anyone would notice the leglessness of a legless woman; but except under very special circumstances no-one would remark the possession of legs in a legged woman. Usage reflects this powerful constraint: hairless men are merely bald, but a hairy man is over-endowed; likewise to identify a woman as 'the one with the legs' draws attention not to legs but to some higher-order property. Indeed, many such adjectives are unipolar - *headless, legless,* etc., the other pole occurring only in combinations - *two-headed, long-legged,* etc. - or in discussions like this one.

This line of reasoning suggests that consideration of a perceiver's interests and expectations may permit a decision about whether a property is salient or not, if manifested by some present object. Roughly, it will be salient if an interest resonates to it or if it violates an expectation. Notice that both object and property have to be considered: whereas an orange banana constitutes a salient presentation of the property 'orange', an orange orange may not. Likewise, the leglessness of a legless book may pass unnoticed.

This still leaves us with the problem of deciding whether a given property

is manifested by a present object. An obvious necessary condition is that the corresponding proposition should be true. However, truth is not sufficient, as the previous examples show. It seems plain that what is needed is that the property should be available to direct perception, other things being equal, in objects that possess it. Notice that this criterion will exclude negative properties like leglessness and disjunctive properties.

This has been a long preamble, and the excuse for it is poor. However, similar preambles might have been constructed for each of our four categories of objects of thought. We have included it in order to make the general point that *a priori* decisions about what will constitute difficult circumstances will always be awkward and will necessarily involve the researcher in metaphysical speculation and debate. We also hope that we have made the specific point that in this context 'manifestness' of a property is independent of its salience and is determined by quite different conditions. Returning to the general point, it is clear that any methodological decision about what constitutes 'difficult circumstances' here will have to be guided as much by the products of research as by aprioristic analysis, so we now turn to discuss these briefly.

It is evident from the most cursory reading of Piaget that he considered certain properties - e.g. the logico-mathematical properties of quantity and measure - to be non-manifest in the sense we have described, to be constructed properties imposed on reality by the intelligence of the child. Moreover, he provided ample evidence that to hold such properties in mind constitutes a major difficulty for young children, a difficulty not surmounted until around 5-6 years. However, the conservations of quantity are by no means the most important or most obvious method adopted for investigating ability to think about properties. Researchers from Piaget and Vygotsky onwards have instead often favoured sorting tasks for that purpose. And these tasks typically involve straightforwardly manifest properties. Moreover,

the saliences of object properties - while undoubtedly variable - surely lie in moderate ranges. The tasks do present difficulties though, and these difficulties can be readily identified with features of the task. Roughly, in a conventional free sorting task the difficulty consists in holding fast to a particular property while other properties change. We tentatively offer the suggestion that this difficulty can be characterized as a difficulty of 'holding X in mind, when X is <u>relatively</u> low in salience with respect to other manifest properties', since the variation in other properties will in general ensure that the salience of the target property is occasionally less than that of other properties. The products of research in this field have burgeoned recently: Sugarman's (1983) investigations show that the rough age-level derivable from Inhelder and Piaget's studies (1964) is strongly dependent on techniques and materials used (i.e. on manifest properties). It seems now that children will meet this criterion at different ages for different properties and materials. If 'sortal' properties are manifest (e.g. 'being a doll') and such properties are not comprehensively 'multiplied' with salient intrinsic properties (e.g. colour properties), then 2-year-olds can hold them in mind. It seems plain that 'thinking about a property' as measured by this admittedly weak criterion is an achievement heavily dependent on circumstances, with the age of achievement ranging from 2 to 6 years, depending on the target property.[9]

This conclusion fits reasonably well with what can be gleaned from the study of language development about the external representation of properties. The acquisition of command of expressions denoting different sorts of property seems to follow a similarly staggered course, with sortal properties, locations and temporary, undesirable states (high salience) being represented very early (cf. Nelson, 1976) and intrinsic manifest 'qualities' of shape, colour, etc., appearing somewhat later (cf. Rice, 1979). Rice's work

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[9] See Chapters 12 and especially 13 for further discussion of this task.

and other indications also presents a considerable puzzle; namely, that external means of representing colours are established later than external means of representing shapes, whereas internal means appear to develop in the opposite order (Bornstein, 1985).[10] Other precise work (e.g. Levine and Carey, 1982; Halpern et al, 1983) suggests fairly substantial décalage between the development of internal and external means.

7. Thinking about Propositions

This is an enormous topic. We have reviewed developmental evidence, from a theoretical standpoint similar to that adopted in this chapter, in Olson and Campbell, to appear (see also Olson and Astington, 1987) and will not attempt to review it here. Our notion is that the investigations of Heinz Wimmer and Josef Perner (e.g. Wimmer and Perner, 1983) form the most direct test of the relevant ability. In these studies children are presented with circumstances in which an adult would ascribe belief in a certain proposition to another, while counting that proposition false. Children younger than 4 years will not do this; instead they ascribe a true belief to the other, even when that other has no grounds for such a belief. Similar findings arise from studies of lying (Wimmer *et al.*, 1984). Lying involves presenting a proposition as true, while believing it to be false, and so involves a similar dislocation between a proposition and relevant factual knowledge. So the analogue of absence for the case of a proposition will be falsity. Notice that this leads us - where Wimmer and Perner may not wish to follow - to the conclusion that children cannot hold a proposition in mind until around 4 years. Their conclusion is a milder one, that their findings show development in the young child's 'theory of mind', rather than a fundamental change in mental function (for discussion of this subtle difference in interpretation, in a different context, see the discussion of Jaynes, 1976 in Witelson and Kristofferson, 1986)

8. Thinking about Worlds

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[10] Chapter 10 pursues these questions further.

Again, we offer the briefest of comments on this ability here. It seems to us that deductive reasoning depends on this ability. A conclusion follows necessarily from certain facts or premises only if there is no world consistent with the premises in which the conclusion is false. The regulation of deduction therefore depends on consideration of worlds different from the presented world: this is the case whether or not the inference proceeds from facts or from suppositions or assumptions. While the investigation of deductive reasoning in children has hardly begun, there is every reason to suspect (as Piaget did) that it is a late achievement. Accordingly, it makes good sense to adopt as a provisional criterion 'holding a world in mind, when that world is inconsistent with the presented world'. There is of course a difficulty in distinguishing this criterion from that adopted in the previous section, but we assume that this can be met by an appropriate definition for world.

The developmental 'stagger' suggested above for mental representation of properties of different types implies corresponding staggers for mental representation of propositions and worlds. It is noteworthy, for instance, that the propositions figuring in Wimmer and Perner's studies invariably involve locations - a property (relation) represented early - as predicates. Likewise, clear cases of early deductive inference such as Donaldson's (1978, p.53) case seem to involve logically simple properties (*dead/alive* in this case). If this view is correct, then we should not expect to find any simple stage-like progression, although the dependence of worlds on propositions and propositions on objects and properties will determine such progressions for fixed combinations of types.

9. The Innatist Alternative

Our proposals amount to a strong claim that cognitive development comprises, amongst other things, a sequence of added representational powers: children can think about increasingly complex kinds of thing as

claim, but chooses to regard the cognitive limitations of young children as merely apparent. Younger children, so the argument runs, have these representational powers, but initially can only exercise them in certain limited contexts, or with adult help and prompting. This is a perfectly natural and appealing move: presumably many learned skills begin life in this context-dependent way. Frequent application of the skill in varied contexts then leads to some release from such dependence. In fact, a similar claim was made long ago by Vygotsky, who argued (cf. Vygotsky, 1978, p.84) that children of apparently similar ability might differ in their 'zones of proximal development', meaning by that phrase that within such zones precocious powers could be exercised given careful choice of materials and adult or peer support. Much the same point is made in an exemplary passage from Babska (1965, p.118):

> 'In Stage I performances giving evidence of the existence of the relevant ability did not occur. There are, relatively speaking, no circumstances or conditions to evoke the ability. Nothing can help the child. What we observe here may be termed lack of ability.
>
> In Stage III there are no conditions, relatively speaking, which can prevent the child from giving a correct performance. There are no circumstances that would prevent the child from solving the given problem. What we observe is presence of ability.
>
> Stage II is the middle one in which one may observe close relations between performance and conditions. In some conditions a high level of performance can be achieved, while in others the opposite is observed. An examination of the relations mentioned above can give us criteria for diagnosing the level that the child has achieved so far in developing the given ability.'

In Donaldson (1978) a test of 'disembedded thinking' - i.e. what we have been calling thinking *simpliciter* - is often represented as an 'unfair' test of children's abilities, systematically under-representing their real competence. In recent developmental debate, however, here and in North America, this

sort of formulation has been extended and generalized towards an innatist position in which formal operational reasoning (or so it would seem to our jaundiced observation) lies within the 'zone of proximal development' of the neonate. A representative view is DeLoache and Brown's (1983, p. 888):-

> '. . two tasks that are structurally similar may give very different estimates depending on a variety of superficial features. Stripping away non-essential features of a task, situating the task in a familiar setting, and making the task content familiar and meaningful are some of the steps that can transform a difficult task in which young children fail into an opportunity for them to display their fledgling competence. The objective of such careful task engineering is not simply to demonstrate that some ability is present earlier than has previously been shown, but to examine the precursors and rudimentary forms of the ability and the conditions of its emergence'

Now Vygotsky's point in arguing for the existence of such a zone was not to legitimize extreme innateness claims but to suggest improvements in the <u>pedagogical</u> assessment of children and in methods of pedagogy. He certainly did not see the performance of the child in an assisted context as providing a truer measure of cognitive powers but rather as refining that measure for pedagogical purposes. Our assessment of Donaldson's position is similar. There is no hint in her writings that that she sees each episode in cognitive development as the spontaneous appearance of an ability present from early infancy. She argues merely that examining 'disembedded thinking' underestimates children's cognitive resources and that appropriate pedagogical techniques exist or can be created to promote the development of these powers in children who often fail to show command of them. We have no weighty objection to this view. However, we would insist that most of the published 'demonstrations' of precocious representational powers 'succeed' either by removing the elements of 'adverse or difficult circumstances' or novelty of task which we have taken as crucial to diagnosis, (see the last quotation) or, more commonly - alas - by straightforwardly tendentious

interpretation of weak and equivocal data. Classic cases of the latter sort are 'demonstrations' (a) of number conservation in 3 year-olds, refuted in Donaldson (1971), and (b) of neonatal imitation (see, e.g. Jacobson, 1979; Hayes and Watson, 1981; Abravanel and Sigafoos, 1984 and Vinter, 1986 for corrective experiment and comment). Numerous cases of the former sort are reviewed (and dismissed) by Gold (1987). Our discussion of this issue should make it clear that those findings, or those that survive critical examination, do not in any way constitute a refutation of the methodology endorsed here. They may provide opportunities for a more effective pedagogy, as Vygotsky suggested, but will not capture thinking, as we have defined it. Naturally it is always possible that a particular tradition of diagnosis may change in the direction suggested by Donaldson. The criterion we have adopted for diagnosis is certainly vague enough to allow such movements to correct true under-estimations.

10. Conclusion

We have offered a framework for a theory of children's thinking and its development. This framework is based firmly on a Piagetian notion of mental representation of X in which that notion is linked to conscious, intentional connection with X. Moreover, our framework - again, like Piaget's - represents cognitive development as the accretion and expansion of representational powers. However, we depart from Piaget in our assumption that development is configured by differences in the substantive 'contents' of thought, rather than by formal differences between distinct kinds of mental operation.

Chapter 9: Content and the Representation of Belief and Desire[1]

1.Introduction

In this chapter I apply some of the ideas about the importance of <u>content</u> developed in the previous chapter to some issues concerning children's ability to represent the beliefs and desires of others - or, in plainer language, to think about their thoughts. I want to start by expressing some general reservations about the theory of mind enterprise as a whole. One of the insights to come out of Europe in the 1920s and early 1930s was the idea that cognitive development involved increase in <u>powers of representation</u>; this was not only a feature of the well-known works of Piaget and Vygotsky but was also strongly implied by the comparative-psychological writings of Wolfgang Köhler. I incline strongly to the Piagetian position that children are initially devoid of representational powers. In short, that there is literally <u>nothing</u> that an infant can think about. According to Piaget, the first signs of such representational powers appear around the end of the second year with object permanence, deferred imitation, insightful solutions to detour problems and so forth.

For Piaget, of course, perception did not imply representation: it was only when the infant could react to an object which was <u>not</u> present, or reproduce an action not recently performed, that it made sense to him to speak of <u>representation</u>. But the representational powers implied by these achievements of late infancy may be quite limited - perhaps only that children can now think about absent objects and anticipate the outcome of certain actions, etc. The central theory-of-mind claim, on the other hand, is that by age 4, children can represent and therefore - to my way of thinking - <u>think</u> <u>about</u> or <u>hold in mind</u> - the thoughts (beliefs, intentions, desires) of another

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[1] This chapter is based closely on a paper delivered to the Canadian Psychological Association's Annual Conference in Halifax, 1988. It is currently under review with the Canadian Journal of Psychology.

actor. If one takes the Piagetian position, then, this sets a spectacularly heavy agenda for the 2 year period from around 2 to around 4: hard work for the child and even harder work for those who would explain the child.

If one is uncomfortable about such a rapid advance in representational powers, then there are two moves that might be made. On the one hand, there is the idea that the powers of the infant might have been underestimated. On the other, there is the idea that the powers of the 4 year-old may have been overestimated. Although my own mind remains more or less closed to the first possibility, it remains open to the second. After all,the key demonstrations of the new powers - the various Wimmer and Perner false belief tasks (see Astington and Gopnik, 1991a) - demonstrate only successful prediction of the actions of others in situations where the judging subject would act differently. We are accustomed to the idea since Vygotsky that young children may use language in a quite transparent way to control the behaviour of others, without explicit knowledge of the linguistic means by which this control is achieved. It may be that 4 year-olds develop practical means for efficient prediction without having any explicit knowledge of these means. In that case we might prefer not to say that the false belief tasks show that 4 year-olds can think about the thoughts of others, but only that they can think about the possible actions of others. However, this is a bleak and sceptical view. Moreover, it fails to do justice to the care taken by Wimmer and Perner in seeking to exclude alternative explanations to the one that they prefer. Perner, too, (e.g. 1991a) has strongly resisted the tendency - now commonplace in North American discussions - to assume that everything of substance in cognitive development is innate and so has accepted the difficult agenda of strong development of representational powers in the period 2 to 4 years.

2. The Content of Beliefs

If we are uneasy about this agenda of strong development of representational powers, but wish to save the general hypothesis that 18 month-olds can't think about anything much and that 4 year-olds can think about the thoughts of others, then what can we do to quell the uneasiness? In Chapter 8 I proposed that development may be staggered for different representational content. Possibly thoughts of rather circumscribed content may be entertained by the 2 year-old, allowing us to maintain the view that there is not very much that they can think about while conceding that they may occasionally think about states of affairs other than the one that currently confronts them. Again, it may be that 4 year-olds can represent the thoughts of others only if these thoughts have a similarly circumscribed content.

Oddly enough, there is already some immediate encouragement for this idea. Surely, if we were to allow that 2 year-olds could represent a certain class of thoughts, a likely candidate class would be thoughts about the location of hidden or absent objects, because of the strong development of object permanence and other searching skills. It may then be no coincidence that the false belief task employed by Wimmer and Perner involves attribution by the subject of beliefs about the location of a hidden object.

There is another version of the false belief task in common use which does not require attribution of a false belief about the location of a hidden object. This is the so-called Unexpected Contents task (in which, say, an eggbox is found to contain marbles) and children succeed with it at roughly the same age[2] (Hogrefe et al., 1986, Experiments 1 & 2; Perner et al., 1987, Experiment

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[2] The relative difficulty of these two tasks is hard to establish, because of variations in procedure, and since within-subject designs are generally avoided here on account of probable strong order effects. While Wimmer and Perner (1983) found no success with 3 year-olds on their Location task, later investigations (e.g. Hogrefe et al., 1986, Experiment 4) show about 20% success. Equally, Perner et al., 1987 found 45% success with 3 year-olds on an Unexpected Contents task, but others (e.g. Wimmer and Hartl, 1991, Experiment 1) have reported only 25-30% success. On balance, the Unexpected Contents task seems to be slightly easier.

2; Wimmer and Hartl, 1991). However, the relative difficulty of constructing the target belief here - a belief about what <u>kind</u> of thing is in the box - and in the standard Location task may be obscured by other differences in procedure. In the Location task the subject (or informed puppet) knows that the chocolate is at B and her knowledge of this is carefully checked. When the owner of the chocolate returns, knowledge of its original hiding place, A, is checked and the subject is carefully informed that Maxi decides to get his chocolate. If Maxi wants his chocolate and thinks that it is hidden at A and is a rational being, he should therefore look at A. In the Unexpected Contents task the child is merely asked,

> *'If I show this box to Y [a second child or puppet], all closed up like this, what will she say/think is in it?'*

Presumably the 4 year-old is supposed to say 'Eggs' because:-
      (a) eggboxes usually contain eggs
      (b) this is an eggbox with contents concealed
      (c) Y is presumed rational

But in fact no attempt is made to verify that the subject knows that Y would assent to (a) and (b). Perhaps if the subject were given the following instruction instead, the task would be somewhat easier:-

> *'Look, here comes Snoopy. I'm going to show him the box all closed up like this. Snoopy sees that it's an eggbox and he knows eggboxes usually have eggs in them. What will he say is in the box?'*

A second point of difference between the Unexpected Contents task and the Location task concerns the degree of commitment to the judgment made. When shown a closed eggbox and asked 'what do I think is in the box', I am certainly disposed to say 'Eggs' but whether it is right to say that I <u>believe</u> that there are eggs in the box is another matter. I am, we might say, making an educated guess, but I would not agree that I knew that there were eggs in the box nor commit myself to other claims implying representation of the hidden

eggs. So the task is very unbalanced: the subject has grounds - as good as they come - for her belief that there are marbles in the box (she just saw them), but Snoopy has rather poorer grounds for the target belief that it contains eggs. It follows that the subject has a correspondingly weak basis for belief ascription. So I am not dismayed by the coincidence that the two tasks are mastered at roughly the same age and in particular, I do not think that it shows that one type of false belief is as easily represented as another.

Once we are alerted to the idea that different sorts of belief might be differently representable, other relevant distinctions between kinds of belief come to mind. The tasks just described both involve updateable, revisable beliefs or guesses. Presumably, it is just this sort of belief which we might first learn was empirically vulnerable. Accordingly, such false beliefs might be the first to be ascribed to other actors. But many of our beliefs, although still <u>contingent</u> beliefs, are not readily revisable, e.g., beliefs about the origins or causes of natural phenomena. Sticking with eggs, our subject might believe that eggs come from chickens. Suppose we established that our subject held this belief, and then introduced little John, whose mischievous father had misinformed him that eggs were laid by pigs. Little John goes on holiday to a farm and the farmer sends him out to collect some eggs. Where would he be predicted to look for eggs? In the chickenhouse or in the pigpen?

Obviously, more examples could be produced but the point may be already clear enough. The sorts of thoughts that a 2 year-old can have may be quite restricted in content and, equally, it may be that the sorts of thoughts that a 4 year-old can represent or think about may be similarly quite restricted.

As a bridge to my next topic, we may note that all the beliefs so far discussed are readily construed in a referential manner, or *de re* as some would put it. They are beliefs about a certain bar of chocolate, or about eggboxes, or about one particular box, or about eggs. However, notoriously, some beliefs are not about any definite thing - in the sense that we don't have

anything in mind that the belief is about. So if I am nearly run over by a speeding car I might form the belief that the driver of the car - <u>whoever she is</u> - is a criminal lunatic. With such beliefs, truth-value changes when a co-designative expression is substituted, so, supposing the driver was in fact my mother, it does not follow that I believe that my mother is a criminal lunatic. Beliefs construed in this fashion as being about who or whatever satisfies the description of the subject of the target proposition are sometimes described as *de dicto* beliefs.

3. Desires and Beliefs

There are many possible ways in which desires and beliefs might be distinguished, and recent discussions by Perner (1991b) and by Astington and Gopnik (1991b) have helped clarify some of these ways. For example, it may be that <u>beliefs</u> are typically shared - thus making the ascription of a different belief hard - but that <u>desires</u> are typically not shared, making the ascription of a different desire somewhat easier. Of course, there are problems here arising from uncertainty about what constitutes the same belief or the same desire. George and Henry each believe that they see a turnip, but George's belief is about George and Henry's is about Henry, so from a third-person logical point of view the beliefs are different, but ordinary usage and commonsense encourage us to say that George and Henry share the belief that they see a turnip. George believes that Henry sees a turnip (Henry is in the garden, while George is indoors); Henry also believes he sees a turnip. Here third-person logic says they have the same belief (that Henry sees a turnip) but ordinary usage and commonsense dictate the conclusion that George and Henry do not share a belief that they see a turnip. If we set aside the peculiar view of logic regarding the individuation of beliefs (or desires), in which two people cannot have the same belief or desire about themselves, then it seems quite plausible that members of the same community, viewing the same world, might share very many beliefs, but not quite so many

desires.

Although beliefs and desires might well differ in the way just described, what I will attempt in this section is to discuss some possible ways of distinguishing beliefs from desires in terms of their typical contents, since I think that these have been ignored or underestimated.

In the first place the content of a belief seems undeniably propositional and our usage reflects this. The verb *believe* takes a full clause as its complement specifying a subject - what the belief is about - and a predicate - whatever is thought to be true of that subject. Desires <u>may</u> have this form, but very many desires seem to have much simpler content.

(i) *X wants Y*   (ii) *X wants to [Verb] Y*

are familiar formulae that fall short of the full propositional specification invariably found with *believe*. Of course such formulae may be treated as elliptical and expanded as follows:-

(i)' *X wants that X has Y*   (ii)' *X wants that X [Verb]s Y*

The notion that desires, like beliefs, have propositional objects is put forward by Searle (1983, p. 29), and, so far as I know, his remarks on the question have not, so far as I know, been countered by other philosophers or linguists, so it is worthwhile to consider them here.

His argument is brief - barely a page - and is easily summarized. He begins by making clear that he is discussing a notion which is more general than that codified by individual English verbs like *wish*, *want*, *desire* and points out that whereas one can say of some previous act,

(1) *I wish I hadn't done it*,

it is 'bad English' to say

(2) *I want/desire I hadn't done it'*.

He considers that (1) expresses an intentional state of desire and that the absence of sentences like (2) is due to some arbitrary syntactic restriction associated with the verbs *want* and *desire*. Although this appears to be an

argument for abstraction, Searle has also slipped in the claim that we may desire past events to have had a different outcome. He then discusses and rejects the *prima facie* evidence that desires may have simple (non-propositional) objects as their contents, namely the existence of sentences like (3), by means of the following argument. The 'surface structure' of

(3) *I want your house*

is said to be misleading. Searle considers the sentence:-

(4) *I want your house next summer*

and argues that *next summer* cannot modify *want* since the sentence does not mean

(5) *I - next summer - want your house,*

since it is perfectly consistent to say

(6) *I now want your house next summer though by next summer I won't want your house.*

What the sentence (4) must mean, says Searle, is

(7) *I want (I have your house next summer).*

It is therefore evident that the content is the proposition expressed by the embedded sentence. He concludes that since all occurrences of sentences *S desires/wants X* can take such modifiers, they must all be considered as expressing attitudes to propositions.

It seems to me that this argument is easily challenged. In the first place, surely we would normally say that what (1) expresses is not <u>desire</u> but <u>regret</u>. Wishes regarding the outcome of past events rarely come true! Nor is the possibility that such past events were wrongly reported a presupposition of such a wish. There is little point in coveting, lusting after or seeking a different outcome. One might feel guilty, or sorrowful, or ashamed, etc., about the actual outcome, but could hardly plan to have acted otherwise! So there is a good case for rejecting the notion that the object of desire could be a past event. Could it be a present event? I think the answer is also no. While

one might enjoy some experience currently in progress (perhaps as we tuck into our caviar, desire lines up with the edge of the spoon!), enjoyable consumption is not desire. So it seems natural to represent desire as an intentional state which is aimed at some unfulfilled, but usually fulfillable, object.

If this is accepted, then it follows that sentence (4) differs from (3) only in that the <u>future</u> time when the desire is to be fulfilled is specified in (4) but unspecified in (3). Since, by hypothesis, expressions of desire always imply a time of fulfilment (which may or may not be specified) there is no need to invoke arguments about what constituent is modified as Searle does. In any case, the locus of adverbial modifiers is always hard to establish with any certainty: they may be said to modify the verb, the verb phrase, or the sentence as a whole, and deciding between these alternatives is never straightforward. In the case of (4), it may be that *next summer* cannot be taken as modifying the verb *want* but this is hardly a sufficient reason for abandoning the analysis *S-V-O-Adv*. Consider the following argument, which is analogous to Searle's. The sentence

(8) *I go to Paris next week*,

predicts a journey, perhaps due to some business obligation. This cannot mean, following Searle,

(9) *I - next week - go to Paris*,

since it is perfectly consistent to say

(10) *I now go to Paris next week but next week I may not go to Paris*. Although the argument looks no worse than Searle's, few would be satisfied with it and none would want to propose an analysis for (8) other than *S-V-Loc-Adv*. Sentences like (3), (4) and (8), which express states to be terminated by consummation at some future time admit both aspectual/temporal modification of the verb denoting the state and specification of the time of consummation.

Looking at Searle's argument from the other end, he proposes the analysis in (7) for sentence (4). But if there is an embedded sentence in such simple statements of desire, that sentence must be rather limited in the form it can take. Although the underlying structure of

(12) *I want a pound of caviar*,

uttered at the delicatessen counter, might look as if it might be

(13) *I want (I buy a pound of caviar)*,

this cannot be, since such an analysis would allow the sentence

(14) *I want a pound of caviar with my Mastercard*

to mean that you want to buy it with your Mastercard. Instrumental and Manner adverbials, which the verb *have* does not permit, are also not permitted as modifiers of the mode of consumption in simple desire statements. So if there is an embedded sentence in simple desire statements, its subject is always *I* and the main verb is always *have*. Finally, if we look instead at desire statements with undeniable propositional objects, then we still find strong restrictions on the form of the verb. It cannot be finite (**I* want you leave today* - cf. *I believe you leave today*) and hence will not accept any modifications of tense. Although some similar verbs readily accept internal negation (e.g. *I promise not to leave*) and this may mean something quite different from external negation (*I don't promise to leave*), *want* and *desire* with internal negation are clumsy at best (*?I want not to leave*), and - if they mean anything - mean the same as external negation (*I don't want to leave*).

So there is a perfectly good case for treating desire as an intentional state which can have as its aim either an object (simple desires), or an event or state (complex desires, with propositions as complements). On the other hand, if for this reason or that it is decided to analyse simple desires as containing an embedded proposition, then that proposition has a very restricted form - in particular, much more restricted than is the case for

beliefs.

Returning to the expansions (i)' and (ii)', it is therefore by no means evident that expanding them in this way does any sort of justice to the content of X's desire. On the contrary, if I want an apple it seems simply absurd to say that I want some proposition to be made true. It is perfectly conceivable that (i) and (ii) correctly characterize the content of X's desire as respectively an object and an action rather than as a proposition. Even if we do translate simple desires in this way as:-

(iii) *X wants that S(ubject V(erb O(bject*

it is plain that in the formula (iii) the default value of S is X and the default value for V is *have*.

Accordingly, and against recent discussions by Perner in particular, there seems to be no good case for symmetry between belief and desire as psychological states. A desire may have a simple object as when *X wants a new dress* whereas a belief must have a propositional object. The content of the desire may simply be a representation of the desired object. Further, although occasionally it may make sense to speak of a *de re* desire, as when X desires not just any new dress but one particular dress on display in a particular high fashion shop, for example, in general desires are for any object that meets a certain vague specification. So the content of a typical desire is a non-referential specification and is therefore *de dicto*. If the content of such desires represent anything then they represent only a possible object satisfying some vague conjunction of properties. In Quine's famous example from *Word and Object* (1960, Chap. 4), the man who wants a sloop may be said merely to crave relief from slooplessness.

In this respect, desires are like pretend play in that the child who pretends that a banana is a telephone most likely does not have any particular telephone in mind and so represents no telephone by means of the banana. Again, although to make the point effectively would involve another lengthy

excursion, I would argue - following Goodman's analysis in *Languages of Art* (1969) - that drawings do not represent in a straightforward manner either. If I draw a fish, there may be no fish that I have in mind. I draw a fish-picture, not a picture of some particular fish. So the recent flurry of studies of children's drawing often beg the representational question. We may put a cup in front of a child, give her pencil and paper and ask her to draw the cup. But unless the child accepts the task as given, as one of producing a representation of that cup, there is little point in pondering the differences between her cup - the cup drawn - and the cup before her.

On the other hand, when we turn to desires in which S and V do not have the default values discussed above, the content may be just as referential and specific as that encountered in a typical belief, and that is because the desires are now about whatever fills the Subject slot in the formula.

4. Some Conclusions regarding Methodology

I will try now to relate some of the distinctions discussed above to current studies of knowledge of belief and desire. There are a number of studies now published, in which children's ability to represent beliefs and desires are compared, with mixed results (Astington and Gopnik, 1991b). Sometimes it seems as though 3 year-olds can represent (think about) the desires of another or their own previous desires; sometimes the desire seems as hard to represent as the usual false belief. My suggestion is that in the first place these studies have all employed rather untypical desires and that it would be premature to draw any firm conclusion from them. I will not comment on Astington's studies in any detail; however, it does seem to me that the desires investigated in them are all of the referential *de re* variety - desires for some specific thing. As I have suggested, such desires are perhaps not the simplest sort of desires and, besides, involve a referential element. In Astington's studies, clear developmental separations between representation of belief and desire seem rather elusive. Possibly such separations would be

clearer for *de dicto* desires.

I will conclude the chapter by discussing two studies conducted by Perner's students, Nicola Yuill (Yuill, 1984) and Julie Hadwin (Hadwin and Perner, 1991). In Yuill's study subjects are presented with the information that a child has a highly specific desire, namely that a specific child should catch a specified thrown ball - 'This boy wants that boy to catch this ball'. There is another possible catcher, a girl. The story is presented with pictures and the thrower's desire is illustrated by means of a 'think-bubble' illustrating the desired outcome. Subjects have to judge whether the thrower will be pleased or sad, given this or that outcome. It turns out that 3 year-olds judge as we would that the thrower is more pleased if the intended catcher does in fact catch it.

This study has many weaknesses and it is hard to know why Perner (1991b) sets such store by it. To begin with, there is no reason why the subject should not share the desire of the story character and so we can have no confidence that the judgments reflect the represented thoughts of the story character rather than the wishes of the subject herself. In the second place, the story character's desire is represented by a drawn 'think-bubble' and - as I have argued above - there is some question about how a 3 year-old takes such drawings - even when they are not think-bubbles! Thirdly, that the girl should catch the ball instead of the intended boy hardly seems a dismaying outcome. This criticism works in the opposite direction from the previous two, since it makes it the more surprising that 3 year-olds should consider this outcome unsatisfactory and a source of sadness for the thrower.

In Hadwin and Perner's study the think-bubbles are presented as representing the thrower's beliefs about the outcome of his throw. In the picture story there is a wall between thrower and catcher so that until he looks over the wall the thrower does not know whether his belief is correct or not. Subjects had to judge whether the thrower would be surprised or not,

given the actual outcome. Hadwin and Perner found that even 5 year-olds had difficulty in making appropriate judgments of surprise. Aside from the difficulties raised in discussion of Yuill's task, this study introduces a further difficulty, for the thrower is apparently represented as having a certain belief, but without any <u>grounds</u> for holding it. Why should he think that the other boy had caught the ball? Or is this merely <u>wishful thinking</u>, equivalent to desire?

Surely we would only predict that the thrower would be surprised if the thrower had some grounds for his wrong belief. Wimmer, Hogrefe and Sodian (1988) and Sodian and Wimmer (1988) have investigated the child's developing knowledge of the grounding of beliefs in perception, inference and so forth. On the face of it, desires are often similarly grounded. My desire for a new dress may be a simple consequence of my perception of the shabby and unfashionable nature of the one that I am wearing now. Or it may arise because my best friend tells me that it's time for a change. Or I may infer from the expressions of contempt on the faces of my peers that something is amiss, etc, etc. So desires may be grounded in just the same range of ways as beliefs. So far as I know, no-one has investigated children's ability to represent desires grounded in these or other specific ways. Notice that in the example just given, the desire for a new dress is clearly to be taken *de dicto*. On the other hand, when the desire is *de re*, for a particular toy, or sweetmeat, or - as in Yuill's study - a desire that a particular person should catch a ball then rather different grounds may have to be adduced. As Astington has argued in recent papers, the intrinsic desirability of certain objects may be a ground for a desire or - as in Yuill's study - the *de re* desire may be left quite ungrounded as a whimsical urge of the child.

Once again I have, I believe, identified good reasons for thinking harder about desires from the point of view of <u>content</u>.

Chapter 10: Contextual Salience and Colour Adjectives

1. Introduction

In general it may be said that children find adjectives more difficult to learn than the other major lexical classes, nouns and verbs. In her survey of 1-2 year-old productive vocabulary Nelson (1976) found few adjectives in use, and those few denoted temporary, often undesirable, extrinsic properties of objects - *hot* , *wet* , *dirty* , *broken* , etc. Adjectives denoting intrinsic properties of objects such as shape, colour or texture, etc. were rare. Although the contrast between 'extrinsic' and 'intrinsic' may be given many meanings, here we intend only that extrinsic properties are subject to change, whereas intrinsic properties are fixed, or change only very slowly.

An obvious characteristic of the extrinsic properties denoted by *hot* , *dirty* , etc. is that they are <u>contextually salient</u> when referred to. That is, the hotness or dirtiness of some object which is normally cool or clean is being remarked upon. It may be that it is because of this contextual salience that such properties come to the attention of young children and thus facilitate their learning of the corresponding adjectives. Therefore, so far as extrinsic properties are concerned contextual salience is more or less guaranteed, since - by definition - the extrinsic properties of objects change from time to time. But what about intrinsic properties, which do not change? Can the slower learning of colour or shape adjectives be explained by the low salience of the properties they denote?

In Chapter 8 an explanation of this sort was proposed, suggesting that young children may encounter a general difficulty in attending to intrinsic properties and in thinking about them. It was argued there that this difficulty can only be overcome by making the property in question salient. Some intrinsic properties may be salient in an absolute sense, such as particularly bright colours or possession of an uncommonly large nose, etc., but in general most intrinsic properties will be salient only contextually. That

is, such a property will be salient only if it violates an expectation or answers to some present need of the subject. As has often been noted, the latter state of affairs is uncommon with colours: it is seldom the case that colour matters much, and it is never easy to contrive experimental situations in which it does matter. However, many kinds of things - some natural kinds, some artefacts - have a characteristic or typical colour, so it is relatively easy to to present colours in such a way that an expectation is violated, most simply by presenting an object or replica object in an uncharacteristic colour, such as a bar of green chocolate, or a pink sugar lump, etc. If the argument of Chapter 8 is correct, such presentations should attract attention to colour, should facilitate elicitation of colour vocabulary and should provide an efficient context for the teaching of new colour terms.

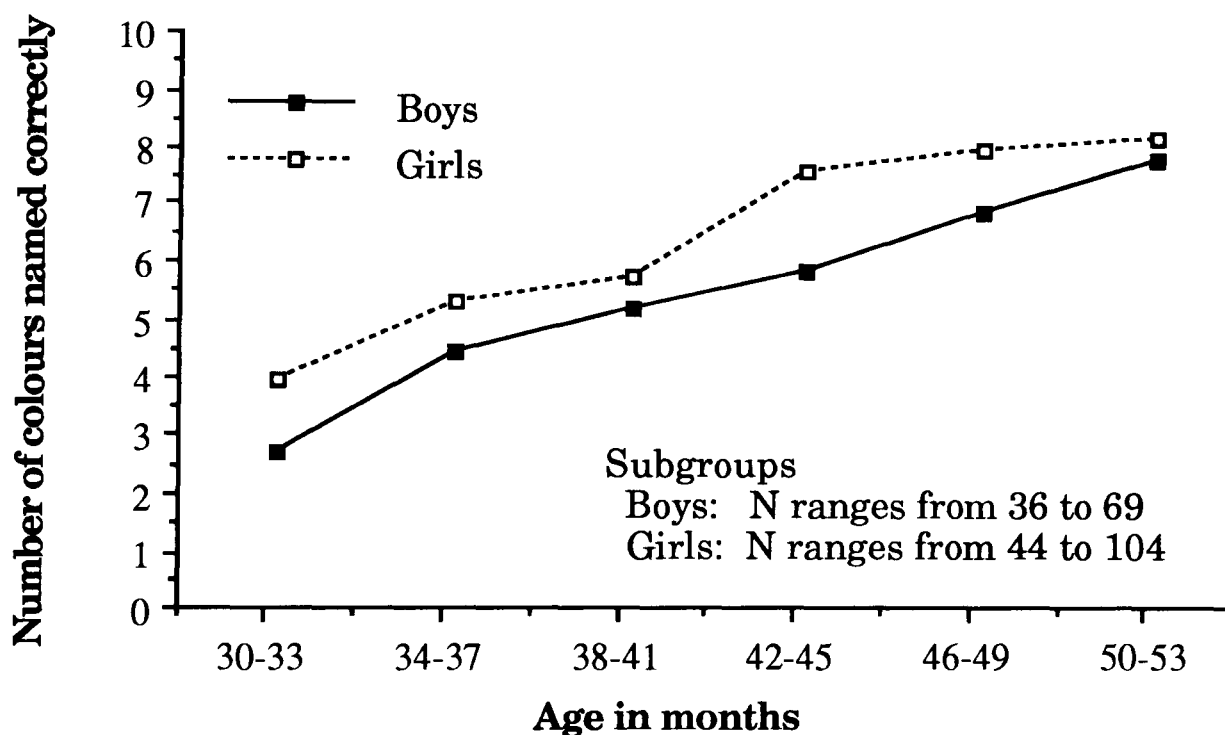Figure 1. Colour naming from 2;6 to 4;5 (data from Johnson, 1977)

There have been only a handful of studies of the development of colour vocabulary: e.g. - with materials used for naming noted in brackets - Heider, 1971 [colour chips]; Cruse, 1977 [cardboard squares]; Johnson, 1977 [cardboard squares]; Bartlett, 1978 [paper strips]; Rice, 1980 [common household artefacts in a range of colours, e.g. combs]; Campbell, Bowe-

Macdonald and Dockrell, 1982 [wooden dolls]; Andrick and Tager-Flusberg, 1986 [colour samples]. Efficient use of basic colour vocabulary (the core adjectives *white* , *black* , *red* , *yellow* , *green* , *blue* , *brown* , *purple* , *orange* and *grey* - see Berlin and Kay, 1969) does not seem to be established until children are approximately 4 years old. There appears to be a moderate sex difference, favouring girls. Normative data supporting these two claims are presented in Johnson, 1977, and reproduced in Figure 1 above.

Moreover, it is extremely difficult to teach new colour vocabulary (cf. Campbell *et al.* , 1982 and especially Rice, 1980) or to establish word-colour associations (Bornstein, 1985a). For example, Rice's subjects required between 283 and 1004 trials to reach criterion on a simple red-green discriminative naming task. As Bornstein (1985b, p. 73) noted, reviewing this material, there is no reason to suspect that this late development has any perceptual cause. Perception and discrimination of colours is an achievement of infancy. Further, (p. 74), acquisition and use of colour nomenclature is 'tardy and problematic'. Bornstein went on to propose a neurological explanation of these difficulties (late maturation and integration of callosal structures and cortical association areas), but - whatever the merits of this explanation - in our view he too rapidly discounted cognitive explanations based on lack of saliency. Bornstein assumes that colours <u>are</u> salient to young children, but the evidence from matching and sorting studies which he cites in support of this opinion (p. 79) does not appear to support it strongly, if indeed at all. The experiment reported below takes as its starting point the possibility that colours are not salient to young children in any absolute sense and investigates the effect of manipulating contextual salience on elicitation of colour names. Three year-old boys and girls were studied, since previous work (e.g. Bartlett, 1978) suggests that children of this age typically know a few colour adjectives, but not all, and understand requests to name colours.

2. Method

Subjects:A sample of 48 normally-sighted children, aged 2;10 to 3;9, and comprising 24 boys and 24 girls, was drawn from local playgroups. These were randomly assigned to two groups, A and B, preserving the balance of the sexes. Group A consisted of 12 boys and 12 girls, mean age 3;3, range 2;11 to 3;10. Group B consisted of 12 boys and 12 girls, mean age 3;4, range 2;10 to 3;9.

Materials : Two sets of replica objects were constructed for colour naming. All objects were constructed from FIMO modelling material, a widely-available non-toxic material which hardens satisfactorily and which is manufactured in an extensive colour range. Nine objects were chosen for modelling, each having a characteristic colour. The 9 FIMO colours used were chosen to match as far as possible the characteristic colours of these objects. Two replicas of each object were constructed, one using the characteristic colour and the other using an uncharacteristic colour to form 2 sets of objects for naming. In the list of objects that follows, the colour used in Set 1 is shown along with its FIMO designation and code. The same 9 colours were used in constructing Set 2:-

Object	*Set 1*	*FIMO code*	*Set 2*
banana	yellow	'yellow HM801'	red
beetroot (with green top)	purple	'purple HM8059'	yellow
sprig of 3 blackberries	black	'black HM809'	green
4x2 chocolate bar	brown	'terracotta brown HM8077'	purple
cucumber	green	'green HM805'	orange
orange	orange	'orange HM804'	brown
sugar-lump	white	'white HM800'	pink
sticking-plaster	pink	'flesh pink HM8045'	black
tomato (with green stalk)	red	'red HM801'	white

Neither a blue nor a grey object was included, since we were unable to think of suitable small objects known to children with these characteristic colours.

Procedure: Each child participated in two naming tasks. Both groups initially named the colours of the objects in Set 1. Group B then named the colours of the uncharacteristically-coloured Set 2, while Group A repeated the original task (Set 1). Within each task, objects were presented to the children in a different random sequence. Children were allowed to handle and to examine each object and were asked 'What colour is this?'. The objects were not named for the children, nor was any feedback given.

3. Results

The results for numbers of colours named correctly in the various conditions are summarized in Table 1.

Table 1. Means and standard deviations of numbers of colours named correctly (max=9)

		Naming Task 1		Naming Task 2	
	N	Mean	SD	Mean	SD
Group A					
Boys	12	4.58	2.61	4.58	2.61
Girls	12	5.33	2.67	5.33	2.77
Group B					
Boys	12	3.92	2.71	5.50	2.02
Girls	12	6.33	1.07	7.91	1.24

A 2 x 2 x 2 mixed-model Analysis of Variance examined the effects of Sex, Task and Condition on numbers of correctly-named colours, with Sex and Condition as between-subject factors and Task as a within-subject factor. There were significant main effects of Sex ($F[1,44]=5.81$, $p<.05$) and of Task, with the Task effect entirely due to the significant Task by Condition interaction ($F[1,44]=51.24$, $p<.001$). The nature of these effects is apparent in Figure 2: girls produce more correct colour names than boys, and Group B (uncharacteristic colours) produce more correct colour names than Group A - although, naturally enough, only in the second naming task. Since no

difference between the performance of the Groups on Task 1 was found, these results are combined below, where appropriate.
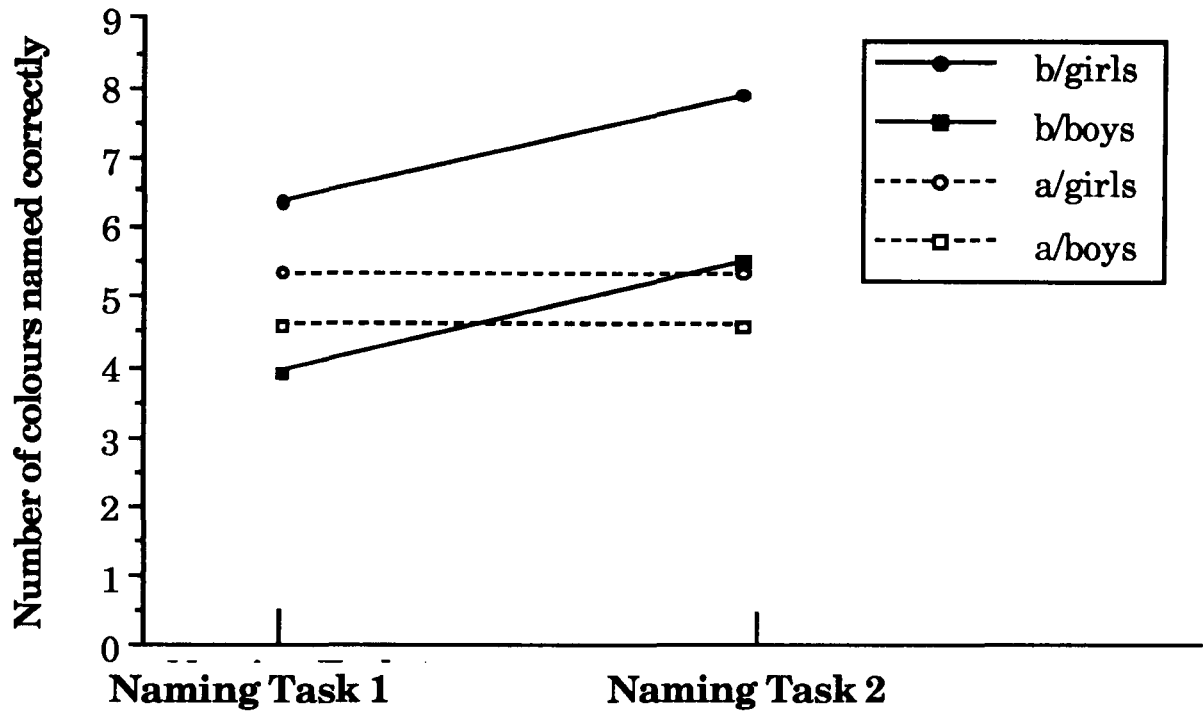
Figure 2. Colours named correctly in repeated naming tasks

The distribution of vocabulary sizes in the first naming task (combined data), and in the second naming task (Group B) are shown in Figure 3.
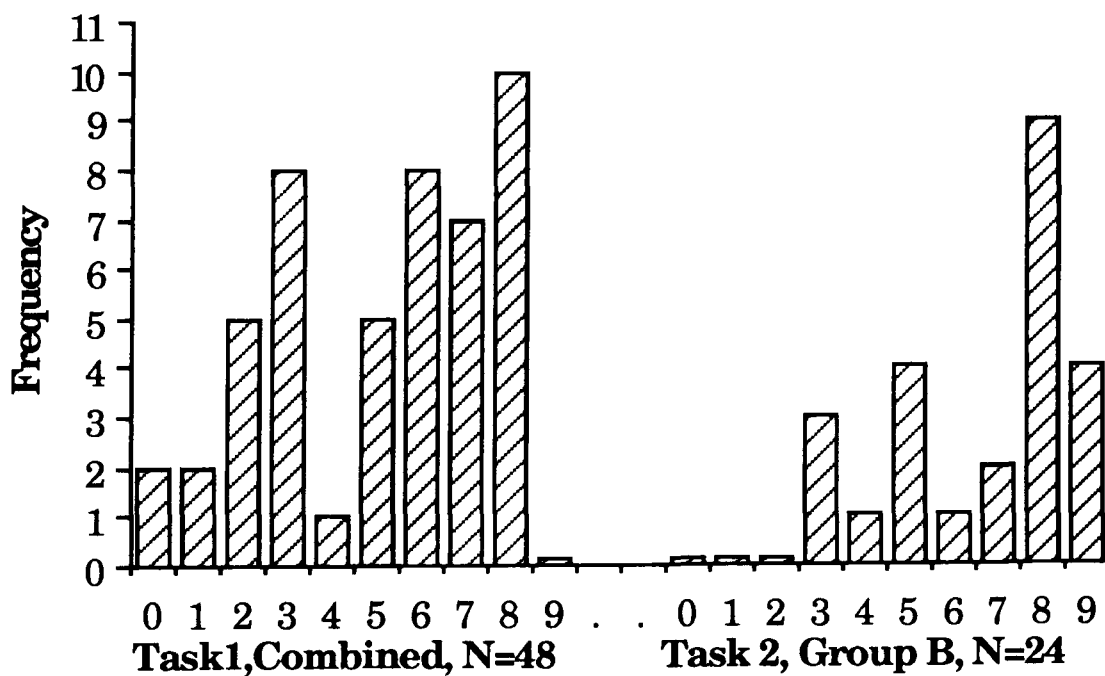
Figure 3. Distributions of numbers of colours correctly named.

The mean number of colours correctly named in Task 1 (n=48) was 5.04, whereas in Task 2 (n=24) it was 6.71. While Group A subjects hardly varied

in the second task, of the 24 subjects in Group B, 5 named 3 more colours correctly in Task 2, 8 produced 2 more correct names, 7 produced 1 more and 4 subjects showed no change. These distributions have a bimodal appearance, suggesting that basic colour vocabulary is acquired in two phases. Bartlett (1978) presented similarly bimodal distributions.

Figure 4 shows the proportion of subjects naming each of the 9 colours correctly in Task 1 (combined data) and Task 2 (Group B only), together with Johnson's results for a sample of comparable age. Comparing our Task 1 findings with Johnson's, inspection of the confidence intervals shows that only the values for brown, pink and purple differ significantly. There is noticeable improvement for all 9 colours in Task 2: comparisons with Group B's performance on Task 1 using McNemar's Test for correlated proportions shows that significant improvement (one-tailed) occurred with white, red, brown and pink.
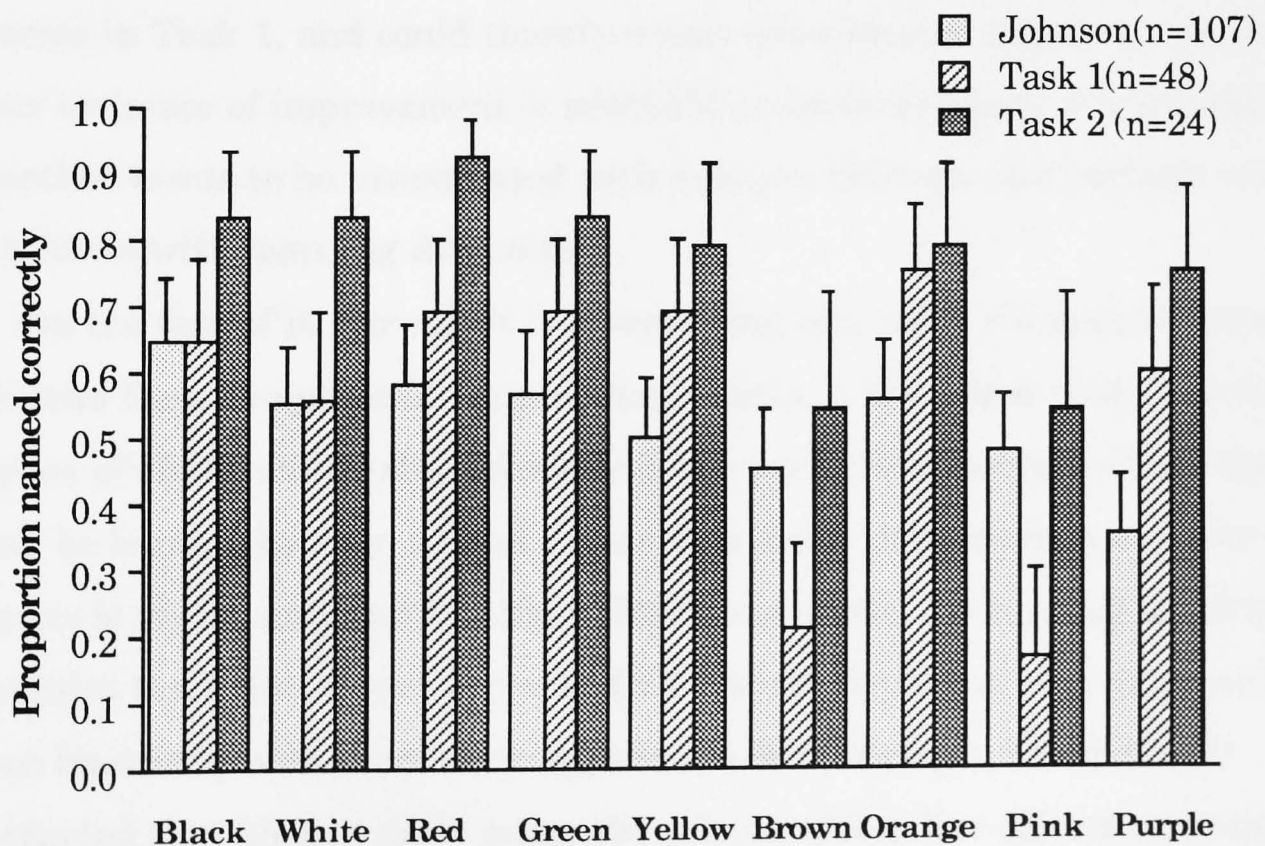
Figure 4. Object Colour and Naming Success

Since some colours are clearly better known than others and since vocabulary size is not evenly distributed, the data were examined for evidence

bearing on the question of order of acquisition. However, no consistency in order of acquisition was found.

4. Discussion

The improvement in naming resulting from use of uncharacteristically-coloured objects shows that conventional methods of colour-term elicitation may underestimate word knowledge. Comparison with Johnson's norms, whose subjects named cardboard squares, suggests that naming objects with no characteristic colour is a similar task to naming characteristically-coloured objects. Other studies of early colour naming, as noted earlier, have invariably used neutral stimuli similar to Johnson's.

The degree of improvement obtained (about 2 colour terms, representing 6 months of normal development) may be considered modest, but is encouraging when compared with the poor return of training found by Rice (1980). Moreover, 8 of the 24 subjects in Group B produced 7 or more correct terms in Task 1, and could therefore only show limited improvement, so that our estimate of improvement is probably an underestimate. Clearly this method needs to be investigated with younger children and perhaps with children with learning difficulties.

On the face of it, our result is a surprising one, since the opposite prediction follows from two plausible arguments. Firstly, it is evident that the colours of types of object with a characteristic colour, or of familiar individual objects, can be learned by rote: a blind person may know that bananas are yellow, that grass is green, and that his dog is black and white. Very young children may acquire their first colour terms in this fashion, ignorant of their meaning, but knowledgeable about their application. For example, Cruse (1977) reported that his son could name the colours of familiar objects at a stage when his colour-naming was otherwise random. Accordingly, we might well have expected subjects to perform better with the characteristically-coloured objects, since such rote-learned knowledge is useful in this task, but useless

with uncharacteristically-coloured objects. However, this was evidently not the case. Secondly, the manipulation resembles that producing the well-known Stroop effect (for review, see McLeod, 1991), in which the print colours of incongruently-coloured colour adjectives are named only with great difficulty. While the Stroop test can only be applied to children able to read, Cramer (1967) reported a variant using drawings of uncharacteristically-coloured objects with 5 year-olds. Subjects took longer to name the colours of these drawings than the colours of coloured squares, and latencies for naming the colours were slower than for naming the objects drawn, thus replicating the Stroop result. It should be noted that a replication by Arochova (1971) was less encouraging, and that both studies used very crude measures of latency. On the other hand, Menard-Buteau and Cavanagh (1984) obtained consistent effects of the Stroop type with adult subjects in a similar task. Following the Stroop result, then, we might have expected the sight of, say, a chocolate bar to incline our subjects to say 'brown' - which response would then compete strongly with the visually-specified response of purple, producing error or confusion.

The result draws attention to the need to seek out effective elicitation procedures in language research. Campbell and Wales (1970:250ff.) made this point strongly many years ago and similar prescriptions have been offered more recently by Crain (e.g. 1991:602). These prescriptions apply equally to psychologists - who are apt to favour studying comprehension over production - and to linguists - who are apt to favour observation of production over experimentation.

Our hypothesis is that uncharacteristically-coloured objects elicit more colour names because these colours - being contextually salient - are forced on the child's attention. On the other hand, to name the colour of a neutral or characteristically-coloured object requires an effort of attention, which effort, we surmise - following the arguments of Chapter 8 - exceeds the mental

resources of very young children. It is perhaps worth considering the stronger hypothesis that contextually-salient presentations are necessary for the <u>acquisition</u> of new colour terms by such children (other than by means of the sort of rote learning discussed above). It is obvious that so far as types of objects are concerned, such contextually-salient presentations must be vanishingly rare - brown oranges, etc., do not occur outside laboratories. However, it may be that with individual objects something rather similar happens when, for instance, a familiar pink toothbrush is suddenly and mysteriously substituted by a blue one.

Chapter 11: Two year-old Learning of an Extrinsic Adjective

1. Introduction

In the previous chapter extrinsic and intrinsic qualities were distinguished and it was suggested on the basis of work by Nelson (1976) that extrinsic adjectives should be learnable by one and two year-olds. As usual in early language learning, it is not to be expected that any deliberate process of teaching by parents is responsible for such learning. Rather, the common supposition is that simple ostension is sufficient. However, for ostension to work with any degree of efficiency, the child must have in mind the same object (proper name learning), or kind of object (common noun learning), or object quality (adjective learning) as has the ostending parent. Because of the preponderance of common-noun-like words in one year-old speech, it is generally assumed that, other things being equal, when a parent points at an object and says *'That's a <novel word>'*, success of the ostension is guaranteed, since the child will automatically take it as designating the basic-level kind to which the object belongs: in other words, other things being equal, one year-olds tend to have basic-level kinds in mind. Success in such cases, though impressive, is not perfect, as the phenomena of early overgeneralization show. Often, (Wales, Colman and Patterson, 1983; Adams and Bullock, 1986) parental naming is based on slightly lower-level kinds. Thinking of 'flower', the child is informed *'This is a rose'* and thus learns to call all flowers *'rose'*.

Although this account explains the relatively effortless one-trial learning of common nouns designating basic-level kinds, it immediately raises the question of how children might learn other sorts of word. So far as proper names are concerned, Katz, Baker and Macnamara (1974) showed that young children were able to make use of both syntactic and pragmatic cues in appropriate fashion: the ostensive frame *'This is <novel word>'* led to proper

name learning more often than the frame *'This is a <novel word>'*, and did so more readily for the kinds of object which 'merit' a personal name, such as dolls, rather than for building blocks. This result was confirmed using more careful procedures by Gelman and Taylor (1984). Subsequent research by Hall (1991) has shown that if the ostended object belongs to an unfamiliar kind, for which the child has learned no common noun, then proper name frames are less effective. In other words, provided parents first introduce nouns denoting kinds, proper name ostension will succeed with objects belonging to these kinds, so long as the kind in question is the sort of thing that merits a proper name.

So far as adjective learning is concerned, little is known. However, the ostensive frame used to introduce proper names - *'This is <novel word>'* - serves equally well to introduce adjectives, and indeed mass nouns (though nothing more will be said of these here). While it might be thought that the children in the experiments referred to above did not make adjective interpretations (since they seemed to understand the novel words as proper or common nouns), this option is more or less denied to them by the procedures adopted. Moreover, these procedures are subject to criticism on other, more general grounds. The method adopted in these experiments has the following steps. First, the child is shown two objects which differ minimally in identity, but belong to the same kind, for example two dolls of different appearance, and then the novel term is introduced by ostension applied to one of these objects. This naming phase is followed by a test phase in which the child is shown the same two objects, together with other objects of different kinds. In the test phase the child is given a variety of simple instructions <u>employing syntactic frames appropriate for the intended word category</u>, for example (proper name learning), *'Dress zav, Put zav in the basket, Show me zav'*, etc. In other words, the child is exposed not just to one ostensive encounter, but to several encounters (ranging from 7 to 10 in the

experiments mentioned), and - taken together - these encounters are syntactically very specific to the intended word category. So adjective interpretations are rather unlikely, and besides, unlikely to be recognized even if made. Moreover, there is a fundamental weakness in this procedure. In the proper name case, a child may very well wrongly understand the novel word as denoting the kind to which the object belongs (thus learning it as a common noun), but nevertheless prefer to pick the previously ostended object in the test phase - after all, why should she not? In that event, she would be misdiagnosed as having learned a proper name, so this condition is liable to overestimate the amount of proper name learning. Conversely, in the common noun case, a child must distribute her responses in the test phase across the available members of the relevant kind in order to be counted as having learned the novel word as a common noun; again, why should she? But if she does not do this, she may be misdiagnosed as having learned a proper name. Again, proper name learning is apt to be overestimated. So the procedure adopted, although adequate for demonstrating differences between ostensive conditions, does not lead to sure diagnoses of what the children have learned. In fact, from the point of view of diagnosis, one would do as well or better to consider only the response to the first instruction, and to follow this up by removing the selected object and asking *'Is there another one here that is/ is-a zav?'*. A resolute *'No!'* (in proper name conditions) would be convincing evidence. However, the fact that such a question is asked at all may lead a young child to abandon a proper name hypothesis. We may conclude that this method of investigation is demonstrative at best.

In view of the observations of the preceding paragraph, there seems to be no difficulty in supposing that (with appropriate objects) the ostensive frame *'This is <novel word>'* could lead to adjective learning. In the present experiment, we compared word-learning using this constant ostensive frame, but varying the kind of object to which it is applied. In the test phase,

we employed the restricted method outlined in the previous paragraph, using as a first 'instruction' the simple question *'Which one is <novel word>?'* , which is syntactically neutral between proper name and adjective interpretations. Familiar objects were used, so as to inhibit kind interpretations, namely toy bears and chairs, and these objects differed in an extrinsic quality (they were intact, or had a missing leg/arm), and in an intrinsic quality (colour). The extrinsic quality chosen is unbalanced in salience, like all such qualities. That is, while the leglessness of a legless bear is salient and worthy of comment, the undamaged state of an undamaged bear is unlikely to be noticed (see Chapter 8 for discussion). In a previous unpublished experiment along similar lines, Campbell, Daley and Smith found that a more drastic extrinsic state - headlessness - was unusable, since children (who had heard a novel word applied to an undamaged doll) would never apply this word to that doll once its head had been removed. Presumably, removing the head deprived the doll of the kind of status which merits a personal name!

Bearing in mind the results of previous research and the suppositions made about the salience of extrinsic and intrinsic qualities, we should expect to find proper name learning with bears but not with chairs, and extrinsic adjective learning with damaged objects but not with intact objects. The only condition in which intrinsic adjective learning seems at all likely is the intact chair condition, since this object neither merits a proper name, nor does it exhibit any salient extrinsic quality.

2. Method
Subjects : Sixty children took part in the experiment (mean age 2;3, range 20-34 months). There were 30 boys and 30 girls. All subjects attended Mother and Toddler groups in central Scotland. Subjects were randomly assigned to 8 experimental conditions (N=6) and one control condition (N=12). There were equal numbers of boys and girls within each condition, and these

assignments were carried out in each Mother and Toddler group visited, ensuring a rough equalisation of differences in catchment, etc.

Materials: Six brightly-coloured toy bears were used ('Bitsy Bears'), three pink and three blue. These creatures were left with the vestigial 'clothing' with which they were supplied, blue ribbons and sashes on the pink bears, and vice-versa. The colours of these favours were much less bright than the dominating 'fur' colour of the bears. In each subset of three bears, one was left intact, while one had an arm removed and the other had a leg removed. Six miniature plastic rocking chairs (normally occupied by 'Sylvanian Family' members) were also used. The backs, seats, arms and rockers of three of these chairs were painted in a bright pink (matching the pink bear fur) and three were painted in a bright blue (matching the blue bear fur). The thin rod-like supports of these components were painted in a darker version of the opposite colour. In each subset of three chairs, one was left intact, while one had an arm removed and the other had a rocker removed. An additional object, a silver and yellow yo-yo, was used as a distractor item. Sets of 5 objects used in testing were assembled from these objects and stored in an opaque bag when not in use. Each set contained one pink and one blue bear, one pink and one blue chair, and the distractor object

Design: Each experimental condition consisted of a training phase, in which the subject learned to apply a novel word to a bear or a chair, and a test phase, in which the subject's understanding of the novel word was investigated. Nine novel words were randomly assigned to subjects in these naming tasks: *cohosh, carvi, saffron, pygmy, tansy, hyssop, malva, comfrey* and *orris*. The choice of real disyllabic unfamiliar words varies from usual practice, which is to use nonsense monosyllables - *mef, zav*, etc. This practice may be criticised, since children who already know many monosyllables may find it odd to be presented with a novel monosyllable, and since some nonsense

monosyllables conform rather poorly to English word structure. Training objects were either bears or chairs, pink or blue, intact or damaged, establishing 8 training conditions. The nature of the damage was partially balanced, as follows: the pink damaged bear and blue damaged chair had missing arms; the blue damaged bear and pink damaged chair had a missing leg/rocker. Following previous practice, training included a contrast object to which none of the target interpretations applied. Thus in bear conditions, this was a different bear (proper name), which was in the opposite state (state adjective), and of the opposite colour (colour adjective). So, if training was carried out with a legless blue bear, the contrast object was an intact pink bear, etc. If training was conducted with an intact object, the nature of the damage exhibited by the contrast object was again only partially balanced, as follows: the pink intact bear and pink intact chair had contrast objects which were armless; the blue intact bear and blue intact chair had contrast objects which lacked a leg/rocker.

Following training, the objects were returned to the bag and this was put in the care of the subject's mother while a short filler task of about 3 minutes duration was administered. The purpose of this task was to make it plausible that the training object might have changed its state (become repaired or broken) in the interval.

In the test phase a set of 5 objects was used. These test sets were assembled in the following manner: an object consistent with proper name interpretation, but differing in state; an object of the same kind and state as the training object but differing in colour and therefore inconsistent with proper name interpretation; two objects of the opposite kind - one pink, one blue, one intact, one damaged; the distractor object. Damaged objects exhibited the same state as the training or comparison object in the training phase. To illustrate these various contingencies, the training objects, contrast objects and test sets used in two of these conditions are shown in Table 1.

Table 1: Objects used in training and testing for two conditions. Following each object description, the relation with the training object is indicated, and the target interpretation with which choice of that object is associated is also indicated. The following symbolic keys are used: for relations, C - same colour, K - same kind, S - same state; for interpretations, PN - proper name, SA - state adjective, CA - colour adjective.

Training Phase 'This is cohosh'	Testing Phase 'Which one is cohosh?'		
Training Object	Blue legless bear	CK	PN
Blue intact bear	Pink intact bear	SK	SA
Contrast Object	Blue legless chair	C	CA
Pink legless bear	Pink intact chair	S	SA
	Silver and yellow yo-yo, Distractor		
Training Object	Blue intact chair	CK	PN
Blue armless chair	Pink armless chair	SK	SA
Contrast Object	Blue intact bear	C	CA
Pink intact chair	Pink armless bear	S	SA
	Silver and yellow yo-yo, Distractor		

As well as the 8 experimental conditions, there was a control condition in which no training phase occurred. The sets used in the test phase of this condition were randomly selected from those used in experimental conditions, subject to the condition that equal numbers of 'armless' and 'legless' sets be used.

Procedure: Testing was conducted only after at least one previous visit to the Mother and Toddler group. Mothers were present but were asked not to intervene. Testing occupied about 10 minutes, including a 3-minute filler task between training and test phases. Test sessions were recorded on videotape. In the training phase, the training object was introduced with the remark

"This is <u>cohosh</u>" (or other unfamiliar word from the 9 used). The contrast object was then introduced with the remark "Look at this!". The experimenter then pointed to the training object, and asked "What did I call this one?". The subject's response was noted and the objects returned to the bag. If the subject's response was inaccurate, the experimenter repeated "This is cohosh" and asked the subject to say the unfamiliar word. This procedure was repeated four times. The bag was then handed to the subject's mother and the filler task administered. In the test phase, which followed immediately, the 5 test objects were shown to the subject and placed on the table. When the broken/repaired training object was shown, the experimenter commented, "Oh, look what's happened! This one's got a bit missing/ put back on". When all 5 objects had been shown, the child was asked "Which one of these is <u>cohosh</u>?", and then, "Is there another one that is <u>cohosh</u>?", and any responses were noted.

3. Results

Training phase: Subjects' success in producing the target novel word was measured by submitting the number of successful productions to ANOVA with 3 between-subject factors, each with 2 levels (object colour, object state and object type). There was only one significant effect: a main effect of object type, with more successes for bears than chairs ($F(1,40)=8.28$, $p< .01$). Mean success in naming bears was 2.71 and for chairs 1.38. Since subject's ages covered a fair range (20-34 months), the correlation with age was examined: it was found to be negative ($r=-.189$) and non-significant.

Test Phase: First choices Control Condition: The 12 subjects in this condition distributed their choices as follows, with two-tailed binomial probabilities indicated: Pink objects - 2, Blue objects - 8 ($p=.11$); Intact objects - 7, Damaged objects - 3 ($p=.34$); Bears - 7, Chairs - 3 ($p=.34$). Two subjects selected the distractor object. These tendencies, though not significant individually, are

confirmed by the modal choice of this group, which was the blue intact bear. This was available for selection on 7 occasions, and selected on 5 of these (p=.0047, one-tailed, assuming probability of .2 for random selection). No other unique type of object was selected more than once.

Experimental Conditions: The 48 subjects in these 8 conditions distributed their choices as shown in Figure 1. In this figure, training object colour is ignored, since inspection showed that it had no effect. Distractor choices are also ignored, since there very few of these (3 in experimental conditions, 2 in control).
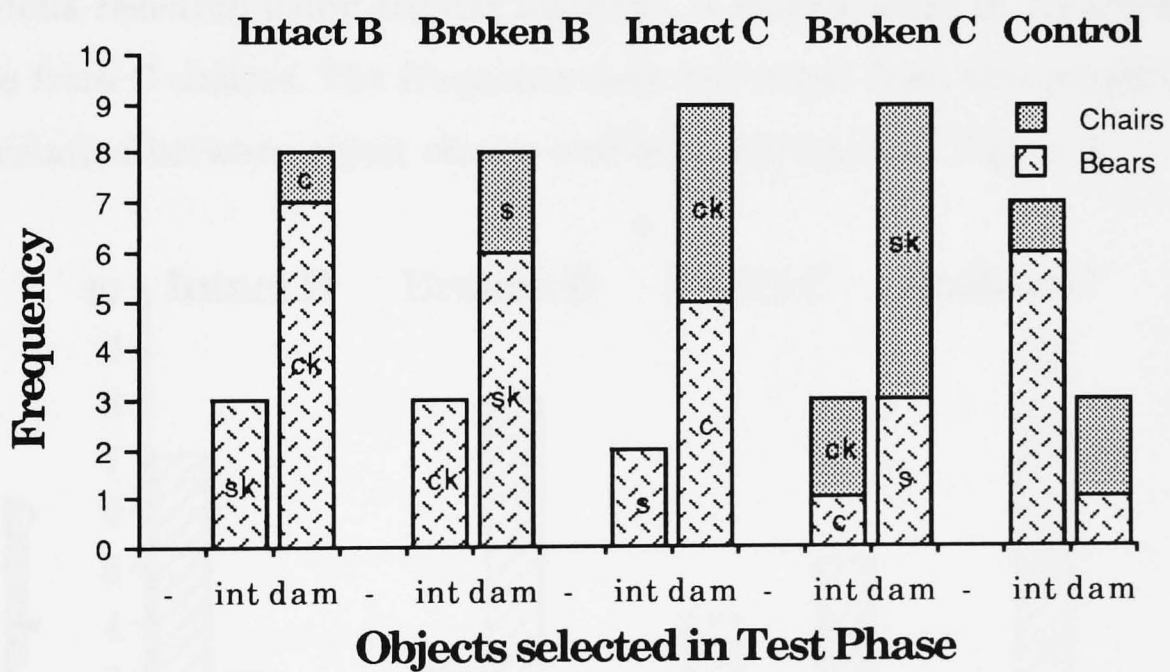
Figure 1: Frequencies of types of first choice for different training conditions. Key: 'int' - intact objects; 'dam' - damaged objects; 'ck' - objects of same colour and kind as training object (same object); 'sk' - objects of same state (intact/damaged) and kind as training object; 'c' - objects matching training object on colour only; 's' - objects matching in state only.

The most obvious feature of these data is that whereas there is a tendency to choose intact objects in the control condition, the opposite tendency is observed in every experimental condition. Comparing each condition with controls using

$\chi2$, the associated probabilites are: Intact Bears, p=.05; Damaged Bears,

p=.05; Intact Chairs, p=.02; Damaged Chairs, p=.04. A second obvious feature

is the tendency to choose bears rather than chairs in the Bear conditions,

whereas in the Chair conditions choices are more evenly distributed ($\chi2$=7.51,

p<.01). Choices marked CK on Figure 1 are consistent with proper name

interpretations of the novel word, and with colour adjective interpretations.

Choices marked C are consistent only with colour adjective interpretations.

Choices marked SK or S are consistent only with state adjective

interpretations. Since proper name interpretations have been highlighted in

previous research using similar methods, it makes sense to distinguish

these from C choices. The frequency data are recast from this perspective of

the relation between object chosen and training object in Figure 2.

Objects Selected in Test Phase Related to Training Object

Figure 2: Key: 'id' - object selected has same identity as training object; 'sta' - object selected is in same state (intact/damaged) as training object; 'col' - object selected has same colour (pink/blue) as training object.
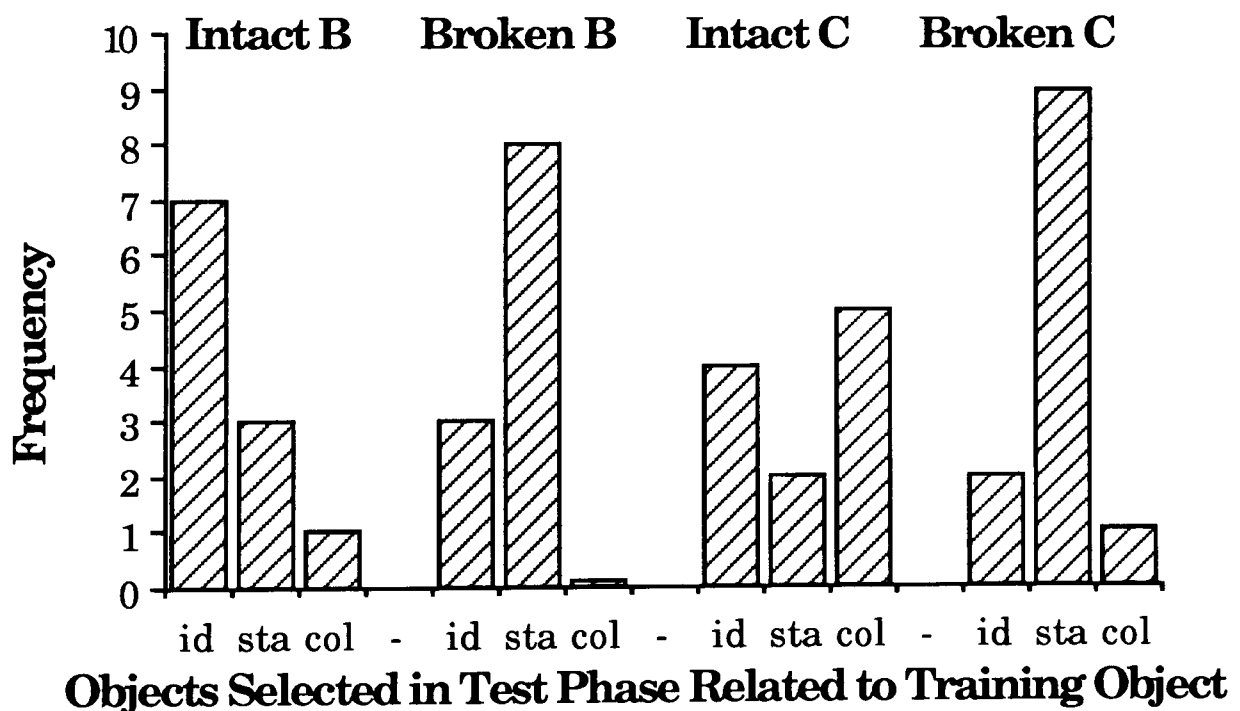
Recast in this way the distributions seem quite different. The overall (4 x 3)

$\chi 2=18.7$, with df=6 and p<.01. Comparing one condition with another, Intact

Bears is significantly different from Broken Bears (p<.05), and from Broken

Chairs (p<.05), PN choices being dominant in the former condition and SA

choices dominant in the latter two; Broken Bears is significantly different

from Intact Chairs (p<.05), SA choices being dominant in the former

condition with choices randomly distributed in the latter; Intact Chairs is

significantly different from Broken Chairs (p<.05), for the same reason.

These results should be accepted only tentatively, since expected values are

low. If the data from both Intact and both Broken conditions are combined, so

as to obtain satisfactory expected values, these combined distributions differ

significantly (p<.01). Plainly SA interpretations are more likely in the Broken

conditions, as expected. While the Broken conditions themselves differ from

chance (assuming random choice of the 5 objects available), the Intact

conditions do not. Since, by hypothesis, PN interpretations should occur only

in Bear conditions, distributions from the two Bear conditions were combined

and compared with the combined Chair conditions. These combined

distributions do not differ significantly. The distribution of choices in Bear

conditions is different from chance (p<.05), due to the low frequency of CA

choices, but the distribution in Chair conditions is not. So although PN

interpretations are more frequent in the Bear conditions, and CA

interpretations more frequent in the Chair conditions, this tendency is

unreliable.

Test Phase: First and second choices. Control condition: The two subjects who

selected the distractor object selected a bear and a chair as their second

choices. The two subjects who refused to nominate a second choice had

selected a bear and a chair for their first choice. The remaining 8 subjects

chose the other object of the same <u>kind</u> as their first choice. Because of the

way in which test sets were assembled, this was <u>always</u> an object of different colour and state from the first-selected object. That is, <u>no</u> control child made choices consistent with SA or CA interpretation of the novel word. While two children's choices were consistent with PN interpretation (those who refused), one had previously selected a chair, for which this is surely not a candidate interpretation. This result seems to show that, despite the familiarity of the objects named, <u>kind</u> interpretations are still the favoured hypothesis.

Experimental conditions: The only response consistent with PN interpretation is to refuse to nominate a second object, having selected the damaged/repaired training object as first choice. Of the 16 subjects whose first choices were consistent with PN interpretation, only one child conformed to this pattern of second-choice refusal (Intact Bear condition). In all, 4 children refused to nominate a second choice, and a further 4 chose the distractor object. Twenty-two subjects made first choices consistent with SA interpretation (17 in Broken conditions); 7 of these made S or SK second choices, all in Broken conditions. Seven subjects made first choices consistent with CA interpretation (discounting those subjects counted under PN interpretation); none of these made CK second choices. If those who made CK first choices are included, then of 23 subjects whose first choices are consistent with CA interpretation, 4 made C or CK second choices, all in Intact conditions.

In view of the result obtained with the Control group, the numbers of children whose first and second choices are consistent with <u>kind</u> interpretation (= common noun interpretation, CN) were examined. There were 31 children whose first choices were consistent with CN interpretation; 16 of these children made second choices of this sort, distributed as follows:-
Intact Bears - 7; Broken Bears - 3; Intact Chairs - 2; Broken Chairs - 4.

In view of the preceding observations, the interesting relationships between first and second choices and the training object seem to be same kind, same state and same colour. The distributions of these choice patterns across conditions is shown in Figure 3, together with residual patterns. This latter category is much larger when both choices are considered, not only because 8 subjects either refused or chose the distractor object on the second trial, but because the patterns of choice S-C and C-S cannot be related consistently to the training object. There were 9 subjects whose patterns were of this inconsistent sort, all in Chair conditions.
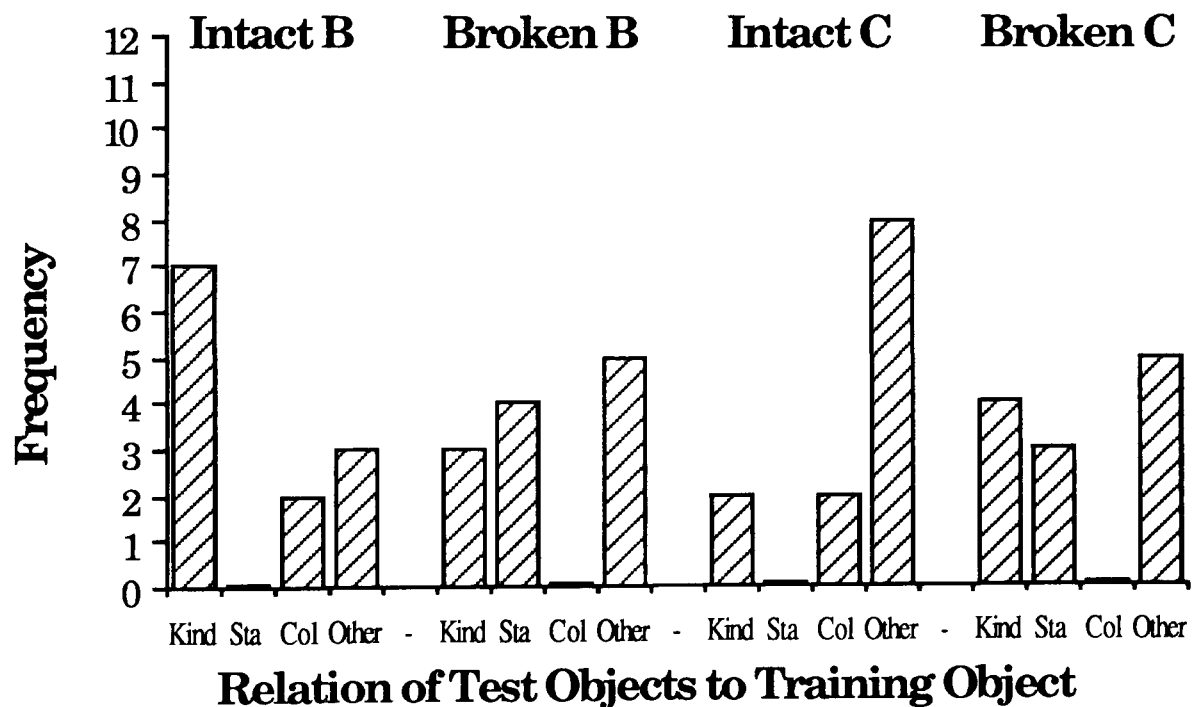
Figure 3: Frequency of types of relation between selections and the training object. Key: 'Kind' - selections both same kind as training object; 'Sta' - both same state (intact/damaged); 'Col' - both same colour.

Inspection of Figure 3 makes it apparent that the only comparison worth making is Intact conditions vs. Broken conditions. Here $\chi^2 = 11.3$ (p<.05), but it should be noted that expected values for state and colour cells (where the difference is located) are low.

4. Discussion

The results obtained in this experiment are puzzling. Despite conditions being propitious for proper name learning in the Bear conditions (bear dolls being a thoroughly familiar object for which not only the common nouns *bear* and *doll* are presumably known to the subjects, but also the specific *teddy* or *teddy bear*), there is actually very little convincing evidence of proper name interpretation. Not only is the proportion of CK choices rather low in the Bear conditions, it is unexpectedly high in the Chair conditions, where proper name interpretation seems absolutely unlikely. Moreover, only one child who selected the training bear on the first test trial replied with *no* to the question 'Is there another one which is <novel word>?'. If we were obliged to choose between proper and common noun hypotheses for these results, common noun learning seems much more likely: in the bear conditions, the 19 first choices of bears were equally distributed between CK and SK, and 10 of these children selected the other bear on the second test trial.

However, it must be borne in mind that we have introduced a difference between the training bear and that same bear in the test phase, namely removal/restoration of a limb. Perhaps individual concepts are defined so tightly for these young subjects that the bear's identity cannot survive this small alteration, or perhaps our procedures were so unconvincing that the children did not consider the modified bear to be the same bear. In connection with the first of these hypotheses, Carey and Diamond (1977) reported a 75% error rate from 6 year-olds in a task of person recognition by means of the face (often miscalled a 'face recognition' task), in which the person was presented first with and then without 'paraphernalia' - a hat, sunglasses, etc., whereas the alternative person (forced choices were made from a pair) had matching paraphernalia. In addition, as noted in our *Introduction*, the fact that we ask whether there is another object bearing the novel name may be sufficient to shake a rather tentative hypothesis loose.

So far as the other interpretations are concerned, the evidence that children will make state adjective interpretations in Broken Bear or Chair conditions is reasonably strong: 17 of the 24 subjects in these conditions selected broken objects (12 objects of the same kind as the training object, and 5 of the other kind) on the first test trial, and 7 of these 17 persisted with such choices on the second trial. So the ostensive frame 'This is <novel word>' will result in extrinsic-state adjective learning, under appropriate circumstances. As expected, there is very little evidence of any colour adjective interpretation: although the Intact Chair condition produced a fair proportion of first choices fitting this interpretation (5 of the 12 subjects), <u>none</u> of these subjects selected the other object of that colour as their second choice.

The most straightforward explanation of the results obtained therefore seems to be that subjects made common noun interpretations of the novel word, except in the Broken Bear and Chair conditions, where they showed a tendency to make extrinsic-state adjective interpretations.

While we have demonstrated what we sought to demonstrate, namely that extrinsic adjective learning from ostensive episodes is possible for two year-olds, the weaknesses of this methodology are very apparent. As we noted in the *Introduction*, it is apt to overestimate proper name learning, and our results reinforce this doubt by showing so little evidence of such learning. Hall (1991) also sheds doubt on the original results of Katz *et al* (1974) and Gelman and Taylor (1984), since his subjects ignored the proper name syntactic cue except when faced with a creature of a familiar <u>kind</u>. If there is anything in the idea that loss of a limb (or some other small alteration) destroys the identity of the object, then it may be that proper name learning will only occur for familiar <u>individuals</u>. It seems likely, given the difficulties of conducting experiments of this kind, and the evident problems of interpreting their outcomes, that methods based on <u>production</u> of the novel word must be developed if substantial progress is to made.

Chapter 12: Two year-old Matching to Visible and Hidden Targets

1. Introduction

Among the properties that objects possess, only a very small number are of psychological interest. For example, the previous sentence did not begin with the word *wheelbarrow*, but this property - on account of its extraordinarily low salience - is hardly of any psychological interest. So negative properties, except in extraordinary circumstances, are of little importance. Likewise, while it is true that the first sentence began either with the word *among* or with the word *wheelbarrow* it is almost inconceivable that this true property could matter to anyone. This argument disposes of disjunctive properties. Similar arguments might reduce the number of psychologically interesting properties to manageable types of infinity, perhaps! Among those which survive, it is customary to make two further distinctions at least. First, some properties define sets which are countable, while others do not. For instance, this is <u>one sentence</u>, composed of <u>ten words</u>. It is usual to call these countable sets <u>kinds</u>, so we may distinguish the relevant properties as properties specifying <u>kind-membership</u>. This sort of property is commonly called a <u>sortal</u> property. Many kinds are of fundamental psychological importance. Other properties do not define countable sets: e.g., every part of a red thing is red, so *red* is not a sortal; cf. no part of a bird is a bird, etc. This makes it possible to count the birds in some defined location, but not the red things. Of those properties which do not define countable sets, some - like the materials from which an object is made, or the colours of these materials, are inherent in the object in the sense that rapid change in these properties is uncommon. We shall refer to these as <u>qualities</u> or, for emphasis, as <u>intrinsic qualities</u>. Others - like the temperature of the object, whether it is clean or dirty, wet or dry, etc. are readily changed without affecting the object's identity. We shall refer to these as <u>states</u> or, for emphasis, <u>extrinsic states</u>. Qualities or states

will be of psychological importance, depending on object, organism and other circumstances, to this or that degree.

In Chapter 8 it is suggested that young children may have difficulty in holding the intrinsic qualities of objects in mind, unless these qualities are salient. It is further suggested that while some intrinsic qualities may be naturally salient, such as particularly bright colours, others will be salient only if they violate an expectation of the subject or answer to some present need of the subject. Typically, in experiments with young children neither of these enabling conditions apply. It should therefore be anticipated that, in tasks requiring some mental effort of attention or memory directed on an intrinsic quality of an object, failure should result. One result that fits with this hypothesis is the absence of adjectives denoting intrinsic qualities from one year-old speech (Nelson, 1976), at a time when nouns denoting particulars and kinds, and adjectives denoting intrinsic states, are readily acquired. Another is the difficulty that young children have in <u>free classification</u>, a task in which objects sharing various qualities are sorted into small groups bound by internal similarity (e.g. Inhelder and Piaget, 1964). With the usual materials and methods, children do not show much competence in this task until they are around 4 years old. While some have made claims for earlier competence (Sugarman, 1983), radical simplifications of both materials and method have had to be introduced to demonstrate such early competence.

Even if the evidence from early naming and free classification is accepted as it stands, it may be objected that these are rather indirect and cumbersome ways of demonstrating a difficulty in directing the mind at intrinsic qualities. Moreover, in the case of free classification it is not obvious that any real mental effort need be involved. Would the redness, roundness and smallness of a small red counter not be immediately apparent? There is, of course, an argument that in order to sort such materials according to their colour,

shape and size it is necessary to <u>abstract</u> the relevant qualities from the global mixture embodied in each object, and to <u>hold them in mind</u> while searching the set of unsorted objects (which may be large and situated well away from the sorted array) for a suitable companion: however, this argument is by no means beyond dispute, despite the promising results of Smith and Kemler (e.g. 1977). Furthermore, success in free classification is usually measured by the amount of <u>consistent</u> sorting, so that a child who puts together some red objects is then supposed to put together blue objects, etc., rather than to shift to, say, round objects for the next sorted group. So the difficulty in achieving this sort of success may be due not to the difficulty in attending to (abstracting) certain qualities, but to the difficulty of understanding that different qualities (red, blue, etc.) belong to the same quality type (colour).

Verbally-directed matching, though a less commonly used technique, is considerably simpler than the free classification task (Campbell, Donaldson and Young, 1976; Daehler, Lonardo and Butatko ,1979; Fenson, Cameron and Kennedy, 1988; Fenson, Vella and Kennedy, 1989): these matching tasks simply require the child to select an object from a set to match ('be the same as' or 'be like') a target object. There is no difficulty in arranging that target and set lie in the same field of view, nor is there any need for the subject to think about higher-order qualities of qualities such as colour, shape, etc. So such a task constitutes a very much more direct test of children's ability to attend to the qualities of an object. That is, if they cannot complete such a simple task, it hard to see what the cause of their difficulty might be other than a difficulty in directing attention to the relevant qualities of the objects. One obvious alternative source of explanation, however, is that they do not understand the words used. Short of using non-verbal methods, it is difficult to exclude this alternative completely. What we have done in the present experiment is to use identically-shaped objects belonging to familiar kind categories (balls and lamps). Since, by hypothesis, there is no difficulty in

attending to such simple quality-specified kinds, the children's success in matching according to kind should provide a good indication of the level of their understanding of the task as presented to them. To reduce the role of searching skills, the size of the object set was limited to four.

Daehler *et al.*, (1979) studied subjects aged 20 to 32 months, using a range of matching tasks. Selection was always from a set of four alternatives. In a picture-picture exact matching task proportions correct were .58 (mean age 26 months) and .85 (mean age 30 months). Object-object exact matching proved considerably easier with .87 of the sample responding correctly (mean age 23 months). A final experiment contrasted object-object exact matching with 'basic-level' object-object matching - in our terms this is kind-matching with variation in colour, size and some other unspecified qualities. At 22 months mean age, proportions correct were .70 (exact matching) and .62 (basic-level matching). None of the matching tasks explored matching based on an intrinsic quality such as colour. It is notable in these findings that even exact matching of replica to target is not entirely straightforward for children approaching three years old. Fenson *et al.*, (1988), using similar methods with two year-old subjects, discovered that the prepotent factor in determining matching success was 'perceptual' similarity - quality similarity in the terms of the present paper - rather than whether target and match belonged to basic-level or to higher-order categories. Thus (p.904), 'it was no easier to match a golfball with a football or a poodle with a collie (basic matches) than to match a sheep with a squirrel or a hammer with a saw (superordinate matches)'. The highest levels of matching performance observed in their study - .79 - were comparable to Daehler *et al.*'s results, and occurred when matching very similar-looking birds, animals, etc. There is ample evidence from this study, then, that two year-olds match objects from quality-specified kinds without much difficulty.

In addition to a straightforward matching task, and following it, subjects

in our study also carried out a matching task where the target object was hidden from view. In this task there is plainly mental effort involved in recalling the characteristics of the hidden object. This task was included in order to see whether this would affect kind-matching and intrinsic-quality matching differently and also because, so far as we have been able to determine, no study of matching to a hidden target has hitherto been published. There have been, however, studies of non-verbal delayed matching to sample (see Overman, 1990, for review). In such tasks the target is presented and a reward recovered from a food well below it; the target is then removed and, following a 10-second delay, replaced by two objects, one of which matches. The subject then chooses one or other of the two objects, and is rewarded or not. This procedure is then repeated, in the most popular version using a novel target object (the so-called trial-unique method), until some criterion of success is achieved. Perhaps surprisingly, children find this task very difficult: Overman reports that around 300 trials were needed to reach criterion with subjects almost three years old! Although, on the face of it, this task could be said to involve a hidden object, in some ways this poor result calls the method in question. Since what is involved here is not strictly speaking matching but merely recognition of an object seen 10 seconds before, it seems very likely that the difficulties are caused by failure to understand the task, rather than by any cognitive difficulty in carrying it out. This possibility is raised in the interesting conference discussion that follows Overman's paper.

2. Method

Subjects : Thirty-eight children took part in the experiment (mean age 2;3, range 1;8 to 2;10). There were 21 boys and 17 girls. All subjects attended Mother and Toddler groups in Central Scotland.

Materials : Two cylindrical covers were used to enclose the sample to be matched, one transparent and one opaque. Objects used were four one-inch diameter balls (hollow practice golf balls), two blue and two green, and four miniature table lamps, two-inches tall, two blue and two green. The balls and lamps were painted with the same blue and green paints.

Procedure : Subjects were tested in a room or area set aside for the purpose. All subjects had met the experimenter on at least one previous occasion. Mothers were present during the experiment but were asked not to intervene. Each child carried out two matching tasks of three trials each, which were video-recorded for later analysis. Subjects first matched objects to a visible target object, then to a hidden target object. In the first task one of the four types of object was randomly selected as a target for matching (green ball, blue ball, green lamp or blue lamp). The object was shown to the child ('Look at this!') and the target object was then placed under the transparent cover ('Look! I' m going to hide this under here'). Then four objects, comprising one of each type, were presented to the child. Thus one was exactly the same as the target object, one was the same colour but a different type of object, one was the same type of object but a different colour and one object differed in both colour and type. The child was then asked 'Can you show me which one of these (pointing to the four choice objects) is the same as the one hiding under here (pointing to the target object under the transparent cover)'. The child was then encouraged to place the chosen object on top of the cover. On the two subsequent trials the child was asked 'Can you show me which one of these (pointing to the remaining objects) is like this one hiding under here

(pointing to the target object under the transparent cover)'. In the second matching task, the target object was of the other type and colour. Thus if a blue ball was the target in the first task, a green lamp was used in the second, etc. The procedure followed was identical except that the target object was placed under the opaque cover. At the conclusion of this task the child was asked to say what was under the cover.

Scoring As a rough measure of success the number of attributes shared by match and target was counted for each trial. Three measures were used in analysis: the scores for trial 1 (S1, range 0-2), for trials 1 and 2 combined (S12, range 1-3), and for all three trials (S123, range 2-4). Scores for visible and hidden target trials are further distinguished as V1, H1, etc.

3. Results
Sex: Inspection of the data showed that subject sex had no effect on any measure, so results are reported without distinction of sex.

Visible vs. Hidden Targets: The cumulated scores for Trials 1-3 are shown in Table 1, together with tests of the null hypothesis that subjects are choosing objects randomly. It is evident that performance with a visible target is better than chance, while performance with a hidden target is not significantly different from chance selection. Performance with a visible target is also significantly better than with a hidden target through the 3 trials.

Age: Although age was not manipulated in this experiment, the ages of children used covered a fair range. Age correlations were therefore inspected, but none proved to be significant.

Table 1: Cumulated scores on successive matching trials where target object is visible or hidden. One-sample t-tests of µ=expected chance score (two-tailed), * - p<.05, ** - p<.01.

	Mean	StDev	t, df=37	Chance score
S1				
Visible Target (V1)	1.47	0.56	5.24**	1
Hidden Target (H1)	1.16	0.72	1.36ns	1
S12				
Visible Target (V12)	2.71	0.61	7.17**	2
Hidden Target (H12)	1.97	0.85	-0.19ns	2
S123				
Visible Target (V123)	3.53	0.60	5.38**	3
Hidden Target (H123)	2.89	0.76	-0.85ns	3
V1-H1	0.32	0.90	2.15*	0
V12-H12	0.74	1.13	4.01**	0
V123-H123	0.63	1.03	3.80**	0

Attributes: On the first trial the object selected could match the target object with respect to colour (C), type (T), both (B) or neither (N). On subsequent trials choices were restricted by previous choices, since objects chosen were removed. The distribution of choices across the 3 trials is therefore shown in the form of a tree for Visible Target (Figure 1) and Hidden Target (Figure 2). In the trees, thickness of line represents the frequency of a particular selection and these frequencies are given by the number adjoining each node. While Figure 1 is complete, Figure 2 presents only the selections in the first two trials, since the absence of structure is already obvious.
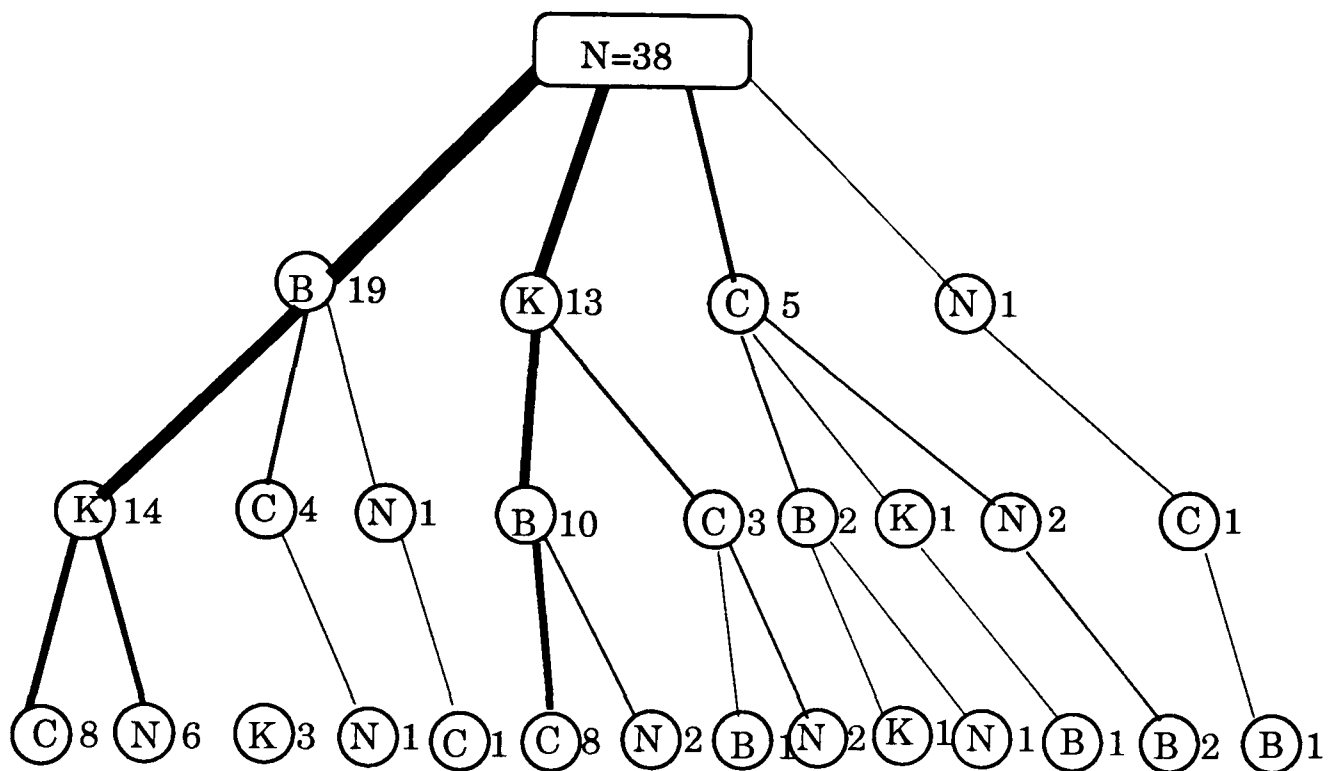
Figure 1: Selections over 3 trials in Visible Target condition. Key: B - selection matches both attributes; K - matches kind of object only; C - matches colour of object only; N - matches neither attribute.
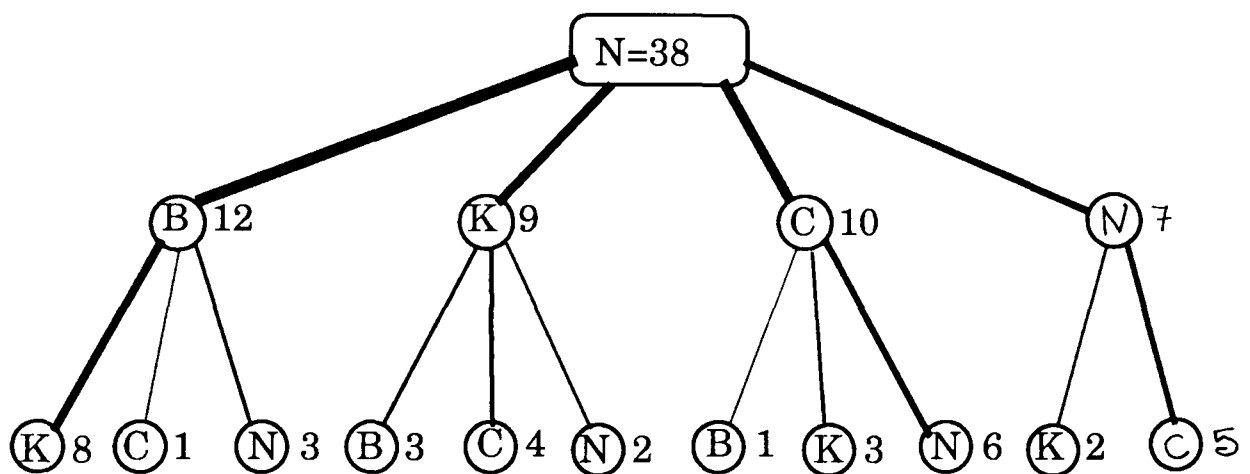
Figure 2: Selections over 2 trials in Hidden Target condition. Key: as Figure 1.

The distribution of selections below each node of Figure 1 were examined, using Chi-square or binomial tests, for significant difference from chance (even) distributions. Those which survive these tests are shown in Figure 3. None of the corresponding distributions in the Hidden Target condition survive such tests.

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                        ┌─────────┐
                        │  N=38   │
                        └─────────┘
              ╱        │        │        ╲
            ╱          │         │          ╲
        (B) 19      (K) 13    (C) 5      (N) 1
        ╱  │  ╲       │  ╲
      ╱    │    ╲     │     ╲
  (K) 14 (C) 4 (N) 1 (B) 10 (C) 3
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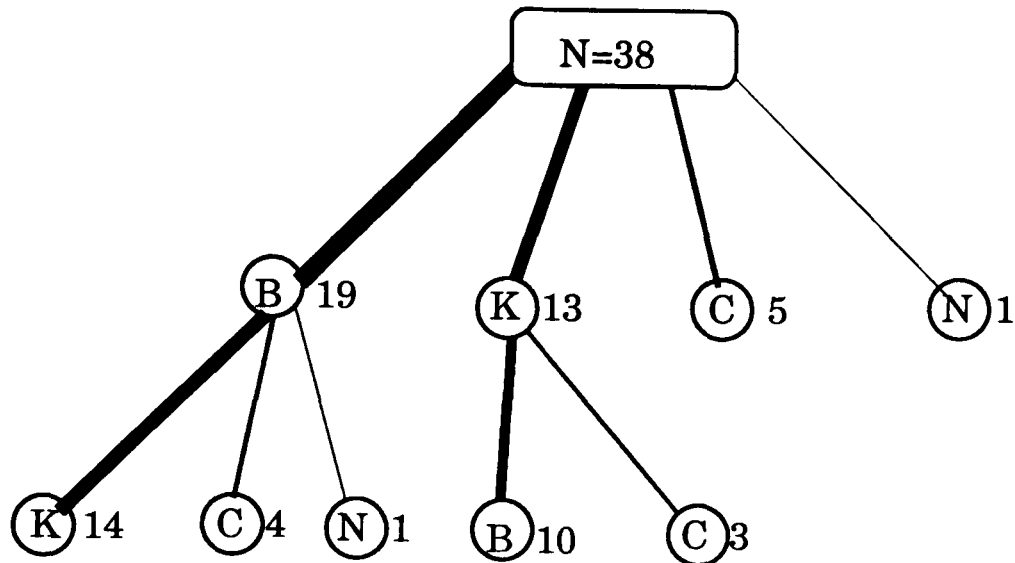
Figure 3: Selection patterns significantly different from chance on Visible
Target task. Key as Figure 1.

Verbal Judgments: Twenty-two subjects replied to the question 'What's under here?', while 16 said they didn't know. Of the 22 who responded, 17 were correct: this proportion is significantly different from the chance expectation of 11 (two-tailed binomial test, p =.017). Only one child mentioned the colour of the hidden target while 21 mentioned the type of object. No child mentioned both. The distributions of scores on H1, H12 and H123 were compared with the distribution of verbal responses using the Chi-square test for association. No evidence of association was found for any measure.

4. Discussion

The results show very plainly that when the target is visible, children in the age-range studied choose objects of the same kind as the target object without much difficulty (32/38 - .85 - on Trial 1, 27/38 - .71 - on Trial 2). These results are broadly comparable with those obtained by Daehler *et al.* (1979) and Fenson *et al.* (1988). However, there is no evidence that object colour is used as a base for matching. Subjects might just as well have been colour-blind. When the target object is hidden, there is no evidence for matching of any sort, not even exact replica - target matching. While memory for the qualities of the hidden object is undoubtedly a potential factor here, the

distribution of choices was perfectly flat even on Trial 1, which immediately followed the hiding of the object without any delay or interruption of attention. So even if memory is one cause of difficulty, it is certainly not the only one. It certainly came as a moderate surprise that children could not match on the basis of kind-membership in the hidden-target condition, since most of the indirect evidence relating to attention to kinds (naming, sorting, etc.) suggests that it should not be much more difficult to recall <u>what</u> a hidden object is than <u>where</u> it is.

Children's verbal judgments regarding the hidden object are hard to evaluate, because of the large proportion of 'don't know' responses, but perhaps the preponderance of (partially) correct judgments amongst those who did respond raises a doubt about whether subjects really understood the task. On the other hand, it may be simply that subjects could better recall the hidden object when not confronted with other objects of different type.

Although not a matching task, the task reported by Sugarman (1987) has some strong affinities with matching. Using 18 to 42 month-old subjects, Sugarman studied search patterns among the objects of a scrambled array, some of which had been provided with attractive concealed stickers. According to Sugarman, subjects younger than 42 months showed a pronounced tendency only to examine objects of the same type as those already found to have stickers. However, there are grounds for taking this as a minor over-simplification. Arguably, younger subjects show such a tendency only when the other, stickerless objects do not share kind-membership with those which do have stickers. For example, when stickers occurred only under green brushes and yellow (solid) triangles, younger subjects showed a comparable level of interest in yellow brushes but next to no interest in green triangles: in the 18-month group proportions of choices in these 4 categories were .48, .22, .25 and .03 respectively (Figure 1B, p.176). Older groups show a similar ordering of choices, although the absolute

differences decrease, reaching approximate equality around 42 months. This may be an indication that a shared intrinsic quality of shape is not sufficient on its own to establish a class of objects as a kind. In terms of the distinctions of the opening paragraph of our Introduction, a quality of shape, such as triangularity, is not necessarily sortal. While every part of a red thing is red, and no part of a bird is a bird, some parts of a solid square are square (i.e., it could be divided into squares). Sugarman's task, therefore, where the material is 'overlapping' - that is, shares intrinsic qualities of shape, colour, etc. - can be used to draw conclusions about matching abilities, e.g., in the case just discussed, that shape matching presents similar difficulties to colour matching to very young subjects.

In conclusion, we think it is evident that verbally-directed matching is a promising method for exploring young children's ability to attend to the qualities of objects. It is also evident that nothing in the present study injures the hypothesis that they find it hard to attend to intrinsic qualities. Only one intrinsic quality, colour, is investigated here and, although there was no sign that two year olds could attend to this quality, further research is needed before succumbing to generalisation. The surprising results obtained in the hidden target condition show that young children's fluent naming of basic-level kind categories may be misleading so far as their grasp of the corresponding concepts is concerned. A very strong hypothesis consistent with these results would be that two year-olds have a problem with kind permanence, just as one year-olds have a problem with object permanence. No doubt this is too strong, but it might provide a starting point for further research.

Chapter 13: Analyzing Free Classifications

1. Introduction

The free classification task has been mentioned in several places in this dissertation. As the discussion of it in the previous chapter showed, success in this task seems to require something more than the ability to focus attention on some quality of the objects sorted. For after sorting, say, all of the <u>red</u> objects available, it is necessary to switch to another quality of the same kind, say <u>blue</u>, if consistent sorting is to be achieved. So success in the task, which is taken by Vygotsky (1962) as evidence of formation of the relevant quality concepts[1], and by Inhelder and Piaget (1964) as evidence of coordination of the intension and extension of the relevant classes, may nevertheless underestimate children's abilities.

This conclusion is also drawn by Ricciuti (1965) and by Sugarman (1983), whose research showed that the 'graphic' work involved in arranging objects on a table-top may constitute an extra difficulty. If the sequence in which children touch or handle objects is examined, then evidence of use of kind concepts is apparent in one year-olds (Ricciuti) and of quality concepts in slightly older children (Sugarman). A similar conclusion was reached by Markman *et al.*,1981, who found that sorting into transparent bags improved performance.

The task has other weaknesses, apart from those just mentioned. When I have talked about it, I have frequently encountered the comment that 'it's just an Art Class', or more precisely, that no demand is placed on the children to sort in this or that way, and so it underestimates for that reason too. Lastly, in this catalogue of methodological frailties, there is the fact that the child's performance is difficult to measure. What counts as 'a consistent sort' or 'a graphic collection', etc. is often left to judgments which are easily discounted or disputed. Moreover, even when the materials invite such comparisons,

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[1] Actually, only of 'pseudo-concepts'; 'true' concepts require further demonstrations

there has been little attempt made to assess the relative importance of the available sorting criteria (usually shape, colour, and size) in determination of the child's performance.

So far as this last point is concerned, the oversight is to some extent excusable. In a sort of 20-30 objects (the usual amount) structured by three attributes, the sorter may have employed one or more of at least 7 different matching criteria in arranging the objects, for objects may match each other on all three attributes (one case), on any combination of two attributes (three cases), or on any one attribute (three cases). So assessment of the relative importance of these criteria will involve examining each sorted array seven times. Further, the research referred to above shows that children find it easier to sort objects together (or to match them, as the preceding chapter showed) when the objects share values on several attributes, and easiest of all when the objects grouped are identical. Kemler and Smith (e.g. 1979) have demonstrated across a range of tasks that young children are much more sensitive to global similarity than to particulate similarity. It follows that, when materials of the kind described in this paragraph are sorted, a child who sorts on the basis of a higher matching criterion, say all three attributes, will necessarily compose groups that match on all lower criteria in the lattice. It is therefore hard to be sure whether a certain amount of, say, colour grouping is really indicative of attention focussed on colour, or whether it is just a by-product of attention focussed on some more global matching criterion.

Faced with these various difficulties, it is hardly surprising that the free classification task has won few followers in the research community. However, it is popular with pre-school children, who sort happily and spontaneously, regardless of instruction. And it is, potentially at least, a rich 'response', capable of providing more and surer information about the concepts of young children than the yes/no answers or reaction times

typically collected. Moreover, while the latter sorts of measure may be preferred when a researcher wishes to investigate differences between conditions, or age effects, or even to draw diagnostic conclusions about a population on the basis of aggregate data, they are next to useless for purposes of individual diagnosis.

In the present chapter I present a set of computer-based methods for analysis of performance in the free classification task in the hope that these methods are superior to previous techniques, and that their use may lead to more disciplined and focussed use of this task with pre-school children. I also demonstrate their application with respect to two different data sets: one of these (Gray, 1986) is used to investigate age differences in consistency of sorting and in the use of different matching criteria in sorting; the other (Kontos, 1989) is used to investigate the causal relationship between the order of handling to-be-sorted objects and the spatial order present in the sorted array.

2. Analysis of sequential and spatial structure
2.1 Grouping structures and measures

The order in which the sorted objects are handled specifies a <u>sequence</u> of objects. Endpoints of such a sequence have just one neighbour, while other objects have two. Neighbouring objects match or not, and whether they match depends on the matching <u>criterion</u>. For example, if a blue square is followed by a red square then these objects match if similarity of shape is the matching criterion, but not if the matching criterion is similarity of colour; if the matching criterion is similarity of both shape and colour, then they do not match either. Suppose they do match. Then the two objects constitute a <u>sequential group</u> under the specified matching criterion. Other objects may be added to this group, provided that they match a member of it. Proceeding in this way, we determine a <u>grouping structure</u> for the sequence, for each possible matching criterion. If there are two bases of matching - such as

shape and colour in the case discussed above - there will be three matching criteria. In general, for N bases of matching, we have 2^N-1 matching criteria. These grouping structures may be represented as vectors $\{n_1,n_2,n_3\ldots\ldots n_k\}$ in which n_1 is the number of groups of size 1, n_2 of size 2, etc., where the largest possible group is of size k. Groups of size 1 are not groups at all, of course, but isolated objects. From these vectors two plausible summary measures of grouping structure are:-

(1) $\text{Gse} = \dfrac{\text{mean group size - 1}}{\text{maximum group size - 1}}$ where mean group size is $\dfrac{\sum iNi}{\sum Ni}$

The subtraction of 1 from numerator and denominator in (1) adjusts for the fact that the smallest possible real group is 2, so that Gse = 0 when all objects are isolates. Thus Gse is a quantity varying between 0 and 1.

(2) $\text{Pse} = \dfrac{\text{number of objects in groups of size} \geq 2}{\text{total number of objects in sequence}}$

For example, for vector $\{9,3,4,0,0\}$, $\text{Gse} = \dfrac{27/16 - 1}{9 - 1} = .085$, and $\text{Pse} = \dfrac{18}{27} = .667$. For vector $\{9,0,1,0,3\}$, Pse=.667 as before; however, mean group size has increased, so Gse=.135. Plainly, these measures respond to different aspects of structure. Gse measures the <u>consistency</u> with which the sorter has adhered to the grouping principle corresponding to the current matching criterion. Pse measures <u>completeness</u> of grouping, but is insensitive to the size of groups achieved in sorting: sorting a set of objects into matching pairs will yield a Pse of 1.0, even though much of the potential for grouping has been ignored.

Spatial arrangement of a set of objects on a table top specifies a 2-dimensional plot of the objects. Unlike the sequential case, calculation of two neighbours for an object is here to some degree arbitrary. There are three difficult cases:-

a) An object may be so isolated that it seems bizarre to count it as having any neighbours

b) An object may be equidistant from more than two nearest objects, so that it is unclear which two to count as neighbours

c) Three or more objects may lie in a line. Consider the end object: obviously the next object is a neighbour, but should the next but one object be counted as a neighbour, supposing that it is next nearest? If the sorter wished to mark a connection between the third object and the first, should s/he not have placed it below, above or on the other side of the first object?

In the analyses reported below, these cases are dealt with in the following way:-

(a) Isolated objects are counted as having only one neighbour, the nearest object. An object is considered isolated if the distance to the nearest object is more than half the distance to the furthest object.

(b) Coins are tossed to determine two neighbours from those available.

(c) Neighbours are selected from those objects accessible from the current object by an uninterrupted straight-line path. So in the case of three or more objects in a line, the end objects are never counted as neighbours.

These decisions are uncomfortable, but necessary if comparability of sequential and spatial structures is to be achieved. Since sequence elements always have two neighbours (apart from the two endpoints), so should spatial-array elements. The decision with respect to (c) is a liberal one. That is, it leads to the attribution of _more_ structure, since end-objects in linear groups must find a second neighbour elsewhere. It may therefore favour spatial structure over sequential structure. Once the determination of neighbours is complete, analysis of spatial structure proceeds exactly as for sequential structure, and Gsp and Psp may be calculated.

Of the two measures computed, only Gse and Gsp are novel. While previous research has rarely examined the structure implied by order of handling, counting the number of objects sorted (which is the basis of Psp) is commonly done, particularly with younger subjects. However, even with this simple measure, it is an innovation to evaluate structure separately for

different matching criteria.

The free sorting task typically involves moving objects from an unsorted 'heap' onto a sorting board or table. So the order in which objects are moved defines a sequential structure and the arrangement on the board defines a spatial structure. Three measures of discrepancy have been examined to compare these structures:-

    (a) dscrp - root mean square of differences between the vectors

    (b) Gsp - Gse

    (c) Psp - Pse

In general, we should expect to find positive discrepancies: much more structure can be achieved in a spatial arrangement, and this is not just because of the liberal rule adopted for counting spatial neighbours. To see this, consider the task of sorting the set of material shown in Figure 1. We might begin by setting out the squares in a line of successive red, yellow and blue colour groups, then set out the circles in a line immediately below, again in successive red, yellow and blue groups. Finally, we repeat this process with the triangles. Such an arrangement will have Gsp=1.0 for all three matching criteria. However, Gse (assuming the objects are moved in the most advantageous way) will be 1.0 for shape and for shape+colour, but only .36 for colour.

To achieve these analyses, the objects in each sort are represented as lists of qualities (ordered by dimensions thought relevant for matching), numbered according to order of handling, and listed in that order, together with up to two spatial neighbours (identified by list order) specified for each object. These data structures are screened for errors and then the various grouping vectors and summary measures are computed. Since becoming interested in this problem six years ago, I have developed a fairly comprehensive set of computer programs to carry out these tasks, and a further stage of analysis to be described below. Some details of these

programs are described below. The procedures followed in this analysis are illustrated for the example sort shown in Figure 1.
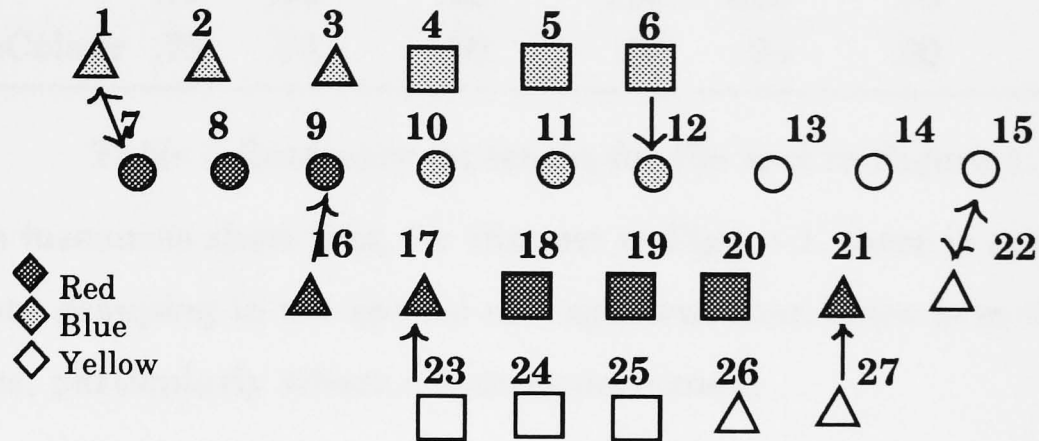
Figure 1: Twenty-seven objects varying in shape and colour. Order of handling is signified by the number above each icon. Spatial neighbours, where not obvious are indicated by linking lines. In the analysis below R is red, T is triangle, etc.

Data structure: BT,2,7; BT,1,3;....BS,5,12; RC,1,8;...YC,14,22
RT,9,17;...YT,21,15;YS,17,24;....YT,26,21

Grouping vectors.

Criterion SHAPE sequential {0,3,4,0,0,0,0,0,1}

spatial{0,1,4,1,0,0,0,0,1}

Note that the match 27-21 combines objects 26-27-21-22 into a single spatial group of size 4.

Criterion COLOUR sequential {0,0,3,0,0,3,0,0,0}

spatial {0,0,0,1,1,0,0,0,2}

Note that the matches 6-12 and 16-9 create 2 large spatial groups; since 21 and 27 do not match, the yellow objects form one spatial group of size 4 and one of size 5.

Criterion SHAPE+COLOUR sequential {2,2,7}

spatial {2,2,7}

Note that 21 and 22 are isolates (no matching neighbours). Since no match occurs across any of the links between rows, the spatial and sequential vectors are identical. Summary measures are shown in Table 1.

Criterion	Gsp	Gse	Gsp - Gse	Measure Psp	Pse	Psp - Pse	dscrp
Shape	.36	.30	.06	1.00	1.00	.02	.24
Colour	.72	.44	.28	1.00	1.00	.00	4.90
Shape&Colour	.73	.73	.00	.93	.93	.00	.00

Table 1: Summary measures for the sort in Figure 1.

These measures show that, for the sort in Figure 1, there is more consistent grouping in the spatial arrangement than there is in the handling sequence, particularly where colour is concerned.

2.1 Computer Programs

These exist at present as a menu-driven package of BBC-Basic programs running on an Acorn Archimedes. Since the programs are pure Basic and make no special operating system calls, they can be adapted without difficulty to run on any machine. The programs take a textfile of sorting data and convert it to a textfile of grouping vectors and/or summary measures, suitable for input to standard packages such as Minitab, BMDP, etc., where further analysis may be carried out. The following programs are provided:-

Data Description This program collects from the user information about the material to be sorted, and any subject or condition variables which are included in the input data file. This information is saved and used by other programs.

Raw Data Checker This program uses the information in the descriptor file to check the raw data file (text) for consistency: input errors are easily made when entering the voluminous and repetitive raw data required.

Datafile Maker This program converts the raw data file into a Basic data file.

Analyzer This program calculates grouping vectors and summary measures as described above, and displays, prints or saves this information. Information selected for saving may be saved either as a Basic data file or as

an ASCII textfile for analysis by statistical packages. The core algorithm for computing grouping vectors operates within a loop which cycles through the 2^{N-1} matching criteria. This algorithm is straightforward for the case of sequential structure. When spatial structure is computed, the algorithm has the following form:-

1. Point to start of object list
   Clear group list

2. If object list is empty
       compute grouping vectors
       exit

3. Pop current object O from object list
   Clear neighbour list
   Push O's <u>matching</u> neighbours onto neighbour list

4. If O belongs (has already been assigned) to a group G
       make G the current group
       go to 5
   Else push new group G onto group list
       assign O to G
       make G the current group

5. If neighbour list is empty
       go to 2
   Else pop current neighbour N from neighbour list

6. [O belongs to G]
   If N belongs to no group
       assign N to G
       goto 5

7. If O and N belong to G
       goto 5

8. [O and N belong to different groups G' and G"]
   If G' precedes G" on group list
       reassign all existing members of G" to G'
       remove G" from group list
       make G' the current group G
   Else reassign all existing members of G' to G"
       remove G' from group list
       make G" the current group G

9. Go to 5

When sequential structure is computed, one or other of the conditions of 5,6 and 7 is always true, so rule 8 is never needed.

Simulator The probability distributions of these measures, particularly the spatial measures, is hard to assess. In the case of sequential measures, work in the theory of runs (e.g. David and Barton, 1962) should provide solutions. Obviously the distributions of Gsp and Gse are sharply skewed towards zero, since values are sparse in the upper range. For the material of Figure 1 a perfect sort yields a G=1, but the next highest value (with, say, shape as criterion) involves 4 groups and a G=5.75/8, which is .72. Because of the resulting uncertainties regarding random expectations, a simulator was constructed. This program generates a random sequence of N objects, drops them randomly into an N x N grid, and works out spatial neighbours according to the rules given above.

The only modest difficulty here is in organizing the search for a second neighbour, which is achieved by reducing the list of potentially blocking objects: a first list consists of the first neighbour together with other close objects rejected in previous cycles because of blocking; this is then reduced to only those objects inside the circle based on the line between the current object and the current candidate neighbour as diameter. Surviving objects are tested for clearance from this line. If any fail to clear, then the candidate neighbour is pushed onto the blocker list, and the loop is re-entered with the next candidate.

Using this simulator, 1000 random sorts of the Figure 1 material were analyzed (running time about 2 hours). Some of the resulting distributions are shown in Figure 2, overleaf.
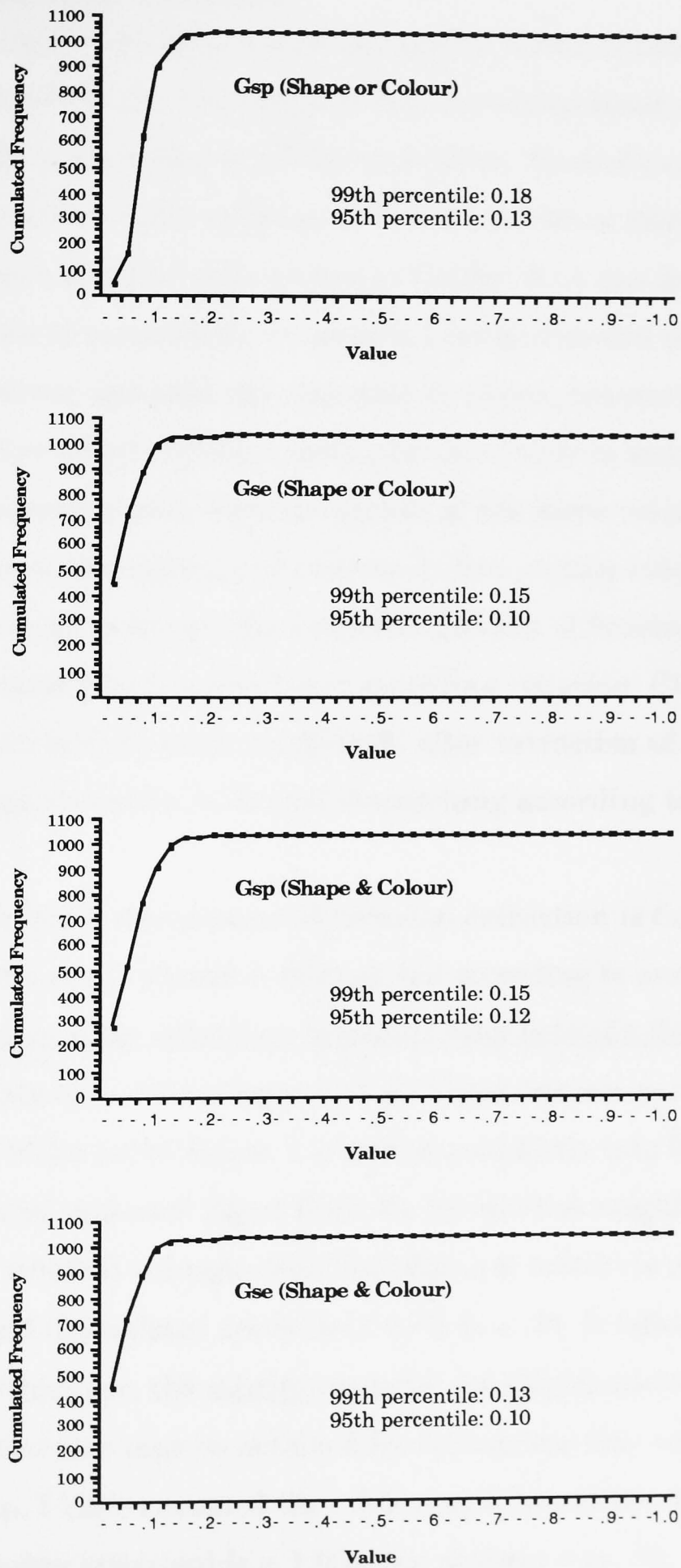
Figure 2: Cumulated frequency distributions for 1000 simulated random sorts
of the material shown in Figure 1.

2.2 Derived similarity extraction

A second stage of analysis which has proved useful is based on the observation made in the Introduction that matching based on higher criteria entails matching according to all lower criteria. Thus if two neighbouring objects match with respect to Shape & Colour, it follows that they match with respect to Shape and also with respect to Colour. So a sort might exhibit a certain amount of consistency on some low-level criterion such as colour, not because the sorter had paid any attention to colour, but simply because, in putting together objects which looked like each other in some more global way, s/he necessarily puts together objects of the same colour. As described in the Introduction, the question of interest in free sorting studies is usually whether and to what degree the subject is capable of forming groups consistently according to some lower matching criterion. Clearly this assessment can only be made confidently after extraction of any <u>derived</u> similarity engendered by more global matching according to a higher criterion.

Once understood, this process of criterion extraction is fairly simply carried out. If a set of objects is fully sorted according to some higher criterion, <u>and no other neighbour relations exist between the objects</u>, then the derived consistencies (G measures) on all lower criteria are easily calculated. For example, if the set of Figure 1 is sorted completely into 9 groups of three identical objects, and each object finds its two spatial neighbours within the group of 3 to which it belongs, then Gsp(shape & colour) is course 1.0, and Gsp(shape) and Gsp(colour) are both $(3-1)/(9-1) = .25$. It follows that in an actual sort of this type, the <u>minimum</u> value for Gsp(shape/colour) will be .25, and the residual Gsp may be obtained by subtracting this value from measured Gsp. I have assumed that this may be generalised to the case where Gsp(higher criterion) is < 1.0 in the obvious way. So, in general, where max() represents maximum group size(),

$$\text{Residual } G(\text{level } i) = G(\text{level } i) - \frac{\max(\text{level } j{>}i) - 1}{\max(\text{level } i) - 1} . G(\text{level } j)$$

In application of this method it is necessary to proceed cyclically, first calculating residuals after extracting the similarity due to matching according to the highest criterion, then reducing these residuals further by successively extracting the similarity due to the several criteria at the next lower level, etc. There are, of course, no 'upward' or 'sideways' effects, since there is always a way of rearranging a lower-order group so that higher-order or same-order groupings disappear. By the same token, 'downward' effects are confined to those criteria which are included in the higher criterion.

3. Application of these methods
3.1 Gray (1986)Data

Gray collected free sorts of 30 objects varying in colour (10 red, 10 yellow, 10 blue), shape (10 rectangles, 10 cylinders, 10 triangles) and size (15 large, 15 small). Colours, shapes and sizes were only partially balanced. There were either 5 or no objects for each combination of two values (e.g., 5 large blue objects, but no blue triangles, etc.) and either 2, 3 or no objects for each combination of three values (e.g. 2 large red triangles, 3 small red triangles, no blue triangles). Her subjects were 20 children in each of three age-groups, mean ages 3;5, 4;2 and 5;2.

These data were analyzed according to the procedures just described. The results for measures Gsp and Gse are shown in Figure 3. The most obvious feature of these results is the strong similarity between spatial and sequential measures. Correlations between these measures range from .74 to .87 (3 years), from .71 to .93 (4 years) and from .45 to .82 (5 years). So the order in which the objects are handled (at least so far as younger subjects are concerned) is strongly associated with the manner in which they are placed on the board.
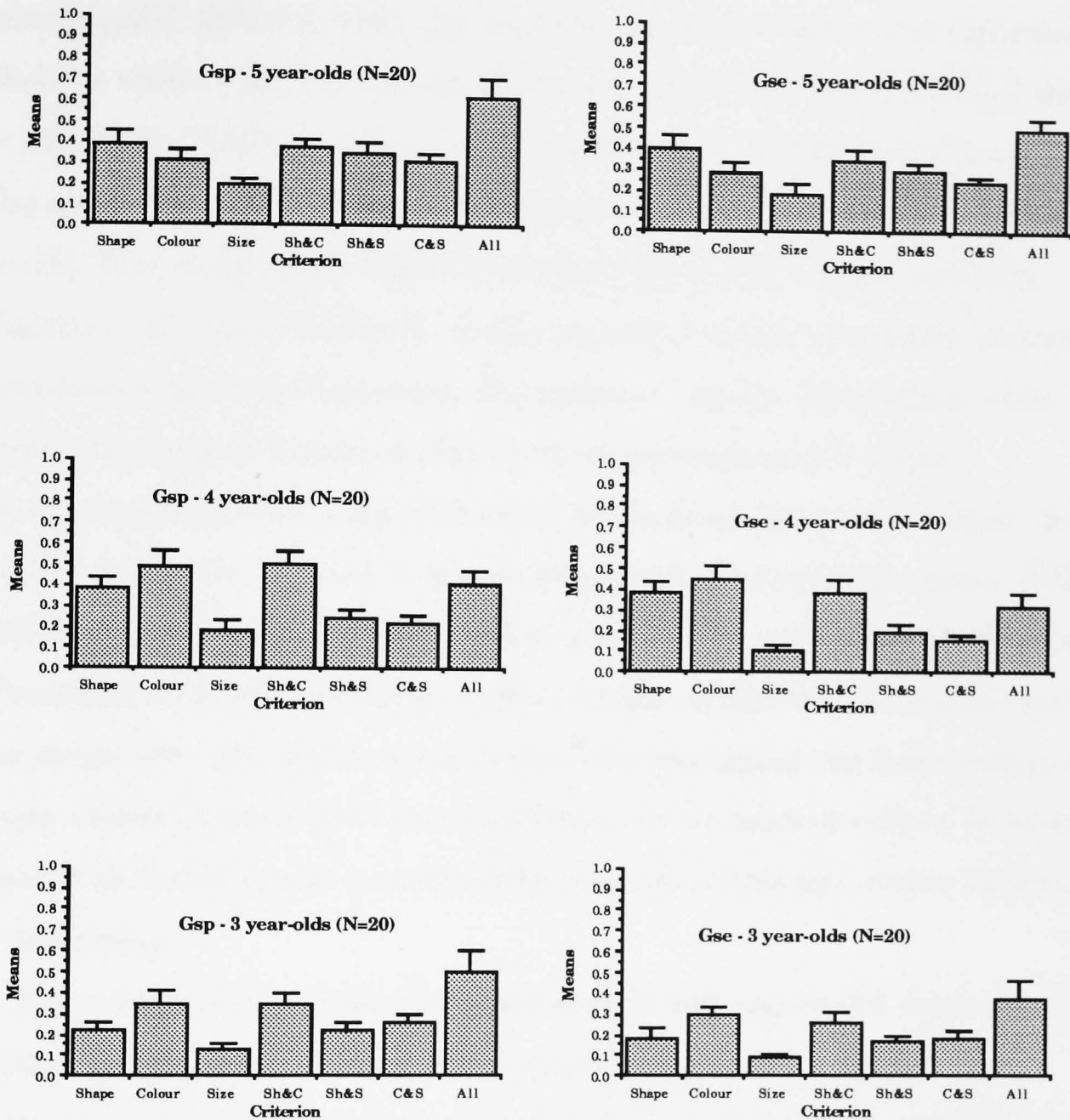
Figure 3: Mean values for G(spatial) and G(sequential) for all matching criteria (Data from Gray, 1985). Error bars are Standard Errors.

The other obvious features are the improvement in performance with age, which is hardly surprising, and the apparent cross-over from colour-based sorting - which is dominant in the 3 year-old group, to shape- based sorting. Analysis of variance confirms these observations.[2] For example, a 3 (age-

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[2] In this analysis 6 subjects who sorted fewer than 11 of the 30 objects were discarded, since the expected G scores for random sorts climb rapidly with falling numbers of objects. Five of these subjects were 3 year-olds, and one a 4 year-old. Of the remaining 54 subjects, only one sorted fewer than 20 objects (18).

groups) x 7 (criteria: shape, colour, size, etc.) x 2 (modes: spatial, sequential) mixed-model ANOVA, with Age as between-subject factor and Criterion and Mode as within- subject factors, shows main effects of Age ($F[2,51]=4.30$, $p<.05$), Mode ($F[1,51]=17.38$, $p<.001$), and Criterion ($F[6,306]=12.72$, $p<.001$). The interaction effect of Age and Criterion is also significant ($F[12,306]=2.39$, $p<.01$). This complicated interaction effect shows (as is apparent from inspection of Figure 3) that focussing attention on one particular variety of consistency may be misleading. For instance, spatial consistency with respect to Shape & Colour & Size exhibits a pronounced U-shape developmentally, with a dip at 4 years, while consistency with respect to Colour shows the opposite U-shape. Examination of residual values of G (following the procedures described in section 2.3) shows that all mean values of residual G for criteria Shape & Size, Colour & Size and Size alone are in the range .04 - .08, which is consistent with simulated random sorting. Other mean values of residual G are inconsistent with random values. It seems clear that Size is barely considered by children of this age, either in selection or in sorting.

The nature of the relation between spatial and sequential measures was further investigated by examining overall correlations with age and comparing partial correlations with Gsp(Psp) and Gse(Pse) partialled out. The resulting correlations are shown in Table 2 below. Evidently these correlations suggest strongly that the children impose more structure on their sorts at the stage of selection of objects from the unsorted heap than at the stage of spatial arrangement.

Criterion

	Size	Colour	Shape	S&C	S&Sh	C&Sh	All
$r_{Age \times Gsp}$	.21	.10	.45*	.33*	.41*	.29*	.38*
$r_{Age \times Gse}$	.32*	.13	.40*	.45*	.50*	.37*	.51*
$r_{Age \times Psp}$	.45*	.35*	.45*	.39*	.47*	.41*	.42*
$r_{Age \times Pse}$	.49*	.41*	.52*	.51*	.60*	.51*	.55*
$r_{Age \times Gsp.Gse}$	.00	.01	.14	-.07	-.05	.02	-.11
$r_{Age \times Gse.Gsp}$	.24	.08	.16	.33*	.32*	.24	.38*
$r_{Age \times Psp.Pse}$	.01	-.14	-.10	-.20	-.16	-.17	-.21
$r_{Age \times Pse.Psp}$	.21	.27*	.31*	.41*	.44*	.38*	.44*

Table 2: Correlations and Partial Correlations between Age and measures of Spatial (sp) and Sequential (se) Consistency- G and Completeness- P (N=60, age-range 3;1 to 5;3).

3.2 Kontos (1989) Data

Kontos collected free sorts of 27 objects varying in colour (9 red, 9 yellow, 9 blue) and shape (9 squares, 9 circles, 9 triangles). Colour and shape were completely balanced, so there were 3 identical objects for each combination of values. Kontos' material is shown in Figure 1.Her subjects were 14 children at each of two age groups, mean ages 3;9 and 4;7. Subjects were assigned to two sorting conditions. In Condition 1 (Closed), the unsorted objects were contained in an opaque bag, while in Condition 2 (Open) the unsorted objects were placed in a disorderly heap beside the sorting board, as usual. The point of this manipulation was to see whether forcing random selection of the objects would produce deterioration of sorting performance. Of course, the Gray results suggest strongly that this should be the case. Results for Gsp and Gse in the 8 combinations of Age, Matching Criterion and Condition are shown in Table 3 below.

Criterion

	Shape				Colour				Shape & Colour	
	Gsp	R	Gse	R	Gsp	R	Gse	R	Gsp	Gse
3 year-olds (Closed)	.12 (.09)	{.09}	.08 (.03)	{.06}	.10 (.05)	{.07}	.06 (.02)	{.04}	.12 (.10)	.07 (.04)
(Open)	.39 (.35)	{.32}	.23 (.24)	{.17}	.26 (.35)	{.19}	.17 (.18)	{.11}	.28 (.23)	.22 (.18)
4 year-olds (Closed)	.17 (.25)	{.15}	.11 (.11)	{.09}	.05 (.03)	{.04}	.05 (.03)	{.04}	.06 (.06)	.07 (.08)
(Open)	.54 (.37)	{.40}	.50 (.40)	{.37}	.33 (.32)	{.19}	.29 (.33)	{.17}	.56 (.37)	.47 (.37)

Table 3: Means and Standard Deviations (in brackets) for Spatial (Gsp) and Sequential (Gse) Measures of Sorting Consistency. Residual Means after Derived Similarity Extraction are shown in curly brackets.

Evidently the manipulation was effective. Values for Gse in the Closed Condition lie comfortably in the ranges determined by simulation (see Figure 2). However, so too do most values for Gsp, showing that successful sorting at these ages depends critically on the imposition of structure at the selection stage. These observations were explored by a 3 x 2 x 2 x 2 mixed-model ANOVA, with Criterion (shape, colour or shape & colour) and Mode (spatial or sequential) as within-subject factors and Age and Condition as between-subject factors. The only significant effects were main effects of Condition ($F[1,24]=24.0$, $p<.001$) and Mode ($F[1,24]=8.54$, $p<.01$). As Table 3 shows, values of Gsp and Gse were consistently higher for Shape than for Colour except, perhaps, in the 3 year-old, Closed Condition data. However, neither the Criterion effect nor the interaction effect implied by this exception was significant. Since numbers of subjects were small, and standard deviations large, in this experiment, it might well be worth investigating further with larger numbers, since it is possible that subjects could use felt shape in the Closed Condition to determine order of selection, and likely that older

children would exploit this possibility more effectively.

3.3 Sugarman's Material (1983)

Some of Sugarman's results suggested precocious sorting ability in late two year-old subjects. These results were obtained using a set of 8 objects varying in 2 colours and 2 shapes, with 2 examples of each object type. The distributions of G obtained by simulation of 1000 random sorts show that perfect sorts are not uncommon with this small set. For example, Gsp(Shape or Colour) was 1.00 for .07 of the sorts, while Gse was 1.00 for .04. Accordingly, Sugarman's findings merit examination with larger sets of objects.

3.4 Concluding Remarks

The techniques of analysis presented here seem well suited to effective exploration of free sorting data. It is clear also from the two datasets investigated above that, if the investigator's interest is confined to using the task as evidence for grasp of concepts, it is unnecessary to record and painstakingly transcribe the spatial arrangements produced, since these simply reflect the order in which objects are taken from the unsorted heap and since that order is much more easily observed and recorded. In fact, it could be recorded in real time. It follows that the various sources of underestimation mentioned in the Introduction, which mostly arise from the activity of sorting itself, do not constitute a sufficient reason for avoiding the task as a measure of conceptual organization.

Chapter 14: Categorization, Early Concepts and Language Acquisition[1]

To characterize the structure of language adequately, linguists require a considerable array of concepts, many of them quite abstract, corresponding to classes of the <u>clause</u>, the <u>phrase</u>, the <u>word</u>, etc. To characterize the content of linguistic expressions a further array of concepts is required, corresponding to the <u>types</u> of <u>objects</u> and <u>properties</u> denoted, of <u>propositions</u> expressed, of <u>modes of expression</u> and so forth. Thus, in order to give an adequate account of how any utterance functions, it is necessary to deploy this army of concepts. Yet young children, 4 or 5 years of age, use and understand the simpler structures of their native language fluently, without benefit of any special instruction and often despite quite unhelpful-looking regimes of child-rearing. In order to do so, it seems as if children must employ mental structures homologous to the linguists' concepts. But it is known from other work that children's ability to construct concepts of arbitrary categories is initially very weak and develops slowly.

1. Approaches to the Problem

This paradoxical observation has one well-known resolution, namely that the necessary concepts do not have to be constructed by children; instead, they are innately specified. In addition it is often proposed that the mental apparatus needed to speak and understand language is encapsulated and isolated from other cognitive resources, that it constitutes a <u>mental module</u> (Chomsky, 1984; Fodor, 1981). This module has several parameters, initially set to default values. In the course of development exposure to the language around them 'triggers' the values of these parameters to appropriate settings, perhaps according to some maturational schedule also regulated by inherited material (see Chapter 6 for elaboration).

Another route towards a resolution involves a number of linked ideas: that

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[1] To be published as Campbell, in press.

to characterize children's early language adequately a much reduced and simpler set of concepts is required; that children are more adept at constructing concepts than hitherto supposed; that to employ a concept explicitly, as a linguist does, is a very different thing from employing it tacitly, as a speaker does; that the contribution of genetic material to the process of acquisition is much more general, thus not encapsulated, or perhaps confined to some specific aspect of language, for instance to production and reception of speech. The general theory of acquisition attempting this sort of resolution is known as <u>semantic bootstrapping</u>. It was first clearly outlined by Macnamara (1972) and has been recently explored by Pinker (1984).

So knowledge of children's categorization abilities, of the sorts of concept they are able to construct and of the innate resources that these abilities imply, is needed in order to make an adequate assessment of the plausibility of the program just outlined. Also, whatever theory of first language acquisition is proposed, such knowledge is needed in order to set upper limits to the possible content of children's utterances. The thought expressed ('what is said') may only partially reflect the thought that prompted expression ('what was meant'), but it is surely absurd to propose that the former exceeds the latter in complexity of <u>content</u>. These relationships are very programmatic and it cannot be claimed that much progress has been made with them. Thus far the most profitable relationship between children's categorization and first language has been the converse one; namely, that study of early language can reveal facts about early categorization (Vygotsky, 1962), although here - by the same reasoning - conclusions about the content of early language can only set lower limits to categorization abilities.

It is evident that there is no clear consensus amongst scholars about either cognitive or linguistic development during this period of childhood: in both fields a range of well-supported views is encountered spanning the two

Palmirini (1980) records a famous and instructive dispute). As noted above, there are even those who deny that the two fields of development are in any way connected.

2. Categorization

From a psychological perspective, categorization is involved whenever an individual treats distinct phenomena as if they were the same recurrent phenomenon. This arises in at least three different ways:

(a) because the individual is biologically disposed to treat the phenomena in this way - these may be called <u>constitutional categories</u>; or

(b) because the phenomena form a natural cluster, isolated from other such clusters - <u>environmental categories</u>; or

(c) because, arising from some purpose of the individual, it makes sense to treat the phenomena in this way - <u>constructed categories</u>.

Examples of constitutional categories might be certain regions of the color solid (Kay and McDaniel, 1978) or certain classes of auditory event (Kuhl and Miller, 1975); a case has been made that some natural kinds such as lions and zebras are environmental categories (Mervis and Rosch, 1981), although not all natural kinds are (Dupre, 1981). It may be that artefactual kinds like <u>fork</u> or <u>spoon</u> provide purer cases of isolated clusters then do natural kinds. The best examples of categories that are clearly constructed are perhaps those categories of number, quantity, relation, etc. whose development was investigated by Piaget and Szeminska (1952). The term 'kind' is used as short-hand for what are sometimes called <u>sortal categories</u>. Sortal categories are categories whose members are readily individuated and, say, counted. Thus, 'dog' denotes a sortal category. 'How many dogs are here?' deserves an answer, and gets one. But 'How many red things are here?' does not. For example, if there is a red handkerchief, should we count each thread, or molecule, etc.? The categories defined by qualities such as colors, shapes, etc. are thus clearly not kinds.

Mervis and Rosch's influential review presented evidence that a particular level within natural kind hierarchies was psychologically privileged. Categories at this level, called basic level categories, are environmental categories inasmuch as within-category similarity is maximal relative to between-category similarity at this level. They argued that this level is the point of entry to the hierarchy for children, pursuing an older insight of Brown (1958), and the level of category most easily manipulated by adults in a range of experimental tasks. For biological hierarchies, this level falls roughly at the level of the genus (e.g. *tiger*, as opposed to *Felid*, or *Siberian tiger*).

Whereas constitutional categorization is presumably automatic, and environmental categorization may come about as the outcome of simple perceptual processes that detect some invariant property that distinguishes the isolated clusters with reasonable reliability or by other simple methods, constructive categorization is presumed to involve mental effort, at least in early stages of the categorization process. The operation of these processes by no means always takes us from the particular to the general, nor does it follow the same pathways in different communities. To illustrate, initial stop consonants may be allocated to numerically distinguished constructed categories of voice onset time (with the aid of suitable instruments). These categories are of course finer-grained than the categories detected by unaided listeners, and these latter environmental categories are different in different speech communities, say Spanish and English. Moreover, although it is unclear whether children form relevant constitutional categories, their discrimination of stops varying in voice onset time is certainly sharpest around environmental category boundaries (Burnham, Earnshaw and Clark, 1991). So we may say that the environmental categories formed have an established constitutional basis, at least.

3. Concepts

It is essential to distinguish between the mental structure which represents a category and the category itself. Within psychological discussions these mental structures corresponding to and representing categories have been called concepts. (This usage is different from that found in most philosophical discussions, following Fregean practice, in which concepts are taken to be abstract entities specifying the intension of a category.) Two important varieties of concept are, or ought to be, distinguished: individual concepts and type concepts. Concepts may represent categories in different psychological functions. Perhaps the simplest such function is recognition. Preliminary definitions would then be:

> Definition (1): An individual concept is a mental structure that enables recognition of the same individual, encountered at different times and places;

> Definition (2): A type-concept is a mental structure enabling recognition of different individuals as being of the same type (i.e. belonging to the same category).

Now most psychologists outside the behaviorist and some present-day cognitive-science traditions have insisted on rather more by way of definition than what is offered above. The definitions given above are minimal in several senses. For instance, recognition can be based on a very partial specification of the recognized individual or type, provided individuals and types are well-separated in the world in question. To take an example from Dennett (1987, p. 290), in a particular country, a coin-operated device may distinguish the desired type of coin from others on the basis of a partial specification of weight and shape, ignoring, say, embossed or engraved marks and inscriptions. As an instantiated type-concept such a device is very defective, though it may work well enough, since the objects inserted form well-separated clusters. If the device were improved in conceivable ways,

then the danger would arise of its rejecting perfectly good coins because of surface imperfections, etc., so it may be seen that attainment of a fully effective instantiated type-concept is a difficult goal. In fact, it is an impossible requirement. A fully effective type-concept for a given coin specifies a history for the coin - that it was minted in a particular place by certain machines. Exactly the same conclusion follows for individual-concepts: ideal individual-concepts will distinguish 'indiscernible' individuals (pennies, twins) and will not be diverted by 'disguise' changes. Ideal individual-concepts will therefore require the specification of a history as well. Though such ideal concepts perhaps cannot be attained, it is a common enough notion that concepts should not be ascribed to creatures or devices, unless they can pick out something like the correct category in most circumstances. This requirement of additional functionality may be characterized as a demand that concepts should be <u>computationally effective</u>.

A second notion of desired functionality, additional to that specified in definitions (1) and (2), is that concepts should be <u>representationally effective</u>: they should allow their possessors to <u>hold the target individual/type in mind</u> [2] when it is absent or competing for attention with other categories. This notion of concept coincides more or less with Piaget's.

A third suggestion for additional functionality is that concepts should be susceptible to combination, so that novel properties, relations and relational properties may be constructed from familiar concepts. There is no doubt that this sort of additional functionality is highly desirable: our creative and imaginative capacities depend on our possessing such <u>productively effective</u> concepts.

These three different characterizations of the additional functionality required need not lead to three different theories of concepts. Instead, it may be reasonable to attempt a theory of concepts that satisfies all three

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[2] This is, of course, the proposal discussed extensively in Chapter 8.

requirements simultaneously. After all, the requirements answer to capacities that work together developmentally. We become creatures that

(a) are not easily fooled (our concepts become computationally effective),

(b) can think about remote objects, etc. (our concepts become representationally effective), and

(c) show some capacity for representational novelty (our concepts become productively effective)

In the present chapter, only the nature and origins of representationally effective concepts are discussed. However, as noted, it is hoped that attainment of such concepts is at least associated with the other two sorts of additional functionality.

Our definitions (1) and (2) above clearly do not define concepts in any of the senses just described, but they are useful notions nonetheless. The structures defined there will be referred to instead as <u>individual</u> and <u>type</u>-<u>detectors</u>.

3.1 Formation of Early Concepts

Traditionally, following Vygotsky (1962) and Inhelder and Piaget (1964), it has been assumed that the free sorting task, in which children form a large collection of diverse objects into groups that share a similar property, depends upon the ability to hold the shared property in mind across the several sorting operations and despite constant change in the other properties. Typically, these tasks involve objects characterized by variation in size, shape, colour or similar simple properties, usually denoted by adjectives - qualities, in a word. So, on the basis of the well-known studies mentioned above (cf. also Kemler, 1983) we may identify the following research finding:

<u>Finding</u> (1): Children in the age-range 3-6 years can hold a quality in mind so as to organise free sorting performance, but cannot readily switch to a different principle of organisation, nor coordinate two such principles in multiplicative fashion.

Such children then, according to the criterion of representational effectiveness, possess concepts of these qualities, although there are still

some limitations to the flexibility with which they are employed. Younger children, although unable to form concepts of these properties, can execute simpler versions of the free sorting task. Ricciuti (1965) showed that if the set of objects to be sorted consists of subsets of objects belonging to different simple kinds, such as dolls and boats, and if one requires only that the subjects should touch or handle these subsets successively (rather than form them into spatial groups), then even 1-year-olds show some ability and 2-year-olds can carry out the task, thus redefined. These findings have been confirmed by Sugarman (1983), who also showed that 2-year-olds will treat locally well-separated categories (for example, a set of green cylinders and a set of red circles) as if they were kinds. So, taking Ricciuti's and Sugarman's results together we arrive at our second research finding:

> Finding (2): Children in the age-range 1-3 years can hold some sortal categories in mind so as to organise free sorting performance.

Such children can therefore form concepts of some kinds, but cannot form concepts of qualities. These sortals cover roughly the same ontological ground as Mervis and Rosch's notion of basic level category. Also, the timing of this achievement, beginning around 18 months, coincides with Stage 6 of object permanence (Piaget, 1954). If this latter achievement is taken as marking the first construction of representationally effective individual concepts, then it would seem that the formation of concepts of simple individuals and concepts of basic level categories are developmentally simultaneous. This is perhaps not surprising, in view of the next finding, after Mervis and Rosch, that individuals play an important role in the formation of such concepts:

> Finding (3): basic level concepts are resemblance structures. For any such concept and the population that employs it, some objects (stereotypes or prototypes) are better examples of the target category than others. Judged membership of such categories depends on similarity to the prototypes rather than on some (set of) common attribute(s).

Readers with philosophical backgrounds will be reminded of similar discussions in traditional metaphysics, notably in connection with the problem of universals (see Armstrong, 1980). In that context (sometimes called <u>first philosophy</u>) the problem is to characterize the notions of object and property (by means of the metaphysical notions of particular and universal - or not, as the case may be) so as to give a satisfactory account of what things there are. The psychological context is different: it is to characterize a variety of mental representations - concepts - so as to give a satisfactory account of how we come to know whatever things there are. However, as noted, there are many affinities between the two sorts of investigation. In metaphysical discussions, whether realist or nominalist in tendency, such resemblance structures have often been proposed as characterizations of properties. In <u>realist</u> analyses, beginning with Plato, there is a single external target against which resemblance is measured, a pure or Ideal Form: in <u>nominalist</u> analyses, e.g. the well-known discussion of games by Wittgenstein (1951), there is an endless chain of global resemblance. However, as has often been pointed out, we are left minimally with the universal properties (relations) of resemblance and with the task of characterizing these. Similarly, in the account of concepts given in finding (3), we are led to wonder on what fundamental capacities the judgment of similarity to prototype depends. Substantial help is provided here by study of the ranges of application of children's first names for <u>basic level</u> categories. Studies by Clark (1973) and especially Bowerman (1978) make it quite evident that judged similarity is by no means global, but rather sharply structured by attributes, features and qualities of shape, colour, texture, etc. Although the theory of the development of word meaning proposed by Clark has now been discarded, it is sometimes forgotten that the data persist, and that these data show clearly that children's apprehension of similarity is strongly structured by these qualities. So, we have the finding:

<u>Finding</u> (4): Children younger than 2 years form concepts with pronounced resemblance structure. However, similarity to prototype clearly depends on formation of concepts of attributes, features and qualities.

4. Paradoxes of Early Concept Formation

The findings (1) to (4) just described generate a formidable developmental paradox:

<u>Paradox</u> (1): Whereas studies of object sorting and handling suggest that children younger than 3 cannot yet form concepts of qualities, the studies of early word use suggest that they must have done so.

The distinction between minimal type-detectors (definition (2) above) and representationally effective type-concepts may be effective in resolving this puzzle. Or, conversely, this puzzle makes it evident that the distinction is necessary. Whereas findings (1) and (2) pertain to representationally effective quality concepts, the bases of similarity in findings (3) and (4) are not quality concepts but quality detectors.

A closely similar puzzle arises when we consider how individuals are recognised. To be sure, an individual concept must be essentially historical. Stage 6 of object permanence is only attained when the infant can construct a history for an individual as it is moved from place to place invisibly. Likewise Piaget's story (1951, p. 225) of his daughter Jacqueline's mistaken recognition of the slug encountered when leaving the house and some hundreds of yards further off suggests a failure to construct such a history. However, it must be presumed that recognition of individuals is often merely heuristic and depends not upon construction of a space-time trajectory but on the detected recurrence of a particular cluster of attributes and qualities. At any rate, under this presumption individual detectors consist of just such cluster-specifications. But this leads to:

Paradox (2): The formation of a type-detector depends on apprehension of the world as consisting of distinct individuals, rather than of a single recurring individual. This in turn depends upon the formation of individual-detectors which define unit categories. But such detectors can only be aggregates of perceived attributes, features and qualities, in other words of type-detectors, completing a vicious circle.

Paradox (2) is surely sensibly resolved by supposing that certain type-detectors of attributes, features and qualities are genetically transmitted. Even the parsimonious philosopher Quine (e.g. 1975) allows some such innate quality space as a cognitive given. These innate type-detectors will then bootstrap the process of acquisition of individual-detectors and then of novel type-detectors, breaking the vicious circle.

5. First Language and First Concepts

According to the previous section, eighteen month old children can form concepts of individuals and of certain environmental categories such as basic level categories. But they cannot yet form concepts of qualities such as shapes or colors. These early concepts are underpinned by individual and type detectors, some of which detect constitutional categories and are therefore innately specified.

Studies of early vocabularly broadly confirm these conclusions. Eighteen month old vocabulary contains many proper names and pronominal expressions denoting individuals, and nominal expressions denoting basic level categories. Moreover, Katz *et al.* (1974) showed that very young children, presented with the contrasting ostensions 'This is X' and 'This is an X' are apt to take X to denote an individual in the former case and a basic level category in the latter.[3]

Expressions denoting qualities are slow to appear in early language, with color adjectives, for example, not well established until the fourth year. Early adjectives appear to denote instead temporary, undesirable properties such as

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[3] Perhaps only if the candidate individual is well-known to the child (see Chapter 11).

hot, wet, dirty, and broken (Nelson, 1976). These extrinsic properties are psychologically salient and command attention, whereas intrinsic properties of shape, color, etc., are always in competition for attention. It may be that the psychological prominence of these extrinsic properties makes it easier for children to form concepts of them.

The prospects for establishing alignments between the developing conceptual apparatus of children and the structures of early language are therefore reasonably promising and such alignments should assist the development of theory in both domains of development. Besides the obvious need for examination of other sorts of early concept than those so far explored, notably of concepts of actions, study of the issues considered here is badly hampered by the lack of suitable metalanguage for describing the content of expressions (meanings) and the content of the thoughts that prompt such expressions. In addition these two perfectly distinct notions are frequently confused or conflated.

Afterword

One of the most awkward questions about the development of representational systems is how the various systems are related and whether there are any identifiable causal connections between them. So far as the latter question is concerned I have assumed, without much argument,[1] that mental representation is causally prior to the other systems. But it may be that if we look beyond the period of the first few years of life considered here there are other causal relations to be considered, and of course, the language that we learn will undoubtedly influence <u>what</u> we think about, and from the earliest years (Choi and Bowerman, in press).

I have presented evidence in Chapter 7 that representational drawing does not appear until late in the 4th year. Why should there be this evident *décalage* between mental representation and language on the one hand (which seem to run fairly closely together) and drawing on the other? There are several hypotheses worth considering. It does not seem likely that it can explained by any perceptual or motor difficulty connected with drawing: as I observed in the Discussion of Chapter 7, children are also slow to use and understand maps and models, and Zaitchik (1990) has offered evidence that they have difficulties with the representational function of photographs, too. As a second try, perhaps the representational content of early language has been over-estimated. I argued in Chapter 2 that the content of one year-old speech is often over-estimated, and it may be that close examination of the content of two year-old talk might lead to similar doubts. Oddly enough, this is a task for the future: most diary studies peter out around age 2;0, when the quantity of speech becomes unrecordably large, and experimental work with 2 - 3 year-old language is not common. A third suggestion is purely theoretical: mental representation is plainly an <u>internal</u> system, and representational drawings, etc., constitute an <u>external</u> system; but

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[1] There is a brief discussion of the point in Chapter 8, Section 4.

representation through language lies somewhere between. Spoken language, though external in the sense of being observable, is very much a system 'of the body', unlike drawing for which a drawing tool and a medium is required. Perhaps we have a developmental cycle here rather like the sensori-motor cycle of primary and secondary circular reactions. Finally, it may be that there are differences in typical <u>content</u> that distinguish linguistic representations from graphic representations. Drawings, maps, photographs and models function in the main to assist us in extending our <u>experience</u> beyond the here and now. On the other hand, we represent (refer to) remote objects and events through language in order to extend our domain of <u>action</u> beyond the here and the now, through the recruitment of others; conversely, others may use the same system to give us new <u>motives</u> and <u>purposes</u>. In sum, there are several lines of investigation of this question which deserve further pursuit.

Another very awkward question about representation is how exactly to characterize it. My answer, developed in the second half of this dissertation, is that representation is co-extensive with thinking. If a child cannot mentally represent some entity, then she cannot think about it; if she cannot think about it, she cannot mentally represent it. To be of use, such a characterization requires criteria to decide when a child is or is not able to think about this or that kind of entity: my own favoured criterion (developed in the later chapters, particularly 8 and 14) is the vague requirement that the child should be able to hold the represented entity in mind under difficult circumstances, where what counts as 'holding in mind' and as 'difficult circumstances' will vary from entity to entity and will depend as much on the outcome of research as on *a priori* analysis. In my Introduction and Overview I mentioned recent characterizations of representation by Millikan (1984) and Dretske (1988); there is also, of course, the characterization offered by Perner (1991), which closely follows Dretske. I will limit my remarks to

Dretske's proposal, endorsed and adopted by Perner, which is that representational systems (of the type to which mental representation belongs) do not simply represent things, but represent them <u>in a certain way</u>. In short, the representations of such systems, like expressions in a language, have a meaning or sense as well as a reference, and, again like language, are capable of <u>misrepresentation</u>. But it seems to me that the apparatus of sense and reference developed by Frege (1952), derives its point from the role that it plays in explaining how statements may be true or false. There are many difficulties in assimilating mental representation and external systems such as the graphic systems to the linguistic statement-making model. For example, there is a brisk and effective refutation of the idea that representations such as images or pictures can make statements, in pp.174-184 of Fodor (1975). And the attempts by Dretske and Perner to present concrete cases of misrepresentation seem clumsy and awkward (cf. Perner's example , p. 20, of a flash photograph 'misrepresenting your beautiful blue eyes as red': the photo represents your eyes perfectly well, and certainly makes no statement that ' your eyes are red'; it doesn't seem to me to misrepresent your eyes at all, except in the uninteresting sense that it 'misrepresents your face as flat and glossy'. Perner has surely confused the <u>medium</u> of the representation with its <u>content</u>).

The idea that sometimes representation is representation 'in a certain way' or <u>representation-as</u> may be traced to a well-known passage in Goodman (1969, p.29), where Goodman is discussing pictorial representation. His general point is to draw a distinction between certain pictures which <u>refer</u> (portraits, landscapes) and therefore clearly represent something, and other pictures which do not; pictures of some particular fish as opposed to fish-pictures (see Chapter 10, Section 3). While the latter pictures denote something, the former do not. Sometimes we have objects which are both, such as the black-horse-picture of a certain black horse,

which both represents the black horse and represents it <u>as</u> black, etc.

Goodman then discusses an event in which he claims to have a picture of a

certain black horse, which he is about to show you: he then shows you a

picture of his horse taken from such a distance that the horse appears only as

a small speck in the photograph. But Goodman does not go so far as to say

that the light-speck-picture <u>mispresents</u> the black horse. Nor should he. <u>How</u>

does it misrepresent the black horse? Does it misrepresent it as a light speck?

Certainly not: if it were a picture of a light speck (whatever that is) it would

not be a picture of the black horse. Goodman uses the word 'mislead', but it is

not the picture that is said to be misleading, but Goodman's statement that he

has a picture of a certain black horse, which he is about to show you. This

context makes such a statement tantamount to a promise to produce a black-

horse-picture. So even if we accept Goodman's claim that his picture

represents his horse 'as a light speck', there is no need to speak of

misrepresentation here. Further, it is not obvious that 'representation-as'

can be generalized from these pictorial cases to other forms of representation.

Perner (Chapter 2) and Dretske (Section 3.6) claim that representation is

always representation-as. But Dretske's discussion, interesting though it is,

seems very unconvincing to me, and Perner's (since he tries to extend

Dretske's idea from representational systems to individual representations)

even more so. Dretske begins by discussing Goodman's black horse story,

saying that black-horse-pictures represent the black horses they are pictures

of as black horses. He then proceeds to discuss an external representational

system, namely a fuel tank/gauge. 'My fuel gauge [presumably indicating

zero fuel - RNC] is not only a representation of an empty gasoline tank, it is

also ... an empty-tank-representation (p. 71)'. This appears just like the black

horse case, but it is not. His fuel gauge is a representation of an empty

gasoline tank <u>only if</u> it is an empty-tank-representation. By contrast, many

different pictures may be pictures of a given black horse, even if they are not

all black-horse-pictures (far away, in a snow storm, in moonlight, etc.). Dretske's next remark makes matters worse, not better. 'That the tank is empty is what it indicates, the information it carries, the comment it makes about that topic'. What topic? Apparently, he means 'the tank', since his next remark is that 'My gas tank is also very rusty, but the gauge does not comment on this feature of its topic'. But surely Dretske has lost his bearings, since the fuel gauge doesn't represent the tank, but the current state of the tank, namely its being empty. But then what are we to make of the claim that the fuel gauge makes the comment 'the tank is empty' about the empty state of the tank?! There are reasons, then, for taking a cautious approach to the criterion proposed by Dretske, and augmented by Perner. I doubt whether it will stand up to scrutiny: moreover, it is hard to see, even if these objections could be met, how the criterion of misrepresentation might be applied concretely to determine the representational status of children's acts.

My criterion for identifying 'true' representation (p. 263) has weaknesses too, notably its miscellaneous and *a posteriori* character. But it has the virtue of applicability, being rooted in established procedures for determining whether a child can represent an <u>object</u> (object permanence tasks) or a <u>thought</u> (false belief tasks), and the possibility of extending it to representing a <u>quality</u> is developed in Chapters 10-13. In these later chapters of my dissertation I have made no explicit reference to consciousness (prominent in my discussions of representation in Chapters 1-4), but the locution (borrowed from Piaget) used instead - 'holding X in mind' - is merely a bowdlerized version of ' being consciously connected to X', so my claim that cognitive psychology must look to consciousness (or directed consciousness at least) to define its agenda persists. Claparède's Law of Awareness (discussed on p.20) may not characterize all forms of directed consciousness, but those which it does characterize perhaps constitute a satisfactory agenda for cognitive psychology, and for cognitive development in particular.

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