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VERBAL CONTROL OF HUMAN OPERANT BEHAVIOUR

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SUMMARY

Considerable evidence has accumulated to show that human operant behaviour differs qualitatively from that of animals, and that adult schedule performance is a function of (i) instructions, (ii) the subject's conceptualisation of the contingency, and (iii) prior schedule performance. Lowe (1979) has argued that these differences can be accounted for by reference to the human capacity for self-directed speech and has predicted, on the basis of Vygotsky's theory, that infants who lack speech should behave like animals. Three studies of children's fixed-interval schedule performance are reported in this thesis. In the first the fixed-interval performance of infants and children was investigated using a variety of schedule values. In the second the effects of instructions and self-instructions on the fixed-interval responding of children between two and a half and nine years of age was investigated. In the final experiment, the effects on fixed-interval responding of prior experience of either a fixed-ratio or a differential - reinforcement - of - low - rates schedule was investigated with infants and older children. It was found that, in agreement with Vygotsky's theory, infant subjects behave in all respects like animals, that a transitional stage in which verbal control of operant behaviour is weak exists approximately between the ages of two and five years, and that older children behave like adults on schedules of reinforcement.

CONTENTS

INTRODUCTION

That there exist important psychological differences between man and the animals seems obvious. After all, "There must be something unique about man because otherwise, evidently, the ducks would be lecturing about Konrad Lorenz and the rats would be writing papers about B.F. Skinner" (Bronowski, 1973). But how can these differences be characterised? Are they in some way qualitative or merely matters of degree? Given the truth of evolutionary theory - that man emerged from ape-like ancestors - then the Biblical assertion that the cattle were created after their kind while man was created in the image of God no longer seems good enough for modern purposes.

In the following pages a modest attempt will be made to clarify some aspects of this problem and to provide, at least in part, an answer through the language and methodology of modern learning theory. The first chapter will describe, in brief, the history of man's attempt to conceptualise his relationship with the other species and will provide the background to what follows. In the second and third chapters experimental evidence that points to important differences between animals and man will be examined in detail. The relevant developmental literature will be examined in the remaining two chapters of the review section, after which the experimental chapters follow.

A small note about terminology: Henceforth the terms 'animals', 'beasts', 'other species' and 'lower species' will be used interchangeably to refer to all animal species other than homo sapiens. This convention is used for the sake of clarity and not in ignorance of the fact that, from the point of view of the biologist, man is every bit as much an animal as a walrus is. Similarly, the term 'man' will be used to refer to humankind in general, again for the sake of clarity. While feminist objections to this practice were noted the alternative seemed unnecessarily cumbersome.

CHAPTER 1: THE IDEA OF A DIFFERENCE

The debate about the relationship between man and the animals is one of the oldest in philosophy and can be traced back to the time of Aristotle and before. It is a debate that, at times, has been connected to a number of other fundamental issues, such as the question of the existence of the animal mind or soul, the alleged immortality of human life and questions about free will and responsibilty. "These debates have also been persistent symptoms of dissatisfaction with the mechanistic paradigm of explanation of seventeenth-century science as applied to biological and psychological phenomena." (Young, 1967).

As we have already noted, that there are differences between man and the animals seems obvious. Indeed it is difficult to see what could be made of the claim that differences do not exist. At least in terms of gross physiological characteristics all the species are different from each other. That is why they are species. So the claim that man is somehow special among all the rest of the Earth's creatures is really a claim about the types of differences that exist. It is a claim that there is something about man that makes him different from ALL the other creatures and that this difference is somehow unique and more important than those differences that separate species in general. It is for this reason that the idea of a difference is not so simple as it might at first appear and needs a little preliminary discussion.

Midgley (1979), for one, has argued that nearly everything is wrong with asking what finally distinguishes man from the animals (and not only because it would be better to ask what distinguishes man AMONG the animals). "As the question is usually put," she says, "it asks for a simple, final distinction, and for one that confers praise."

Midgley's objections to this are broadly of two sorts. Firstly she notes that many of the attributes that appear to distinguish man from the animals are a particular combination of, and further development of, abilities found in some lower species. (Man is the greatest engineering species but beavers are pretty good too.) Secondly she notes that there is not one thing that has been proposed to mark off man from the animals but a whole cluster of characteristics (eg. reason, culture, self-consciousness, tool-using, laughter, etc.).

Leaving aside the possibility that Midgley's remarks amount to prejudging a question that is essentially empirical, the problem seems to be, as she rightly observes, that we do not know how an extra-terrestrial visitor would regard human-animal differences in relation to the differences that separate the other species. It would seem, therefore, that our consideration of the problem is inevitably made from our own perspective as human beings. It may be argued, however, that if some writers seem to have fallen into the trap of being uncritically chauvinistic, just as many, perhaps in an effort to avoid this pitfall, have gone too far in the opposite direction and have avoided the discussion of those differences that may be real. This is an error no less important than that which its perpetrators wish to avoid. Even if there were no precise, single difference at the behavioural level, and even given that many of man's most remarkable achievements are mirrored to a lesser extent in the behaviour of beasts, this should not prevent us from considering why it is that man seems to have, as the Bible puts it, "dominion over the fish of the sea and over the fowl of the air, and over the cattle, and over the earth, and over every creeping thing that creepeth in the earth". For surely it is a legitimate aim of scientific research to seek after the reason or reasons why this is so, why man is undoubtedly better than animals at doing so many things. And if, in the process of seeking this explanation, we appear to be conferring praise upon ourselves, then so be it. We would perhaps do well to remember that undisputed divisions are found elsewhere in nature (nobody argues that classifying living things as either plant or animal amounts to praising ourselves for being animal) and this fact alone makes the idea of a division between ourselves and the beasts seem less unreasonable that it might otherwise appear, and therefore worthy of investigation.

If, then, questions about difference are worth asking (and can be asked quite properly), what sort of differences should we be looking for? Philosophers of the past have tended to answer this question in one of two ways. On the one hand, some have argued that man's achievements mark only a difference in degree between man and the animals - that man is just the same as the apes, for example, but a little cleverer, a little less hairy. An extreme example of this viewpoint was put forward by, among others, Erasmus Darwin, in his 'Zoonomia' of 1794, where he maintained that ALL human and animal behaviour is learnt (even breathing and swallowing) and by the French eighteenth century naturalist

Condillac who argued that the apparent differences between the species arise purely from the different ranges of experience that their bodies allow them (Warden, 1927). We may refer tentatively to this kind of hypothesis as a CONTINUITY HYPOTHESIS and the difference it refers to as a QUANTITATIVE DIFFERENCE.

The alternative to the continuity hypothesis is that which has been argued by many other philosophers to the effect that man represents a radical departure from other animals, that his behaviour results from the action of processes found nowhere else within the animal kingdom. This kind of hypothesis may be tentatively referred to as a DISCONTINUITY HYPOTHESIS and the difference to which it alludes as a QUALITATIVE one. Discontinuity hypotheses have been held by, among others, many modern linguists and by philosophers ranging from Descartes in the seventeenth century to Sartre in our own.

It is immediately admitted that these terms are not as clear and precise as we might want them to be. Furthermore, we will see that within the two broad groups are differences of opinion as great as those which divide them. It is hoped that in the following chapters these terms will be sharpened up, but they will suffice to allow us to examine briefly the history of the argument. Unfortunately space allows us only to trace the barest origins of each approach and to see only in outline how they are represented in modern biology and psychology.

THE ORIGINS OF THE CONTINUITY HYPOTHESIS

Surprisingly, it may seem, the idea of a continuous progression from plants, via beasts to man is one of the oldest in philosophy.

It is known that ancient man was much concerned with the lives of animals long before written history. The art of Cro-Magnon man, both mobile (artifacts) and parietal (paintings) testifies to an intense interest in the behaviour of animals. Cave murals, such as those found at Altimira in Spain, detail animal form and motion but for the most part neglect plant life, inanimate objects and even, to some extent, man himself. (Whereas the animal species portrayed can be clearly identified the few plants painted cannot.) Archaeologists generally believe that these paintings are the work of a guild of artists although there is less agreement about the significance of the paintings - whether, for example, sexual or magical symbolism is coded into the way the paintings are distributed (Leakey, 1981). However, primitive man's concern with animal life may reflect the fact that his survival must have depended upon his ability to understand the beasts which either threatened his safety or provided him with food. Unfortunately the existing record does not tell us much about what Cro-Magnon man or his descendants in neolithic and later epochs actually thought about animals in relation to himself. We can perhaps conjecture that the differences between the conditions of existence of man and those of animals may well have been less obvious then than they are today.

Although animal cults were a common characteristic of early civilisations - in early Egyptian times, for example, each district had its own local divinity which, more often than not, was an animal - it is only with the coming of the Ancient Greeks and the birth of the scientific movement in the sixth century B.C. that a systematic attempt was made to clarify and understand the natural world (Warden, 1927). Although careful observations of animal behaviour were made before his birth, the most important figure in this respect was Aristotle.

The idea of a 'great chain of being' (Lovejoy, 1936) stretching from the lowliest order of creation to the highest was developed by Aristotle from the Platonic belief in plenitude - that the cosmos, of necessity, exemplifies the full range of conceivable kinds of living beings. It was an idea widely held by occidental philosophers until the seventeenth century. Aristotle is in fact generally regarded as the father of natural history (Warden, 1927). He wrote three sizeable works on animals ('de Partibus Animalium','de Generatione Animalium' and 'Historia Animalium' dealing roughly with the parts, reproduction and natural history of animals respectively) together with a number of lesser works on the same subject. The extent of his knowledge about animals can be judged from the fact that reference is made to over five hundred species of animals in the 'History'. Indeed, the extent of the observations recorded by Aristotle, both of dissected animals and of animals in their natural habitat, has been taken as evidence that the original collection of the data must have been carried out by a team of researchers (Alan, 1970). The three most notable features of Aristotle's view of nature can be summarised as follows:

(i) He saw all living things as existing on a continuum in terms of the complexity of their psychical faculties with man towards the top of the continuum.

(ii) He viewed all natural processes as teleological, the expression of an all-pervading design. "Nature acts like an intelligent workman." (De Generatione.)

(iii) He was a vitalist. The soul was seen as the final cause in Aristotle's system, determining the organisation, movement and purpose of all animals.

Our interest in Aristotle concerns the way in which he described the psychical faculties by which the animals (in which category Aristotle explicitly included man) were graded. Plants were said to have the most elementary psyche, being capable only of taking nourishment, growing and reproducing. Animals, on the other hand, were observed to roam the earth and to prepare food, indicating to Aristotle that they had memory and imagination. (The physiological seat of these faculties was thought to be the heart. The brain was regarded as a kind of cooling tower.) Man was regarded as one step further up the ladder. He alone had the ability to reason. "For of all living beings with which we are acquainted man alone partakes of the divine, or at any rate partakes of it in a fuller measure than the rest." (De Partibus Animalium.) Hence man's senses were seen as not merely orientated towards survival, but towards science and philosophy. None the less:

> "Nature proceeds little by little from things lifeless to animal life in such a way that it is impossible to determine the exact line of demarcation, nor on which side thereof an intermediate form should be." (Historia Animalium.)

Moreover, in an earlier passage Aristotle states that:

> "In the great majority of animals there are traces of psychical qualities or attitudes, which qualities are more markedly differentiated in the case of human beings. For just as we pointed out resemblances in the physical organs, so in a number of animals we observe gentleness or fierceness, mildness or cross temper, courage or timidity, fear or confidence, high spirits or low cunning, and, with regard to intelligence, something equivalent to sagacity. Some of these qualities in man, as compared with the corresponding qualities in animals, differ only quantitatively: that is to say, as man has more or less of this quality, and an animal has more or less of some other; other qualities in man are represented by analogues and not identical qualities: for instance, just as in man we find knowledge, wisdom and sagacity, so in certain animals there exists some other natural potentiality akin to these. The truth of this statement will be more clearly apprehended if we regard to the phenomena of childhood: FOR IN CHILDREN THERE MAY BE OBSERVED THE TRACES AND SEEDS OF WHAT WILL ONE DAY BE SETTLED PSYCHOLOGICAL HABITS, THOUGH PSYCHOLOGICALLY A CHILD HARDLY DIFFERS FOR THE TIME BEING FROM AN ANIMAL." (Historia Animalium, emphasis mine.)

Curiously, Aristotle nowhere made reference to the distinction between learned and unlearned behaviour, a distinction which has become interwoven with the continuity-discontinuity dispute in recent times. The concept of instinct had to wait for Galen who, of all the Greeks after Aristotle, stands out against a background of scientific decline. That decline extended also to the Romans, who bothered little with the study of animals (although a few anecdotal studies by Pliny the Elder and others do survive), and even to the birth of Christendom and through to the Middle Ages.In fact, it was only the rediscovery of Aristotle (via Arabic) in the thirteenth century by Albertus Magnus that revived interest in observing and understanding the natural world. Until that time Christian theologians were too preoccupied with other issues to give lower beasts more than a passing thought and, although it was held by some such theologians that there was an important difference between man and the animals (there had to be, otherwise animals might be morally responsible and therefore open to redemption and immortality) it remained for Descartes in the seventeenth century to popularise the doctrine of discontinuity to the extent that it could be accepted by scholars and informed laymen alike (Lovejoy, 1936; Warden, 1927; Young, 1967).

DESCARTES, DISCONTINUITY AND LA METTRIE

It is probably not exaggeration to say that Cartesian dualism has had a greater effect on the development of psychology than any other philosophical doctrine. Indeed, Descartes' insistence on the separation of body and mind is still hotly debated by both philosophers and psychologists today.

It is clear that Descartes' philosophy was developed from a desire to reconcile his scientific aims with his acceptance of the religious doctrines of his time. The seventeenth century was an era of great development in biology and other sciences. During the preceding few years, Galileo had asserted the unity of terrestrial and celestial mechanics and Harvey had discovered the circulation of the blood. Descartes, both a philosopher and a physiologist was naturally inclined towards a mechanistic interpretation of the human body and its actions. His belief in free-will and the immortality of the soul, however, prevented him from formulating a completely materialist account of human behaviour. The answer was to propose that, while certain human activities might be purely mechanical in nature - blinking for example - the more complex aspects of human behaviour were governed by a non-material mind which, he curiously maintained, interacted with the body via the pineal gland. It was from this perspective that Descartes considered the behaviour of animals.

Descartes' views on human-animal differences are normally dated from his 'Discourse on Method' (1637) but he had in fact been considering the problem for some years before that. In his private notebook of 1619 he stated that: "From the very perfection of animal actions we suspect that they do not have free will" (Rosenfield, 1941). Apparently impressed by the automata (machines that mimicked human action) that were very much in vogue at the time, he went on to develop the opinion that animals were really nothing more than complicated machines, devoid of mind. This viewpoint appeared to offer a number of theological advantages. Firstly it was possible to deny animals immortality. Secondly, it dissolved moral qualms about eating and abusing beasts. By far the most important aspect of this doctrine, as compared to Aristotle's, was that no half-way compromise was allowed in the system. It was possible either to have a mind or to not have one. In Descartes' estimation humans had minds and animals did not. The difference between animals and humans was therefore extremely sharp or, in the terms outlined earlier, qualitative.

In his fifth Discourse on Method, Descartes pointed to two sources of evidence to support his contention that only humans have souls:

> ".... I gave particular emphasis to showing that, if there were such machines which had the organs and appearance of a monkey or of some other irrational animal, we would have no means of recognising that they were not of exactly the same nature as these animals: instead of which, if there were machines which had a likeness to our bodies and imitated our actions, inasmuch as this were morally possible, we would still have two very certain means of recognising that they were not, for all that, real men. Of these the first is, that they could never use words or other signs, composing them as we do to declare our thoughts to others. for one can well conceive that a machine may be so made as to emit words.... but not that it may arrange words in various ways to reply to the sense of everything that is said in its presence, in the way that the most unintelligent of men can do. And the second is that, although they might do many things as well as, or perhaps better than, any of us, they would fail, without doubt, in others, whereby one would discover that they did not act through knowledge....
>
> Now by these two same means one can also tell the difference between men and beasts. For it is particularly noteworthy that there are no men so dull-witted and stupid, not even imbeciles, who are incapable of arranging together different words, and of composing discourse by which to make their thoughts understood; and that, on the contrary, there is no other animal, however perfect and whatever excellent dispositions it has at birth, which can do the same." (Descartes, 1637).

Descartes' ideas on the animal soul changed somewhat throughout his life, perhaps partly because of the publicity that they attracted and the negative way in which the academic world received them. (A considerable amount was written by Descartes' contemporaries discussing and objecting to various aspects of his theory, the idea that animals were soulless attracting a good deal of criticism.) Towards the end of his life, however, even though Descartes allowed animals certain rudimentary sensations and, in a letter to Sir Henry More in 1649, admitted to being unable to prove his assertion that animals' lack souls, he still argued that their lack of language made this most probable:

> "For language is the one certain indication of latent cogitation in a body, and all men use it.... whereas on the other hand NOT A SINGLE BRUTE SPEAKS, AND CONSEQUENTLY THIS WE MAY TAKE FOR THE DIFFERENCE BETWEEN MAN AND BEAST." (Quoted in Young, 1967.)

Reactions to Descartes included many attempts to allow animals a soul and thus to give then credit for some measure of intellect. (Descartes' letter to More was in response to More's outraged reaction to: "The internecine and murderous view which you bring forwards in the Method, which snatches away life and sensibility from all animals;" cf. Williams, 1979.) A few, it was true, saw the danger to Christian doctrine that lay in the other direction. However, over a century passed before la Mettrie (1747), reasoning that if animals did not require a soul perhaps man did not require one either, advocated the extension of the doctrine of LA BETE MACHINE to L'HOMME MACHINE.

La Mettrie represents a major turning point in the history of ideas - one of the obvious 'breaking points' between science and religion. Drawing his ideas directly from Descartes, and influenced by the iatromechanist school of biology, he tried to restate the mind-body problem as a problem of physics (Vartanian, 1960). To this end he cited evidence from a wide range of phenomena - the effects of drugs, fatigue, diet, disease and sexual desire - to support his view that mental life depends on bodily processes. He maintained that such correspondences were sufficient to validate his analysis which, in today's terms, might be thought of as a primitive mind-brain identity theory (advanced in more recent times by Place, 1956; Smart, 1959; and Fiegl, 1960).

It should be stressed, however, that la Mettrie's concept of a machine was more complex than a mere 'dead mechanism' and that he was careful to assign man purposeful and self-sustaining qualities. (The concept of 'irritability' was seen as the key to autonomous muscular activity.) Moreover - and for the purposes of this discussion this is the important point - the behaviour of man was seen to differ in degree, not in kind, from that of animals. Although man had a moral sense (in the form of certain neural mechanisms) he was not regarded as unique in this respect. Of special interest is la Mettrie's treatment of language. According to la Mettrie, man's scientific abilities had to be understood in terms of the effect of language over centuries which had, in effect, turned man's brain into a sort of 'self-playing piano'. All thought was therefore seen as symbolic in nature. Noting the absence of language in lower species, la Mettrie was probably the first person to advocate the teaching of sign language to apes.

Not surprisingly, la Mettrie's ideas were not well received at the time. In France, 'L'Homme Machine' was immediately banned. Only in Prussia, where la Mettrie was in exile, were his views received with any sympathy. None-the-less, la Mettrie's argument was but one obvious manifestation of a tendency that had begun before Descartes and which has had more effect on our conception of ourselves than perhaps any other influence since the rise of Christianity - the emergence of a modern, systematic, science of biology.

THE DIFFERENCE AND THE RISE OF BIOLOGY

"Both chemistry and biology are eighteenth-century sciences in inspiration," remarks Gillispie (1960) in his study of the origins of scientific ideas, while Lovejoy (1936) states that, "It was in the eighteenth century that the conception of the universe as a Chain of Being, and the principles which underlay this conception - plenitude, continuity, gradation - attained their widest diffusion and acceptance". The exact relationship between these two developments is an issue beyond the scope of this thesis and best left to historians. None-the-less, the parallel emergence of these two paradigms to the extent that they became dominant had important consequences in the realm of ideas about human-animal differences.

The term 'biology' was actually coined by Lamarck in 1802 "to lend cosmic unity to natural history, that descriptive study of living nature which classified detail until the mind reeled in boredom along ordered rows of trivia" (Gillispie,1960). Yet much progress had been made in the century before Lamarck with respect to two major ideas - ideas which came to dominate the rise of biology as a science and in the development of which Lamarck played a crucial role. These ideas were, of course, the classification of the species and the theory of evolution.

From the point of view of this brief history the interesting aspect of the drive to develop a system by which species could be classified is the doubt that was prevalent about whether or not species actually existed at all, a doubt that was as widespread among the classificationists as it was among educated laymen. A number of large encyclopaedias of natural history were compiled during the eighteenth century - Linnaeus' 'Systema Naturae' (1734), Buffon's 'Natural History' (fourty four volumes, beginning in 1749) and Erasmus Darwin's 'Zoonomia' (two volumes published in 1794) to give but three examples - all devoted, among other things, to accurately labelling and describing the breadth of nature, yet it is clear that, in many cases at least, species names were considered to be no more than convenient markers for certain points along an infinitely varying continuum. Buffon, in his 'Natural History', for example, attacked the whole idea of species with the comment that, ".... in reality individuals alone exist in nature...." (a position he later abandoned) whereas Bonnet, in his 'Contemplation of Nature' of 1769 stated that:

> "If there are no cleavages in nature, it is evident that our classifications are not hers. Those which we form are purely nominal, and we should regard them as means relative to our needs and to the limitations of our knowledge. Intelligences higher than ours perhaps recognise between two individuals we place in the same species more varieties than we discover

> between two individuals of widely separated genera." (Quoted in Lovejoy, 1936)

As for man's position with respect to other animals, the writers of this period held views which strike us as surprisingly modern. Linnaeus, for example, recognising a degree of ambiguity in man's place in the chain of being placed him in his system close to bats and monkeys while reserving only the Socratic injunction "Know thyself" for the species description (Gould, 1980).

Bonnet was even more forthright, arguing that the orang-utan so closely resembled man that it was extremely difficult to decide whether he should be regarded as a seperate species or a mere variety.

It is worth remembering that these arguments were formulated approximately a hundred years before Darwin. And it is important to note that this belief that "nature makes no leaps" was not restricted to professional zoologists. Indeed, for some time before the controversy raged over Darwin's 'The Origins of the Species' (1859) both the scientific establishment and the public were concerned about the possibility of a 'missing link' in the chain. Sailors testified to seeing men with tails, for example, and the showman P. T. Barnum, a skilled judge of what the public wanted, advertised among his attractions in 1842 "the preserved body of a Feejee mermaid.... the Ornithorhincus, or connecting link between the seal and the duck; two distinct species of flying fish, which undoubtedly connect the bird and the fish; the Siren, or Mud Iguana, a connecting link between reptiles and fish,.... with other animals forming connecting links in the great chain of Animated Nature" (Lovejoy, 1936). In the light of this concern it perhaps seems odd that the publication of Darwin's theory evoked the outcry that it did. The difference, of course, between the views of Darwin and the views of Bonnet and his contemporaries was that, for Bonnet at least, the observed similarities between species implied nothing about their origins.

Darwin's own views on the relationship between man and the animals emerged quite early in his efforts to formulate a comprehensive theory of evolution and preceded his discovery of natural selection (Gruber, 1981; Howard, 1982; George, 1982). However, they were not made public until much later, partly because he wished to develop them to a point where they would be resistant to the kind of attack which he so accurately forsaw. ("Mention persecution of early Astronomers," appears ominously in his 'C' notebook, dated

1838.) Although Darwin began to make systematic remarks about human psychology in his 'M' notebook in 1838 (ed. Barret and Gruber, 1974), 'On the Origin of Species' (1859) carries little mention of man, who had to await 'The Descent of Man' (1871) and 'The Expression of Emotions in Man and Animals' (1872). Despite the enormous impact of Darwin's theory, the exact value of some of his ideas about the human mind is difficult to assess.

In the 'M' and 'N' notebooks can be traced the attempt to formulate the systematic and crudely materialistic psychology that Warden (1927) has described as naive. Certainly, Darwin seems to have toyed with the idea that thought was a kind of secretion of the brain, and it could be maintained that a rather cavalier attitude towards some difficult philosophical issues is embodied in such comments as, "Origin of man now proved - Metaphysics must flourish - He who understands baboon would do more towards metaphysics than Locke" ('M' notebook, 1838). However, the systematic psychology never emerged in its entirety and, in his published works, Darwin confined himself mainly to speculation about the origins of human and animal behaviour, placing them within the range of evolutionary theory in general and natural selection in particular.

Darwin was clearly and obviously a continuitist, as was his disciple Huxley, who was more concerned than Darwin to popularise an evolutionary account of man's nature (and less hesitant to offend). Huxley (1874) commented:

> "But though I do not think that Descartes' hypothesis can be positively refuted, I am not disposed to accept it. The doctrine of continuity is too well established for it to be permissible to me to suppose that any complex natural phenomenon comes into existence suddenly, and without being preceded by simpler modification; and very strong arguments would be needed to prove that such complex phenomena as those of consciousness, first make their appearance in man"

(Huxley went on to propound the doctrine of epiphenomenalism - that psychic processes are a collateral product of the brain's working.... "as completely without any power of modifying that working as the steam-whistle which accompanies the work of a locomotive engine is without influence upon its machinery.")

Two general lines of argument were used by Darwin and his followers to support their continuitist position. First, Darwin claimed that in man could be found traces of behavioural processes found in animals. Second, he argued that animals displayed elements of reason and the higher mental faculties normally ascribed to man. For Darwin no hard boundary existed between instinct and reason and the instincts found in animals were not the inflexible patterns of species-specific behaviour they have sometimes been thought to be in later times. "A little dose.... of judgement or reason, often comes into play, even in animals very low in the scale of nature." (Darwin, 1859.) Thus, Darwin was able to avoid recognising the possibility of a qualitative change between man and the animals. To this end 'The Expression of Emotions' contains much anecdotal evidence explicitly comparing the expressions, gestures and displays of both man and beast. However, this did not make Darwin ignore as unique an acheivement as human language, which, he claimed, owed its origins to "the imitation and modification, aided by signs and gestures, of various natural sounds, the voices of other animals, and man's own instinctive cries." (Darwin, 1871.) For:

> "A great stride in the development of the intellect will have followed, as soon as, through a previous considerable advance, the half-art and half-instinct of language came into use; for the continued use of language will have reacted on the brain, and produced an inherited effect; and this again will have reacted on the improvement of language. The large size of the brain in man, in comparison with that of the lower animals, relatively to the size of their

> bodies, may be attributed in chief part.... to the early use of some simple form of language, - that wonderful engine that affixes signs to all sorts of objects and qualities, and excites trains of thought which could never arise from the mere impression of the senses, and if they did arise could not be followed out." (Darwin, 1871)

As is well known, the effects of Darwinism on the development of the biological sciences and medicine in general are hard to overestimate. It is undoubtedly true that the psychologists of the late nineteenth and early twentieth centuries were much influenced by his thought and by the place he accorded man in nature. Wundt and Mead took Darwinism as the basis of their respective social psychologies (Farr, 1980) and such figures as Freud and Watson, the founders of psychoanalytic theory and behaviourism respectively, incorporated evolutionary theory into their approaches. For Freud, man's behaviour, and particularly his problems, could be traced to the growth and interaction of drives and instincts in his mind. Watson took a different path. Rejecting mentalistic talk and assuming continuity between the species, he extended the prevailing view of animal learning to man.

THE CONCEPT OF A DIFFERENCE AND MODERN PSYCHOLOGY

The idea of the difference is surprisingly complex. The above brief historical survey should go some way towards showing how complex it is. Aristotle and Darwin, we have seen, are considered to be theorists of continuity, whereas Descartes is commonly supposed to be, historically, the most important advocate of the case for discontinuity. Yet, when we compare Aristotle's views, or even Darwin's, to Descartes' later assertion that "language is the one certain indication of latent cogitation in a body", we can see elements of similarity. Indeed, a tendency to define man through language has been manifest in both continuity and discontinuity approaches to human nature (Hawpe and Richards, 1979).

But if both discontinuity theories and continuity theories emphasize the importance of language, where do the two types of theory really differ? In some cases, for example in that of Descartes, it would seem that the presence of language is important ONLY in that it is seen as an indication of something else (a soul) whereas, in the case of other theorists, for example la Mettrie, language does not make man fundamentally different from the animals because it is seen as JUST another (perhaps rather specialised) way of behaving.

It is as if one is required to believe that man either is or is not an animal when, in reality, both assertions may be, in different ways, correct. To use Ryle's (1979) metaphor, philosophers have fallen victim of their 'deflationary' and 'inflationary' tempers. Yet, given an acceptance (contra Midgley) that there may be an important difference between man and the lower species, it does not necessarily follow either that man is nothing but an animal or that he is something else (a soul) as well. For surely, talk of a difference or differences between man and the animals does not preclude the fact that many things about the two are obviously similar. From a different perspective it might be maintained that there has been a failure to view the matter 'dialectically'.

Janet Malcolm (1982), in her remarkable journalistic enquiry into the current state of American psychoanalysis, describes a conversation between herself and her analyst-informant (pseudonymed Aaron Green) about the work of the analytical theorist Kohut, contrasting in particular, Kohut's approach as shown in his paper 'The Two Analyses of Mr. Z' (1979) with the approach of more orthodox analysts, such as Brenner (1973):

> "'Have you ever seen Kohut?'
> 'I've heard him talk twice.... At the last meeting of the American Psychoanalytic, in the Waldorf, Kohut made a brief, charismatic appearance. His disciple Paul Ornstein from Cincinnati, had given a speech spelling out the Kohutian line and holding up 'The Two Analyses of Mr. Z.' as if it were the miracle of the loaves and the fishes. Then Kohut himself appeared, like God or Lenin come down from heaven.... He didn't say much, but he betrayed it all in one sentence. "WHAT IF MAN IS SIMPLY NOT AN ANIMAL?" he asked rhetorically at the end of his homily. Meaning "Let's forget the drives.... Let's forget that we're DRIVEN organisms." Freud's hypothesis of the drives has never been acceptable to the public, or palatable even to many within psychoanalysis itself. There has been a persistent attempt to whittle away at the radicalism of psychoanalytic theory.... That's why I like Brenner so much. Brenner is willing to to draw radical inferences.... to go all the way. I believe that Brenner's view will prevail, because, for all its harshness and reductionism, it contains a more profound and complex and interesting statement about human nature than any of the revisionist views do. To say "Man is not an animal" is to say nothing that banal people haven't always said. To say that our essential humanity resides in precisely that part of our nature which is most instinctual, primitive, and infantile - ANIMAL - is to say something radical.'" (Emphasis Malcolm's.)

When we look at other areas of modern psychology we see immediately that some of the most important divisions within it, divisions that hold psychologists apart with a strength of feeling rarely encountered in other sciences, are the direct result of the wholesale importation of opposing conceptions of human-animal differences from earlier centuries. The basic ideas have not changed, only the sophistication of the theoretical devices and experimental techniques with which they have been defended. If Descartes and La Mettrie were to be suddenly resurrected today then, by brushing up on selected data, they would feel intimately at home in any of a number of university departments that are committed to the enlargement of their original hypotheses. Psychoanalysis is generally treated with little interest by academic psychologists today (Farrell, 1981) but similar disputes to that voiced by Aaron Green are reflected in many of the different approaches to the study of man that are prevalent. For example, three different theoretical approaches to the study of human nature which are currently receiving some support, and which (traditionally) take strong positions on the issue of human-animal differences are sociobiology, psycholinguistics and behaviourism.

Sociobiology is a term that has recently become popular, perhaps more among zoologists than among psychologists. The term was first used by Wilson (1975) to describe an approach to human and animal behaviour that attempts to reduce it to, in the main, genetic phenomena and principles of natural selection. This kind of analysis, particularly when applied to human behaviour (for example by Wilson, 1975, 1978; and by Dawkins, 1978), has proved to be highly controversial. There is too little space here to consider the merits of sociobiology, except to mention that, in general, opponents have argued that much of the theory is speculative and unsupported by data, and that sociobiologists have failed to give due weight to the influence of culture and learning. (The reader is referred to Sahlins, 1977; Midgley, 1979; Gould, 1980; Reynolds, 1980; and Ruse, 1982 for discussions of these issues.)

The term psycholinguistics has been used to refer to the study of language performance and acquisition. Over the past twenty-five years or so the major figure in this area has been Noam Chomsky, who has argued that learning theory accounts fail to explain the child's rapid acquisition of language adequately and that, therefore, human beings must be born with innate linguistic knowledge (Chomsky, 1957, 1965, 1978, 1980). Chomsky's theory, then, is similar to that of the sociobiologists to the extent to which it emphasises the role of genetic inheritance in determining important categories of human behaviour, but diametrically opposed to that of the sociobiologists to the extent that it claims the existence of an unbridgeable divide between the functioning of the animal mind and the minds of men. Again, space is too limited here to consider these issues in detail. Suffice it to say that Chomsky's nativism remains as controversial as the ideas of the sociobiologists. (The interested reader is referred to Cohen, 1976; de Villiers and de Villiers, 1978; and Slobin, 1979 for further details.)

For the purpose of the present thesis the third approach mentioned - modern behaviourism - demands a more extended examination. The behaviourist approach is, therefore, the subject of the next chapter.

CHAPTER 2: BEHAVIOURISM AND HUMAN-ANIMAL DIFFERENCES

It is not possible, in the limited space available, to give a full account of modern theories of learning and of the behaviourist approach to psychology which has, at times, informed and, at other times, been informed by those theories. All that can be attempted here is a brief sketch in the hope that a few common illusions can be dispensed with.

The origin of the movement now known as 'behaviourism' can be traced to two major developments that began to make themselves felt in the early twentieth century: (i) the emergence, following the theory of evolution, of rudimentary experimental analyses of animal learning and (ii) the almost simultaneous failure of what may be termed the 'introspective' approaches to human psychology prevalent at the time.

Of particular importance in the study of animal learning were the contributions of C. L. Morgan (1894), I. P. Pavlov (1927), and E. L. Thorndike (1911). Lloyd Morgan is most remembered for his (by today's standards) anecdotal studies of animal behaviour and for his insistence that only relatively simple mechanisms were required to explain quite complex animal behaviour.

I. P. Pavlov's contribution was the systematic elucidation of what has come to be known as 'the conditioned reflex'. Pavlov was originally interested in the mechanisms of digestion and, to that purpose carried out a series of experiments with dogs in pre-revolutionary Russia. As is now well known (and taught at school to almost every biology student), Pavlov noticed that his dogs would salivate on hearing their keeper approach with food. In other words, they had learned to associate the food with the keeper's approach. Thus, salivation, an unconditioned reflex to food in the mouth, became a conditioned reflex to the sound of the keeper's feet. Pavlov spent most of the rest of his life investigatiing this process by carefully controlled experimentation. Considering himself first and foremost a physiologist rather than a psychologist, he frequently speculated about the brain mechanisms underlying conditioning and nourished the hope that his research might be relevant to human medicine. He was cautious in his extension of his theory of the conditioned reflexes to man but believed that it might lead to an explanation of fairly complex psychic phenomena.

> "It is obvious that the different kinds of habits based on training, education and discipline of any sort are nothing but a long train of conditioned reflexes. We all know how associations, once established and acquired between definite stimuli and our responses, are persistently and, so to speak, automatically reproduced, sometimes even although we fight against them." (Pavlov, 1927)

Pavlov's work carries many attempts to find analogies between human behaviour (especially pathological behaviour) and animal behaviour. However, his views on human-animal differences developed considerably towards the end of his life and we shall examine them in more detail in a later chapter.

Thorndike mapped out what, at first sight, appears to be a similar process to that described by Pavlov. In a famous series of experiments he placed a cat in a box from which the only means of escape was by use of a concealed catch. On escaping the animal was rewarded or 'reinforced' with food. Thorndike observed that the cat progressively learned to escape from the puzzle box with greater efficiency. This was explained in terms of the celebrated Law of Effect - the connection between a stimulus and a response (Thorndike labelled his approach 'Connectionism') was strengthened or weakened as a result of its consequences (satisfying or unsatisfying). Thorndike thus represents something of a departure from Pavlov's concern with the stimuli that elicit behaviour to a concern with how behaviour changes the environment which, in turn, alters the animal's disposition to respond in a certain way. In fact, Thorndike's system was a form of natural selection operating in the lifetime of the organism - successful responses multiplied, unsuccessful responses perished.

J. B. Watson (1913, 1924) was the first to use the term 'behaviourism'. (See Bergman, 1956; and Burnham, 1968 for historical details.) Disappointed by the results of the introspective method and disillusioned by the important failures of agreement between different introspectionists (failures of agreement that could not be resolved by appeal to objective data because no such data existed), Watson took the simple expedient of advocating that psychology should be an objective science.

> "Why don't we make what we can observe the real field of psychology? Let us limit ourselves to things that can be observed, and formulate laws about only those things. Now

> what can we observe? We can observe behavior - what the organism does or says." (Watson, 1924.)

Watson was much influenced by current research on animal learning and, in particular, by Pavlov's studies which were then filtering to the West from Russia. In a sense applying Lloyd Morgan's Canon to man, Watson sought to extend Pavlov's theory to account for the most complex human activities. Thus, the child's acquisition of language was accounted for in terms of simple stimulus-response associations, as was human thought and creative behaviour. Watson thus stood for a strong continuitist approach to human behaviour:

> "Psychology as the behaviorist views it is a purely objective experimental branch of natural science. Its theoretical goal is the prediction and control of behavior. Introspection forms no essential part of its methods, nor is the scientific value of its data dependent upon the readiness with which they lend themselves to interpretation in terms of consciousness. The behaviorist, in his efforts to get a unitary scheme of animal response, recognizes no dividing line between man and brute. The behavior of man, with all its refinements and complexity, forms only a part of the behaviorist's total scheme of investigation...." (Watson, 1913).

It is instructive to consider Watson's early behaviourism - which is now universally regarded as simplistic - from the perspective of modern science, and to look at it in terms of the development of the scientific world view in general. The development of science has sometimes been described in terms of a 'growth in objectivity' (Gillispie, 1960). Kantor (1968), taking an extremely broad view in an admirable attempt to dispel some of the mystique that has grown up around the behaviourist position, has stated:

> "By behaviorism we understand the study of behavior as some confrontable thing or process; thus the term 'behaviorism' is equivalent to the term 'science'.
>
> In astronomy, behaviorism is the study of the interaction of stars, suns, planets, galaxies and celestial radiation. In physics, behaviorism is the study of various activities, objects or properties of objects.... In biology, behaviorism signifies that the investigator observes the behavior of nucleic acids, cells, organs, and organisms, as well as various organic processes such as photosynthesis, metabolism, growth, reproduction, and disintegration. In psychology, behaviorism is the study of the interactions of organisms with other organisms or objects; in other words, the subject matter of the science of psychology consists of definite confrontable events just as do the subject matters of other sciences."

In other words, the emergence of behaviourism amounts to, in essence, little more than a paradigm shift to a natural scientific approach to the study of man.

The extent to which Watson's version of behaviourism is, by modern standards, crude, both empirically and conceptually, must be borne in mind in any consideration of the nature of the behaviourism of today. Behaviourism may be best viewed historically as a 'research programme' (a shared view of the problems facing the science and the best methods of approaching their solution - Lakatos, 1974), that has evolved in complexity around a small number of central tenets. In fact, one of the most common misunderstandings about behaviourism is the assumption that the term applies to one, coherent theory about human behaviour. The term is better thought of as a rather general label used to describe a range of broadly similar but not identical theoretical positions. The common features of modern behaviourisms can be roughly summarised as: (i) an emphasis on a functional analysis of behaviour, (ii) a rejection of the kind of mentalistic theories first advocated by Descartes and (iii), a commitment to the application of theories of learning to important social problems (Day, 1976). Different behaviouristic systems specify these features in different ways. For example, different behaviourists have emphasised different functional relationships and have not always agreed with each other about which relationships are important. Again, all behaviourisms reject the Cartesian form of mentalism that regards the mind as non-physical and the ultimate cause of behaviour, but most behaviourisms have allowed some reference to internal events, either as hypothetical mediators of stimulus-response relationships or as behaviours which themselves require explanation and which may form part of highly organised sequences of responding. (For futher discussion of modern behavioural views on internal events, see, for example, Moore, 1980; and Creele, 1980).

The first half of the twentieth century saw something of an explosion of behaviourist theories of learning by such people as Watson, Guthrie, Hull and others (Hergenhahn, 1976, gives condensed accounts), most of which are now largely forgotten. However, one behaviourist who began to write in the 1930s, and whose approach remains influential today is B.F. Skinner. Skinner's fame to some extent rests on the boldness with which he has attempted to extend his account of learning in animals to the analysis of human social behaviour. Moreover, many of the concepts and experimental techniques Skinner developed during his research with animals have proved to be of enduring value.

For these reasons, then, a few essential elements of the Skinnerian system need to be mentioned before we can proceed.

Skinner's approach is essentially atheoretical in the special sense that he regards attempts to explain learning in terms of underlying processes occuring within the organism as unnecessary for technological progress (Skinner, 1950). Instead, Skinner's concern has been with the formulation of descriptive laws that link environmental events to changes in behaviour. This is not to say, as has often been assumed, that Skinner regards the organism as an empty 'black box', it is rather that he regards the investigation of the underlying mechanisms of learning as another sort of problem, best studied when a full analysis of what the organism does and under what circumstances has been carried out. Thus:

> "A successful independant experimental analysis of behavior is a necessary half of any physiological investigation. The requirement is not always recognised by those who try to tell the physiological Inside Story. On the contrary, it is often implied that behavior cannot be adequately described until more is known about the nervous system. A science of behavior is said to be highly phenomenological and is said to show a studied indifference to brain mechanisms - to what is inside the black box. But we cannot say that what goes on inside is an adequate explanation until we know what the

> black box does. A behavioral analysis is essentially a statement of the facts to be explained by studying the nervous system. It tells the physiologist what to look for. The converse does not hold. We can predict and control behavior without knowing how our dependant and independant variables are connected. Physiological discoveries cannot disprove our experimental analysis or invalidate its technological advance." (Skinner, 1969).

Skinner (1935), along with Miller and Kornorski who made the same point independently in 1928, drew attention to the importance of distinguishing between the two types of learning described by Pavlov (respondant, Pavlovian or classical conditioning) and Thorndike (instrumental or operant conditioning). In the former, the organism learns to emit a response (usually autonomic, ie: a reflex such as salivation) in the presence of stimuli which previously would not elicit the behaviour. In the latter, the circumstances under which a particular response occurs is not important but its consequences are. Hence, the organism learns to operate on its environment and the response involved is usually muscular, ie: a motor act. Furthermore, in the case of operant conditioning (but not classical conditioning) completely new responses can be learned. This can happen by the method of 'successive approximations' or 'shaping'. For example, a rat being trained to press a lever can first be rewarded or 'reinforced' for moving close to a lever then, when that has been learnt, for touching it and finally for moving it downwards with its left paw. Thus, for Skinner, operant conditioning is of much greater interest than classical conditioning because it gives the animal greater flexibility and leads to behaviours of greater complexity.

Several different types of consequences of operant responses - operants - were described by Skinner (1938). A reinforcer was defined as any environmental event that, when consequent upon behaviour, alters the future probability of that behaviour occurring. Extinction occurs when a behaviour that has previously been reinforced is reinforced no longer.

The consequences of a response are not the only important stimuli in the Skinnerian system. Also of importance are stimuli that occur simultaneously with the availability or non-availability of reinforcement. For example, if a rat is only reinforced for pressing a lever when a red light close to the lever is turned on, then the light will become a discriminative stimulus and the rat will learn to press the lever only when the light is on. (For more complete definitions and descriptions of these processes see, for example, Blackman, 1974; Catania, 1979; Davey, 1981).

Skinner (1938, 1956) developed a particular method for the investigation of operant conditioning which has proved to be of lasting utility. The most interesting effects of operant conditioning occur when not every response in a response class produces reinforcement. Thus, for example, a rat may be reinforced for pressing a lever on every fifth lever press, or a child may be reinforced every hour on the hour for doing his homework. To investigate the effects of such schedules of reinforcement in animals, Skinner devised an experimental chamber which came to be known (through no effort of Skinner's; cf. Skinner, 1972) as a 'Skinner Box'. In a typical Skinner box designed for pigeons there is a grid on which the bird stands (and is free to move about), one or more keys which it can peck, and a number of lights that can act as discriminative stimuli. In addition, a small food hopper can be lowered, temporary access to which functions as the reinforcer. Onset and offset of lights and reinforcement can be determined by an observer or automatically by means of a computer.

It has been suggested that the most revolutionary characteristic of Skinner's approach is the way in which it allows learning to be SHOWN (Cullen, Hattersley and Tenant, 1981). Whereas other learning theorists described the results of their experiments in terms of, for example, the time taken to escape from a puzzle box or to run through a maze (perhaps represented by means of a graph) Skinner was able to demonstrate learning itself as it occurred in the Skinner box. One way in which he was able to keep an accurate record of moment to moment changes in an animal's behaviour was by the device known as a 'cumulative recorder'. A pen passed over a drum of paper turning at a constant rate. Whenever the animal responded the pen automatically moved slightly across the drum. Thus, the resultant mark on the paper represented the number of responses made and the time taken to make them. The slope of the curve denoted the momentary rate of responding. By convention, the delivery of reinforcement during an experiment is represented by a brief deflection of the pen, causing a downward slash through the curve.

In recent years there has been something of a shift in emphasis in the study of animal learning. This shift has manifested itself in a number of ways.

(i) Classical and Operant Conditioning. Skinner, Kornorski and Miller argued that classical and operant conditioning are two different kinds of learning, the former involving autonomic (involuntary) responses and the latter involving skeletal (motor) responses. In recent times, this distinction has been brought into question by evidence showing that autonomic responses may be operantly conditioned and that motor responses may be classically conditioned.

The evidence on the operant conditioning of autonomic responses remains controversial. The technical problems involved are immense. The possibility that a change in autonomic activity may result from the conditioning of a motor response (eg. that a change in heart rate may result from an overall increase in motot activity) must be ruled out; this is usually done by administering the drug curare, which paralyses the somatic nervous system. In addition, care has to be taken that classical conditioning is not responsible for any changes that occur - eg. that the reinforcer is not acting as an eliciting stimulus. A number of researchers have claimed to demonstrate, by means of careful experimentation with animals, operant conditioning of such functions as heartrate and blood distribution.

However, replications of these studies have not always been successful (cf. Brady and Harris, 1977).

Evidence of the classical conditioning of motor responses is clearer. Brown and Jenkins (1968) showed that a pigeon would peck a key if the key was repeatedly lit as non-contingent reinforcement was delivered. The peck - a response normally made to grain, the reinforcer - was made to the key that was paired with it. This phenomenon, known as 'autoshaping', has been shown to occur even when a key peck is made to lead to a delay of reinforcement (Williams and Williams, 1969).

(ii) Species-specific Constraints on Learning. Although Watson underestimated the importance of innate determinants of behaviour, it is probably true to say that no behaviourist has denied that the organism's innate biological endowment plays an important role in determining what can or cannot be learnt (Skinner, 1966). However, behavioural analyses of both human and animal behaviour have tended to stress the importance of environmental, rather than evolutionary, factors in determining what the organism does. In the last twenty years or so more has been learnt about species-specific constraints on learning. Thus attempts to teach animals to perform certain activities for commercial purposes (eg. to perform in television advertisements) have sometimes been thwarted by the intrusion of species-specific responses (Breland and Breland, 1961). Learning in the laboratory has also been found to be selective - rats learn to avoid flavoured water after drinking it has been paired with an injection causing sickness but not after it has been paired with an electric shock (Garcia and Koeling, 1966). For these reasons, the study of the effects of species differences in learning experiments has assumed importance.

(iii) Processes Underlying Conditioning. Finally, conditioning seems to be a more complex process than has sometimes been thought and it is now widely accepted that "Simple associative learning is simple in name only." (Mackintosh, 1976). An example of a phenomenon that has often been thought to be problematic is 'silent learning'. In a classical conditioning experiment carried out by Rizley and Rescorla (1972) an animal experienced a light and a tone (two neutral stimuli), paired together repeatedly and, in a following condition, a light and an electric shock. The light came to elicit a 'conditioned emotional response' by virtue, presumably, of its contiguous presentation with the shock. More surprisingly, the tone, when presented alone, also elicited such a response. The relationship between the light and the tone had thus been learnt 'silently' - without any overt behavioural manifestation. Evidence such as this, together with the evidence that classical and operant conditioning may not be distinct processes and that not all responses are equally conditionable to all stimuli, has led a number of psychologists to theorise about the associative processes that are assumed to underlie conditioning (Rescorla and Wagner, 1972; Mackintosh, 1976; Dickinson, 1980). Some psychologists, eg. Dickinson, have gone as far as to claim that the aim of animal learning theory is to discover the 'cognitive' processes involved in learning and that this goal represents a fundamental shift away from the programme of behaviourism. Interestingly, these psychologists seem to tacitly share the assumption of many behaviourists that no fundamental differences exist between learning processes in animals and those in man. Instead of taking Watson's step of extending Lloyd Morgan's Canon to man, however, they attribute to animals the kinds of information-processing mechanisms previously attributed to man (Blackman, 1983).

These developments, however, are probably not of great importance to the present argument. It is not clear, first of all, that there need be an essential contradiction between the research programme of Skinner's behaviourism - which aims for a functional account of human and animal learning - and the attempt by cognitive animal psychologists to construct a theoretical model that explains learning in terms of underlying mechanisms (although what those mechanisms actually are may, of course, be disputed). Furthermore, although it is now held by some that the difference between operant and classical conditioning may be one of procedure, the distinction is useful and is still employed, even in those textbooks that advocate a cognitive approach. In addition, the techniques pioneered by Skinner - the Skinner box and the cumulative recorder - remain of enduring value and have led to some important and surprising discoveries. The most important of these with respect to the present thesis has been made by investigating the performance of many species of animals on schedules of reinforcement. The effects of schedules have been found to be highly reliable and replicable across species (Ferster and Skinner, 1957; Zeiler, 1977; Richelle and Lejeune, 1979). In other words, given a particular schedule, those animal species that have been studied seem to behave, in the main, in the same way. This leads to a natural test of the continuity hypothesis of human behaviour - if simple continuity theory is correct we would expect human behaviour to obey the same lawful relationships that have been observed with animals on schedules of reinforcement.

HUMAN PERFORMANCE ON SCHEDULES OF REINFORCEMENT

In fact, Skinner began his work with the white rat (following on from a long tradition of American research into animal learning) and, together with the pigeon, that species has been the most common occupant of the operant laboratory ever since. In addition (as already mentioned) other animal species have at times been tested on schedules of reinforcement. There are, of course, a number of good reasons for carrying out research with animals - rats are more tolerant, more manipulable, more reliable and cheaper to maintain than human subjects. It is possible to do things to rats which would be ethically unacceptable if done to people. The rat is available for experimentation at any time, his history before the experiment begins can be controlled with at least a degree of precision, and he never turns up late for an appointment.

None the less, many of the claims of learning theorists have been based on the argument that operant research in particular is relevant to man. Skinner remarked in 'The Behavior of Organisms' (1938)"Let him extrapolate who will", yet extrapolation has been the keynote of behaviourism in recent times . Thus it has been claimed that the kind of experimental analysis of behaviour that has been so fruitful with animals can be usefully extended to account for such diverse aspects of human life as economic affairs (Lea, 1981); psychopathology (eg. Ullman and Krasner, 1975; see also Eysenck's work, eg. 1957, 1981 based on a Pavlovian model of human behaviour); education (Skinner, 1968) and even art (Skinner, 1972). Skinner has further argued, on the basis of such research, that the time has come for psychologists to enter the business of designing cultures (Skinner, 1953) and that the concepts of 'free will' and 'dignity' are now dispensable (Skinner, 1971). Indeed, so loud have been these claims that some writers, both within psychology and without, have cast a worried eye in the behaviourists' direction in the apparent fear that powerful techniques for manipulating human behaviour may already exist and that the behavioural scientist (in a Frankensteinian guise) may already be on the loose (Fransella, 1975; Packard, 1978). Yet curiously, although one might imagine from the most recent writings of Skinner and his followers that the study of the variables controlling human, as opposed to animal, performance is the experimental behaviourist's greatest concern, the fact of the matter is that there is a suprising paucity of articles published each year which deal with the basic analysis of human behaviour (see Buskist and Miller, 1982a for a literature survey). Indeed, when one looks at basic, laboratory operant research with humans, it would appear that the absolute amount that was published in the major behaviourally orientated journals between 1970 and 1980 was actually less than that published in the previous decade (Buskist and Miller, 1982b).

Despite this apparent dearth of basic human research, extrapolations of the kind noted above have been defended explicitly on the grounds that, "Past research has shown that nearly all the results of animal experimentation are just as true of humans as they are of animals" (Whaley and Malott, 1971). But is this assertion justified? On the contrary, the short answer would seem to be that, when the experiments that have been carried out with humans are studied carefully, the results obtained are markedly different in important ways to those obtained in analogous studies with lower species.

To see that this is so it will be necessary to examine this body of research in some detail. For our purposes it will be useful to look initially at the results of experimental studies carried out with human ADULTS and postpone the examination of work with other populations until later.

HUMAN PERFORMANCE ON SCHEDULES OF POSITIVE REINFORCEMENT: A SELECTIVE REVIEW

(i) Interval Schedules

As noted above, some of the most interesting effects of reinforcement occur when reinforcement is not scheduled for every response. There are many different ways of scheduling reinforcement. Sometimes a number of different schedules have been employed within one experiment, as, for example, in the case of multiple schedules (in which several different schedules are presented in series, each for a predetermined period and correlated with a particular stimulus) and in the case of concurrent schedule experiments, in which two or more schedules are available to the subject simultaneously.

Time is the major dimension through which behaviour varies. The importance of time in understanding the behaviour of both animals and humans cannot, therefore, be overestimated. A central issue in the study of animal behaviour is the way in which the animal's behaviour adapts to the temporal relationships between significant stimuli in its environment. Two types of schedules - interval schedules and schedules that differentially reinforce low rates of responding (DRL schedules) have proved fruitful sources of information on this matter (Richelle and Lejeune, 1979).

On a fixed interval schedule a reinforcement is given for the first response made a fixed interval of time or greater after the previous reinforcement. Animal performance on these schedules is typified by a pause after reinforcement, followed by a steady acceleration of responding until the next reinforcer is delivered (Ferster and Skinner, 1957). This pattern is known as a 'scallop' and is shown on the left of Figure 1. After lengthy exposure to the schedule, the post-reinforcement pause is sometimes followed by an abrupt transition to a high-rate of responding that persists until reinforcement. However, detailed analysis of this 'break and run' pattern has shown that it is, in fact, only a variant of the scallop in that, after the PRP, responding is positively accelerated (Dews, 1978; Lowe and Harzem, 1977). Either the scallop or the break and run pattern of responding have been found in a wide range of species including mice, prairie dogs, horses, guinea pigs, hamsters, bats, beagle dogs, rabbits, prosimians, chimpanzees, baboons, a number of monkey species, racoons, chickens, budgerigars, vultures, crows, ravens, gourami fish, goldfish (Richelle and Lejeune, 1979), newly hatched chicks (Marley and Morse, 1966) and even neodecorticate rats (Oakley, 1983). The few cases in which anomalous behaviour on FI schedules has been reported probably reflects problems of methodology rather than fundamental species differences (Richelle and Lejeune, 1979).

Various aspects of FI performance have been shown to be related in an orderly fashion to the interval length. The rate of responding during the interval, for example, has

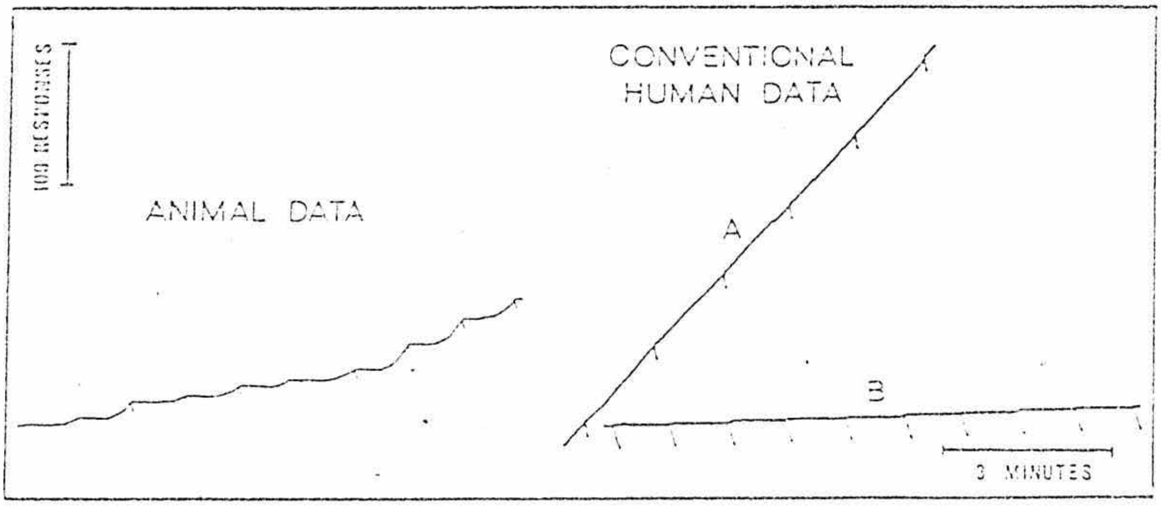

Figure 1: Cumulative records showing representative response patterning on fixed-interval schedules of reinforcement. From Lowe (1979).

been shown to decline with an increase in the interval length (Wilson, 1954) and the postreinforcement pause has been shown to be a negatively accelerated function of the schedule value (Branch and Gollub, 1974). It has been claimed that a number of important and less-than-important human activities reflect the operation of a fixed-interval contingency, for example checking to see whether the post has arrived (Logan, 1969), placing bets in a betting shop (Dickerson, 1979) and passing bills in the United States Congress (Weisberg and Waldrop, 1972) where, apparently, activity is very low after a recess but increases towards the end of a session. Similar interpretations of everyday phenomena in terms of the fixed-interval scallop can be found in most introductory text books in psychology (Poppen, 1982).

Although a majority of fixed-interval studies with humans have produced results differing widely from those obtained with animals, a small number of studies have claimed to report animal-like behaviour on these schedules in the laboratory. All of these have involved unusual response requirements or reinforcements and none have shown any relationship between responding and the schedule parameter comparable to those observed in animals.

Holland (1957, 1958a) reported placing five subjects on FI schedules varying between thirty seconds and four minutes as part of a research study of human vigilance. An observing response was used in which the subject was required to press a key to illuminate a dial in order to detect deflections of a needle. Cumulative records from a representative subject were presented, showing a pause after reinforcement, followed by a usually (but not always) abrupt transition to a terminal high-rate of responding. No numerical response measures were presented but the records of the one subject showed little variability with respect to running rate (the rate of responding during the interval excluding the post-reinforcement pause), although the post-reinforcement pause did seem to increase with the interval length. The cumulative records of a subject on a mixed FR 40 FI 3minute schedule were also presented and the interval components apear to be scalloped, although the grain is rough.

The observing response was also used by Azrin (1958) as part of a study of the effects of noise and by Laties and Weiss (1963) who were interested in the effects of a concurrent task on human operant behaviour. Azrin, interestingly, could not obtain a scalloped pattern when the response pressure required was fifteen grammes and had to increase the pressure to several hundred grammes. No detailed measure of responding was given for either condition and, from the cumulative records presented it seems that a pause-respond pattern was perhaps prevalent. This is apparently so, too, for the Laties and Weiss study (in which, in addition to the FI contingency a 'limited hold' was in operation, requiring the subjects to respond within a fixed time of the end of the interval), although less so for those sessions in which the subject was made to perform a concurrent subtraction task (see Chapter 3).

Two further studies in which a pause-respond pattern has been reported for human subjects involved multiple schedules, in which several different types of schedules (including, of course, in these cases fixed interval schedules) are presented in sequence, each correlated with a distinct discriminative stimulus. Schroeder and Holland (1968) used a variant of the signal detection task in which the responses measured were the subjects' eye movements as they glanced at a dial. The schedule used was a multiple FI-FR-DRL (FR and DRL schedules are explained below). Scallops are claimed for the fixed interval components and indeed, for two of the three subjects, the relevant portions of the cumulative records do appear to be positively accelerated. The third subject's performance also appears scalloped, but less so, perhaps because the relevant record is highly grainy. The second study, by Gonzalez and Waller (1974), involved a handwriting response. A multiple FI-limited hold FR schedule was used and responding in the FI component was pause-respond. Rate of responding was found to be insensitive to changes in the schedule value and it seems doubtful whether the resemblance to animal responding is anything other than superficial.

Set against these studies are a considerable number of experiments in which widely different results have been reported. Generally, these results have fallen into two distinct classes - a constant high-rate pattern of behaviour with little or no postreinforcement pause, and a low-rate pattern consisting of, for the most part, one response per interval, just as reinforcement becomes available. These patterns are shown on the right of Figure 1.

The high-rate pattern of responding was that observed by Azrin before he increased his manipulandum pressure. Blair (1958), using a variant of the Holland vigilance task in which the response was orientation of the head towards the dial, also obtained a relatively constant high-rate of responding in most of his subjects (one was described as behaving in a similar manner to Holland's subjects), although Blair's results have to be treated with caution because even less detail is given than by Holland.

Less equivocal examples of high-rate behaviour have been reported in a wide variety of FI conditions by Baron, Kaufman and Stauber (1969) and Weiner (1962, 1969, 1970b). For example, Baron et. al., using a multiple schedule with five different fixed interval components of monetary reinforcement obtained a constant high-rate of responding from their subjects. Weiner (1962), on the other hand, obtained comparable results using an observing response similar to that used by Azrin and Holland. Still further results of the same kind were obtained by Weiner in his subsequent studies using a more conventional manipulandum and point reinforcement, later exchanged for money. No relationship between response rate and interval value has been reported in any of these studies.

The low-rate pattern described above has been observed in a number of studies. For example, uninstructed subjects on a FI 27 second schedule in a study by Buskist, Bennett and Miller (1981) all responded at a low-rate. Both Bullock (1960) and Lippman (1973) have reported subjects responding at a low-rate in the FI component of multiple schedules.

Matthews, Shimoff, Catania and Sagvolden (1977) also reported examples of human FI performance which appear to be generally low-rate, except for an occasional scallop.

A number of studies have detected both patterns of human FI responding. For example, two out of three subjects in a study by Weiner (1964b) responded at a high-rate on FI 25 seconds and the third responded at a low-rate. Lippman and Meyer (1967) found that three out of ten uninstructed subjects on FI 20 seconds responded at a high-rate and the rest at a low-rate. However, a study by Buskist, Miller and Bennet (1980), which explicitly used food as a reinforcer in order to replicate as closely as possible those procedures used with animals produced both the low-rate and the high-rate patterns and, in the case of two out of seven of the subjects, an accelerated pattern of responding, similar to that found in lower species. Even greater variability in response patterning was reported by Lowe, Harzem and Hughes (1978), who found that some fixed-interval trained subjects responded at a low-rate, some at a high-rate, and some with some kind of combination of the two, or with an intermediate, moderate-rate pattern. (None produced scallops.) Similar results were found by Harzem, Lowe and Bagshaw (1978), using a response initiated FI (which requires a response after reinforcement to initiate the fixed-interval and which, in animals, has been found to produce similar behaviour to simple FI schedules). The type of performance produced by subjects - low-rate or high-rate - has also been found to be affected by a number of variables such as instruction (Lippman and Meyer, 1967), response cost (Weiner, 1962) and conditioning history (Weiner, 1969, 1970b), all of which will be discussed in the next chapter.

Interestingly, Lowe and his colleagues have identified one procedure - a variant of the fixed-interval schedule - which does seem to reliably replicate the results of animal studies with humans. The procedure used consisted of giving the subjects access to a DRL schedule (on which they were only reinforced if they waited a fixed amount of time before responding - see below) on one key, with additional access to a digital-clock (showing time into the interval) on a second key (Lowe, 1979). Thus subjects could use the second key in order to obtain information about the availability of reinforcement on the first. In effect, this amounts to a fixed-interval schedule of reinforcement, where reinforcement is access to points (later exchangeable for money) on the DRL key. It was found that performance on the clock key was scalloped and obeyed the same parametric relationships as those found in the animal literature. A binary-clock control group (the clock signalled whether or not reinforcement was available but gave no information about time) on the other hand produced a pause-respond pattern in which the pause was a fixed proportion of the schedule value. Lowe (Lowe, 1979; Lowe, Harzem and Hughes, 1978) has shown that this effect of the digital-clock still holds when an FI (instead of DRL) schedule is programmed on the reinforcement delivering key, or when only one key is used on which is programmed both access to the digital-clock and fixed-interval reinforcement. An interesting replication of this finding has been carried out from quite another perspective by Harris and Wilkins (1982) who observed clock checking behaviour in human subjects who had been asked to make a particular response at a particular time. The subjects were required to watch a film. They were also given several sheets of paper, on each of which was marked a target time. At the target time for each sheet the subjects were required to hold up the sheet towards a camera, which was recording what happened during the experiment. It was found that, as each target time approached the subjects checked the clock behind them with increasing frequency, producing a scalloped cumulative record.

These results, however, which have been obtained by a rather unusual technique, do not alter the general picture that has emerged. On FI schedules at least, even when free of the variables of instruction and reinforcement history briefly alluded to above, the dominant result is not one that easily supports the continuitist position.

The fixed-interval schedule is not the only variety of interval schedule in common use with animals. Variable Interval (VI) schedules are characterised by a varying time interval between reinforcement availability. On VI schedules animals typically respond at a relatively low but constant rate which varies inversely with the mean inter-reinforcement interval (Ferster and Skinner, 1957; Catania and Reynolds, 1968). Cumulative records are usually observed to have a rough grain. The very short postreinforcement pause, which usually can only be seen on close inspection, has been shown to be positively related to the mean interval length (Harzem, Lowe and Priddle-Higson, 1978).

Employing his observer response procedure Holland (1958a) was the first to investigate human VI performance, using his observing response paradigm. His cumulative records show a consistant, relatively high response rate, similar to that found in animals. Cumulative records shown by Lindsley (1960), who used a more conventional knob-pulling response with six adult subjects, also show a constant, high-rate performance, although Bagshaw (1980) has argued that the responding of some of Lindsley's subjects was much steadier and at a higher rate than that commonly found in animals. High-rates (consistently higher than on FI and less than those found with a pigeon control group) have also been reported by Bullock (1960) from his multiple schedule procedure, although from the data given it is not clear how constant and consistent the performance of his subjects was.

An exception to this general high rate picture of human VI performance has been reported by Zeiler and Fitz (1975), as part of a larger investigation. The baseline VI 33 seconds performances of their eight human subjects were briefly described, although cumulative records were not shown. In contrast to the data given by Holland, Lindsley and Bullock, Zeiler and Fitz reported that, although five of their subjects responded at a high rate (up to six responses per second), three of their subjects responded at less than one response every ten seconds. A similar picture of variability among human adult subjects responding on a VI schedule is given by Kaufman, Baron and Kopp (1966), as part of a study of the effects of instructions on human schedule performance (see next chapter). A control group of seven subjects was given only minimal instructions, reminding them to try and earn as many point-reinforcers as possible. (The actual schedule was a VI 1 minute.) The cumulative record shown for one of the subjects in this group shows a degree of unevenness not apparent in the studies mentioned above. Furthermore there was great variability in the subjects' rate of responding. One subject responded only once a minute on average, and another six times a minute. In contrast, the highest rate found in this group was one hundred and forty-four responses per minute on average! A great deal of variability in response rate is also shown by seven subjects who were 'shaped up' rather than instructed to use the response device - a manipulandum - in a study by Matthews, Shimoff, Catania and Sagvolden (1977). The subjects were also required to make a consummatory response, which Matthews et al. maintained was necessary in order to obtain animal-like responding (see below). The lowest final rate of responding recorded for these subjects was 21.2 responses per minute, whereas the highest rate was 145.5 responses per minute, nearly seven times greater. One subject even changed response rate abruptly during testing. Despite the results of Holland and Lindsley, then, it is not possible to conclude that adult subjects produce the high rate response pattern found in animals, although they often do.

Holland's study cited above is especially interesting because the two subjects tested were exposed to a range of VI values during the experiment. It was reported that the subjects' rate of responding decreased with increasing schedule value - a result identical to that found with animals. A similar 'matching' of response rate to rate of reinforcement has been reported by Weiner (1962) and Bradshaw, Szabadi and Bevan (1976). In contrast, however, Wearden, Lochery and Oliphant (1982) replicated almost exactly the procedure used by Bradshaw et al. and failed to produce matching. Moreover, great variability existed in their subjects' day to day performances and, by selecting the data from five of their six subject's best 'matching day' it was possible to create a picture not greatly different from that obtained from animals. For two of these five subjects, the days on which they matched least followed the days on which their behaviour was most animal-like, suggesting that, for these subjects at least, there was no trend towards matching as time went on.

A matching relationship between response rate and rate of reinforcement has been frequently reported in the animal literature with respect to concurrent VI schedules (de Villiers, 1977). A concurrent schedule is one which allows the subject to choose between two alternative and independent schedules of reinforcement. There are two oft used ways of arranging such conditions. In one, the subject is allowed access to two separate response devices delivering separate reinforcement. In the other the subject responds on only one response device but is allowed to use a seperate switch to change from one of the schedules in operation to the other. A change over delay (COD) - a period after change-over during which reinforcement is not available - is quite frequently used with both methods to prevent the adventitious pairing of reinforcement with the change-over. On concurrent VI schedules animals generally match both their rate of responding on each schedule (Hernstein, 1961) and their time of responding on each schedule (de Villiers, 1977) according to the relative probability of reinforcement on each. Rachlin (1980) has argued that this finding is relevant to human behaviour in situations in which decisions are made relatively automatically, for example when a choice is made between two supermarkets, one of which is closer but more expensive than the other. Recently, however, there has been a certain amount of controversy about whether human adults obey this 'matching law'.

A number of studies have claimed matching with adults on concurrent VIs, for example: Schroeder and Holland (1969), recording eye movements on a vigilance task; Baum (1975), measuring absolute time spent watching two displays on a vigilance task; and Conger and Kileen (1974), measuring verbal behaviour to two differentially reinforcing experimenters during a group conversation. In a series of studies Bradshaw and his colleagues at Manchester (Bradshaw, Szabadi and Bevan, 1976; Bradshaw, Szabadi, Bevan and Ruddle, 1979; Bradshaw, Ruddle and Szabadi, 1981; and Ruddle, Bradshaw, Szabadi and Bevan 1979), measuring response rates on conventional manipulanda, have also reported matching, as have Buskist and Miller (1981), also using conventional manipulanda but using food as a reinforcer. In addition, Cliffe and Parry (1980), having used concurrent VIs to quantify a paedophile offender's preference for slides of women against slides of men, followed by his preference for slides of men against slides of children, used a version of the matching law to predict with reasonable accuracy the subject's preference when given a choice between the slides of women and the slides of children (which had not previously been presented together). The matching law has been successfully used to make similar predictions of preference with animals (Miller, 1976; Farley and Fantino, 1978).

In contrast, a number of studies have claimed varying degrees of deviation from the matching law with adult human subjects. Catania and Cutts (1963), using a concurrent VI-Extinction schedule, found that subjects pressed frequently on the non-reinforcing key, although the addition of a changeover delay of three seconds did cut down such 'superstitious' responding. Schmitt (1974), using the changeover switch method, found that the relative response rates of his five subjects did not closely match the relative rates of reinforcement. (Schmitt also failed to find matching when reinforcement magnitude rather than reinforcement rate was varied, but failure to match according to reinforcement magnitude has also been found in the animal literature, eg. by Fantino, Squires, Delbruck and Peterson, 1972). Wurster and Griffiths (1979), also using the changeover switch method, found some sensitivity to rate of reinforcement but significant deviation from the matching law from two out of three subjects. Oscar-Berman, Heyman, Bonner and Ryder (1980), in a study of the VI performance of patients suffering from Korsakoff's disease (a form of brain damage caused by continuous excessive alcohol ingestion), found that a normal control group performed with great variability, undermatched significantly and, in the case of one of the five subjects, even exhibited negative matching (ie: response rate was inversely related to rate of reinforcement). Pierce, Epling and Greer (1981), using a procedure similar to Conger and Kileen's, also found negative matching in three out of five subjects. Finally, Horne and Lowe (1982), using a variety of concurrent VI procedures in six seperate experiments with over thirty subjects, found wide deviations from matching, including, on occasion, exclusive preference for one schedule, a result unknown in the animal literature.

A careful study of those publications for which matching has been claimed complicates the overall picture still further. For example, in Baum's (1975) study, the data from three of the seven conditions in the main experiment - nearly half the data - were discarded "because they produced performances that tended to be unrepresentative of later performance". In the study by Schroeder and Holland (1969), despite their claim that the data was animal-like overall, one subject out of the six changed over with unusual rapidity until the changeover delay was increased from one to two and half seconds and, when this was done, two of the other subjects displayed almost exclusive preference for the schedule delivering most reinforcement. Finally, it is worth noting that, with the two studies involving verbal behaviour cited above (Conger and Kileen, 1974; and Pierce, Epling and Greer, 1981) it is difficult to see how the experiments could have been made to appear remotely like a normal conversation. (The reader might like to speculate how he would feel if two people differentially praised his or her own verbal behaviour, irrespective of its content.) Interestingly, although Conger and Kileen reported that none of their subjects were aware of the contingencies, Pierce et al. in a follow-up note (Epling, Pierce and Greer, 1982) reported that, during their experiment, one of the experimenters was not cued to deliver reinforcement appropriately due to an equipment failure. Despite the fact that one subject was not reinforced for a ten minute period she exhibited no change in her rate of responding. Epling et al. concluded that her behaviour must therefore have been a function of the demand characteristics of the experiment (ie: she did what she thought she was required to do in order to be a good subject) rather than the schedule. The implication was that the same could well have been true of the behaviour of the other subjects.

A final issue worth considering with respect to concurrent schedules is the often all-too-difficult problem of what is or is not to count as animal-like behaviour anyway. In the Ruddle et. al. (1979) study, only two subjects showed near to perfect matching, two overmatched and two undermatched. The results were deemed to be animal-like, as indeed they were. But HOW animal-like exactly? Bradshaw, et. al. (1981), in their review of the literature on human concurrent VI performance cite the Oscar-Berman et. al. (1980) study as showing evidence of matching in humans, whereas Wearden et. al. (1982) conclude the opposite from the same study. Oscar-Berman et. al. did not explicitly commit themselves either way, concluding that, "Other studies of normal human concurrent performance.... have obtained greater correspondence to the matching law for reinforced behaviour.... but there was still a positive correspondence between response and reinforcement ratios." Lest it be thought that the present writer is suggesting bias on behalf of Bradshaw and his colleagues, it should be noted that they cite the Wurster and Griffiths (1979) paper as an example of failure to match, whereas Wurster and Griffiths say, "Although matching was not obtained responding changed in the same way it frequently does in animal studies in which qualitatively similar reinforcers are delivered in both components." Matching, it would seem, is, at least in part, something which is as much in the eye of the experimenter as it is in the behaviour of the responding organism.

(ii) Differential Reinforcement of Low Rates

Interval schedules, as mentioned above, are not the only means by which temporal discrimination can be investigated. On an interval schedule the subject is not penalised for responding before reinforcement becomes available. However, it is possible to set up such a schedule. On a differential reinforcement of low-rates (DRL) schedule, reinforcement is given for the first response occuring at or beyond a fixed amount of time after the previous response. In other words, subjects are only reinforced if they wait a certain amount of time before responding. On DRL schedules, animals generally respond at a low-rate, matching their responses to the interval duration, but sometimes producing premature bursts of responses (Blackman, 1974; Harzem, Lowe and Davey, 1975). In addition to rats and pigeons, species in which this pattern has been observed include gerbils, mice, guinea pigs, hamsters, prosimians, monkeys, crows and goldfish, although great differences have been found in the length of the DRL interval to which each species can successfully adapt (Richelle and Lejeune, 1979).

As with other schedules, the earliest report of human DRL performance was made by Holland (1958a), using his observer response technique. Two subjects performed at DRL 30 and, like animals, their responding was described as low-rate, with occasional short bursts of responding just before reinforcemnt became available. Schroeder and Holland (1968) also reported DRL responding, both for a DRL 10 second condition early in the experiment and for a DRL 10 second component of the multiple schedule that comprised the major part of their study. In both cases, as with Holland (1958a), the two subjects responded at a generally low-rate with occassional bursts. Lane (1960) also reported animal-like responding on DRL schedules, using a vocal response. However, such animal-like behaviour has not always been reported in the literature.

In particular, Weiner (1964c, 1965a, 1969, 1970b) has reported a series of studies, using a more conventional telegraph key response, in which human subjects responded with much greater efficiency than animals. For many subjects, after a short period on the schedule, every response produced a reinforcer, ie: they were able to space their responses to match the schedule value with great accuracy.

It should be noted that the kind of behaviour observed by Weiner is very similar in appearance on the cumulative record to animal responding. What differentiates it from animal behaviour is the accuracy of the temporal discrimination - premature bursts of responding do not occur and each response is made just after reinforcement becomes available.

One particular area of interest that has attracted much research attention with respect to DRL schedules is the question of collateral behaviour. Animals have often been observed to reliably perform various stereotyped patterns of behaviour (eg. moving about the cage, climbing) between responses. Furthermore, providing subjects with the opportunity for collateral responding (eg. with blocks of wood to chew) has been shown to improve the efficiency of the animal's performance (Laties, Weiss and Weiss, 1969). Does the availability of alternative responses have a similar effect in humans? Generally, the answer has been a qualified 'yes'. Bruner and Revusky (1961), for example, using a DRL 8.2 second schedule with a 2.05 second limited hold found that their four subjects (high school boys) pressed three irrelevant keys in a constant pattern. However, a number of researchers have questioned whether such collateral behaviour is 'mediational' or 'superstitious'. The distinction revolves around whether or not the collateral responses function to allow the subject to time the interval or whether they ultimately have no effect on responding but merely reflect the adventitious reinforcement of irrelevant behaviours which fill in the vacuum between reinforcement producing responses.

Stein and Landis (1973) argued that, if collateral responses have a mediating role then disruption of them should lead to a disruption of responding on the relevant key. Using apparatus similar to that employed by Bruner and Revusky and a DRL 5 second schedule, they denied their subjects access to the three irrelevant keys halfway through the experiment. Fifteen of their twenty female undergraduate subjects responded on the irrelevant keys during a five minute period prior to their removal, and the responding of all these subjects was adversely affected by the change - generally, response rates became higher. The subjects who made few responses on the irrelevant keys were, as expected, little affected by their absence and, across all subjects, a modest but significant negative correlation was discovered to exist between the number of collateral responses made in the first phase and efficiency of responding in the second.

Flynn and Tedford (1976) further addressed this issue, arguing that if collateral responding was genuinely mediating DRL responding, then increasing the response effort required for the collateral response (thus slowing its execution) would decrease responding on DRL, whereas reducing the collateral response effort would increase the DRL response rate, thus adversely affecting responding. On the other hand, they argued, if the collateral response was merely superstitious, then disrupting such responding by either method would decrease DRL efficiency. Subjects were placed on a stationary excercise bicycle, on one handle bar of which was placed the response button, with the hope that pedalling would manifest itself as a collateral response - a hope justified in the case of nine out of the ten subjects.

The effort required for pedalling was remotely adjusted and, for six of the subjects, responding on the DRL 20 second schedule usually (but not always) changed in the expected direction, providing broad support for the mediation hypothesis.

It is worth considering at this point, whether the distinction between superstitious collateral behaviour and collateral behaviour with a mediating function has any validity, and whether the kind of collateral responding found in man is the same (functionally) as that found in animals. This is an issue which will be returned to in the next chapter.

For the moment however, it can be noted that, as was the case with interval studies, the results of studies of DRL responding provide little comfort for the continuity theorist.

(iii) Ratio Schedules

So far we have looked at temporal discrimination - the ability of the subject, whether animal or human, to make his responses in accordance with the various temporal relatinships that exist in his environment. But reinforcement need not necessarily be contingent upon the passage of time. Indeed, it is comparatively easy to think of circumstances under which the availability of reinforcement depends upon the amount of responding that has already occurred. On a fixed ratio (FR) schedule, reinforcement is given when a subject has responded a certain fixed number of times after receiving the previous reinforcement. On these schedules animals commonly pause after reinforcement and then respond at a relatively high constant rate until the next reinforcer is delivered (Ferster and Skinner, 1957). It has been shown that the length of the postreinforcement pause on a FR schedule is a direct function of the schedule value (Felton and Lyon, 1966).

Skinner (1953) has suggested that many examples of fixed ratio schedules can be found in operation in Western culture. The most obvious example he gives is 'piece-rate'. On the assumption that such contingencies will generate the high-rate of responding found in animals, Skinner notes a number of ethical dilemmas presented by their use: "A fixed-ratio schedule may, in fact, be too effective. It leads not only to high levels of activity, but to long working hours, both of which may be harmful. A bricklayer paid in terms of the number of bricks laid may 'burn himself out' in a few years. Another objection.... is that the increased return to the worker which follows conversion to such a schedule often seems to justify increasing the ratio."

There have been a number of laboratory studies of human fixed ratio responding. Bullock (1960) reported that, on his multiple schedule, the rate of responding by his normal subjects during the FR component was indeed greater than their rate of responding during VI, and a generally high-rate of responding by human adults on FR schedules is the picture given in the literature overall.

Holland (1958a), with his vigilance procedure, tested seven subjects on a variety of fixed ratio schedules, ranging from FR 36 to FR 200. There is little evidence that the rate of responding, as evidenced by the representative cumulative records shown, declined with schedule value. Nor is there evidence of the regular postreinforcement pauses found in the animal data. Instead, PRPs were few and far between and entirely absent at the higher schedule values. High-rates with only brief pauses were also found by Schroeder and Holland (1968) on their multiple schedule (the FI performance on which was described above) on which the recorded responses were eye movements. In a series of studies by Weiner (1964a, 1964c, 1965a, 1970b) human performance on FR schedules was consistently found to be high-rate with no postreinforcement pause. A similar pattern of responding was reported for two FR subjects in the study by Matthews et. al. (1977) cited above (although a few pauses do appear at the end of the one printed record).This absence of PRPs is a marked departure from the animal literature. In addition, from the one FR cumulative record published in the Matthews et. al. study, it is not possible to discern a difference in overall rate between the brief segment of FR 30 shown and subsequent FR 120 performance.

Weiner (1966) investigated human preference when given a choice of ratio schedules. He used a two component multiple FR schedule, with the addition of a separate switch which the subject could use at any time to changeover to the schedule not then in operation. (This arrangement is similar to the changeover method of presenting concurrent schedules, with the main exception that the ratio schedules are regularly changed independently of the subject's performance on the changeover key.) Various ratios were used and, in one condition, the rate of reinforcement was kept equal on both components while the magnitude of reinforcement on one component was made four times as great as on the other. It was found that the subjects consistently used the changeover switch to change to the component with the lowest ratio or the greatest magnitude of reinforcement. These results are consistent with animal data collected from similar experiments. Interestingly enough, there is no indication that, in the baseline conditions in which no changeover switch was present, the subjects' rates of responding differed in different FR components, although in some cases it is difficult to tell for certain from the records given.

A similar result to Weiner's with respect to ratio preference was found by Striefel (1972), using the dual response-device method of presenting concurrent FR schedules. At discrepant ratio values all the subjects showed exclusive preference for the lower ratio schedule. (One subject did, however, show near to exclusive preference for one manipulandum, even when no difference existed between the ratios.)

Ratio schedules do not necessarily have to be fixed - the number of responses required to obtain a reinforcer can be varied in a way analogous to the way in which the interval is varied in a VI schedule, the result being a variable ratio (VR) schedule. On variable ratio schedules animals usually respond at a very high constant rate with extremely short pauses after reinforcement (Ferster and Skinner, 1957; Priddle-Higson, Lowe and Harzem, 1976).

Skinner (1953) and Blackman (1974), among others, have suggested that the variable ratio schedule may be the schedule effectively in operation on fruit machines and that this may account for the high-rate of responding of some humans thereon. (In fact, the scheduling of fruit machines may be somewhat more complex. The magnitude of the reinforcer, for example, varies greatly, and a subject's performance on such a machine is presumably affected by a number of variables in addition to reinforcement.) Furthermore, it has been suggested by a number of authors that VR schedules might prove to be an even more potent method of maintaining high work rates in industry than ratio schedules (Aldis, 1961). That said, few investigations of human VR performance have actually been carried out.

In the Weiner (1966) study cited above, one condition allowed the subjects to choose, via the changeover switch, between a VR 40 component and a FR 40 component. The subjects showed no peference either way, a result contrary to the findings of a number of animal studies (Fantino, 1964). In addition, there is no evidence from the records of any pausing after reinforcement.

One way in which VI and VR behaviour have been compared in the animal liturature is by yoking two such schedules together. One animal responds on a VR and another animal paired with it, responds in a Skinner box in which reinforcement becomes available only when the animal in the VR condition has received reinforcement. Thus, both animals receive reinforcement at the same rate and at almost identical times. Yet one is exposed to a ratio contingency and the other to an interval contingency. When this happens it has been found that the rate of responding of the VR animal is generally higher than the rate of responding of the VI animal (Zuriff, 1970).

This was done in the Matthews et. al. (1977) study discussed above with respect to VIs. The VI data described above was in fact generated by yoking a VI subject to another subject responding on a VR schedule. In nearly all cases, the cumulative records of the VR subjects appear to be constant high-rate, although it is difficult to tell whether there was any postreinforcement pause. For all the subjects whose performances were shaped up (some subjects were instructed and the data from these will be discussed in the next chapter) the rate of responding of the VR member of the pair was considerably greater (as much as ten times greater and more) than the rate of the VI member.

It thus appears that, as we saw with other schedules, the results of studies of human ratio performance provide little comfort for the continuity theorist.

CLASSICAL CONDITIONING AND THE LIMITS OF THE CONTINUITY HYPOTHESIS

If the data from human schedule research is disconcerting to those who would wish to regard human behaviour as continuous with that of animals, then what about the results of studies of classical conditioning? On examination, it turns out that the results of such studies have been equally anomalous.

Perhaps the most famous (or infamous) reported case of classical conditioning in a human subject was reported by Watson and Rayner (1920), who conditioned an infant, Little Albert, to fear white furry objects by making a loud, frightening noise while the child played with a white rat. As has already been mentioned, Watson was in fact tireless in his efforts to describe complex human activities in terms of sequences of habits which could in turn be reduced to conditioned reflexes. One of his most famous examples went to the heart of human creativity:

> "Why can't those of us, who are not literary, write a poem or an essay? We can use all the words the literary man uses. The answer is, it is not our trade, we do not deal in words, our word manipulation is poor; the literary man's is good. He has manipulated words under the influence of emotional and practical situations of one kind or another, as we have manipulated the keys of the typewriter or a group of statistics, or wood, brass and lead.... How does Patou build a new gown? Has he any 'picture in his mind' of what the gown is to look like when it is finished? He has not, or he would not waste his time making it up; he would make a rough sketch of it or he would tell his assistant how to make it. In starting upon his work of creation, remember that his organisation about gowns is enormous. Everything in the mode is at his finger tips, as is everything that has been done in the past. He calls his model in, picks up a new piece of silk, throws it around her; he pulls it in here, he pulls it out there, makes it tight or loose at the waist, high or low, he makes the skirt short or long. He manipulates the material until it takes on the semblance of a dress. HE HAS TO REACT TO IT AS A NEW CREATION BEFORE MANIPULATION STOPS. Nothing exactly like it has ever been made before. His emotional reactions are aroused one way or another by the finished product. He may rip it off and start over again. On the other hand, he may smile and say, 'Voila, parfait!" In this case the model looks at herself in the mirror and smiles and says, 'Merci, monsieur.' The other assistants say, 'Magnifique!'.... Not until the new creation aroused admiration and commendation, both his own (an emotional reaction either verbalised or unverbalised) and others' would manipulation be complete (the equivalent of the rat's finding food." (Watson, 1924; emphasis Watson's.)

In recent times, with the development of improved experimental methods, the whole issue of classical conditioning in humans has become highly controversial (cf. Dawson, 1973; Brewer, 1974; Davey, 1983). Despite a few cases in which animal-like classical conditioning has been reported (Bridger and Mandel, 1965; Mandel and Bridger, 1967; Fuhrer and Baer, 1969, 1980; Wilson, Fuhrer and Baer, 1973;) the results of laboratory studies of human classical conditioning have been almost universally problematic. A small example of the anomalies observed can be found in a number of studies in some subjects have failed to condition (ie: to develop a conditioned response to a conditioned stimulus). Dawson and Biferno, 1973, for example, failed to produce a conditioned response in some subjects when the UCS was a shock, the CS a tone, and the CR a galvanic skin response (a change in skin resistance correlated with anxiety). As we shall see, these are not the only anomalies that have been observed in the human classical conditionig literature.

And so, the picture with respect to classical conditioning is not all that different to that with respect to operant conditioning. Possible reasons for these results will be discussed in the next chapter, in which the effects on operant and classical conditioning of certain factors, present in humans but absent or of less importance in animals, will be discussed. It is in the examination of these factors that the strongest case against a simple continuity hypothesis can be made.

CHAPTER 3: SOME VARIABLES AFFECTING HUMAN PERFORMANCE

In Chapter 2 it was shown that human schedule performance is not always similar to that produced by animals and thus presents problems for a continuitist account of human learning. This finding, however, is only a beginning. Unless it can lead us to information about what it is about human learning that makes it different from learning in infra-human species it will remain nothing more than an intriguing anomaly of interest to academic operant conditioners but of not much relevance to anyone else. As a step toward understanding why humans are unique in this respect it will be necessary to examine some important variables that affect human schedule performance. These variables consist of verbal instructions given to the experimental subjects, the subjects´ awareness (or lack of awareness) of the contingencies, the cost of making a response and the subjects´ history prior to conditioning. Only when these factors have been examined will it be possible to proceed to a theoretical account of what it is about humans that makes us, in the operant chamber and outside, unique.

(i) Instructions and Self-Instructions

One of the most obvious and, therefore, important variables absent in animals but intrusive in human operant research is that of instruction. A number of research studies have specifically examined this problem. One of the earliest of these was carried out by Holland (1958b), who found that, while most of the subjects run on FR schedules in his previous studies (1957, 1958a) responded at a high, constant rate, one did not. On the contrary, on FR 36 he responded at a low-rate and on FR 60 the shape of his cumulative record was like an inverted ´S´ - high-rate at the beginning and end of the interval, low-rate in the middle. Holland reported that this subject counted out responses and his rate of responding was paced to his counting. When two further subjects were instructed to count out responses, it was found that they produced no postreinforcement pause, and that responding accelerated towards the end of the interval. One of these subjects responded, overall, at a much faster rate than the other.

Weiner (1970a) also used FR schedules to investigate the effects of instructions, in his case using rate of extinction following withdrawl of the reinforcer as the dependant variable. Weiner gave his subjects information about the number of reinforcers available that either accurately or inaccurately predicted the onset of the extinction condition. As might be expected, those subjects who had been correctly informed ceased responding much more rapidly than those subjects who had been led to believe that more reinforcers would be forthcoming.

Comparable results have been obtained when instructions have been used in conjunction with other schedules. In the study by Lippman and Meyer (1967), in addition to the non-instructed subjects on a FI 20 second schedule, three subjects were given high-rate instructions ("After getting a point you can get another by pressing a certain number of times. The number of button presses will vary somewhat from point to point.") and three were given low-rate instructions ("After getting a point, you can obtain another by pressing the button after a certain amount of time has elapsed. The amount of time will vary from point to point."). In most cases the pattern of responding obtained, high-rate and low-rate, reflected exactly the instructions given.

Various attempts have been made to investigate the effects of different types of instructions in more detail. For example, Kaufman, Baron and Kopp (1966) gave undergraduate subjects on a VI 1 minute schedule either (i) minimal instructions, (ii) response instructions telling the subject which key to press (iii) VI instructions specifying a VI contingency in non-technical language, (iv) FI instructions and (v), VR instructions. The reinforcing event was access to two choice keys, one of which the subject was required to press. These keys, in turn, produced point reinforcement at random but with increasing probability throughout the experiment in order to support the subjects' belief that they were taking part in a concept-learning experiment and (presumably) succeeding in learning the concept. The results for responding on the main response key were roughly as expected: subjects given VR instructions responded at the highest rate, with those given VI and response instructions close behind. Those given FI instructions responded at a rate even lower than those given minimal instructions. Thus performance was related to instructions, rather than the contingency alone. Baron, Kaufman and Strauber (1969) further investigated the effects of various combinations of instructions and feedback on the performance of adults on a multiple FI 10, FI 30, FI 90, FI 270, Extinction schedule. Those subjects who were instructed about the contingencies and given information about the amount of reinforcement earned (which would be collected at the end of the experiment) responded at appropriately low rates. Those given instructions but no feedback responded at a high-rate on FI 10 but at decreasing rates on higher values. Those given no instruction but feedback responded at a high-rate on all but the FI 270 and extinction schedules and those given neither feedback or instructions responded at a high-rate throughout. Interestingly, once responding had been established, giving feedback or instructions when they had previously been absent produced little effect, although the addition of response cost (discussed separately below) did serve to bring down rates.

A rather more exotic study of the effects of different types of instructions on simple FI 27 second performance was carried out by Buskist, Bennet and Miller (1981), who told some subjects to respond a certain number of times in each interval, to complete each interval within a certain time interval, or some combination of both. The results reflected the instructions given although some individual differences were observed. For example, of those told to respond between twenty and twenty three times in each interval, three produced pause-respond patterns whereas two subjects responded at a high-rate, paused and then pressed once more to collect reinforcement. Of those given the same instruction but who were asked in addition to complete twenty intervals within nine minutes and ten seconds one subject responded at a slow, steady rate and four produced inverse scallops (negatively accelerated responding) without any kind of postreinforcement pause.

More complex schedules have been employed to study the effects of instructions, with broadly the same kinds of results. T.W. Frazier and Bitetto (1969), using an observer response, found that uninstructed subjects on a concurrent FI, FR, DRL schedule were sensitive to the different schedules (ie: responded appropriately on each component schedule) but that similar subjects on a concurrent FI, VI, DRL schedule were not (they produced VI type responding on each component). Instructing the subjects about the contingencies, however, produced clearly differentiated responding on the latter schedule, including positively accelerated responding in the FI component. In another study, J.R. Frazier (1973), using a complex variant of a FR schedule in which, during some conditions, reinforcement was delayed by up to 15 seconds demonstrated a similar effect. Subjects who were instructed that the delay depended on their rate of responding responded at a lower rate than those given correct or minimal instructions during the delay conditions. However, the difference between the three groups was non-significant during the no-delay conditions.

A final and perhaps extreme example of the way in which instructions can affect operant behaviour involves several studies in which hypnotic suggestion has been used to affect schedule performance. Ferster, Levitt, Zimmerman and Brady (1961), for example, used suggestions such as 'You are afraid' and 'You are getting tired' to depress responding on a form of VI 3 minute schedule. More recently, Baer (1979) used time-slowing suggestions to decrease the rate of responding of five suggestable subjects on a similar schedule.

It is perhaps not surprising to note that adult human classical conditioning has also been found to be strongly influenced by instructions. For example, Dawson and Grings (1968), using a tone as the CS, a shock as the UCS and change in skin conductance as the response, found that apparent classically conditioning can be demonstrated by informing a subject that a relationship exists between the CS and the UCS, even if this is not the case. Wilson (1968), also using shock and measuring changes in skin conductance, exposed subjects to two differently coloured lights, one of which was correlated with shock, the other of which was not. Subjects were then instructed that the relationship between the shock and the lights would be reversed and on subsequent trials (in which no shock was in fact given) the subjects responded accordingly, thus demonstrating both instructed conditioning and instructed extinction. Instructed extinction has also been reported by others (eg. Colgan, 1970), as has an instructed reduction in the magnitude of the conditioned response in the continued presence of the UCS (Prokasy and Allen, 1969). More recently, Dawson, Catania, Schell and Grings (1979) demonstrated an instructional analogue of Kamin´s (1969) blocking effect. Subjects were trained using a combination of coloured lights and tones as the conditioned stimuli, shock as the UCS and changes in skin conductance as the CR. They were subsequently told that either the lights alone or both the tones and the lights would preceed shock. Responding was appropriate to the instructions.

It thus appears that instructions can radically affect a human adult subject´s performance in both operant and classical conditioning experiments. In a sense, this is to say no more than that people will usually do what they are told to do. However, the conclusion is important, even though it is commonsensical, because the effects of instructions have often been ignored when attempts have been made to assimilate human behaviour, whether it be economic, pathological or whatever, to the animal model. Instructions clearly have no bearing on animal performance. Just what the significance of this finding is for the experimental analysis of behaviour in general will be discussed below. However, it is worth noting that some, for example Matthews et al. (1977) have taken this finding to be central to an understanding of human-animal differences, arguing that almost any instructions can have a major effect on outcome and that the only safe way to try and obtain animal behaviour is to resort to response shaping in which the subject is successively reinforced for greater approximations to the desired response during the initial phase of an experiment. To support this contention they have provided data for both uninstructed pairs of subjects and pairs of subjects to whom the response was initially demonstrated on their yoked VI-VR experiment. The pairs of subjects who were shaped produced generally more animal-like responding, both in terms of response patterning and in terms of the difference in rate between the two members of each pair. However, it should be noted that shaping the human subjects, unlike shaping animals, proved to be an extremely rapid process.

(ii) Awareness and Beliefs About the Contingencies

Related to the question of how performance is affected by instructions is the question of how it is related to the subject´s own awareness of the contingencies and his beliefs about the determinants of his own behaviour.

Some social psychologists, particularly those interested in ´attribution theory´, have collected data relevant to the present discussion. ´Attribution theory is a term originally given to the study of the causes people attribute to the behaviour of others (cf. Kelley, 1967; Jones, Kanouse, Kelley, Nisbett, Valins and Weiner, 1972; Nisbett and Ross, 1980). However, attribution theorists have also turned their attention to the ways in which people percieve their own behaviour. From this standpoint it has been argued that a person´s performance may be related to the causes to which he attributes his success or failure at a particular task (Weiner, Frieze, Kukla, Reed, Rest and Rosenbaum, 1972). A number of early studies in which beliefs about contingencies were manipulated and the effects on performance observed can be interpreted in this light. To take one example, James and Rotter (1958) required subjects to report whether they had seen an X or an 0 during trials in which stimuli were presented very briefly, using a tachistoscope. False feedback was given so that subjects were told that they were correct either on all trials (CRF) or on fifty percent of trials (VR2). In addition, subjects were told that success depended upon luck or that it depended on skill. After a number of trials no further responses were reinforced. Not surprisingly, resistance to extinction was greater in the luck condition for those subjects who had been reinforced on the VR schedule. In the skill condition, however, the reverse was true and those who had been reinforced on CRF persisted responding for longer than those who had received partial reinforcement. (This finding has obvious implications for theories of gambling and suggests that attention should be paid to the gambler's beliefs as well as to the contingencies operating in his environment).

A more commonly used way of investigating the relationship between beliefs about contingencies and performance has been to question the subject during or after an experiment. This technique has yielded important and interesting data. However, it might be objected that it amounts to asking the subject to introspect about his reasons for behaving as he does. It might further be objected that asking subjects to introspect is an odd and unreliable move for behaviouristically orientated researchers to make - after all, did not Watson (1924) formulate behaviourism as a direct challenge to the introspectionist schools of the time? In answer to this it should be noted first of all that Watson´s objections were aimed at approaches to psychology that relied exclusively on introspection - thus denying the possibility of resolving differences of opinion by reference to public data. Since Watson´s time, a number of theorists have argued that introspection, in a limited form, may be acceptable, useful or even vital to the furtherance of psychology (Bakan, 1954; Joynson, 1970, 1972, 1974; Radford, 1974; Valentine, 1982). Radford, for example, notes that the use of introspection has not always been viewed as in conflict with scientific rigour, that it does not denote just one activity and that, within any particular setting, it is difficult to tell a priori whether any method, be it introspection or anything else, will be reliable.

Of particular importance here is the question of what is being inspected when introspection occurs. According to the old view, the view objected to so vehemently by Watson, what is being inspected is the mind or the soul.

Another view that might be defended is that it is the inside of the body that is some how being observed. However, according to the celebrated argument of Wittgenstein (1953, see also Kenny, 1973) and formulated in a slightly different way by Skinner (1969) it is not possible to construct a language that describes private events directly. This is precisely because language is a social activity and description depends upon the public use of terms. Private events and processes can only be described by metaphor. (A ´sharp pain´ is the sort of pain that would be felt if a sharp object penetrated the flesh.) Attribution theorists in particular have taken this objection seriously, arguing that people often infer the cause of what they do by observation of their own overt behaviour and their surroundings (Nisbett and Valins, 1972). Thus, it has been experimentally demonstrated that a person´s evaluation of his own emotions is often based on cues external to himself; that, for example, his description of his behavior when aroused (as threatened, anxious or excited) depends on the cause he attributes to his arousal (Schacter and Singer, 1962; cf. Pennebaker 1980 for a recent evaluation of this line of research). Similaly, Bem (1967) has argued that people describe their own attitudes partly on the basis of observation of their own overt behaviour.

An important and controversial study in this context, which addressed the problem of introspection directly, was carried out by Nisbett and Wilson (1977) who asked subjects to describe what they did and their reasons for doing so after a number of standard undergraduate-type psychology experiments. While the subject´s reasons almost invariably tied with what they thought they had done, their descriptions of what they had done were often wildly inaccurate. On the face of it, it might be argued that this evidence strongly suggests that nothing useful will come out of questioning subjects after operant conditioning experiments. However, it should be said that attempts to replicate Nisbett and Wilson´s results have sometimes been problematic (cf. Ericsson and Simon, 1980; Grover, 1982). Moreover, for other reasons the case for scepticism is not so clear.

In the first place, as Radford emphasises, it is better not to make hasty a priori judgements before the data has been examined. In the second case the above discussion reveals the extent to which the term 'introspection' has sometimes been misapplied. Not all cases of 'introspection' involve subjects attempting to describe the determinants of their own behavior or, for that matter, their own internal 'mental', 'cognitive' or covert states. First person reports in psychophysics, for example, do not fall into this category. In the case of operant research, to bring the discussion back to the work under review, it may be simply muddle-headed to apply the Nisbett and Wilson formula. Thus, when a subject is being asked to state his reasons for behaving as he does, he is not being required to look inside himself and observe something else (his 'reason'), he is simply being asked to state his belief about the contingencies in operation. Whether or not the reasons a subject delivers (or, in other words, his disposition to describe the experiment in a particular way) are related to his performance is a matter for empirical investigation.

As a matter of fact, the results of questioning subjects about their reasons for performing as they do has revealed a close correspondance between their verbal reports and their actual behavior. For example, both Lippman and Meyer (1967) and Leander, Lippman and Meyer (1968) found that non-instructed FI subjects who reported an interval formulation of the contingencies responded at a low-rate whereas those reporting a response-based formulation responded at a high rate. A similar relationship has been observed in other studies of FI responding, for example those studies carried out by Lowe and his colleagues (Harzem, Lowe and Bagshaw, 1978; Lowe, Harzem and Bagshaw, 1978; Lowe, Harzem and Hughes, 1978; Lowe, 1979) and by Buskist, Bennett and Miller (1981).

On other schedules, performance has also been found to relate to the subjects' awareness of the contingencies. Thus Wearden et al. (1982) reported that several of their subjects developed time-based conceptualisations of the contingencies and that their performance related to the conscious strategies they developed on VI schedules. One subject reported using a complex strategy involving counting the passage of time and pressing the response button a certain number of times.

DRL schedules have proved to be a particularly fruitful source of subjects' self-reports. Bruner and Revusky (1961) reported that their subjects (who, it will be recalled, were provided with three irrelevant keys in order to assess their collateral behaviour) all believed that responses on the irrelevant keys were necessary to produce reinforcement. Flynn and Tedford (1976) in their excercise bicycle study similarly reported that all their subjects believed that pedalling (the collateral response) was involved in the task. Catania and Cutts (1963) reported a similar relationship between formulation of the contingencies and inappropriate responding on the extinction component of their concurrent VI-Extinction experiment. (One subject, for example, reported being aware that he was on a VI schedule but that he believed that the required response was two presses of the extinction key, followed by one press of the VI key.)

The relationship between collateral responding and formulation of the contingencies was explicitly investigated by Stein and Flanagan (1974), who asked their subjects to write down their thoughts about the experiment at three minute intervals while responding on a DRL 5 second schedule. All but one of the ten subjects reported a time based hypothesis. The type of verbalisation produced by the end of the study was not related to final efficiency but time based verbalisers produced much less collateral behaviour on the three irrelevant keys.

A similar but more complex experiment was carried out by Catania, Matthews and Shimoff (1982), who required subjects to make written guesses about the contingencies while responding on a multiple random-ratio, random-interval (Mult RR-RI) schedule. Points were awarded not only for responding on the schedule but also for the guesses. In some cases, guesses were differentially reinforced to encourage either more accurate or less accurate formulations of the contingencies. In other cases, guesses were simply instructed (accurately or inaccurately, eg. "Write ´press slowly´") or reinforced at a fixed rate regardless of accuracy. The results of these procedures were varied. Generally, successfully shaped guesses tended to be reflected in corresponding changes in the rate of responding, even when such responding was inappropriate to the schedule (eg. low-rate responding on the RR component). Little systematic variation in response rate was found in those subjects who were reinforced non-differentially for guessing or in those subjects who had failed to shape. Great variability, on the other hand, was found in the results of those instructed to write down their guesses in a particular way. In some cases, responding was sensitive to the changes in schedule, in some cases it was not. In some cases responding varied with guessing and in other cases it did not.

A number of interesting issues are raised by such reports linking contingency awareness to schedule performance. The first concerns the relationship, between the subject´s apparent conceptualisation of the contingencies and his actual strategy. Clearly, a relationship between contingency awareness and overt performance exists (as detailed above) but does a subject´s formulation of the schedule - his hypothesis about what is ´going on´ - reflect any internal, ´mental´ strategy that he might be employing to mediate his own behaviour? In a number of studies of FI and DRL responding, subjects explicitly mentioned using such strategies, suggesting that such a connexion exists. For example low-rate responders in a number of FI studies (Laties and Weiss, 1963; Lowe, Harzem and Hughes, 1978) and DRL studies (Flynn and Tedford, 1976) reported counting out the length of the interval. Interestingly enough, although counting seems to have been the most common strategy used it was not the only one. One of Laties and Weiss´ subjects reported using Shakespeare´s verse to time the interval, whereas Flynn and Tedford´s subjects reported "counting - both forward and backward, stereotyped motor responses and singing the first four bars of the school fight song." Now it is immediately conceded that these reports need to be treated with some caution by virtue of the arguments outlined above. However, there are several reasons for believing that these particular self-report data are reliable. In the first place, Wittgenstein´s private language argument does not apply to reports of counting and similar activities because counting (being verbal) is in principle an observable activity (even if in some cases it is covert). Thus there is no reason to believe that subjects cannot learn to describe their own counting (just as they can describe the counting of others). In the second place, overt counting has actually been observed on some occasions, for example by Holland (1958b).

It might be expected that, if such strategies are so important in determining the pattern of human operant behaviour, then any concurrent task affecting the subject´s ability to regulate his own behaviour verbally might have a major effect. Some studies have addressed this issue. For example, Laties and Weiss, in their 1963 study, made their subjects perform a concurrent subtraction task, the effect of which was to alter their behaviour in different ways. For three of the subjects in particular the effect was to make the pattern of responding more scallop like, in two cases by decreasing the terminal rate of responding on FI 100 seconds and in one case by increasing the rate of responding early in the FI 150 second interval. All the subjects in this study reported using some kind of strategy - most often covert counting - to time the interval. One (the subject who reported reciting Shakespeare to himself at first) later used subtraction itself to minimise the effects of the concurrent task during a latter phase of the experiment. Lowe (1979) reported a similar study in which the pause of low-rate responders on a FI 120 second schedule was sometimes dramatically reduced when a subject was required to perform a concurrent shadowing task (in which he was required to repeat random numbers as they were played through headphones). Interestingly, the behavior of subjects whose responses produced digital clock stimuli (see Chapter 2) was much less disrupted.

Given that contingency awareness is related to overt performance and also to certain covert strategies used by adult human subjects in operant experiments, those circumstances in which a subject´s apparent belief about the contingency and the actual contingency are at variance call for attention. As we have seen, this has been seen to occur in a number of studies, particularly those involving time based schedules. High-rate responders on interval schedules who report a response-based formulation of the contingencies represent one obvious example. Subjects who produce superstitious or collateral behaviour on DRL schedules can be thought to represent another. Indeed, the relationship between contingency awareness (or misawareness) and such collateral behaviour brings the whole concept of human superstitious behaviour into question. What may have been ´superstitious´ in these studies is the subjects´ hypotheses about what the schedule involves - overt behaviour appears to have followed from the hypotheses. If this is the case, then the kind of responding observed on irrelevant keys in DRL experiments with human adults, although passingly similar to the collateral responding observed in animal studies, may be functionally very different.

In passing, it is worth noting that, as was the case with the effects of instructions, a relationship between awareness and conditioning similar to that found in the operant literature has been found in research on human classical conditioning. For example, Baer and Fuhrer (1968, 1970) found that successful classical conditioning was observed only in those subjects who reported awareness of the contingencies during a post-experiment interview. In order to investigate this effect in more detail, Dawson and Biferno (1973) devised an ingenious method by which their subjects´ awareness of the contingencies during a classical conditioning experiment could be assessed as the experiment progressed. The CS was the last tone in a series of tones, the UCS a shock, and the response measured was the galvanic skin response. The experiment was described as an auditory perception task in which the relationship between physiological arousal and perception was to be measured, the purpose of the electrical shock being to enhance physiological arousal. Seven tones were presented in each trial. The subjects were asked to say whether the fourth tone was the same as the first and whether the last tone was the highest or lowest of the series. Subjects were also asked to report their expectancy of electric shock by pressing one of five buttons labelled according to various degrees of certainty from ´certain no shock´ to ´certain shock´. Subjects were also given a questionnaire at the end of the experiment in order to further assess their awareness of the contingencies. In general, it was discovered that conditioning occurred only when awareness was reported, a result that was repeated by Biferno and Dawson (1977).

This discussion of the role of awareness in operant conditioning connects with a related issue which has recently been a matter of some controversy - namely whether, in humans, conditioning without awareness can occur at all. Arguing that it cannot, Brewer (1974) has claimed that, "There is no convincing evidence of classical or operant conditioning in human adults." Brewer argued his case with a survey of a wide range of human classical and operant conditioning studies, all of which cannot be covered in the following discussion. Briefly, his claim was that, in those studies, in many cases where conditioning appeared to have occurred, awareness had been demonstrated, or at least NONAWARENESS had not been demonstrated. Non-conditioning, on the other hand, seemed to be related to an absence of awareness.

Brewer´s argument is interesting precisely because of the importance that he assigns to awareness. According to Brewer, the concepts of classical and operant conditioning explicitly preclude the possibility that a subject may be aware of either the contingencies that are operating or his own behaviour. Thus, the existence or non-existence of conditioning in human adults is seen to depend entirely upon whether conditioning without awareness can be demonstrated.

We have already seen that, with respect to classical conditioning, there are a number of studies in which only aware subjects appeared to condition. Within the operant literature, a number of early studies were also devised in order to test whether conditioning without awareness could occur. The most famous of these involved the conditioning of verbal responses, and indeed it was claimed that it was possible to alter what a subject said without allowing him or her to become aware of the contingency (Greenspoon, 1955; Verplanck, 1955). For example, Greenspoon had his subjects say a series of nouns to the experimenter, who regularly said either "mmm-hmm" or "huh-uh" following either plural or singular responses. It was found that "mmm-hmm" was an effective reinforcer of both response classes, whereas "huh-uh" was not. At the end of the experiment subjects were questioned and there appeared to be no evidence of awareness of what had happened. Interestingly, Traux (1966) analysed transcripts of client-centred psychotherapy sessions, carried out by Carl Rogers, and found a similar sort of effect - Rogers typically followed different types of statements made by his client with different types of verbal responses so that the number of positive statements made increased throughout the session. Of course, in this case it was not possible to assess the awareness of the client.

However, as Brewer pointed out, the post-experimental questionnaires used by Greenspoon and Verplanck were not particularly searching. Levin (1961) used more rigorous questionnaires, beginning with very non-specific questions but moving on to more direct questions at a later stage, and in a similar study to Greenspoon´s (subjects were reinforced by the word "Good" following the use of certain pronouns) found that only those subjects who were aware appeared to condition. Similar results were obtained by Dulany (1961) Eriksen (1962) and Spielberger and DeNike (1966). Attempts have been made to condition other responses (for example eyeblinks - see Grant, 1973 - and covert finger movements - see Hefferline and Keenan, 1963) but again Brewer has argued that either conditioning has not been demonstrated or nonawareness has not been demonstrated. Does this mean that Brewer´s arguments have to be accepted? Probably not.

Taking the empirical arguments first, Brewer has overstated his case. Despite all the cases to the contrary, there are a few convincing examples of classical conditioning without awareness (and to destroy Brewer´s argument there need only be a few). Fuhrer and Baer (1969); and Wilson, Fuhrer and Baer (1973), for example, succeeded in producing evidence of conditioning without awareness in subjects using masked designs (in which the true nature of the experimental task was hidden from the subjects). Furthermore, Bridger and Mandel (1965); Mandel and Bridger (1967); and Fuhrer and Baer (1980) found that galvanic skin responses, in subjects who had been conditioned using shock, did not extinguish immediately even though the subjects were told that no more shocks would be given and the electrodes were removed. These latter results have led Dawson (1973) to argue that, whereas the aquisition of a response may be mediated by awareness, subsequent performance may be automatic.

Within the operant conditioning literature, there are also some convincing examples of conditioning without awareness. Kennedy (1970) argued that conditioning may precede awareness, which may be the result of subjects observing their own responses. Using a verbal conditioning task and asking the subjects to write down their thoughts as the trials progressed, he found that, in aware subjects who appeared to condition, an increase in responding occurred before the subjects were able to verbalise the contingency. A similar result has been found by Mercier and Ludouceur (1980).

Perhaps the most convincing example of operant conditioning without awareness, however, was demonstrated by Rosenfield and Baer (1969), using a ´double-agent´ procedure. The subject was a psychology postgraduate who had read the conditioning without awareness literature and was sceptical. He was recruited to take part in a conditioning without awareness experiment in which he was to be the EXPERIMENTER. He was instructed to hold a series of structured conversations with his ´subject´ which would involve him making a number of prompts in order to keep the subject talking. After a baseline period, and consultation with the authors, it was decided to try and reinforce the subject´s tendency to stroke his chin by means of discrete nods. Of course, it was the subject who was the real experimenter, and who stroked his chin to reinforce the pseudoexperimenter´s use of certain prompts. Conditioning occurred and the pseudoexperimenter only caught on - quite suddenly and with great surprise - when a hidden recording switch used by the real experimenter began to squeak. Regular and intensive questioning was carried out throughout the experiment - the pseudoexperimenter was encouraged to discuss pseudodata and was asked for his opinions about how the pseudostudy was progressing. At one stage he reported being fairly confident that his pseudosubject was conditioning, at other times he was not too sure, but until the end he never mentioned any suspicion that ´the joke´, so to speak, was on him. Rosenfield and Baer (1970) repeated this result using a version of the double-agent procedure designed to remove any possibility of experimenter bias. Two psychology undergraduates were this time asked to condition a non-fluent speaker to speak more fluently via an intercom. Actually, the subject in this case - who was supposed to voice nouns to the pseudoexperimenters - was two tape recorders. If the pseudoexperimenters began a trial with a certain prompt the fluent tape was turned on, rather than the non-fluent tape. Again, conditioning occurred and awareness was not detected, even using the extensive questioning procedure recommended by Levin.

That it is difficult to demonstrate conditioning without awareness is not surprising. In the case of verbal conditioning, as Eriksen (1962) has pointed out, conditioning without awareness may not be possible because the criterion for awareness is so closely tied to the response. More generally, as Dixon (1966) has suggested, post-experimental questioning may well produce awareness. Thus, failure to detect unaware conditioning may possibly be an artifact of the type of close questioning favoured by

Levin and Brewer. An intriguing extension of this argument has been made by the attribution theorists Nisbett and Wilson (1977) on the basis that subjects sometimes report being aware of things they have not, in fact, done. Nisbett and Wilson maintain that in any experiment subjects will try and formulate a coherent account of what is going on. With respect to conditioning experiments, Nisbett and Wilson argue, it is almost impossible to stop subjects from accurately observing their own behaviour. Therefore, the subjects' verbal reports will almost always match their performance.

Finally, Brewer gives hardly any weight at all to the results of experiments in which subjects who are unable to speak have been conditioned. Thus, successful classical conditioning has been demonstrated with the severely retarded (Hogg, Remington and Foxen, 1979), as has operant conditioning (Fuller, 1949; Rice and McDaniel, 1966; Keehn, 1967; Rice, McDaniel, Stallings and Gatz, 1967; Rice, 1968; Murphy and Doughty, 1977; Remington, Foxen and Hogg, 1977, Haskett and Hollar, 1978; and Murphy, Doughty and Nunes, 1979). Operant conditioning has also been demonstrated with people asleep (Lindsley, 1957); with people recovering from anesthesia (Lindsley, Hobika and Etster, 1961); and also, of course, with infants (Lancioni, 1980 - and see Chapter 5). It seems unlikely that whatever mechanisms we may presume to underlie conditioning should suddenly cease to function at some point in childhood (except in brain damaged individuals), or upon awakening from sleep or the operating table. It also seems unsatisfactory to attribute awareness to severely brain damaged, sleeping or just young individuals for the only criterion of awareness (as Brewer uses the term) is what the subject says. And if it should not be speech, then why not attribute awareness to animals - in which case we equate awareness with reactivity and either hold that it is irrelevant to conditioning or reach the non-sensical conclusion that animals cannot condition.

To some extent Brewer concedes the weight of these arguments. As he points out, there is considerable anecdotal evidence of conditioning without awareness in alert human adults. What is difficult is demonstrating it in the laboratory. The interesting questions are: What is awareness? (Brewer leaves awareness relatively unanalysed.) Assuming that the conditioning mechanisms in animals have been carried over to the adult human brain, then why is it so difficult to demonstrate their pure function? Perhaps processes unique to man - higher cognitive processes that develop in chldhood - in some way intervene when humans are placed in the conditioning laboratory. This is an issue we will return to at the end of the chapter.

(iii) Response Cost

Another variable that has been found to affect performance greatly is the cost of a response. As Bagshaw (1980) points out, in practice the term ´response cost´ has referred to a number of generally aversive events consequent on or contiguous with each response, including the initial expenditure of effort necessary to make the response, mild aversive stimuli such as noise and a direct debit of reinforcement. It is not clear that these are functionally the same.

Response effort was implicitly manipulated by Azrin (1958) when he increased the force required to make an observing response from fifteen grammes force to several hundred grammes force, thus changing the pattern of his subjects´ responding on FI from high-rate to scallop-like. Azrin also gave subjects on FI and FR schedules a short burst of noise every time they made a response and the effect of this procedure was to lengthen the post-reinforcement pause.

In a long series of studies, Weiner investigated the effects of response-produced loss of reinforcement on a variety of schedules. For example, Weiner (1962), giving 100 points reinforcement at the end of each interval (later exchangable for money), found that, on a FI 1 minute schedule, a 1 point loss of reinforcement made contingent on every response was sufficient to switch subjects' responding from the high-rate FI pattern to the low-rate pattern. On the other hand, Weiner (1964a) found that a similar point-loss contingency on a FR 40 produced little effect.

Weiner has shown that point loss can have an effect on VI schedules (1962) and during extinction after exposure to FI schedules (1964b). In both cases, response rate was reduced in the cost condition but returned to some extent when the cost contingency was removed.

Some of the most dramatic effects of point loss were demonstrated by Weiner (1962) by varying cost during the inter-reinforcement intervals on a FI schedule. Seven different variants of a FI 1 minute schedule were used: (i) no cost; (ii) cost; (iii) cost/no cost in alternate intervals; (iv) alternating 30 second periods of cost and no cost - the first 30 seconds of each interval being cost; (v) alternating 30 second periods with the first period in each interval being no cost; (vi) alternating 15 second periods of cost and no cost with the first 15 second period in each interval being cost; and, finally, (vii) alternating 15 second periods with the first period in each interval being no cost. Two subjects (previously) trained on FI, performed for 2 hours on each of these conditions, a white light signalling the operation of a cost contingency. Subjects were then exposed to a seven component multiple schedule consisting of ten minute periods of each of the above conditions for 140 minutes daily. The resulting cumulative records reflect the presence or absence of cost in each of the conditions to a surprising degree. For example, when cost occurred in the first half of each interval but not in the second, a break-and-run pattern was produced. When no cost occurred in the first thirty seconds a reversed break-and-run pattern or ´knee´ was produced. In the fifteen second alternating cost/no cost conditions even multiple break-and-runs and multiple knees were produced.

Interestingly enough in the light of our discussion of the effects of instructions and awareness (above), Weiner (1965b) found that, if subjects were instructed to imagine a response cost contingency on a FI 10 second schedule, the rate of responding decreased. However, the decrement was not as great as that produced by the same subjects in a real cost condition.

(iv) Conditioning History

A final variable that has been found to radically influence schedule performance in humans is a subject's conditioning history. Little information is available about the effects of conditioning history on animal behavior. Some effects of prievious schedule experience on performance on fixed time schedules (in which reinforcement is given at regular intervals, irrespective of responding) have been investigated (Richelle and Lejeune, 1979) although the effects that have been identified seem to be highly species specific (Lowe and Harzem, 1977). With more conventional schedules, Skinner and Morse (1958) have described transient effects on FR and FI performance after extensive training on DRL schedules. Patterning on FR in particular was found to be grainy (as revealed by the cumulative records) and on FI responding after the postreinforcement pause was relatively low-rate (although the usual scallop pattern could be discerned). None the less, after a short exposure to FI, the effects of the previous schedule had diminished. More recently, Urbain, Poling, Millam and Thompson (1978), as part of a drug study, ran rats on either DRL 11 seconds or FR 40 and then transfered them to FI 15 seconds. Again, an effect due to history was observed. In particular, the rate of responding on FI of the FR history subjects was high compared to that of the DRL history subjects and the pauses after reinforcement were shorter. None-the-less, the data given (in terms of mathematical descriptions of curvature) indicates that, for subjects with both histories, some degree of scalloping was present soon after the change to the FI schedule. With increasing exposure to FI the FR subject´s performance became more accelerated and the rate of responding dropped. Similarly, the rates obtained from the DRL history subjects increased.

Weiner, in a long series of studies, has investigated the effects of conditioning history in man. For example, Weiner (1964c) placed three adults on a FR 40 schedule and three adults on a DRL 20 second schedule. The response was movement of a microswitch and the reinforcement was points, exchangable for money. After training, the subjects were all moved on to a FI 10 second schedule. Those who had previously experienced FR responded at a constant high rate whereas those who had experienced the DRL responded at a low rate. As shown in Figure 2, similar results were found by Weiner (1969) and, in addition, it was discovered that the experience of a DRL contingency, either before or after exposure to a FR contingency, almost totally removed the effect of exposure to the FR on subsequent FI performance.

Weiner (1965a, 1969, 1970b) has further investigated the interaction of these history effects and the effects of response cost (outlined above). One of the effects of FR histories, it was discovered, was to maintain high rate responding on a FI schedule, despite response cost. Thus two subjects exposed to FR 40 continued to respond at a high-rate on FI 10 seconds with a one point response cost, despite the fact that, in doing so, they substantially reduced the amount of reinforcement they were receiving (Weiner, 1965a). Subjects exposed to FI 10 seconds (without cost) or DRL 20 seconds showed no such ´maladaptive responding´. In the same study, Weiner investigated the effects of FR 40 and DRL 20 second histories on VI 10 second performance and found that the former, as expected, led to high-rate responding and the latter to low-rate responding. In this case it was the low-rate responding on the VI

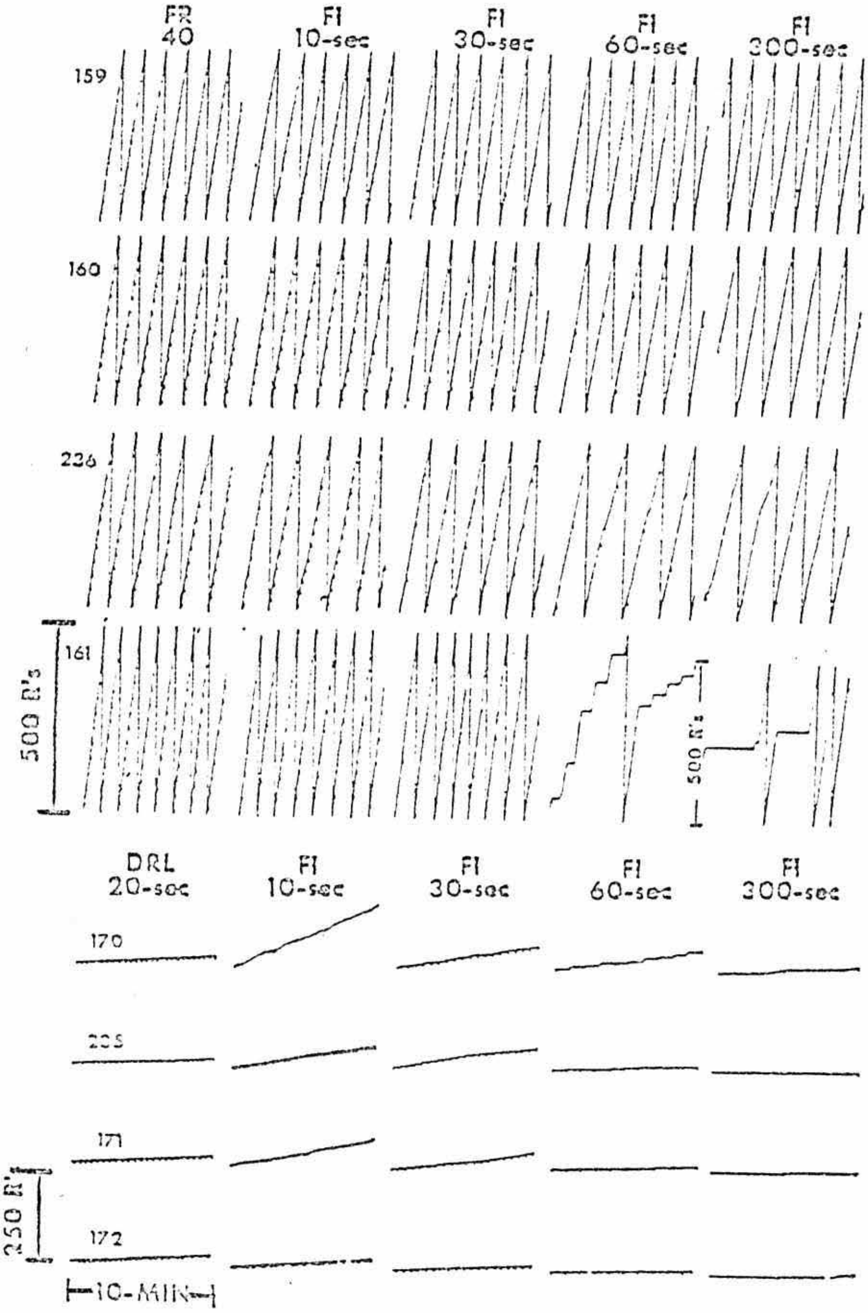

Figure 2: FI responding of human subjects following histories of FR or DRL reinforcement. From Weiner (1969).

(caused by the DRL history) which was maladaptive since the most efficient way to respond on a VI schedule is at a moderately high rate (in order to obtain each reinforcer just as the interval is up). Weiner (1970b) has labelled this kind of behaviour ´human behavioural persistence´ (the term ´behavioural rigidity´ has also been used) and suggested that it (and conditioning history) may be related to certain pathological behavioural repertoires found in psychiatric patients.

The relationship between conditioning history and collateral behaviour was investigated by Stein (1977), whose subjects received conditioning histories of FR 20 or FI 10 seconds with cost before moving on to DRL 10 seconds. As might be expected, those subjects who had previously responded on FR were less efficient on the DRL, responded more on the irrelevant keys provided and were more affected by the removal of those keys than those subjects who had previously responded on FI. The FR history, it would seem, had left them with a disposition to respond at a high rate and, therefore, to produce collateral behaviour.

The effects of conditioning history, as outlined above, help to make sense of human performance on some other, complex schedules. For example, Weiner (1972) trained two subjects progressively on a mixed FI 10 second-DRL 10 second schedule, a mixed FI 10 second-FR 80, and a mixed DRL 10 second-FR 80. Not surpringly, on the mixed FI-DRL schedule, both subjects learned to respond at a constant low-rate, on the mixed FI-FR they responded at a constant high-rate, and on the mixed DRL-FR they alternated between the low-rate and the high-rate patterns.

Similar history effects have been claimed for concurrent schedules. Frazier and Bitetto (1969) found that most subjects on concurrent FI, DRL and VI schedules (subjects had a choice between THREE schedules) responded at a low-rate whereas responding on concurrent FI, DRL and FR schedules was more variable - some subjects apparently learned to respond differentially to the different schedules and responding on the FI component was generally higher than in the concurrent FI-DRL-VI condition. However, there was a great deal of intersubject variability in the results and not all studies of human concurrent responding have produced data so directly consistent with a history effect interpretation of performance. Sanders (1969), for example, obtained low-rate behaviour in the FI component from subjects on concurrent FI and FR schedules (although the subjects of this experiment had previous experience of DRL and, remembering Weiner's results, we might hypothesise that this was responsible for the outcome). More directly contradictory to Frazier and Bitetto's results were the results of a study by Poppen (1972) in which subjects were trained, in different conditions, on both concurrent FI-DRL schedules and concurrent FI-FR schedules. Although the results were complex, responding in the FI component concurrent with the FR schedule was generally low-rate compared with responding in the FI component concurrent with the DRL schedule. As a result, Poppen reached the conclusion that the effects of concurrent schedules represented yet another way in which human learning differs from that of animals.

In a more recent study, however, Poppen (1982) looked at human behaviour on concurrent FI-FR and FI-DRL schedules in further detail. On the basis of two experiments, Poppen noted an interaction between responding on the two components. Initial FI responding was, in general, either high rate or low rate, both for subjects on concurrent FI-FR and for subjects on concurrent FI-DRL. However FI-FR, subjects who were initially exposed to schedule values that allowed one or more ratios to be completed before reinforcement became available on the FI component typically responded at high rates on the FR component early in the FI interval, only switching to the FI component towards the end of the interval. Subjects initially exposed to a low FI value and a high FR value, however, tended to respond at a high rate on FI and to ignore the FR component altogether. Similarly, subjects on a FI-DRL schedule, in which the FI value was initially high compared to the DRL value responded at a high rate on FI, switching to the DRL component about three times each interval, thus ´pacing´ their responding on DRL. However, if the initial FI value was very large compared to the DRL value (so that the pacing strategy became very costly) subjects tended to respond on DRL only.

When the parameters of the various components were changed, Poppen found that subjects tended to persist with the pattern of responding already established in much the same way that Weiner´s subjects had done on FI schedules following FR or DRL. Poppen thus concluded that history effects played a major role in determining human behaviour on concurrent schedules.

SOME EXPLANATIONS

What conclusions can be reached from our survey of the human operant literature? It seems quite clear that, on schedules of reinforcement, human behaviour is anomalous when compared to that of animals. The response pattern of human adult subjects is rarely the same as that of animals in comparable circumstances, and when it is so the similarity is usually superficial. Finally, human operant behaviour is affected by a range of factors which have little or no role in determining animal behaviour - in particular, instructions. This latter variable is, of course, entirely absent in the life of animals and, as has been noted in the previous chapter, has often been the focus of theories of man's uniqueness.

A number of people have attempted to formulate a coherent theoretical account of these differences between human and animal performance and to bring them within the range of broadly behaviouristic approaches to human functioning. Until quite recently, these attempts have focused only on particular phenomena, rather than on the whole range of anomalies outlined above.

For example, Weiner (1973) drew attention to the human failure to respond to response cost after exposure to FR schedules and argued that this could be accounted for by reference to (i) conditioning histories that fail to provide subjects with appropriate behavioural repertoires (hence the non-occurrence of behavioural persistence after exposure to DRL schedules); and (ii) current contingencies that fail to make net gains of reinforcement dependent on change. Yet this analysis is obviously flawed for the following reasons: it addresses only FI performance, although it could conceivably be extended to account for anomolous behaviour on other schedules; it ignores altogether the effects of instructions and the importance of awareness; and it begs the very question at issue as the human response to conditioning histories is one of the anomalies in need of explaination.

Another attempt to bring human schedule performance within the range of animal learning theory lies in the claim by some authors, eg. Matthews et. al. (1977) that the absence of a consummatory response in human subjects could be the source of many of the differences found between human and animal behaviour. In their study of human VR and yoked VI performance, Matthews et al. found the behaviour of subjects who were required to make a consummatory response -

in the form of a button press to claim their reinforcer - to be closer to that of animals (as reflected in the cumulative records and the different rates of responding in the yoked subjects) than the behaviour of subjects who were not required to make a consummatory response. However, in other studies in which a consummatory response has been required of subjects, wide deviations from animal-like behaviour have been observed. Thus, Buskist, Miller and Bennet (1980) used food as a reinforcer on an FI schedule and obtained high-rate respondng from some subjects, low-rate responding from others, and animal-like responding in only two of their seven subjects.

Matthews et al. have further pointed out that, whereas animal subjects are typically shaped up to respond on schedules of reinforcement, in humans the response is usually established by the use of instructions. They thus argued that, in the case of human subjects, where the reinforcer used is typically not very powerful, the effects of the instructions often over-ride the effects of the contingencies per se. As detailed above, to test this, they compared the performance of most of their subjects - who were response-shaped - to several for whom the response was demonstrated, and reported more animal-like performances for the shaped subjects.

As noted by Lowe (1983), although this account, with its emphasis on the effects of instructions, has much to recommend it, it still leaves much to be explained. Anomalous performances are often produced when subjects are not instructed or receive only minimal instructions. Furthermore, the effect of instructions alone is not sufficient to account for the persistent history effects found with human subjects. Lowe (1979) has argued that the experimenter is not unique in his capacity to formulate instructions. According to this view, the subject comes to the laboratory with an ability to formulate his own verbal hypotheses about the contingencies in operation. These hypotheses, in the form of self-directed instructions, subsequently comes to control the subject's performance. The merit of this suggestion lies in the way it can be used to account for non-instructed human performance. Thus, according to the theory, subjects on FI schedules, even when uninstructed, tend to reach their own conclusions about the nature of the experiment. The theory can also be used to account for behavioural persistence or rigidity. Subjects exposed to FR contingencies, for example, may formulate response-based conceptualisations of the contingencies (eg.

"The more I press, the more I get,") which subsequently carry over to control later behaviour on FI schedules.

It is perhaps not surprising that classical conditioning theorists have at times advanced similar hypotheses to account for the anomalies reported in the human classical conditioning literature. Thus, a ´two processes´ account of classical conditioning has been advanced (cf. Razran, 1971; Baer and Fuhrer, 1973; Mandel and Bridger, 1973), according to which human classical responding is controlled both by those associative processes found in animals and shared by man, and also by higher cognitive process that are unique to man. The two process theory thus manages to explain the importance of awareness and instructions in determining a human subject´s response to paired stimuli, while at the same time allowing for occassional examples of apparently animal-like conditioning without awareness.

These theories - Lowe´s verbal regulation theory of human operant behaviour and the two process theory of human classical conditioning - have a long, shared intellectual pedigree. As we have seen, most of Pavlov´s life as a psychologist was concerned with describing and explaining the formation of conditioned reflexes - the process we now know as classical conditioning - and thus accounting for learning in animals (Pavlov, 1927; Gray, 1979 - see Chapter 2). However, towards the end of his life, Pavlov developed an interest in psychiatry and sought to extend the theory of conditioned reflexes to account for human psychopathology. It soon became apparent that conditioned reflexes, at least as they were found in animals, were not sufficient to explain the full range of human behaviour and it was in this manner that Pavlov came to theorise about human-animal differences.

According to Pavlov (1941), two separate ´signalling systems´ - neurological mechanisms linking perceptual input to motor (response) output - can be identified, one shared by both animals and man (the first signalling system) and the other exclusively possessed by the human species (the second signalling system).

> "When the developing animal world reached the stage of man, an extremely important addition was made to the mechanisms of nervous activity. In the animal, reality is signalized almost exclusively by stimulations and by the traces they leave in the cerebral hemisphere, which come directly from the special cells of the visual, auditory or other receptors of the organism. This is what we, too, possess as impressions, sensations and notions of the world around us - with the exception of the words heard or seen. This is the first system of signals of reality common to man and

> animals. But speech constitutes a second signalling system of reality which is peculiarly ours, being the signal of the first signals. On the one hand, numerous speech stimulations have removed us from reality, and we must always remember this in order not to distort our attitude to reality. On the other hand, it is precisely speech which has made us human.... However, it cannot be doubted that the fundamental laws governing the activity of the first signalling system must also govern that of the second, because it, too, is activity of the same nervous tissue."

Nor was the importance of speech missed by Watson, the founder of behaviourism; for, although Watson´s treatment of human behaviour is often thought to exclude reference to private events such as thinking, nothing could be further from the truth. Thus, for Watson (1924):

> "The behaviorist advances the view that what the psychologists have hitherto called thought is in short nothing but talking to ourselves."

It was MENTALISM that Watson objected to, rather than what we commonly refer to as ´mental events´. Watson went on to suggest that, during childhood, the trick of speaking covertly so that no one can hear is rapidly learnt, and as evidence for this he cited the observation that young children often talk to themselves out loud. Furthermore, Watson too noted a difference between learning in animals and learning in man (although perhaps he assigned it less importance). When discussing learning curves obtained from animals, for example, Watson (1924) maintained that:

> "This animal curve of learning illustrates the details of learning possibly better than one taken from the human field because most of the curves showing the learning of humans contain many complications. When we work with rats we keep the stimulus fairly constant.... The human being becomes bored while learning. Other things stimulate him. Inner speech (thought), for example, may always be a disturbing factor."

This view, that language mediates behaviour, was subsequently taken up and elaborated by future generations of behaviourists, such as Kuenne (1946); Kendler (1964); and Reese (1962). It also appeared in the writings of B. F. Skinner (1957, 1969), who drew a distinction between contingency governed behaviour and rule governed behaviour. Thus, according to Skinner (1969):

> "Analyses of contingencies are related to behavior.... when they are used as guides or rules. The behavior of a person who has calculated his chances, compared alternatives, or considered the consequences of a move is different from, and usually more effective than, the behavior of one who has

> merely been exposed to unanalysed contingencies. The analysis functions as a discriminative stimulus. When such a stimulus is perfectly correlated with reinforcement, the behavior under its control is maximally reinforced.... Many proverbs and maxims are crude descriptions of contingencies of social or nonsocial reinforcement, and those who observe them come under more effective control of their environment."

Clearly if self-generated verbal rules are an important determinant of human behaviour, then evidence should be available from other sources to indicate that language is of great importance in regulating human behaviour. It is to such studies of the relationship between language and motor responding that we must turn next.

CHAPTER 4: LANGUAGE, SELF-REGULATION AND DEVELOPMENT

Psychologists have long been preoccupied with elucidating the relationship between language and thought. The kinds of questions that have been asked are: Can thought occur without language? Does language act as a mental tool useful but not necessary for thinking or is the essence of all thought words? And in the domain of developmental psychology the question of whether cognitive development precedes language development, succeeds it, or emerges independantly of it has been hotly debated. Yet the issues are, to some extent, definitional. Ryle (1979), for example, argues that:

> "In its sweeping form, this doctrine (that thinking is just saying) is false. A delirious man babbles things but is not thinking; nor am I thinking what I am saying when I have a jingle or a catch-phrase or a ritual formula running in my head or off my tongue. Conversely, the composer at the piano and the child at the jig-saw puzzle are thinking; but they need be saying nothing aloud or to themselves; and if they are saying anything, their saying it probably does no good, and may do some harm."

None-the-less, there are those who would say, against Ryle, that if I have a ritual formula running in my head or off my tongue, I am thinking of the formula. Furthermore, a clinical psychologist may diagnose a patient´s babbling, depending on the circumstances, as ´thought disordered´. All this, however, is not to cast doubt on Ryle´s objection. Animals are clearly capable of a wide range of activities without the aid of speech. If we allow these activities to be described as ´thoughtful´ then thought can obviously proceed without language. On the other hand, man appears to be the possessor of unique skills that may (or may not) crucially depend upon his ability to use words. The issue, then, is not one of whether thought is independant of speech but one of the extent to which the ability to use language transforms the range of man´s activities. If man can perform intellectual tasks with the aid of language which he cannot perform without, then we can say that some forms of thought, reasoning or ´cognitive´ activity depend upon language.

Traditionally, one way of looking at the relationship between language and thought has been to investigate the abilities of human subjects who suffer from various forms of language deficit. (Some of the experiments that will be described in the following chapters could be thought of in this way.) These subjects may be mentally handicapped, deaf, brain damaged or just young. However, this research strategy is not as simple and straightforward as it might seem and the interpretation of studies of individuals with speech deficits has proved problematic. To begin with, the exact nature and extent of the language deficit is not always apparent. Furthermore, individuals who lack language are usually (in the Western world at least) subject to intensive educational procedures designed to ameliorate the very problem under investigation. Finally, even when the extent of the language deficit is reasonably well described, the description of associated cognitive deficits has often been anecdotal and unsystematic. Even when objective tests have been administered the significance of the results has been a matter of some controversy. Of particular importance is the necessity of using tests which do not rely heavily on the subject´s comprehension of words as the use of such tests will obviously not allow a distinction to be made between speech and intellect.

´Aphasia´ is the term given to the group of speech disorders that result from brain damage of one sort or another. It is primarily on the basis of the study of the aphasias that attempts have been made to locate the major language functions in the brain. In most people, lesions that produce language deficits have been found to occur in the left cerebral hemisphere, although in a few people language deficits have been found following damage to the right hemisphere (Geschwind, 1979).

The range and complexity of the disorders that result from damage to these areas presents serious problems for those who would look to aphasiology for information about the relationship between language and thought. Aphasic patients are rarely entirely lacking in verbal abilities and the exact nature of the verbal deficiency observed varies according to the site and extent of the damage and also, presumably, according to the premorbid organisation of language functions in the brains of the individuals affected.

Benson (1980), in a review of the literature, has detailed some of the common abnormalities found in the language abilities of aphasic patients. With respect to conversational skills, some aphasics are nonfluent: their speech is sparse, produced with considerable effort, and is marked by poor articulation, short phrase length, inappropriate accent (dysprosody), and a preferential use of substantive words. Others are fluent: their speech is plentiful and easily produced, with good articulation but a tendency to omit words and to combine syllables incorrectly (known as paraphasia). In addition, some aphasics have marked difficulty in repeating words or phrases, whereas some others show an uncontrollable tendency to repeat whatever is said to them. Word-finding difficulties are also very common and nearly every aphasic has some difficulty naming things (eg. objects, bodily parts, colours, actions, etc.), although the degree of this disability, and the circumstances under which it manifests itself, varies greatly from patient to patient.

With respect to verbal comprehension, a wide range of deficits have also been observed. Many aphasics comprehend frequently used words but have difficulty with rarely used words. Others understand concrete terms such as names, but have difficulties with propositions, possessives, verb tenses and other relational or syntactic aspects of speech. Finally, of course, many aphasics have difficulties with reading and writing.

Clearly, aphasia is a phenomenon that requires investigation from the standpoint of a wide range of disciplines - particularly - in addition to neurology - psychology, linguistics and psycholinguistics. Given the complexity of aphasia it is perhaps not surprising that there is some confusion about the different types of aphasia that exist as discrete syndromes. Indeed, the number and variety of aphasic disorders has been much debated since the middle of the nineteenth century and many different classificatory schemes have been proposed. These cannot be detailed here, and the reader is referred to Benton and Joynt (1960), Critchley (1970) and Benson (1980) for details.

Equally debated has been the extent to which aphasia affects a person´s ability to think. Opinions have ranged from those who have closely identified language with thought, and who have therefore regarded the aphasic as cognitively disabled, to those who have maintained that behind the aphasic´s communication difficulties lies ´locked in´ intelligence.

The neurologist Hughlings Jackson (1958), working at the end of the nineteenth century, described the aphasic patient as "lame in thinking". According to Jackson, most aphasics retained their understanding of verbal propositions as revealed by their ability to follow instructions. It was only in their production of speech that they were limited. Goldstein (1948), however, believed that the relationship between the ability to speak and thought was a closer one than Jackson would allow. Goldstein regarded aphasia as an essentially unitary phenomenon and maintained that the aphasic´s thought manifested a ´concrete´ rather than an ´abstract´ attitude towards the world. Indications of the abstract attitude, absent in whole or part in the case of aphasics, were said to include the ability to verbalise acts; the ability to shift from one method of classification to another; the ability to form hierarchical concepts; and "the ability to form an attitude towards the merely possible". Using a series of sorting tasks, Goldstein and Sheerer (1941) demonstrated that aphasics did indeed have the kind of difficulties associated with a loss of abstract attitude. For example, they found that aphasics were particularly bad at categorising objects according to abstract properties such as function, or at changing from one method of classification to another.

Unfortunately, the application of systematic intelligence tests has not clarified the nature of the aphasic´s deficit. For reasons mentioned above, it is not appropriate to use intelligence tests that require an understanding of verbal instructions or the production of verbal responses. On the other hand, a moment´s reflection may lead us to doubt whether the assessment of higher intellectual functions could ever be entirely divorced from language. Attempts to use relatively non-verbal tests such as Raven´s Progressive Matrices (a test in which the subject is required to pick the missing piece to a pattern) or the performance subscales on the Weschler Adult Intelligence Scale (which include such tasks as joining coloured blocks together to make designs) have generally found evidence of depressed performance, together with a recovery in performance that accompanies the recovery of speech (Bay, 1962; Zangwill, 1964, 1969). However, the interpretation of these findings is problematic. Zangwill, for example, has argued that the aphasic´s intelligence is probably greater than can be demonstrated by the available techniques.

Similar problems to those discussed with respect to aphasia affect studies of thinking in the deaf. The degree of hearing impairment found in deaf children varies considerably, the most common form consisting of an inability to percieve high-frequency sounds while hearing at low-frequencies remains unimpaired (de Villiers and de Villiers, 1978). There can be no doubt that a significant reduction in the ability to percieve oral speech severely affects language acquisition. None-the-less, a considerable proportion of deaf children in the West learn to speak remarkably well if sufficient efforts are made to provide them with clear speech input (Fry, 1966). Even when a child´s hearing impairment is so severe that the learning of oral language seems impractical, efforts are usually made to teach some kind of sign language. Such sign languages as are commonly used with deaf children have been subject to considerable investigation in recent years. It has generally been found that, in terms of structure at least, sign language is comparable to oral language and that the acquisition of sign language by deaf children follows a similar course to the acquisition of oral language by hearing children (Bellugi and Fisher, 1972; Dale, 1976).

The extent to which the language deficits that undoubtedly do exist (usually in the form of a slowing down of the language acquisition process) affect the deaf child´s intellectual abilities has been as hotly debated as the equivalent question in aphasiology.

Oleron (1953), summarising the research that had been carried out until that time, rejected the possibility that the deaf child´s conceptual thinking could be viewed simply as manifesting an absence of Goldstein´s abstract attitude. Instead, Oleron argued that concept formation requires two conditions, a perceptive condition in which the objects to be sorted are perceived, and a conceptual condition, in which the objects are grouped according to class.

> "A sorting task.... cannot be correctly carried out unless the first (conditions).... are subordinated to the second.... However, this subordination may be imperfect or lacking, as when the subject lends too much importance to the observed data and tends to consider them as independant entities."

Oleron supported this assertion with the observation, from one of his own studies (1951), that when deaf subjects were asked to explain the reasons for their sorting, the answers given were seldom logical, rarely included reasons for the actions taken, and hardly ever included the idea of a common class in abstract form. Oleron (1953) argued, however, that:

> "Unlike Goldstein's 'concrete attitude' this does not constitute an abnormal state. Rather it indicates a stage of incomplete development, similar to an earlier stage found in normal children."

In contrast, a rather different view of the deaf child's abilities has been argued by Furth (1966, 1971), again based on the results of a number of studies. Furth claimed that deaf children show normal abilities on a surprisingly wide range of tests and that this provides support for a Piagetian position (to be discussed below) to the effect that thought is independant of and prior to language. According to Furth, it is only on a few tests, requiring 'verbal mediation', that deaf children do worse than normals.

In fact, the extent to which the subjects in the studies reviewed by Furth were actually linguistically deficient has been a matter of some argument. As noted above, deaf children are not always as lacking in language as is sometimes supposed. Bornstein and Roy (1973) in particular have pointed out that, in the studies cited by Furth, linguistic deficiency has not been demonstrated and that deaf children in the age ranges concerned typically have considerable verbal skills.

Furthermore, the distinction made by Furth between tasks requiring thought alone and those requiring verbal mediation immediately plunges us back into the definitional problems with which this chapter began. As discussed above, the position taken here is that, because animals can perform a wide range of complex activities without language, obviously language is not necessary for all kinds of complex behaviour. The interesting question is: How important is language in those most complex of activities which we count as uniquely human? If it should turn out that those activities that are uniquely human are precisely those that Furth deems needing of verbal mediation, then this would provide evidence to support the view that much (but not all) of complex human behaviour is dependant upon speech. In fact, as Blanck (1965) has pointed out, "There are numerous studies in which animals who are obviously nonverbal have mastered many of the complex types of learning tasks cited by Furth".

Furth goes some way to countering this criticism when he specifically maintains that deaf children demonstrate "logical thinking" in the absence of a symbol system. However, as Cohen (1977) points out, Furth´s data simply do not support this conclusion. His deaf subjects have been shown to be lacking or at least retarded in important logical skills. When the probable linguistic abilities of the deaf children studied by Furth are taken into account, it is difficult to disagree with Cohen who observes that the claim that these deficits may be the result of a general lack of stimulation, rather than a lack of language, is doctrinaire and virtually self-validating. After all, if any results that place deaf children behind normals are to be explained away, either in this fashion, or because the skills concerned require verbal mediation and therefore ought not to be considered, it is difficult to imagine exactly what deaf children would have to lack in order to support any view opposed to Furth's.

What do the results of studies of these linguistically disadvantaged people tell us, then? For the reasons outlined above the findings are difficult to interpret and far from conclusive. None-the-less there does seem to be some (albeit weak) support for the view that language is an important, perhaps even the crucial component of those higher forms of mental activity that we associate with man. There certainly does not seem to be any evidence that would justify rejecting this hypothesis out of hand.

LANGUAGE, THOUGHT AND DEVELOPMENT

This is a convenient point at which to stop and consider some theoretical accounts of the relationship between thought and speech. Most of the attempts that have been made to understand this relationship have been constructed in the context of developmental psychology. This is not surprising because it is during the first few years of life that a child acquires language, and it is during just this period that a number of remarkable transformations in the child's abilities occur. This obvious codevelopment of language and intelligence is one of the things that make a link between speech and thought so intuitively plausible. A comment by Flavell (1977) makes this clear:

> "The infant 'knows' in the sense of recognising or adapting familiar, recurring objects and happenings, and 'thinks' in the sense of behaving towards them with mouth, hand, eye, and other sensori-motor instruments in predictable, organized, and often adaptive ways. His is an entirely unconscious and self-unaware, non-symbolic and nonsymbolizable (by the infant), knowledge-in-action or know-how type of cognition. It is the kind of noncontemplative intelligence that your dog relies on to make its way in the world. It is also the kind that you yourself exhibit when performing many actions that are characteristically nonsymbolic and unthinkable by virtue of being so overlearned and automatized...."

In other words, there is not much if anything that an infant can do that a dog, say, cannot do. On the other hand, by the third or fourth year of life the child has mastered many aspects of self-control and self-regulation, this achievement being described by Flavell as, "one of the really central and significant cognitive-developmental hallmarks of the early childhood period" (Flavell, 1977).

Is this achievement, then, related to language? As we have seen, Watson might advance the view that it is, but developmental psychologists have by no means reached agreement on the matter. In fact, different schools of developmental psychology have arrived at notably different answers to the question. Soviet psychologists, for example Vygotsky, have followed Pavlov in stressing the importance of language in cognitive development. In contrast, however, Jean Piaget and his followers in Geneva have arrived at a quite different account of the way in which the child's mind grows, and have argued that cognitive development is relatively independant of the development of speech. It is interesting to note that both Vygotsky and Piaget, like Watson, were moved to speculate about the relationship between language and thought, in part, by the observation that children often talk to themselves with little apparent regard for the presence or absence of others. This kind of verbal behaviour has been described, variously, as egocentric speech, private speech, self-directed speech and inner speech. As these labels all originate from particular theoretical standpoints, Zivin (1979) has suggested that

´acommunicative speech´ is a term that suitably describes all instances of speech that do not have any obvious communicative intent and, for the most part, this term will be used in the more detailed examination of the Soviet and Genevan positions that follows.

THE THEORY OF VERBAL SELF-REGULATION

Soviet psychology has been heavily influenced by two intellectual traditions: on the one hand, following from the nineteenth century reflexology of Sechenov, an emphasis on psychophysiological studies and the concept of the conditioned reflex; on the other, following the revolution of 1917, an attempt to develop psychological principles consistent with Marxism (McLeish, 1967; Harris, 1979). Of particular importance, with respect to Marxism, is the theory of dialectical materialism developed by Marx and especially Engels (1925). Essentially, dialectical materialism is a philosophy that, in addition to the usual tenets of materialism (roughly that, "There is nothing in the world over and above those entities which are postulated by physics" - Smart, 1963) emphasises that the universe is in a constant state of change. Particular importance is given to those changes that are qualitative and take the form of sudden leaps from one state of affairs to another. According to dialectical materialists, such changes may result from the slow build up of small, quantitative changes. Thus, according to Marx's theory, quantitative changes in a capitalist society, for example the increasing impoverishment of the working class, may eventually cause conflicts which bring about a revolutionary transformation to a communist society.

The influence of this world-view on Soviet psychology cannot be overemphasised. Soviet developmental psychology tends to be materialist (higher mental functions are seen as the property of highly organised matter), emphasises stages of development, and assigns a central role to the socially and historically determined relations between people (as opposed to, for example, innate capacities). As a result, although Western behaviourists have not been so concerned with child development, and although behaviourism does not represent an attempt to develop a psychology consistent with a particular ideology in the way that Soviet psychology does, none-the-less the two traditions have much in common. Both reject Catesian dualism in favour of some kind of materialism and both emphasise the importance of environmental (rather than biological) forces.

As we have seen, the emphasis on the role of language as a regulator of behaviour comes from Pavlov. Pavlov began his work well before the revolution of 1917, but his research was regarded favourably by Lenin, who saw in it the basis for a psychology compatible with Marxism. On this view, the emergence of the second signalling system (speech) acquired by means of the first signalling system (conditioned reflexes), amounts to a dialectical leap from one mode of functioning (common to all species) to another (exclusive to man).

It was Vygotsky (1962, 1978) who extended Pavlov´s distinction between the two signalling systems into a theory of child development. According to Vygotsky (1962):

> 1) In their ontogenetic development, thought and speech have different roots. 2) In the speech development of the child, we can with certainty establish a preintellectual stage, and in his thought development, a prelinguistic stage. 3) Up to a certain point in time, the two follow different lines, independantly of each other. 4) At a certain point these lines meet, whereupon thought becomes verbal and speech rational."

Vygotsky and subsequent Soviet developmentalists have held that the way in which a child performs when aided by an adult (for example, by means of instruction) may be a more accurate and sensitive indicator of future performance when unaided than standardized intelligence tests (Sutton, 1980). Thus, Vygotsky (1978) described ´the zone of proximal development´:

> "It is the distance between the actual developmental level as determined by independent problem solving and the level of potential development as determined through problem solving under adult guidance or in collaboration with more capable peers.... If a child can do such-and-such independently, it means that the functions for such-and-such have matured in her.... The zone of proximal development defines those functions that have not yet matured but are in a process of maturation, functions that will mature tomorrow but are currently in an embryonic state."

It was in the light of this idea that Vygotsky argued that the acquisition of the ability to be regulated by others forms an important part of the development of the child´s ability to regulate himself. Thus, according to Vygotsky, as the child grows, he first learns to produce and be guided by social speech and, in particular, to follow the instructions of significant adults in his environment (Wertsch, 1975). The child then goes through a stage during which he overtly instructs himself and follows his own self-instructions. Finally, social and self-directed speech become differentiated. Self-directed speech becomes private and internalised, becoming inaudible and invisible to others and losing many of the components of social speech, so that it becomes a highly abreviated form of covert verbal activity. Vygotsky supported his theory with a number of observations of children speaking to themselves, which led him into an early disagreement with Piaget. Piaget (1926) had previously denied the social and cognitive significance of such speech (see below), but Vygotsky argued that the fact that acommunicative speech had a function for the child could be inferred from the observation that such speech occurred most often when the child was engaged in the solution of difficult tasks.

Vygotsky's pupil Luria (1959, 1960, 1982) elaborated the theory, using a well known series of experiments, by which means he mapped out the stages of child development during which the child acquires the ability to use speech to regulate his own behaviour to the full.

Luria reported that, at the end of the first year of life, the child is usually able to be directed by speech to a limited degree, but if directions come into conflict with external conditions they cease to be effective. Thus, according to Luria, if the child is asked for one object when another is nearer he reaches for the nearer one. Up to the end of the second year, the child continues to show evidence of inertia in the face of instructions. If asked to take rings off a stick after he has begun to put them on he continues to put them on; or if asked to reach for one object after having been repeatedly asked to reach for another he continues to reach for the object he has previously grasped.

Even when the directive role of simple commands has been established by the end of the second year, according to Luria more complex speech fails to regulate behaviour. If told, "When the light comes on press the rubber bulb," (the one choice discrimination paradigm) the two year old will look for the light at the beginning of the sentence and press the bulb at the end. With slightly older children, in the second half of their second year, it was found that, although they could press the bulb when the light went on, they persevered in pressing afterwards, despite being instructed to press only once.

Luria found that three year olds could usually master the one choice discrimination task. However, with a more demanding two choice discrimination task, in which the child was asked to press when one light came on but not when the other was illuminated, Luria found that three year olds would press to both lights. Thus Luria maintained that, even in the case of the three year old the control of motor behaviour by means of speech is still not fully developed.

In a series of studies, Luria and his colleagues tried to improve the child's performance on the above tasks by making the child produce appropriate verbal responses. Thus, on the two choice task, for example, the self-instructions, "Press" and "Don't press" had different effects at different ages. The child of two to two and a half years found the task so difficult that, according to Luria, the effort required to produce the verbal responses at the right times, if he could produce them at all, completely inhibited motor activity. The three to three and a half year olds, on the other hand, could make the appropriate verbal responses to the appropriate lights in the two choice paradigm. However, although the "Press" self-instruction had the required affect on performance, the "Don't press" self-instruction was also accompanied by pressing. This led Luria to conclude that, despite the apparent effectiveness of the positive self-instruction, speech still had no semantic control over behaviour. Instead, its function was ´impulsive´. A similar observation was made with children of this age using the one choice paradigm, on which the self-instruction "Press" did seem to eliminate perseveration. However, it is important to note that, at this stage in development, the self-instruction often followed the response, so that, although it appeared to be effective, it usually functioned by inhibiting perseveration, rather than by initiating action (Luria, 1960). Only at about four years of age, according to Luria, could self-instructions completely and effectively control the child´s performance.

It will be useful, at this juncture, to compare the Soviet approach with that of Piaget and the Geneva school. The Piagetian approach to cognitive development stands in sharp contrast to that of the Soviet psychologists, the former being ´structuralist´ in orientation, rather than ´functionalist´. Structuralism is a philosophy that has found favour in some quarters of such diverse disciplines as anthropology, linguistics and literary criticism, in addition to psychology. The common elements of different structuralist approaches in different areas have been considerably debated and the interested reader is referred to such discussions as those by Caws (1968) and Broughton (1981), and to Piaget´s own account (1971). Suffice it to say, for present purposes, that central to Piaget´s approach is a search for mental structures that are said to underlie the child´s behaviour. These structures, it is argued, can be inferred from an examination of the child´s activities at a particular age, and are regarded as sufficient explanations of the child´s abilities. Their development (common to all children) is held to be governed by relatively inflexible transformational laws. Thus, in contrast to the Soviet approach, cognitive development is seen as the unfolding of a sequence of necessary stages, little affected by the influence of society.

Piaget has changed his position on the relationship between language and thought somewhat over the years. As noted above, his original position (Piaget, 1926) was opposed to that of Vygotsky. Like Vygotsky, Piaget had seen children talking to themselves in the classroom but, unlike Vygotsky, he did not attribute a social root to such speech. Instead the speech was said to be ´egocentric´ in that the child was not said to be able to recognise the viewpoint of possible listeners. This ´egocentric speech´ then was seen as a spurious consequence of (indeed a visible manifestation of) the child´s primitive mental structures. The effect of cognitive development on speech was to make it less egocentric.

Later, Piaget (1954) shifted his position slightly, arguing that, "A symbolic function exists that is broader than language and encompasses both the system of verbal signs and that of symbols in a strict sense...." Though denying that language was the only source of logical operations he seemed to accept a weak version of the Vygotskian hypothesis to the extent that: "Language indefinitely extends the power of these operations and confers on them a modality and a universality which they would not have otherwise, but it is by no means the source of such co-ordinations."

The extent of the difference that remains between the two leading accounts of acommunicative speech is, at least in part, a matter of interpretation. Brown and Desforges (1979), in a review of Piaget´s work have argued that Piaget was never sufficiently clear about the relationship between language and thought whereas Zivin has argued that Piaget and Vygotsky may have been, after all, talking about two rather different phenomena, or at least different aspects of the same phenomenon. "If one sees Piaget´s serious emphasis to be on communicative misfiring and socially inappropriate use of words, then it becomes clear that, as defined by function, Piaget is referring to one kind of speech and Vygotsky to another. Piaget means using or not using words in accord with consensually agreed upon social convention. Vygotsky is talking about using words for one´s own idiosyncratic cognitive-motoric self-regulation." (Zivin, 1979).

There is obviously insufficient space here to critically evaluate Piaget´s theory - and the vast amount of research that it has generated - in any depth. However, two criticisms that have been made about Piaget´s work are worth noting in passing because of their relevance to verbal regulation accounts of intellectual development.

As mentioned above, Piaget divided the period of the child´s mental growth into a number of separate stages, characterised by the kinds of cognitive failures and successes peculiar to each. It has been recently suggested that Piaget sometimes underestimated the child´s abilities because he was not sufficiently careful about the way in which he instructed his subjects (Donaldson, 1978; Brown and Desforges, 1979). If this is so, then it may be that Piaget has given relatively little weight to the effects of language on cognitive development simply because his methodology was not sensitive enough to identify its important role.

The second, related criticism has more direct bearing on the theory of verbal self-regulation. It now appears that Piaget may have also underestimated the communicative abilities of younger children and, in particular, their ability to adapt to the perspective of the listener (Garvey and Hogan, 1973; Martlew, 1980; Fluck, Donnelly and Hick, in press). If this is so, it seems less likely that the acommunicative speech of younger children could be the result of communicative misfiring.

These criticisms obviously put Piaget´s theory of acommunicative speech into doubt, but they do not provide positive support for the Soviet account. It is to evidence that does seem to support Vygotsky´s theory that we shall now turn.

SOME EVIDENCE

i) Direct Observations of Private Speech

Speech that appears to have a regulatory, rather than a communicative function, clearly manifests itself often during child development. Flavell, in an unpublished survey of parents employed at an American university (reported in Jarvis, 1968), found that over ninety per cent of those questioned had observed such speech. As we have already seen, many theorists have attached importance to the occurrence of such spontaneous acommunicative speech (ACS) in children (particularly Vygotsky and his followers). A considerable number of studies have reported such speech being produced by children employed in a wide range of tasks, for example during discrimination training (Kuenne, 1946); memory tasks (Flavell, Beach and Chinsky, 1966; Conrad, 1971); jigsaw and drawing tasks (Kohlberg, Yaeger and Hjertholm, 1968; Deutsch and Stein, 1972; Goodman, 1981); delayed gratification tasks (Mischel, Ebbesen and Zeiss, 1972), free play (Rubin, Hultsch and Peters, 1971; Dickie, 1973; Rubin, 1979) and reading (Roberts, 1979). Many of these studies have produced anecdotal evidence of a quite impressive nature, illustrating the efforts to which some children will go in order to regulate their own behaviour, at least in part by means of speech. For example, in the study by Mischel et al. (1972) the following behaviour was observed from children who were asked to wait alone in a room, and who were told that, if they reached for a small, available reward before the experimenter returned they would lose the opportunity to gain a larger reward:

> "When the distress of waiting seemed to become particularly acute, children tended to reach for the termination signal, but in many cases seemed to stop themselves from signalling by abruptly creating external and internal distractions for themselves. They made up quiet songs ('Oh this is your land in Redwood City'), hid their heads in their arms, pounded the floor with their feet, fiddled playfully and teasingly with the signal bell, verbalised the contingency ('If I stop now I get ————, but if I wait I get ————), prayed to the ceiling, and so on."

Similarly, Conrad (1971) observed that approximately half his subjects, who were required to perform a delayed matching task, spontaneously spoke, usually naming items in the task.

A number of issues are raised by these observations. How does acommunicative speech vary with age and with the type of task the child is engaged in? Can it be shown that such speech has, as Vygotsky suggested, a regulatory function, or is it a mere artifact of the child's limited social and cognitive development?

In a series of studies, Flavell and his colleagues (Flavell, Beach and Chinsky, 1966; Keneey, Cannizo and Flavell, 1967; Corsini, Pick and Flavell, 1968) investigated rehearsal during memory tasks in six and seven year olds. The subjects were given items to remember and were observed to see whether their lips moved (suggesting that they were subvocally repeating the names of the items over to themselves). It was found that those subjects who most subvocalised performed best on the memory tasks but that this skill could be taught to the poorly performing subjects. These findings suggest, as Vygotsky argued, that ACS does play an important regulatory role.

The first attempt to carefully and systematically analyse the content as well as the occurrence of acommunicative speech was carried out by Kohlberg, Yaeger and Hjertholm (1968).

Kohlberg et al. developed a verbal coding system that has been used in a number of subsequent studies, and which was based on a reading of Vygotsky, Piaget and George Herbert Mead (1934). According to their system, speech is recorded, transcribed, and scored into six major categories.

These categories are conceptualised as occurring in a five level developmental sequence as follows:

Level I. Presocial Self-Stimulating Language
    1. Word play and repetition.
Level II. Outward-Directed Private Speech.
    2. Remarks addressed to nonhuman subjects.
    3. Describing own activity.
Level III. Inward-Directed or Self-Guiding Private Speech.
    4. Questions answered by the self.
    5. Self-Guiding Comments.
Level IV. External Manifestations of Inner Speech.
    6. Inaudible muttering.
Level V. Silent Speech or Inner Thought.

By using this system, Kohlberg et al. carried out four studies in which they attempted to evaluate Vygotsky´s theory of acommunicative speech against Piaget´s, arguing that, if Vygotsky was correct then ACS should follow a curvilinear course (first increasing with age, then decreasing), whereas, if Piaget was correct then ACS should steadily decline with age. (For reasons discussed above, this interpretation of the difference between Vygotsky and

Piaget is a dubious one.)

Kohlberg and his colleagues began by classifying speech as either social or (using Piaget´s term) egocentric. In the first study four and six year olds were observed by experimenters for two hours in a classroom situation, and it was found that the four year olds produced a greater proportion of egocentric speech than the six year olds. Furthermore, as might be predicted from Vygotsky´s theory, the amount of egocentric speech produced by the bright six year olds was less than the amount of egocentric speech produced by the average six year olds (although no significant difference was found between the amount of egocentric speech produced by the bright and average four year olds). The second study more or less replicated this result with a much larger sample of children (one hundred and twelve), ranging in age between four and ten years, who were observed while making designs, again in a classroom.

Kohlberg et al. used their extended system of classification (described above) in their third study which was a one year follow up of the performance of twenty six of the four, five and seven year olds who took part in their second study. In general, the proportion of egocentric speech produced by the children followed the predicted course, increasing in the younger average intelligence subjects but otherwise declining with age. Individual subcategories of egocentric speech showed different age trends. The amount of muttering, for example, increased with age, whereas the amount of self-guiding comments showed no tendency in either direction. The extent to which subjects described their own activity, however, declined with age. Kohlberg et al. concluded that this data supported Vygotsky.

In their final study, Kohlberg et al. investigated the relationship between egocentric speech and task difficulty. According to Vygotsky, it will be recalled, more overt self-regulatory speech should be produced in difficult task conditions than when the task being attempted is comparatively easy. Kohlberg et al. required four and a half to five year old children to thread beads on a string, build a tower of bricks, and solve two jigsaws of differing difficulty. As expected, the amount of egocentric speech increased with task difficulty. When the egocentric speech was broken down into its different subcategories, however, a rather more complex picture emerged. The proportion of self-answered questions, for example, increased with difficulty, whereas the proportion of muttering generally declined. The proportion of self-guiding statements, the most common type of egocentric speech over all, increased with task difficulty over the two easiest tasks, then declined to a certain extent over the two hardest tasks.

Subsequent studies utilising Kohlberg's scale have tended to confirm some aspects of Kohlberg's findings, but have cast some doubts on others. For example, Rubin, Hultsch and Peters (1971) used the scale to code the acommunicative speech of four year olds during free play. The children were observed when alone, with a peer and with a familiar adult. It was found that significantly more ACS was observed in the alone condition than in the other conditions. First born children were observed to produce more ACS than other children but little difference was found in the type of speech emitted by the oldest and the youngest children in the range (with the exception of mumbling, which occurred more often with the older subjects), a result that is perhaps not surprising given the narrowness of the age range studied.

In contrast, Deutsch and Stein (1972), using the Kohlberg scale with four and five year olds, did manage to replicate Kohlberg's finding that older children produce a greater proportion of the higher levels of ACS as defined by the scale. In addition, Deutsch and Stein also investigated the effect of task failure - brought about by interrupting the children before they could finish a jigsaw and telling them that they had run out of time - and found that this led to an increase in acommunicative speech on subsequent attempts.

Beaudichon (1973) also produced results to some extent consistent with Kohlberg but without using Kohlberg´s scale. Seven year olds and five year olds were observed solving a number of problems and it was found that speech judged to be involved in immediate regulation (interpretative or descriptive speech) occurred more frequently in the seven year olds than in the five year olds, whereas the reverse was the case for all other types of speech. With an increase in task difficulty, however, the five year olds produced more of all types of speech than the seven year olds.

On the basis of these studies, then, there is some reason to believe that ACS varies with age roughly in accordance with Vygotsky´s theory. There is also some (though less) evidence to suggest that task difficulty may affect the amount of ACS produced. These studies, however, give little information about the relationship of the verbal activity recorded to ongoing motor activity. Two more recent studies by Rubin (1979) and Goodman (1981) have looked at the relationship between ACS and task performance in greater detail, in an attempt to shed light on this problem.

Rubin (1979) reported two studies in which children were observed in free play. In some contrast to the findings detailed above, he found that the total amount of ACS emitted by children between four and eleven years of age did not seem to vary much. However, some age trends were detected in some of the subcategories, repetition occurring more often in the younger children, questions answered to the self reaching a peak at about seven years, and expletives (a category not employed by Kohlberg) generally increasing with age. In the second study, with three to five year olds, it was observed that the relationship between ACS and ongoing activity depended upon the activity in which the subject was engaged. In exploratory or constructive play, ACS during motor activity exceeded ACS preceding or following it. During the transition from one type of play to another, however, comments occurred more often prior to and during motor acts than following them.

The most detailed analysis of the relationship between ACS and motor activity to date has been carried out by Goodman (1981), who observed the jigsaw solving behaviour of four year olds. A number of findings were made as the result of an analysis too complex to be described here. Puzzles solved most quickly were those in which most verbalisations occurred. Most verbalisations occurred during individual behavioural acts (eg. the placing of pieces); however descriptions of activity and plans were most likely to occur during trial and error attempts to place pieces and during failed attempts to place the pieces correctly. Moreover, a variety of temporal relationships between speech and motor activity were detected. Verbalisations of plans and thoughts were most likely to follow or be followed by errors in placing the pieces; descriptions of activity were most often preceded by the successful placement of pieces; and questions asked and answered by the subject were usually preceded by failed attempts to place pieces and succeeded by the successful placement of pieces by trial and error.

Interpretation of these results as a whole is problematic for a number of reasons. The most important concerns the relationship between the formal and functional aspects of acommunicative speech. Such concepts as ´private speech´, ´self-directed speech´ and ´social speech´ are primarily functional. That is, they relate to the origins and consequences of particular speech acts; what these speech acts do for the speaker. Attempts to identify such categories in an observational situation (where the determinants and especially the consequences of individual utterances are beyond the control of the experimenter) pose many problems. It is likely that, often, the description of a particular verbal response as either, say, social or self-directed speech will be based upon limited information, primarily of a structural nature. (The verbalisation, "I must put this piece here," may well be regulatory, but our recognition that it is so will depend upon a number of factors, for example the use of the phrase "I must....", the occurrence or nonoccurrence of a motor act following the speech act, etc.)

This reliance on structural information may conceivably lead to a number of confusions. "I must go to the bank," is a statement that could be emitted by an adult and which might have either a social or a regulatory function. It is the type of comment that could be said alone or equally well in response to a question from a companion. There are some speech acts, on the other hand, which seem to be neither social or regulatory. For example, the category of ´remarks addressed to nonhuman subjects´ in Kohlberg´s scheme could be seen to include what Skinner (1957) has described as ´magical mands´ (ie: commands that can have no effect, such as shouting, "Stop! Stop!" while pumping the brake pedal of a car). It is possible that this kind of speech has no regulatory function (certainly the car will stop no quicker for it) and is the result of some kind of generalisation from the use of commands in social situations.

These kinds of issues become accentuated when, as with Kohlberg´s system, the experimenter attempts to divide self-regulatory speech into a number of sub-categories. In fact, a number of writers have suggested alternative schemes or variants of Kohlberg´s (eg. Fuson, 1979; Meichenbaum and Goodman, 1979) but the same general problems apply. In part, this reflects the fact that few attempts have been made to provide a functional account of language acquisition.

Obviously, future naturalistic research into ACS will have to analyse the relationship between individual speech and motor acts in even more detail than the studies cited above. Of particular importance will be the development of methods of identifying appropriate functional speech categories. Despite these problems, however, some conclusions can be reached on the basis of the research carried out to date.

In the first case, a great deal of evidence detailing the naturally occurring control of motor behaviour by verbal behaviour has been collected. It is true that this has varied from task to task and from condition to condition (a finding in itself not too surprising given the complexity of the phenomenon), but enough acommunicative speech has been documented to allow us to be reasonably confident that it has a regulatory function.

With respect to the effect of age, interpretation of the findings is difficult. None the less, most studies have observed a decline in ACS with age, together with an increasing predominance of what Kohlberg has suggested are the more mature forms. This finding, however, needs to be accepted with a degree of caution. As Meichenbaum and Goodman (1979) have observed, most psychologists at one time or another have discovered their colleagues speaking aloud to themselves. Common sense, therefore, suggests that acommunicative speech does not always go completely covert with age.

ii) Related Psychophysiological and Physiological Evidence

According to Vygotsky, the ultimate fate of ACS is its internalisation, so that it is no longer directly observable to an external onlooker. Over the years a certain amount of inferential evidence has accumulated to suggest that adult thought consists, at least in part, of such internalised speech. Thus, Landauer (1962), for example, found that adult subjects took almost exactly the same amount of time to think through a series of numbers as they did to say them out aloud.

Arguing in the reverse direction, Benjafield (1969) has claimed that thinking out aloud is an accurate indication of inner thought on the basis that, when think-aloud protocols are compared to retrospective introspections, they prove to be more elliptical and present-orientated, and contain more indefinite referents. Benjafield rejected the alternative argument that thinking aloud was not really thinking but doing something else as well because think-aloud protocols are usually in the present tense (suggesting that the speaker is not saying what he has already thought) and contain plans and evaluations.

Evidence such as this, however, provides only relatively weak support for Vygotsky's theory, simply because it is so indirect. One way of investigating hidden verbal behaviour or 'inner speech' more directly is to search for microactivity of some sort in the speech musculature.

Early attempts to measure covert speech have been described by McGuigan (1978). A variety of crude devices were used, including rubber bulbs placed on the tongue, tambours placed on the larynx, and levers attached to various parts of the speech apparatus. Some studies seemed to support the view that inner speech was involved in problem solving whereas others produced negative results. One surprisingly often cited experiment by Thorson (1925) involved the use of mechanical levers, connected to a suction cup attached to the tongue, which allowed a magnification of tongue activity by approximately four times. Thorson concluded that no relationship between thought and speech muscle activity could be detected. However, McGuigan (1978) has argued that, considering the insensitivity of the equipment, Thorson´s data is, if anything, supportive of the inner speech hypothesis.

The measurement of covert speech activity was revolutionised by the invention of the electromyogram (EMG), a device that can measure the electrical activity of muscles via electrodes placed on or beneath the skin, and which can be made hundreds of times more sensitive than any of the devices previously used for the same purpose. By placing EMG electrodes on the skin of the neck, chin, lips and tongue, it became possible to measure with some accuracy speech-muscle activity not visible to the eye.

Despite the sensitivity of the EMG, however, the interpretation of data collected by it is not without problems. Obviously, the attachment of electrodes to various sites around the neck and mouth may conceivably have a confounding effect in an experiment. A more serious problem is faced when an attempt is made to establish that any increase in EMG activity is actually indicative of inner speech, rather than a more general increase in muscular arousal due to the demands of the experiment. One method frequently used for this purpose is the comparison of lip, chin and tongue activity during a task with increases in the activity of non-speech muscles. Using this approach, inner speech has been detected with adult subjects during a number of tasks, for example during imagined speech (Jacobson, 1931, 1932); during silent reading (Faaborg-Andersen and Edfelt, 1958); during memory tasks (Garrity, 1977b); during the formation of counterarguments to disagreeable arguments (Cacioppo and Petty, 1981); and even during the solving of apparently visual tasks, such as Raven´s Progressive

Matrices (Sokolov, 1969, 1972). These studies in themselves provide considerable evidence of the role of inner speech in problem solving, yet some researchers have been moved to seek for even more exacting evidence that the observed increases in EMG activity actually reflect speaking, rather than something less specific.

In a review of the anatomical literature, Faaborg-Andersen (1965) found that the muscle groups involved in normal speech and those observed to be involved in apparent inner speech are the same, but that the amplitude of inner speech activity is less than that of overt speech. A similar finding has been reported by those experimenters who have compared the EMG traces of subjects when they speak with the traces produced when subjects are asked to think the same words to themselves (eg. Jacobsen, 1932; Sokolov, 1967, 1969, 1972).

Another approach has been to compare EMG activity during verbal tasks with EMG activity during visual tasks. Thus, for example, McGuigan (1970a) found that subjects asked to draw an oval produced less inner speech than subjects asked to copy words. Similarly, Sokolov (1969, 1972), although reporting speech muscle activity during the solving of visual problems, found that this was less than the activity observed during verbal problem solving.

A more recent method, which puts speech muscle activity under even greater scrutiny, was devised by Locke and Fehr (1970). Reasoning that, if apparent inner speech really is speech, the covert utterence of some words should be reflected by more muscular activity than the covert utterence of others, they required adult subjects to memorise sequences of words for short periods of time. Some words included labial phonemes (ie: sounds which, when voiced, required strong lip movements, as in "Bomber") whereas others did not. As expected, the remembering of labial words was accompanied by more lip EMG activity than the remembering of non-labial words.

In the light of this evidence, it is interesting to note that the same pattern of covert muscular activity is not observed in deaf people as in the hearing, an unsurprising result given the difficulties that deaf people have learning language (see above). Max (1935, 1937) found increased EMG activity in the hands of deaf people during problem solving. Similar evidence has been reported by Novikova (1961) and McGuigan (1971) who observed that, for those subjects who could both speak and sign, EMG lip and arm activity occurred in synchrony.

Once this evidence that inner speech is indeed involved in problem solving, as Vygotsky supposed, is accepted, a number of further questions arise. It is possible to inquire, first of all, whether there is any relationship between cognitive ability and the amount of inner speech produced. McGuigan (1970a) found that poor adult readers tended to produce more inner speech during reading than good adult readers, as did Edfelt (1959). McGuigan further suggested that the poor reader's increased speech activity reflected the facilitative effect of inner speech and the perceived difficulty of the task (ie: the poor readers were producing more inner speech to compensate for their difficulties), a view supported by the observation that speech EMG activity generally increases with task difficulty (Faaborg-Andersen and Edfelt, 1958; Bassin and Bein, 1961; Sokolov, 1969, 1972). This finding is reminiscent of Vygotsky's view that more overt acommunicative speech would be produced by a child in situations of great difficulty than in situations that posed few problems.

With respect to children, it is pertinent to inquire whether the tendency to produce inner speech shows any age trends. Perhaps because of the obvious uncomfortable aspects of EMG research, little work has been carried out with children. McGuigan, Keller and Stanton (1964), however, observed significant speech EMG activity in six to eleven year olds during silent reading. McGuigan and Bailey (1969) repeated these measures with some of the same children three years later and found a general decrement in the amount of speech muscle activity, again as would be expected in the light of Vygotsky's theory. At the other end of the age range, Garrity (1975, 1977a, 1977b; Garrity and Donoghue, 1977) observed evidence of inner speech in children as young as four years of age.

The above review has been, of necessity, rather brief, and the interested reader is referred to McGuigan (1970b, 1978); Garrity (1970a); and Cacioppo and Petty (1981) for more extensive coverage of the available evidence. Despite the brevity of the present account, however, the extent to which the EMG data supports the verbal regulation hypothesis should be clear. None the less, important questions remain to be answered. For example, is speech muscle activity necessary for thought?

Watson (1924) saw thought as activity, primarily of the speech musculature, but of other muscles as well. Furthermore, linguistic habits were, on Watson's account, reducable to chains of conditioned reflexes. However, it is not clear that muscle activity need always accompany verbal thought; it is possible to conceive of the internalisation of regulatory speech occurring to such a degree that, even though the brain centres responsible for inner speech are active, no detectable reflection of this can be found in the speech muscles at all. Modern theories of covert verbal behaviour see such activity as controlled centrally (cf. Sokolov, 1972; McGuigan, 1978; Luria, 1982) and the peripheral activity of the speech muscles is presumably on this account dispensible. Whether it is, in fact, dispensible is, of course, a matter for empirical investigation.

One way of approaching this question has been to inhibit or paralyse the speech muscle in some way in order to see what effect this has on thinking. A variety of techniques have been employed to this end. For example, attempts have been made to occupy the speech musculature with competing tasks or to restrict the activity of the speech muscles by means of mechanical restraint and anaesthesia.

Sokolov (1972) asked subjects to recite well memorised poems while attempting a range of tasks. He found that perception of speech was severely disrupted and that subjects typically reported being unable to remember what had been said to them. With continued practice, however, Sokolov's subjects became skilled at speaking and remembering verbally presented information at the same time. Sokolov also tested the effects of articulatory interference on other tasks, such as arithmetical problem solving, the memorising of drawings and visually presented words, and the translation of texts from one language to another. Thus, in one study, Sokolov reported that reciting a poem delayed the solution of an arithmetical problem by adults more than the mechanical retardation of articulation, the repeated enunciation of the same syllable, or the continuous squeezing of a balloon. Furthermore, the extent of the delay due to reciting a poem increased with the difficulty of the arithmetical problem, as might be expected. In general, Sokolov found that articulatory interference had a similar negative effect on the other tasks studied, that this effect was most marked on tasks which involved verbal material, and for child subjects rather than adults. However, with increasing practice, subjects were able to overcome the affects of articulatory interference.

On the surface, these results are entirely consistent with Vygotsky's hypothesis but, on closer examination it seems that the picture is not so clear. It should be noted, for example, that although articulatory interference was shown to have a negative effect in by far the majority of cases, in a few instances subjects performed their tasks faster in the interference condition than in the control condition, a finding that Sokolov noted as particularly problematic. More generally, as McGuigan (1978) has pointed out, it is virtually impossible to guarantee that articulatory interference will completely suppress task-relevant sub-vocal speech. Some evidence to support this claim has been reported by McGuigan and Rodier (1968), who recorded chin EMG activity while adult subjects simmultaneously listened to a tape and read silently (articulatory interference again). It was found that EMG activity and breathing rate increased (compared to a control period) when the tape contained prose, but not when it contained meaningless material such as white noise or speech played backwards. The suggestion is that the increase in EMG and breathing rate may have been the result of the subjects producing two types of sub-vocal speech (monitoring of the taped message and reading) at the same time.

Of course, the same general problems apply to the other techniques that have been employed to suppress sub-vocal activity. In addition, the further methodological peculiarities of these techniques have also to be taken into account. Thus, when mechanical restraint has been employed, it seems to have produced less of an effect than articulatory interference. Sokolov (1972) found that restraint (subjects were asked to grip their tongues between their teeth) had an insignificant effect in the case of adults, but some effect in the case of children. Similarly, McGuigan (1978) found that mechanical restraint had no effect on the performances of silent reading college students, although it did seem to produce an increase in tongue EMG, perhaps in compensation. Reflect for a moment and it becomes obvious that clamping the tongue is unlikely to interfere significantly with covert speech. The results obtained by this method, therefore, are not surprising. Equally unsurprising are the negative results that have been obtained by the application of local anaesthesia to the speech muscles (described in detail by McGuigan, 1978) as it appears just as unlikely that this technique will have a significant effect on covert speech production.

Perhaps the most drastic attempt that has yet been made to investigate the effects of preventing articulation was carried out by Smith, Brown, Toman and Goodman (1947). One of the authors (Smith) was given a dose of curare (d-tubocurarine), a drug that completely paralyses the skeletal musculature (causing death by asphxia unless, as in Smith´s case, the subject´s lungs are artificially ventilated). Smith was completely unable to move or speak for a number of minutes but, upon recovery, it was apparent that, while paralysed, he had understood and remembered what was happening around him. Furthermore, just before being overcome completely by the curare, when Smith was almost but not quite totally paralysed, he was capable of answering yes-no questions correctly by means of a prearranged code of eyebrow twitches. This would appear, then, to be evidence that thought can occur without muscle movement although, as McGuigan (1966) has pointed out, the experiment was not without its inadequacies of design. (It would have been better if some means had been devised for Smith to answer standard intelligence test questions, for example).

This leads on to the general problem of which parts of the brain are actually involved in inner speech. Can physiological or psychophysiological evidence help to solve this problem in any way? As we have already noted with respect to aphasia, some of the conceptual and technological problems involved in investigating the functioning of the brain are extremely complex, and there is not enough space to go into these issues in any detail here.

In passing, it may be noted that an increasing amount of evidence is being brought to bear on this problem. Thus, Luria (1982) has argued on the basis of clinical evidence that the frontal lobes appear to be involved in the higher cognitive functions associated with inner speech. Obviously, we should expect the speech areas of the brain to be involved. A number of promising techniques for investigating the brain directly have been developed in recent years. For example, Larsen, Ingvar and Skinhoj (1978) have described the preliminary results of applying their method of measuring the brain´s regional blood flow (which should increase in those areas of the brain involved in a task). Their data so far suggest that those parts of the brain most active during inner speech (for example, when one is counting in one´s head) may be the premotor cortex and the frontal lobes, rather than the major speech areas damage of which is most commonly associated with aphasia. Another promising technique has been described by Fried, Ojemann and Edeerhand (1981), who worked with patients whose brains had been exposed for neurosurgery. The patients were conscious (as is usual during operations on the brain) and were required to perform either a visual or a verbal matching task (to say whether two lines were orientated at the same angle, or whether the names of two objects projected on a screen, rhymed). In the case of the verbal task, on some trials the subjects were asked to voice the names of the objects, on others they were asked to work out the answer in their head. By direct measurement of evoked potentials it was found that the motor speech areas seemed to be involved, but that the premotor areas were more active than the motor areas during the silent solving of this kind of problem.

It seems likely that in future years increasingly sophisticated techniques will reveal much more about the organisation of inner speech in the central nervous system.

iii) Experimental Studies of Speech-Motor Behaviour Interaction

A third class of studies relevant to the present discussion consists of those in which the production of speech is manipulated - directly or indirectly - in order to assess its effect on performance.

Luria´s experiments, desribed earlier in the chapter, obviously fall into this category. In the late 1960s and afterwards, a number of attempts were made to replicate Luria´s results in the West. As these have proved to be somewhat problematic, these will be examined in some detail.

In fact, replications of Luria´s work have tended to focus on two rather different aspects of his theory. On the one hand, some studies have tended to focus on the changing ways in which instructions affect the growing child´s performance on various tasks; on the other hand, some studies have tended to focus on the effects of self-instructions.

Generally, studies of the effects of instructions have produced results consistent with those of Luria. Indeed, it is difficult to see how this could not be so as common sense suggests that instructions are likely to have increasing control over motor behaviour with age. Golden, Montare and Bridger (1977) used a delay task originally designed by Luria (subjects were instructed to make a particular response, for example finding a cookie hidden under a box, and a short delay was introduced before they were allowed to make the response). It was found that three year olds learnt to perform correctly more quickly than two year olds. In a similar study, Birch (1966) investigated the effect on lever pressing of instructions to press, presented either periodically or contingent upon the release of pressure, and of a buzzer (which the subjects had been preinstructed to respond to as if it were a command to press), also presented periodically or contingently. Again, performance increased with age. Moreover, the youngest subjects (two to four years of age) could only respond to the instructions and not the buzzer. Beiswenger (1968) used a variant of the Luria two choice paradigm (subjects had to respond to a green light and not to a red light - later these conditions were reversed) and found that three and a half to four and a half year olds could only respond correctly on sixty per cent of trials (just above chance level) whereas subjects over five and a half years of age could perform correctly on more than ninety per cent of trials, a result that is in broad agreement with those of Luria. Finally, similar results were obtained by Stommen (1973), using a version of the game ´Simon says....´, also conceived as a variant of the Luria two choice paradigm. If the experimenter gave a command prefaced by "Simon says...." (eg. "Simon says touch your nose!") the child was required to respond appropriately whereas, if the experimenter used an unprefaced command the child was required to do nothing. As in Beiswenger´s study, it was found, in line with Luria´s theory, that two and a half year olds responded whatever the experimenter said, and only those over five years of age could respond only to the prefaced commands. To the extent to which we can accept Vygotsky´s argument that the child´s ability to be regulated by instructions is an important indicator of his future ability to regulate his own behaviour, these results, then, provide evidence supporting the Vygotsky-Luria theory of cognitive development.

It was when attempts were made to replicate Luria´s research with self-instructions that difficulties became apparent. Joynt and Cambourne (1968) used versions of both the one choice and the two choice discrimination tasks, together with another task described by Luria (the dual response task) in which subjects were required to respond twice to a stimulus. The subjects in the study ranged from one and a half to seven and a half years and were required to (i) press continuously to a red light until told to stop; (ii) press with appropriate "Squeeze" self-instructions on the one choice, two choice and dual response tasks (in which latter case the self-instruction used was "Squeeze, squeeze"); (iii) respond as in (ii) but with more complex self-instructions ("I must press now," in the one choice task, "Squeeze" and "Don´t squeeze," in the two choice task, and "Press twice," in the dual response task); and (iv) respond as in (ii) but in silence. In general, the results supported Luria´s theory. Responding on the one choice task occurred correctly in the younger subjects only with the aid of the "Squeeze" self-instruction, although the older subjects could perform just as well either in silence or with the more complex self-instruction. No effect resulting from self-instructions was detected in the two choice task (in which performance improved with age) but it was found that only the oldest subjects could perform the dual response task with the aid of the "Press twice" self-instruction, indicating that only in their case did the meaning of the self-instructions determine the response.

Jarvis (1968) carried out a similar study, using the two choice task with four groups of children, of mean ages 3 years 9 months, 5 years, 6 years, and 6 years 7 months. Subjects performed in three counterbalanced conditions: unaided; with self-instructions to "Push" as appropriate; and with the self-instruction "Don´t push" as appropriate. The older children performed better than the younger children but again no effects due to vocalisation were found.

Miller, Sheldon and Flavell (1970), in perhaps the most frequently cited of all these early studies, also used the two choice task, assigning subjects of mean age 3 years 2 months, 3 years 6 months, 4 years 1 month, and 4 years nine months to one of four conditions: unaided; with appropriate "Squeeze" self-instructions; with appropriate "Don´t squeeze" self-instructions; or with both kinds of self-instructions. Again, errors decreased with age but no facilitating effect due to the self-instructions was detected. Moreover, Flavell et. al. observed that those children who performed with self-instructions typically responded before speaking and they argued that this evidence in particular cast Luria´s theory into doubt.

The results of Jarvis and of Miller et al., when taken at face value, certainly do seem to count against Luria´s account. However, Wozniak (1972), on the basis of a careful scrutiny of the methodology employed in each study, has argued that in fact they do not. In particular, all of the above studies used warm-up procedures in which the subjects were pretrained on the task to ensure comprehension. For example, in the Jarvis study, subjects were pretrained to a criterion of six correct responses to the positive stimulus with no concurrent response to the negative stimulus, thus completely negating the whole excercise. In addition, Jarvis´ study in particular used children of the wrong age to test Luria´s account. In both the Jarvis study and the Miller et. al. study the interstimulus interval was so short (600 milliseconds and 1.5 seconds on average respectively) that perseverative responding was virtually impossible. Finally, of course, Miller et. al. were wrong in assuming that, for Luria´s account to be correct, the verbal response had to precede (or ´mediate´) the motor response (see section on Luria´s theory above). Wozniak concluded by arguing that none of the above described studies (even the Wilder and the Joynt and Cambourne studies, with their suggestions of support for Luria) amounted to fair tests of Luria´s theory.

In the time since Wozniak´s review was published, several further attempts have been made to replicate Luria´s work with self-instructions and these attempts have proved much more informative than the earlier studies that we have already reviewed.

A variant on Luria´s task that allowed a detailed exploration of the effects of self-instructions was designed by Meacham (1979), who addressed directly the observation by Miller et al. that speech sometimes followed, rather than preceded motor action. Meacham, like Wozniak, was critical of interpretations of Luria´s work that insisted that speech must always come between an environmental stimulus and a response and thus mediate behaviour. Meacham argued, on the contrary, that verbal responses following motor responses could regulate behaviour by describing the outcome of completed motor activities and, in older children, aid subsequent performance by allowing the child to remember the goals of action. As Meacham pointed out, failure to desist from responding on the two choice task may simply mean that the child has forgotten which stimulus he is required to respond to.

Children in three age groups (mean ages of three years four months, four years five months and five years and seven months) were required to ride a bicycle in response to tape recorded sequences of commands. Half the children were told to repeat the commands out aloud. Two sequences were used. The first, consisting of "Go"s and "Stop"s, was analogous to Luria´s two choice paradigm. The second consisted of "Up"s, "Back"s and terminated in "Stop" for comparison. Pretraining, to ensure that the subjects understood the instructions, was carried out with a toy truck, not on the bicycle. Meacham found, as had Miller et. al. (1970), that verbal responses typically followed motor responses, but that, if the motor response made had been inappropriate, change to the appropriate motor response was more likely to occur if the child had verbalised the command, and usually happened following the verbalisation. Interestingly, Meacham noted that Stommen (1973) and a number of other investigators had observed that young children seemed to give no indication of recognising that they had responded inappropriately when they did so. Furthermore, it may be remembered that Goodman (1981), in her careful analysis of naturally occurring acommunicative speech, found that some types of verbalisation, in particular descriptions of plans and thoughts and questions asked and answered by the self, tended to follow unsuccessful attempts to solve problems, in line with Meacham´s findings.

Another analysis of the temporal relationships between speech and motor activity was carried out by Tinsley and Waters (1982), who were again critical of the view that, to support Luria´s account, speech had to precede action. Instead, Tinsley and Waters argued, speech may well occur at the same time as action. In the first of their two experiments, they asked two year olds to hit an object once with a hammer in silence and then with either (i) a simultaneous and appropriate self-instruction ("One"); (ii) a simultaneous inappropriate self-instruction ("Toy"); or (iii) the appropriate verbalisation preceding the response. In line with expectations, they found that both types of self-instructions facilitated responding if they occurred at the same time as the response but that instructions to speak before responding actually decreased responding. This supports Luria´s suggestion that if speech can be made to control behaviour at this age it is not the meaning of the words that is crucial. Tinsley and Waters also found that, as Meacham suggested, children who responded inappropriately were rarely aware of doing so.

In a second experiment with two and half to three and a half year olds and three and a half to four and a half year olds, Tinsley and Waters compared simultaneous appropriate verbalisations with a silent condition, using two tasks - their version of the one choice task and a more complex task in which the child was required to hit three coloured pegs in a certain sequence. (The self-instruction in this case was "Red, green, blue", the colours of the pegs.) In both age groups the self-instructions facilitated performance, especially on the difficult task.

These experiments, then, provide evidence that tends to support Luria´s account. However, it appears that verbal regulation involves more complex processes than has sometimes been supposed. It is perhaps possible to distinguish a number of different ways in which verbalisations may affect behaviour, either by occurring before, after or during responding. This is an issue we will return to shortly. In his review of replications of Luria's theory, Wozniak (1972) noted that there was much that supported the theory to be found in the results of studies which were not originally designed to replicate Luria's findings. It is to such studies that we must now turn.

One example of a research tradition that is highly relevant to Luria's theory consists of studies in which the effects of self-instructions on lever pressing have been assessed. One example of this kind of research is a study carried out by Bem (1967), in which three year old and four year old subjects were pre-assessed on a number concept task. The subjects were shown a series of lights and were then required to press a lever once for each light that had been illuminated. It was found that the four year olds could do this reasonably well but that the three year olds generally failed to press the lever the correct number of times. The three year olds were subsequently taught to count the lights while they were illuminated and then to count out lever presses, with the result that their performance improved to a level comparable to that of the four year olds.

A series of similar experiments was earlier reported by Loovas (1964), who looked at the relationship between speech and motor behaviour in more detail than Bem. In one of his studies, Loovas found that when five and six year olds were reinforced for saying "fast" in the presence of one light and "slow" in the presence of another, the presentation of the lights during lever pressing had an effect if the subjects had previously been taught to respond appropriately to instructions from the experimenter to respond quickly or slowly. However, without such training the lights had little effect. (Loovas claimed that this was because verbal control of motor responding is usually weak in five and six year olds. An obvious analogy can be seen between the results of this study and Vygotsky's claim that it is necessary for the child to learn to follow the commands of others before he can learn to regulate his own behaviour.) In another of Loovas' studies, subjects were taught to say "fast" and "slow" in the presence of two lights and were then required to turn off the lights by means of lever presses. The pre-training had a non-significant effect on the response latency of six year old subjects, but an increasing effect on response latency in older subjects. In another experiment, subjects between four and a half and just over five years of age were taught to count quickly or slowly in the presence of lights and were subsequently exposed to the lever. Loovas found that, for these subjects, lever pressing did vary with the presentation of the lights and, furthermore, that the subjects generally made one lever press per counting response. These studies serve to highlight the different ways in which speech can regulate behaviour. In the case of the six year olds and older children, the words "faster" and "slower" do appear to control behaviour and it is the meaning of the words that appears to have been important. In the younger subjects taught to count, the tempo of motor responding seems to have been controlled by the tempo of counting.

Two related studies that need to be considered in this context were carried out by Meichenbaum and Goodman. Meichenbaum and Goodman (1969a) looked at the effects of the overt self-instructions "letter" (semantically irrelevant), "faster" and "slower" on finger tapping in five year olds and seven year olds. In all cases, utterance of the word "letter" decreased response rate, "slower" decreased it further still and "faster" increased it, indicating both the semantic control of motor behaviour and also a general decrement in response rate brought about when the subjects were required to do the irrelevant task (saying "letter") at the same time as finger tapping. When the experimenter said the words instead of the subjects doing so, "letter" had no effect on response rate and "slower" and "faster" had effects in the expected directions. Meichenbaum and Goodman (1969a) also instructed their subjects to whisper the self-instructions to themselves so that the experimenter could not hear them (a covert self-instruction condition) and found that the difficulty of the task led to a general and equal decrement of response rate irrespective of the actual self-instructions in the case of the younger subjects, but an effect similar to that brought about by overt self-instructions in the older subjects.

In their second study, Meichenbaum and Goodman (1969b) attempted to relate verbal self-control to impulsivity. Five to six and a half year old subjects were taught to say "push" to one light and "don´t push" to another and the lights were presented to them in a random order. Subjects were instructed to turn off one of the lights by pressing a pedal with their feet, but not to turn off the other light.

Both an overt verbalisation and a silent (assumed covert verbalisation) condition were included in the design. In the overt condition no difference was found between the impulsive and the non-impulsive children whereas, in the silent condition the impulsives produced significantly more errors than the non-impulsives. Wozniak (1972) notes that these results clearly support the Soviet position as, according to Luria, at five years of age speech should have semantic control over motor behaviour. The high number of errors made by the impulsive children in the silent condition suggests that they failed to use the self-instructions covertly.

In the studies we have reviewed so far, subjects have been instructed to produce certain self-instructions and the effects on task performance have been assessed. However, the verbal behaviour of subjects need not necessarily be manipulated by direct instruction. An alternative approach is to manipulate verbal behaviour by means of reinforcement. The effects of doing this on motor behaviour has been the subject of a number of studies.

One of the earliest examples of this kind of research was carried out by Loovas (1961), who used trinkets to reinforce three and a half to four and a half year old children for aggressive verbal behaviour towards small dolls (eg. "Bad doll!" or "Doll should be spanked!"). Subjects were then given access to a lever, the manipulation of which caused a doll to strike another doll. It was found that those subjects who had been reinforced for aggressive speech pulled the lever more often compared to both a baseline condition and an untrained group. A similar study by Sherman (1964) involved subjects between the ages of four years and two months and six years and eight months being reinforced for talking about a particular toy, with the result that the subjects were more likely to play with that toy in a subsequent play session.

Risley and Hart (1968) analysed this kind of interaction between verbal and non-verbal behaviour in more detail. Subjects were reinforced for reporting playing with particular toys and it was found that this procedure had little effect on the subject´s subsequent selection of toys. However, such an effect could be brought about by reinforcing the subjects only when they truthfully reported playing with the toy in question. After exposure to this contingency reinforcement of verbal behaviour alone did have an effect on toy preference. Actually, it is difficult to know exactly what the mechanism behind this behaviour change was. However, Risley and Hart´s experiment led to an interest in what has come to be known as ´correspondence training´. Thus, Israel and O´Leary (1973) reasoned that, for such training to be maximally effective, reinforcement would have to be contingent upon verbal behaviour preceding motor behaviour. In their study, Israel and O´Leary reinforced their subjects for announcing that they were going to make a particular response and then doing it. They found that this procedure was more effective than reinforcing the child for making a particular response and then announcing it.

It appears, then, that the manipulation of verbal behaviour, either by means of instruction or by means of reinforcement, does bring about a corresponding change in motor responding in accordance with Luria´s theory. It is perhaps not surprising that attempts have been made to apply these findings in clinical settings. The most important development in this area in recent years has been Meichenbaum´s (1977) ´self-instructional training´ (SIT). In SIT, subjects are explicitly taught to instruct themselves in a variety of situations. The training is carried out by means of instruction, demonstration and reinforcement. Although SIT has been used with a wide range of patient populations, studies in which it has been used with children are of obvious importance to the present discussion. In fact, SIT has been used particularly with impulsive children (usually above six years of age) on the grounds that such children often fail to regulate their behaviour by means of speech, which is why they are impulsive. Generally, the results of these studies have been promising (cf. Palkes, Steward and Kahana, 1968; Meichenbaum and Goodman, 1971; Bornstein and Quevillon, 1976; Camp, Bloom, Herbert and Van Dooninck, 1977; Meichenbaum, 1977; and Nelson and Birkimer, 1978). In fact, in most of the studies of the effects of SIT carried out to date, the extent of the training has been limited and, although significant behaviour change has been reported, more evidence is needed to show that the results of SIT generalise to situations outside therapy. Even so, the fact that SIT appears to work at all provides yet further support for Luria´s theory.

CONCLUSION

There can be scarcely any doubt, given the evidence reviewed above, that speech is an important component in human thought, and that the ability to use speech in this way develops roughly in the manner described by Vygotsky and Luria. Overt acommunicative speech appears to have a regulatory role in the lives of young children; covert or inner speech seems to play an important role in adult problem solving; and studies in which the effects on motor behaviour of manipulating verbal behaviour have been assessed support Luria's account of the growth of this skill.

At this point it might be profitable to integrate these findings. Table 1 summarises the relevant studies of the effects of self-instructions detailed above according to the apparent functional relationship observed to exist between saying and doing. The table also includes information about the ages of the subjects and the types of tasks used. As alluded to at several points above, a relationship seems to exist between the nature of the verbal control established and the ages of the children studied. In particular, at least three functional relationships between speech and behaviour can be discerned.

(i) Speech occurs simultaneously with motor responding. This seems to happen in the case of very young children. The tempo of speech seems to control the tempo of responding. One motor response occurs to every verbal response and

Table 1: Developmental trends in speech-motor interaction.

Observed relationship	Experimental studies	Age range
Speech occurs simmultaneously with motor activity.	Luria (1959, 1960)[1.]	3-$3\frac{1}{2}$yrs (approx.)
	Loovas (1964)[2.]	$4\frac{1}{2}$-5yrs (approx.)
	Bem (1967)[3.]	3-4yrs (approx.)
	Tinsley and Waters[4.] (1982)	2-$3\frac{1}{2}$yrs (approx.)
Speech occurs after the motor response.	Luria (1959, 1960)[1.]	3-$3\frac{1}{2}$yrs (approx.)
	Miller, Sheldon and[5.] Flavell (1970)	3-5yrs (approx.)
	Meacham (1979)[6.]	3-6yrs (approx.)
Speech occurs before the motor response.	Luria (1959, 1960)[1.]	4yrs+
	Loovas (1964)[2.]	5yrs+
	Meichenbaum and[7.] Goodman (1969a)	5yrs+
	Meichenbaum and[8.] Goodman (1969b)	5yrs+

1. Luria tasks (one choice; two choice; dual response).
2. Lever pressing.
3. Number concept lever task.
4. Hitting (one choice; sequential).
5. Two choice lever pressing task.
6. Riding bicycle (two choice).
7. Finger tapping.
8. Pedal pressing (two choice).

whatever semantic control speech has over behaviour appears to be weak. In Luria´s terminology, it is the impulsive aspects of speech that control behaviour.

(ii) Speech occurs after the motor response. On the one hand, in Luria´s studies and in that of Miller et al., speech seems to inhibit further responding. On the other hand, in the case of Meecham´s study, the verbal response seems to determine the character of any further corrective response. The extent to which these two types of verbal activity differ from each other, and also from simultaneous verbal self-regualtion, remains unclear. The meaning of the utterance may be less important when speech inhibits motor action than when it determines the nature of the corrective response. It is not inconceivable, therefore, that the type of verbal self-regulation described by Meecham, in which speech serves to help the subject remember the goals of action, marks the transition from (again using Luria´s terminology) the impulsive to the semantic control of behaviour.

(iii) Speech occurs before the motor response and has semantic control over motor behaviour. This is what has sometimes been called verbal mediation (Reese, 1962; Kendler, 1964). It seems obviously somewhat different from

(i) above, and perhaps also from (ii) also. This type of verbal control is observed only in subjects five years of age and older and is exemplified by the work of Meichenbaum and Goodman.

Of course, if this account is correct, then it in no way implies that the end point of child development is reached when the child has acquired the ability to verbally self-regulate. For the child to effectively use language to his own advantage he needs not only the ability to follow his own speech but appropriate verbal repertoires with which to instruct himself. To take an extreme example, in order to think nuclear physics it is first necessary to acquire the language of the nuclear physicist.

It may be argued that, in the early life of the child, some concepts are acquired relatively late and others relatively early. It has been claimed, for example, that the child can describe simple functional relationships and make infer ences of causality by the age of three years (Flavell, 1977; Bullock, Gelman and Baillargeon, 1983), but that the ability to use conventional time units and remember complex temporal relationships does not develop until the fourth and fifth years (Friedman, 1978). The ability to use quantative concepts, on the other hand, seems to develop in stages, the three year old being capable of making judgements of relative magnitude while only older children are capable of making precise numerical judgements (Flavell, 1977). A crucial difficulty here is that different researchers have investigated concept development in different ways so that what has been investigated is not always clear. In some cases, it appears that verbal concepts (ie: the ability to use words in certain ways) have been measured whereas in other cases it is the ability to make discriminations that has been assessed. As we shall see in the next chapter, this confusion can lead to difficulties.

Leaving this issue aside for the moment, it may be noted that according to the verbal regulation hypothesis, we should expect the acquisition of language to be at least a necessary requirement for adult-like responding on schedules of reinforcement. It seems, then, that the development of verbal self-regulatory skills may be reflected in schedule performance at different stages in childhood. This possibility will be considered in the next chapter.

CHAPTER 5: DEVELOPMENTAL ASPECTS OF HUMAN OPERANT BEHAVIOUR

We have seen that, when attempts have been made to condition human adults, the results have not usually been in accord with the results of animal studies. On the contrary, wide differences seem to exist between human adult and animal learning (Chapter 2). It has been suggested that these differences are the result of the human adult's ability to regulate his own behaviour by means of speech (Chapter 3). Developmental studies indicate that, not surprisingly, this ability depends on age (Chapter 4). If human adult operant behaviour does reflect man's ability to use language in this way, as, for example, Lowe (1979) has suggested, and if the Vygotskian theory of verbal regulation is even approximately correct, then one obvious prediction is that the operant behaviour of children should reflect their changing capacity to control their own behaviour by means of speech. In other words, the older a child is the less animal-like his behaviour should be. An infant who lacks language altogether should behave, in all respects, like an animal. As a matter of fact, no systematic attempt has been made to test this prediction. However, a certain amount of relevant evidence does exist, and it is this that will be examined in the present chapter.

SIMPLE OPERANT CONDITIONING AND SOME CONSIDERATIONS

Numerous studies have succeeded in demonstrating simple operant behaviour in infants and, as Lancioni (1980) has noted in a detailed review of the literature, it has been shown that a variety of responses can be brought under operant control. Indeed, so much research on simple operant conditioning with infants has been published to date that only a small selection of the available evidence can be discussed here.

Among the responses that have been successfully conditioned are vocalisation (eg. Finkelstein and Ramey, 1977); smiling (eg. Etzel and Gerwitz, 1967); sucking (eg. Kobre and Lipsitt, 1972); visual fixation (eg. Watson, 1969); headturning (eg. Watson and Ramey, 1972); and manipulation (eg. Lipsitt, Pederson and De Lucia, 1966). In some experiments operant conditioning has been demonstrated with subjects only a few days old (see below for several examples) although, in most cases, infants of at least three months have been studied.

An equally wide number of stimuli have been demonstrated to be effective reinforcers of infant behaviour. Thus, Lancioni found that social stimulation (eg. speech, smiling), tactile stimulation, mother´s face, mobiles, flashing lights, bells, music, multicoloured toys, milk and edibles have all been used at one time or another to change infant behaviour. In a few studies, reinforcement has been varied systematically in an attempt to isolate the crucial components. Thus, Ramey and Ourth (1971) and Millar (1972) have investigated the effects of a delay between the response and reinforcement and discovered that, for a stimulus to be effective, it has to occur within two seconds of the response. Millar and Schaffer (1972) also investigated the effect of position with a visually presented reinforcer and found that, at six months, a stimulus displaced sixty degrees from the direction of an infant´s gaze ceases to function as a reinforcer, whereas, at nine months, displacement of the stimulus does not seem to have such a drastic affect.

It seems, then, that there can be little doubt that infants can be operantly conditioned.

SCHEDULE PERFORMANCE IN CHILDREN AND INFANTS

Although simple operant conditioning has been amply demonstrated in infants and young children comparatively little schedule research has been carried out. On the one hand, the goal of operant research with children has often been the solution of practical, rather than theoretical problems. On the other hand, similar sorts of assumptions about the continuity of human and animal learning as those prevalent, until recently, in the study of adult operant behaviour, have perhaps been accepted by the child psychologists. In any case, more effort has been made to study which reinforcers are effective and which responses can be conditioned than to investigate the way that schedule performance is related to age and development. When schedule work has been carried out with children, the exact ages of the subjects have not always been reported so that, while the data that does exist is of the interest, the age course of operant performance cannot be specified in any detail.

(i) Interval Schedules.

There has apparently been no attempt to investigate fixed interval performance in infants and the only research available has been carried out with children above the age of four years. Long, Hammack, May and Campbell (1958), using trinkets as reinforcers and a telegraph key response, studied the performances of a large number of children aged between four and eight years, on a number of schedules, including different values of FI. Some children failed to produce stable performances, their cumulative records showing sections of high-rate, low-rate, scalloped and inverse-scalloped behaviour. Other children responded at a constant high-rate, similar to the pattern produced by animals on VI (or some adults on FI). Few children produced scallops and, when they did, the pattern was transient, and was replaced within a few sessions by either the high-rate pattern or by irregular responding. Unfortunately, Long et al. made no attempt to relate the behaviour of their subjects to their ages which, as we have seen, were within a wide range. Furthermore, no parametric data were given so that direct comparison with the results of animal or adult research is not possible. However, Long and his colleagues did suggest that a scalloped performance was most likely to be produced (a) if subjects had previously been exposed to a VI schedule; (b) after shifting from small to large FI values; and (c) if no prior experience was given on FR schedules. Long (1962), using the same methodology with similarly aged subjects also found that, on multiple FR-FI schedules, responding in the FI component was generally high-rate and undifferentiated from responding in the FR component (see below).

Broadly similar results have been described with four to five year olds by Zeiler and his colleagues. Zeiler and Kelley (1969) used cartoon slides as reinforcers and a lever press as a response on a FI 30 second schedule concurrent with extinction (ie: a second lever was available but did not produce reinforcement). There was a change-over delay of 3 or 6 seconds so that reinforcement was not immediately available on FI after responding on the extinction lever. No evidence was found of pausing or scalloping on FI but an effect due to prior exposure to an FR schedule was discovered (see below). De Casper and Zeiler (1972), with similar aged subjects, investigated FI 1 minute and FI 1 minute 15 second behaviour on multiple schedules, the other component being FR 20. Both high-rate and low-rate performances, not dissimilar in appearance to that of adults, were observed and were found to relate to conditioning history (again see below). No further work with

FI schedules has been reported. From the data described above, then, all that can be concluded is that children above four years of age either tend to behave like adults on FI, or fail to produce a stable pattern of responding at all.

As with FI schedules, little work has been done to investigate the way that variable interval performance changes with age. Long et al. (1958) also tested children on VI and found that their subjects tended to respond at a steady, stable rate, but no evidence was collected to assess whether the rate was systematically related to the schedule value. The only attempt to study VI performmance with infants has been made by Weisberg (1969), who used various multiple and chain schedules consisting of VI, DRO and extinction components. His subjects were between fifteen and twenty five months old. Although detailed information about response patterning was not given it is clear that responding on the three types of schedule was well differentiated, so that responding on VI was at a much greater rate than responding on DRO and extinction. This finding is certainly not inconsistent with the results of animal studies. However, as no other data for the VI schedule performance of children exists, no firm conclusions about the nature of infant VI responding can be reached.

(ii) DRL Schedules

Again, little work has been carried out with young children on DRL schedules. Stein and Landis (1975) exposed eight year olds to a DRL 6 second schedule for half an hour. Points exchangeable for candy were given as reinforcement and a telegraph key was used to collect responses. Subjects were divided into two groups, impulsives and reflectives according to performance on Kagan´s Matching Familiar Figures Test. Non-instructed impulsives responded at a much higher rate than non-instructed reflectives (though the rate dropped throughout the experimental period in all cases). However, both groups produced a similarly increasing number of collateral responses on three non-effective keys. Impulsives also emitted more bursts of responding than reflectives. Throughout the half-hour the relative frequency of responses occurring more than 6 seconds after the previous response increased, indicating increasing adaption to the schedule.

Pouthas (Pouthas, 1981; Fraisse, 1982) has studied the DRL performance of younger children, using a manipulandum response and slides with an accompanying soundtrack as reinforcers. Two - year - old subjects were exposed to DRL schedules of values increasing up to 10 seconds, and four year old subjects were exposed to DRL values increasing up to 15 seconds. (Subjects were moved from one DRL value to the next after they had learnt to obtain a reinforcer with thirty per cent of responses or after sixty responses, whichever was sooner.) With minimum instructions, three out of the nine two year olds showed evidence of temporal discrimination on DRL 10 seconds, whereas four out of twelve of the four year olds showed evidence of temporal discrimination on the DRL 15 second schedule. Fraisse (1982), on the basis of this research, commented that: "One conclusion is clear: At least some children between two and five years old are capable of adapting to intervals of ten to fifteen seconds. It is possible to obtain better results with rats, but only after a tremendous number of trials." Pouthas (1983) subsequently extended her work by investigating the DRL 5 second performances of subjects aged between two and seven years. She found that all her subjects reduced their rate of responding during exposure to the schedule. However, only her subjects above four and a half years of age showed an increasing proportion of long interresponse times (IRTs) compared to the number of short IRTs, indicating that only these children had learnt to make the appropriate temporal discrimination during the six sessions of testing.

None the less, it is not clear that infants are incapable of making temporal discriminations. Weisberg and Tragakis (1967) investigated the DRL performances of even younger subjects. Five children between fifteen and forty-one months of age were tested using a lever to collect responses and snack items as reinforcers. The subjects were exposed to a DRL 2 second schedule, followed by sessions on DRL 10 seconds and, in the case of three of the subjects, further sessions on DRL 18 seconds. Response rates dropped during training in accordance with expectations and the final cumulative records are remarkably animal-like (showing, for the most part, one response per interval, with occasional response bursts, either just before the availability of reinforcement or just after reinforcement had been delivered). Indeed, the similarity with animal data is surprising considering the limited number of sessions given (between five and ten on DRL 10 seconds, up to thirteen on DRL 18 seconds). In contrast to Pouthas, Weisberg and Tragakis noted that, as training progressed, the proportion of short IRTs decreased and the proportion of longer IRTs (greater than the schedule value) increased, providing evidence of temporal discrimination.

The evidence, then, suggests that children of varying ages may be able to adapt to DRL schedules. Although the evidence on temporal discrimination is not consistent, it seems that, in some circumstances at least, infant subjects may be able to adjust the spacing of their responses in the same manner as animals.

(iii) Ratio Schedules

Fixed ratio performance has been more widely investigated with children and infants than any of the time-based schedules. Long et al. (1958) found that their FR subjects sometimes produced pauses after reinforcement if the initial ratio value was between 20 and 60 and if the shift towards larger ratios was gradual. However, on the FR components of multiple schedules Long (1962) found that performance tended to be uniformly high-rate. Zeiler and Kelley (1969) and De Casper and Zeiler (1972) also found that their subjects on the FR components of concurrent or multiple schedules responded at a relatively stable and high rate.

FR schedules have been used with infant subjects by a number of researchers. Weisberg and Fink (1966) investigated the FR performance of five infants ranging between fourteen and a half and nineteen and a half months of age. They used a lever to collect responses and snack items as reinforcers. The subjects were exposed to increasing FR schedules for between four and nine sessions, finishing with FR 10 in four cases and FR 15 in the fifth case. In general, all subjects produced a break and run pattern, similar to that produced by animals. Some variability was observed, however, particularly in the post-reinforcement pause. Weisberg and Fink noted that similar results have been obtained with animals close to satiation (eg. by Ferster and Skinner, 1957). Following FR training, Weisberg and Fink placed their subjects on extinction and found results broadly comparable to those obtained from animal subjects on extinction following exposure to FR schedules.

Siqueland (1968) investigated the FR responding of thirty - four newborns aged between fifty - one and ninety - six hours old. The response used was head turning and the reinforcer was a non-nutritive nipple. Training was only carried out for fifteen minutes, some subjects being trained on FR 2, some on CRF and others on what amounted to a DRO 20 second schedule. As expected, the ratio trained babies responded at a higher rate and took longer to extinguish than the others. No other data were shown. Bosack (1973) conditioned fifty babies of a similar age (thirty one to one hundred and three hours old) on either FR3, FR5, FR7 or FR10 schedules, using sucking as a response and nutritive feed as a reinforcer. However, in this study, training lasted for only five minutes. None the less, responding was highest on FR 7, second highest on FR 5 and third highest on FR 3. Responding on FR 10, on the other hand, was not significantly greater than responding to CRF using non-nutritive feed (a control).

Perhaps the most ambitious use of FR schedules with infants was made by Sheppard (1969), whose subject was his own three month old son. Throughout the many conditions two responses were collected - vocalisations and leg kicks. Reinforcement was varied throughout the experiment and consisted of either five seconds of vibration (delivered via a vibrator taped to the subject's palm); five seconds of flashing lights and a recording of mum's voice; or a five second recording of mum's heartbeat (used only once). In all, sixty - seven sessions were carried out, usually on consecutive days, each lasting between twenty and thirty minutes. During testing the subject lay in an air crib and all recording of data and delivery of reinforcement was by means of automatic equipment. At first vocalisation was successfully trained on FR 3 but when multiple FR DRO schedules were introduced there was no differentientation between responding on the two components. Sheppard argued that this was because the lack of reinforcement on the DRO component led to crying which, of course, delayed reinforcement further. Kicking was also succesfully established on FR 5. Towards the end of the study, Sheppard introduced both response devices (a microphone and a leg-operated microswitch) into the crib together and presented his son with alternating five minute blocks of FR3 reinforcement for kicking and FR3 reinforcement for vocalisation, each correlated with a discriminative stimulus. By this means the infant was allowed to decrease his rate of producing one response without undergoing a reduction in reinforcement. In this case responding was well differentiated and changeovers from one type of responding to the other were very rapid. Although no parametric analysis was carried out on the data, the cumulative records from throughout the experiment are particularly impressive and seem, by and large, to correspond to the break and run pattern where appropriate. However, some variation is observable and, as Sheppard noted, the pause following reinforcement was sometimes very short. On balance, then, the available data suggest that the FR performance of infants is similar to that of animals.

A little work has also been carried out on VR schedules with infants. In particular, Brackbill (1958) found that infants on a VR 2 schedule responded at a higher rate and took longer to extinguish than infants on CRF. However, no cumulative records were given of the data and it is impossible to reach any conclusion about the nature of the infants′ behaviour.

To summarise: no systematic attempt has yet been made to relate schedule performance to age. On the whole, however, the limited evidence there is suggests (but no more than suggests) that infant subjects may condition in much the same way as animals (cf. the work of Weisberg and his colleagues) whereas there is some indication that older children (eg. those in the studies of Long and Zeiler and their colleagues) may behave differently on reinforcement schedules. It will be recalled that, in the case of adult responding, performance is influenced by a variety of factors (which were described in Chapter 3). It is the influence of these variables on the behaviour of children that we must now turn to.

VARIABLES FOUND TO AFFECT ADULT PERFORMANCE

Few attempts have been made to investigate with children the effects of those factors found to be important with respect to adults. None the less, some information is available.

(i) Instructions

It is not possible to instruct infants. Little evidence is available to suggest at what age instructions become an effective determinant of the operant behaviour of the child. Stein and Landis (1975) found that the temporal discrimination of their eight year old subjects on DRL schedules could be improved by instruction, reducing the differences observed between the performances of impulsive children and reflective children. Pouthas (1981) also instructed her subjects, having first exposed them to DRL schedules with minimum instruction. The exact instructions given were: "You should not press the button all the time, but you should wait a little to see the pictures". Some changes were observed in the behaviour of two of the two and a half year old subjects. In the case of the four year olds, however, three of the six subjects who had not previously shown evidence of learning the DRL contingency achieved temporal discrimination on the DRL 10 second schedule. In her later study, Pouthas (1983) instructed some of her three and a half and four and a half year old subjects on DRL 5 seconds and found that the instructions were immediately effective in the case of the older children but less effective in the case of the three and a half year olds.

Because of her relative lack of success using instructions with her younger subjects, Pouthas advanced the hypothesis that poor performance may reflect a lack of time concepts, suggesting this as an alternative or at least additional hypothesis to Lowe's suggestion that schedule performance would reflect differing levels of verbal skill. Generally, conclusions based on isolated failures of this sort should be treated with caution because the literature on developmental psychology is littered with false negative results obtained by the use of inappropriate instructions (Donaldson, 1978; Brown and Desforges, 1979, cf. Chapter 4). More particularly, Pouthas' results cannot entirely be attributed to a lack of time concepts as temporal discrimination on DRL schedules has been found with uninstructed infants (Weisberg and Tragakis, 1967) and, of course, with animals (cf. Richelle and Lejeune, 1979). Of course, the central issue here is the question of what counts as evidence of a concept. If concepts are to be inferred from non-verbal performance, are we to attribute concepts to animals in some cases? If they are to be inferred from verbal behaviour or a subject´s response to instructions should we regard conceptual behaviour as uniquely human? In passing, it should perhaps be noted that some writers (eg. Anderson, 1976) have suggested that information about temporal relationships is encoded linguistically in adults and that verbally retarded and deaf children are particularly bad at remembering temporal sequences (cf. Blank, 1974). This suggests that, in one sense of ´concept´ at least, some concepts may be closely related to the ability to verbally self-regulate.

In summary, then, it appears that all that can be inferred from the above research is that instructions can have an effect on the behaviour of children above the age of four and a half years. The effect of instructions on younger children remains in doubt. The role of time concepts, if any, in operant responding will be further considered in subsequent chapters.

(ii) Conditioning History

Again, few data exist on the effects of conditioning history on the performance of children of different ages. However, some circumstantial evidence has been reported. In Long's (1959, 1962) experiments with multiple FR-FI schedules, subjects between four and seven years of age rarely showed evidence of responding at different rates to the different schedule components, although stimulus control could be established by a variety of procedures (eg. by differentially reinforcing responding on the two components, by satiating the subjects, and by increasing the size of the FR). Long (1962) also noted that a low rate of responding could be established on the FI component of a multiple FR-FI schedule by adding a DRL contingency to the FI component during training.

Zeiler and Kelley (1969) obtained similar results. When their four to five year old subjects were changed from FI to FR, response rates increased. However, when subjects were changed from FR to FI, "The rates remained at the FR level for much of the session. One child.... declined in rate by the middle of the session and changed from a steady rate to a more erratic pattern; the other.... revealed similar changes by the end of the day. These data indicated that when fixed-intervals generated a moderate or low rate, fixed-ratios increased and stabilized responding, but that the high steady rate generated by FR persisted through much of a single day of FI". Also in a similar vein to Long (1962), De Casper and Zeiler (1972) noted evidence of behavioural rigidity in four and five year olds on their multiple FR-FI or DRL schedules. No difference was found between performance on the FR and FI components until subjects were exposed to a DRL component, after which responding on FI became low-rate.

Taken together, then, this evidence indicates that behavioural rigidity is a characteristic of the operant behaviour of older children. No relevant data exists for infant subjects.

CLASSICAL CONDITIONING

There thus seems to be no clear evidence of instructional effects or behavioural rigidity in children below four years of age. We have also seen that the available evidence suggests at least provisionally that infants operantly condition in the same way as animals on schedules of reinforcement.

This leads to the interesting question of whether infants can be classically conditioned in the same way as animals. As discussed at various points in Chapter 3, respondent conditioning in adults seems to be controlled by the same verbal processes which (it has been argued) control operant behaviour. Unfortunately, the effects of verbal self-regulation are often not as obvious in the case of classical conditioning as in the case of operant conditioning and are therefore usually inferred by asking the subject about his knowledge of the contingencies or by observing the effects of instructions. As a consequence, developmental trends in classical conditioning are likely to be less obvious and harder to detect than comparable trends in operant conditioning.

None-the-less, considerable evidence has accumulated to show that infants can be classically conditioned. To take two of the many examples in the literature, Fitzgerald and Brackbill (1971) conditioned eye blinking in infants approximately one month of age using a puff of air as the UCS and a tactile stimulus to the subjects' cheeks as the CS and Lipsitt and Kaye (1964) conditioned sucking in infants using an auditory CS and feed as the UCS. However, classical conditioning in infants seems to be no simple matter. In a review of the literature, Fitzgerald and Brackbill (1976) found that success or lack of success in conditioning seemed to depend upon the particular combination of stimulus and response employed. Thus, despite their success with eye blinks, Fitzgerald and Brackbill (1971), using the same CS, failed to condition an autonomically mediated pupillary response. None-the-less, the pupillary response has been successfully conditioned within a temporal conditioning paradigm in which a bright light was presented at regular twenty second intervals (Brackbill, Fitzgerald and Lintz, 1967). With animals, of course, it has frequently been observed that not all responses are equally conditionable to all stimuli (Seligman, 1970; cf. Chapter 2).

Clearly, an awareness of the contingencies of the sort found in adults can play no role in classical conditioning during infancy. Obviously, it would be interesting to know at what stage in the child's life awareness does become an important determinant of performance in the classical conditioning experiment. Fitzgerald and Brackbill (1976) found no relationship between conditionability and age during the first nine months of life, although they did find some evidence to suggest that autonomic responses may be more readily conditioned than skeletal responses in very young infants. The question of to what extent classical conditioning remains unaffected by verbal processes in later years, then, remains a tantalising and unanswered topic for future research.

CHAPTER 6: THE AIMS OF THE EXPERIMENTS

It has been argued that the essence of scientific theory, as opposed to the metaphysical, is the generation of predictions (ideally of events that have previously gone unobserved), the accuracy of which can be tested empirically (Popper, 1972; Chalmers, 1978). As noted at the beginning of Chapter 5, such a prediction is that children too young to speak will behave like animals on schedules of reinforcement. It might be further argued that, during the early stages of a child's acquisition of self-regulatory skills, suitable instructions will significantly alter a child's responding in the direction of more adult-like behavior. We have seen that, although the evidence that already exists certainly does not suggest that the contrary is the case, to date there has been little or no attempt to relate human schedule performance to age.

The broad aim of the series of experiments outlined in the following chapters was to put these predictions to the test. Fixed-interval schedules were chosen as the principal means by which the operant behavior of children of different ages could be assessed. This was because it is on such schedules that the most marked discrepancies between human and animal responding are observed. In the first study, the uninstructed fixed-interval performance of children between infancy and the age of nine years was investigated, and the way that such behavior varies with changes of the schedule parameter observed. In the second, the effects of different kinds of instructions and self-instructions on the fixed-interval performance of children was evaluated. In the final study, the effects of histories of exposure to FR and DRL schedules was studied, thus allowing an assessment of how performance on these schedules varies with age, as well as allowing an assessment of the interaction between age and the effects of conditioning history.

In the case of the older children in the studies to be outlined, it was found that a simple manipulandum would function as an appropriate response device. A combination of reinforcers that had been shown to be effective in the literature (social reinforcement, snack items, flashing lights, music and cartoon slides) were used together in order to lessen the possibility of satiation. In the case of the infant subjects, close attention was also made to the conditioning literature. However, it was felt that some of the responses commonly used in infant operant research (eg. vocalisation, sucking, head turning and foot kicking) were inappropriate for our purposes as they were either difficult to measure, not rapidly repeatable or related to consumatory behaviour. Manipulation and panel touching have, however, been shown to be appropriate responses for operant research with infants above four months of age by a number of investigators (eg. Lipsitt, Pederson and DeLucia, 1966; Weisberg and Fink, 1966; Millar, 1972; Hopkins, Zelazo and Kagan, 1973; Finkelstein and Ramey, 1977) and it was these responses that were collected in the experiments that are described in the following chapters.

With respect to reinforcing infants, attention was paid to ensuring that (i) the reinforcer was delivered within one second of the appropriate response and (ii) the infant received a variety of reinforcers in order to reduce the chance of satiation. Also with the infant subjects it proved important to maintain a flexible attitude towards experimentation with respect to selection of the reinforcers, location of the equipment, etc. These were guided by the parents.

Finally, it should be noted that research with children of all ages posed many practical problems. For example, some children were very shy and a great effort was required to gain their confidence. Co-operation would depend upon many day to day factors, such as how happy the child was at home or at school. These difficulties were accentuated in the case of the infant subjects, with whom problems connected with toileting and teething proved to be of particular importance.

CHAPTER 7: A STUDY OF UNINSTRUCTED FIXED-INTERVAL RESPONDING IN INFANTS AND CHILDREN

Previous studies of the fixed-interval performance of children (eg. Long, Hammack, May and Campbell, 1958; De Casper and Zeiler, 1972) have produced either the high and low-rate patterns of responding found in human adults (cf. Figure 1), or a broken, inconsistent pattern of responding not found in either adults or animals. None of these studies explicitly investigated the relationship (if any) between response pattern and age or other measures, nor did they investigate their subjects' sensitivity to the schedule parameter, or attempt to assess their hypotheses about the contingencies.

These were the aims of the present study.

Subjects

Children and infants in four age groups took part in this experiment. Group One consisted of two boys and a girl aged between seven and a half and nine years of age at the beginning of the experiment, with a mean age of eight years and one month and a mean developmental language age of more than seven years (the maximum score) on the Reynell Developmental Language Scale (Reynell, 1977). Group Two, consisting of two girls and a boy, ranged between five and six and a half years of age, with a mean age of five years and ten months and a mean developmental language age of six years nine months. The children in Group Three, two girls and a boy, were between two and a half and four years of age at the beginning of the experiment, with a mean age of three years and three months and a mean developmental language age of three years and nine months.

Group Four consisted of four infants, Ann, Jon, Pat and Will, all of whom were too young to be assessed on the Reynell scale, but who were aged respectively nine, ten, nine and thirteen months at the experiment´s beginning.

The nine subjects in the oldest three groups were recruited from local schools and nursery groups. The infants were recruited via personal contacts and a newspaper advertisement.

The ages of the individual subjects are given in Table 2.

Table 2: Ages and Reynell Developmental Language Scale scores of the individual subjects at the start of Experiment I (years and months)

Group	Subject	Age	RDLS score*
1	SR	8,3	7+
	IP	8,7	7+
	LR	7,6	7+
2	LC	5,9	6,6
	MK	6,6	7,0
	HP	5,3	6,9
3	JJ	4,0	3,9
	SM	2,6	2,9
	TS	3,10	4,3
4	JON	0,9	n/a
	ANN	0,10	n/a
	PAT	0,9	n/a
	WILL	1,1	n/a

* maximum score = 7yrs.

Apparatus

The equipment used varied according to the age and abilities of the subjects.

The equipment used with the oldest three groups of children was located in an appropriately decorated room in the Department of Psychology, University College of North Wales, and is shown in Figures 3 and 4. Subjects sat in a chair in front of a large screen and a table, on which was placed a large manipulandum requiring approximately 670 grammes force response pressure. The experimenter was hidden behind the screen, together with an Apple II Plus microcomputer which registered responses with an audible click. Reinforcement, consisting of a series of coloured lights, flashing in a vertical column, accompanied by pop music and a cartoon slide projected on the screen for ten seconds, was delivered automatically by the computer. In addition, a glove puppet ('Sooty') manipulated by the experimenter appeared above the screen and dropped a potato crisp or piece of fruit down a chute into a tray on the table, from where it could be collected and consumed by the subject.

A version of the apparatus used with the infant

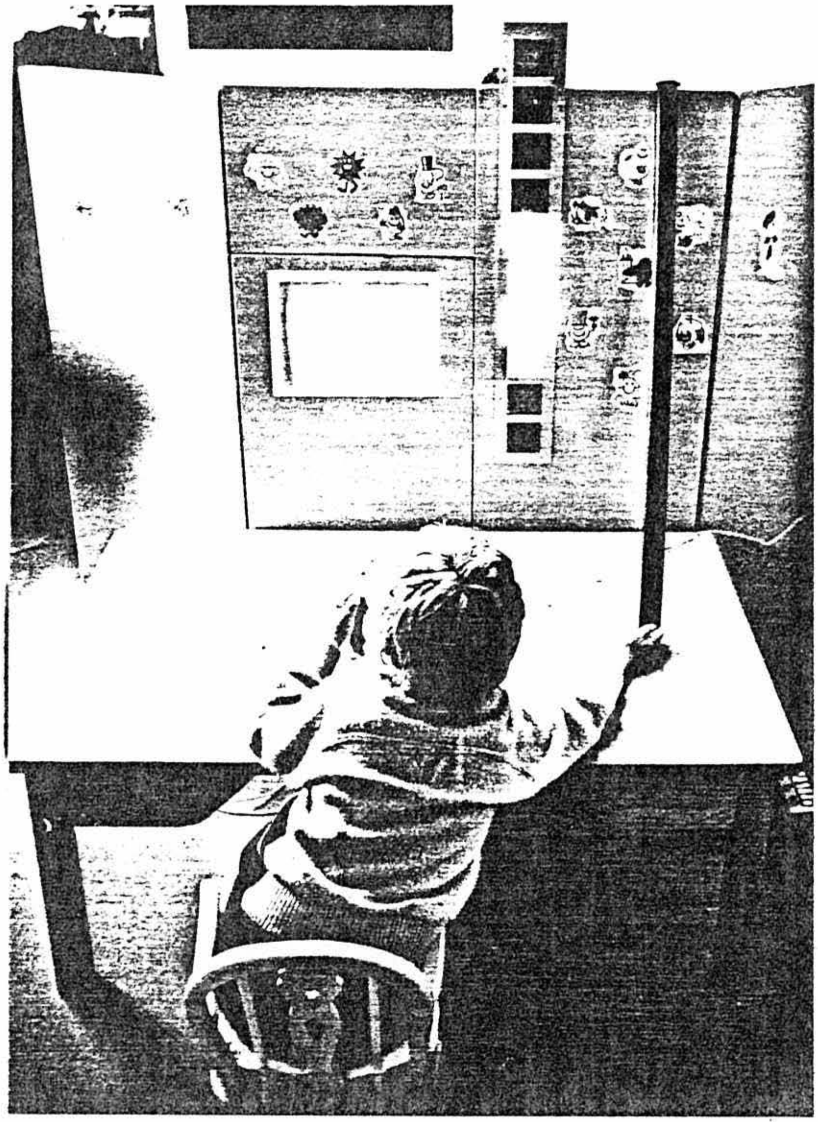

Figure 3: Apparatus used with older subjects, front view

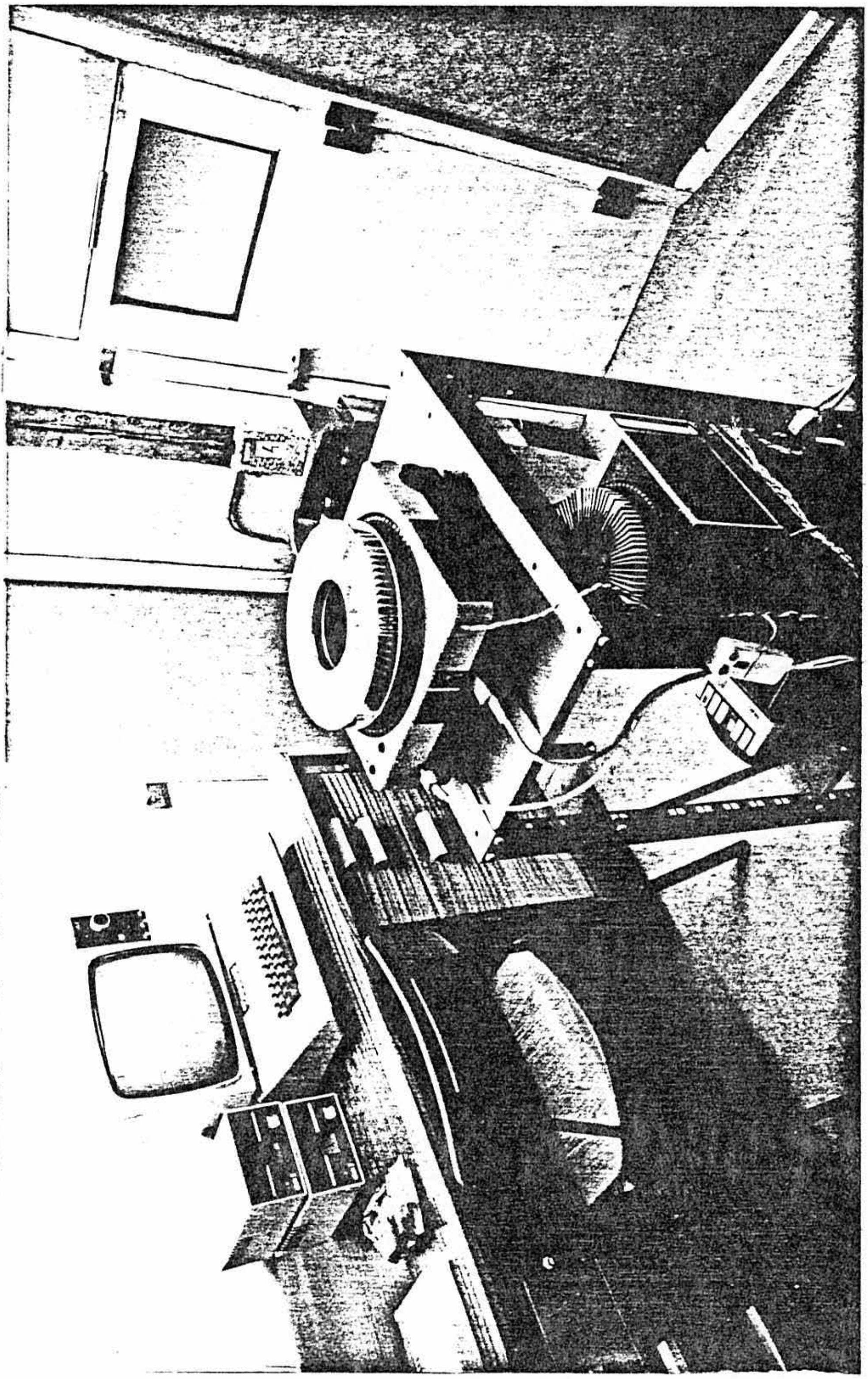

Figure 4: Apparatus used with older subjects, rear view.

subjects is shown in Figure 5. It was necessary to use portable equipment so that the subjects could be tested in their homes. Preliminary experience with these subjects indicated that it would also be necessary to vary the equipment slightly to take into account individual differences with respect to response and reinforcer preferences. In the case of three of the subjects, Ann, Jon and Pat, the response device was a metal cylinder, 40 cm long and 11 cm in diameter, mounted on a wooden stand, as shown in Figure 5. The cylinder was placed within reach of the subject who sat in a high chair (Ann and Pat) or on mother´s knee (Jon). Touching the cylinder activated an electronic touch switch. The fourth infant, Will, sat in a high chair in front of which was placed a spherical manipulandum, 10 cm in diameter, which required a response pressure of 100 grammes force.

With all infants, responses produced an audible click and were registered by an Apple II Plus microcomputer. Reinforcement was delivered manually and was signalled by a brief tone from the microcomputer. The reinforcer used was determined after a considerable amount of preliminary work with the subjects and consultation with their parents. It was found that four seconds of music played from a small

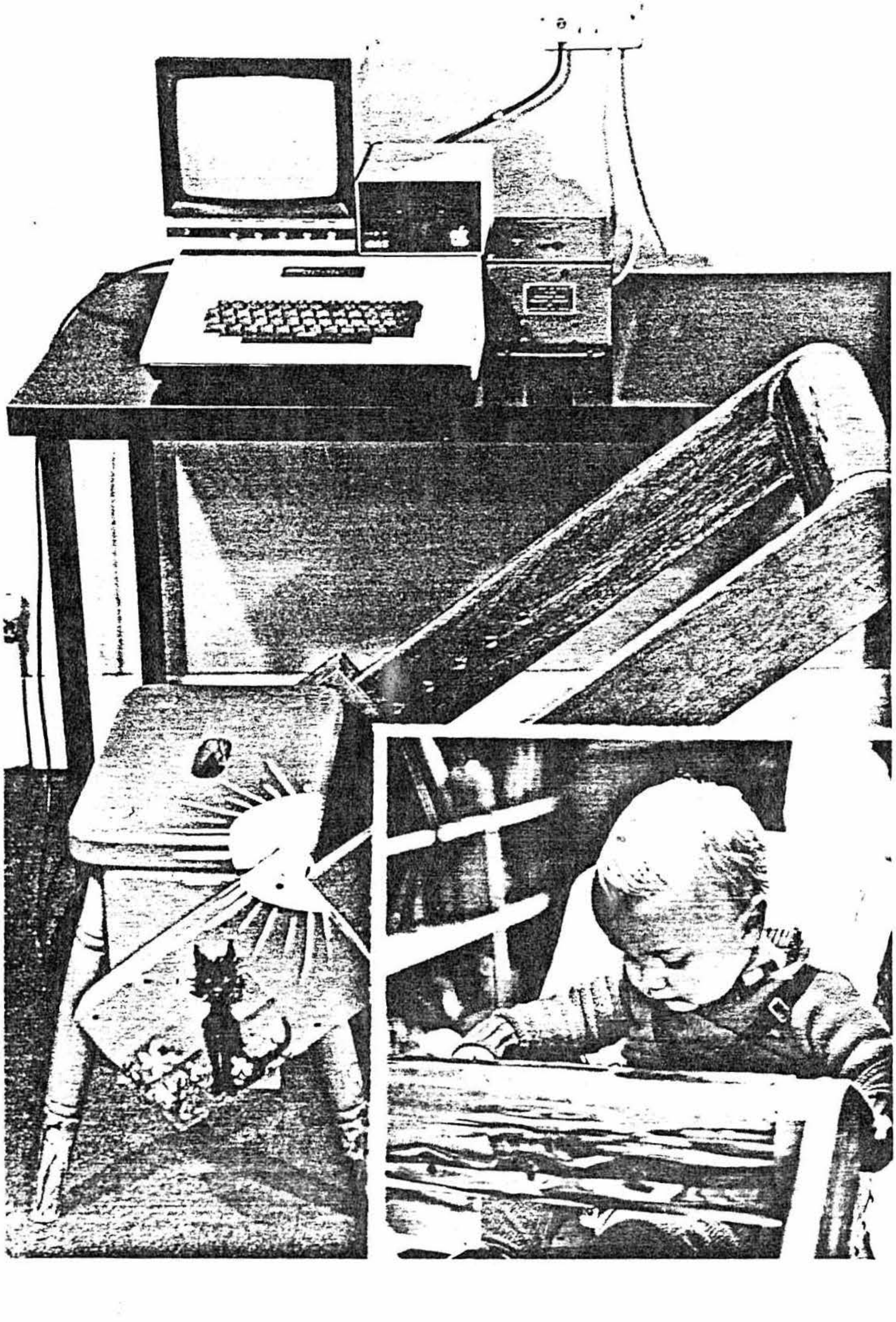

Figure 5: Apparatus used with infant subjects. Subject inset.

music box (together with associated social reinforcement) was highly reinforcing in the case of Ann. Small snack items, such as ´Monster Munchies´ were found to be effective with Jon. For Pat and Will the reinforcer consisted of ten seconds of pop music, accompanied by lights flashing on a portable flashing light unit and the presentation of a toy doll.

Procedure

The three groups of older children were introduced to the experimental situation in the company of a familiar adult, usually a parent. The experimenter sat in front of the screen initially, pulled the manipulandum and collected a reinforcer, saying, "Look what happens when I press the lever. Now you have a go."

The experimenter than went behind the screen and the subject was left to press on continuous reinforcement (CRF) for ten reinforcers, usually followed by a short period on FI 20 seconds.

The children were then taken up to FI 40 seconds. In some cases (IP, LC, HP, TS, JJ and SM) this happened over a number of sessions while in others (SR, LR and MK) the subjects were placed straight on FI 40 following CRF. The familiar adult was phased out of the experimental situation within one or two sessions of the experiment's beginning. Sessions usually lasted ten to fifteen minutes unless terminated prematurely by the child, which sometimes happened if he or she had been upset for some reason (eg. illness, emotional problems). Sessions were usually carried out three or more times a week with each subject. The subjects were exposed to schedule values in the following order: FI 40, FI 70, FI 25 and FI 40 seconds. The cumulative records were inspected to ascertain whether responding was stable* before the subjects were moved on to the next schedule value. Some of the older children produced stable responding very quickly and spent only five or so sessions at each schedule value. Some of the younger children, however, were kept at different schedule values for much longer - for more than twenty sessions in one case - before moving on. The total number of sessions carried out at each schedule value with each subject is shown in Table 2.

Most sessions were tape-recorded. After completing each schedule value, the subjects were asked, "What makes Sooty work?" Each subject was also tested on the Reynell Developmental Language Scale (Reynell, 1977), a standard

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* over three consecutive sessions

measure of language development (mean scores on which were reported for the three groups above). Individual scores on the RDLS are given in Table 2.

The schedule values experienced by the infant subjects were, in the case of Ann, a FI 20 second schedule, followed by FI 10 seconds and then FI 30 seconds. Jon experienced the same schedules in the order FI 20, FI 30 and FI 10 seconds. Pat experienced three different schedule values in the order FI 20, FI 60 and FI 40 and Will experienced the same schedule values in the order FI 60, FI 20 and FI 40 seconds. As with the older subjects, testing was continued at each schedule value until inspection of the data showed that responding was stable over three consecutive sessions. Sessions lasted for approximately eight to ten minutes, unless interrupted, and were carried out on most week days. The number of sessions carried out at each schedule value with these subjects is shown in Table 3.

RESULTS

Figure 6 shows individual cumulative records from early

Table 3: Design of Experiment I. (Session numbers shown in brackets.)

Group	Age range	Subjects	Schedule values
1	$7\frac{1}{2}$-9yrs	SR	FI40(6),FI70(6),FI25(5),FI40(5)
		IP	FI40(5),FI70(5),FI25(5),FI40(5)
		LR	FI40(10),FI70(5),FI25(5),FI40(5)
2	5-$6\frac{1}{2}$yrs	LC	FI40(13),FI70(7),FI25(6),FI40(5)
		MK	FI40(7),FI70(5),FI25(5),FI40(7)
		HP	FI40(7),FI70(5),FI25(5),FI40(5)
3	$2\frac{1}{2}$-4yrs	JJ	FI40(16),FI70(10),FI25(6),FI40(6)
		SM	FI40(26),FI70(7),FI25(7),FI40(7)
		TS	FI40(11),FI70(10),FI25(6),FI40(6)
4	Infants	JON	FI20(18),FI30(11),FI10(11)
		ANN	FI20(10),FI10(10),FI30(6)
		PAT	FI20(14),FI60(9),FI40(9)
		WILL	FI60(14),Fi20(12),FI40(12)

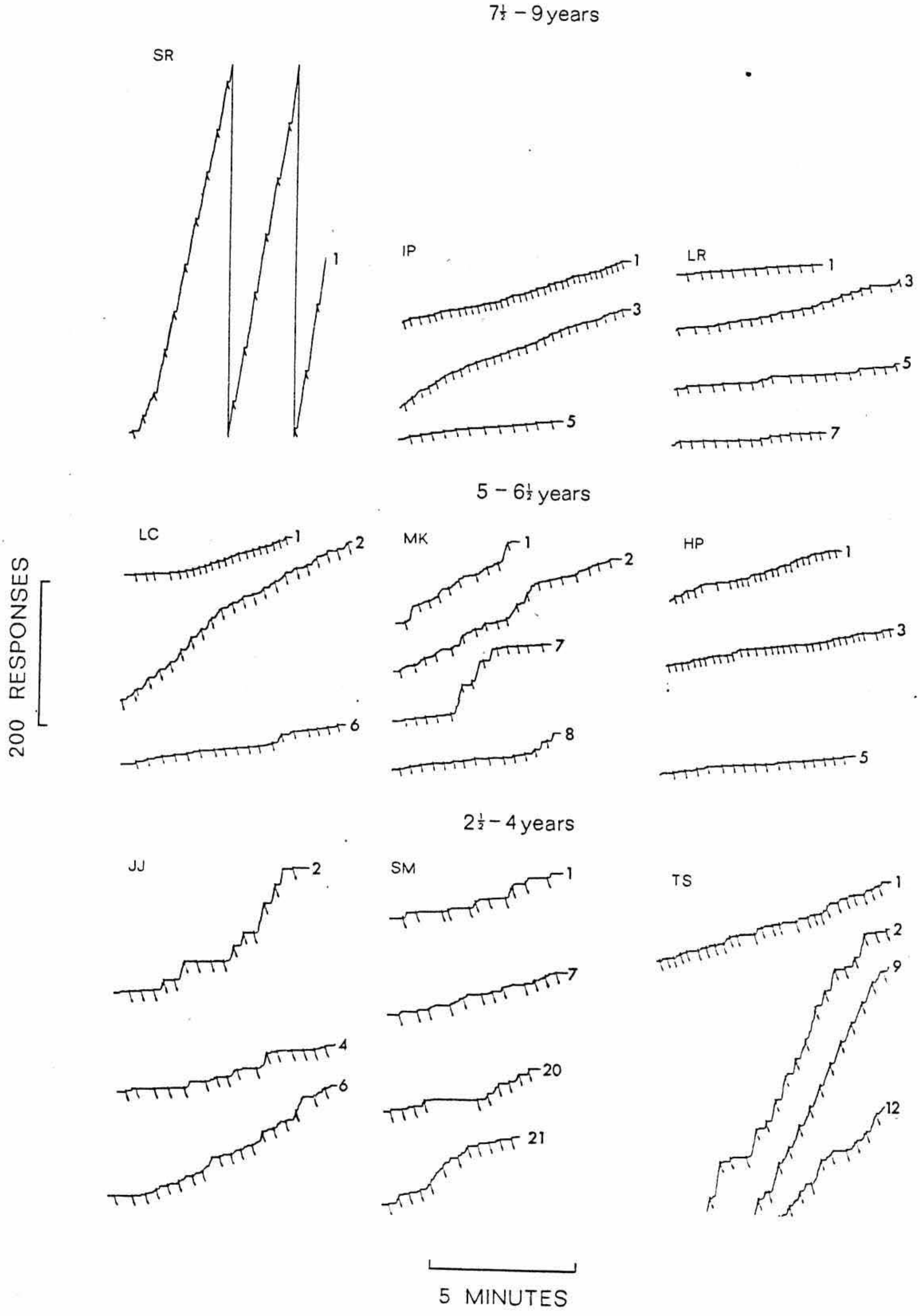

Figure 6: Cumulative records from initial sessions of subjects in Groups One, Two and Three, Experiment I.

in the development of the FI responding of three of the subjects from the oldest three groups. It can be seen that two of the subjects in the oldest group (shown at the top of the figure) quickly developed a constant low-rate pattern of responding. One, IP, performed at a rapidly decreasing rate on FI 20 seconds for the first two sessions following CRF. The other, LR, was placed immediately on FI 40 seconds following CRF and immediately produced the low-rate pattern. In contrast, SR, proceeding from CRF (shown) to FI 20 seconds immediately began to respond at a constant high-rate, a pattern that persisted thereafter.

The development of FI responding for the three subjects in Group Two (five to six and a half years) is similar, though more prolonged, to that of the two low-rate responders in the oldest group. LC, beginning on FI 20 seconds after CRF, produced a transient, almost scalloped performance when the schedule value was increased to FI 40 seconds (session 2) but, by the next session, she had begun to produce a low-rate pattern of responding which persisted thereafter. HP, on progressing from FI 20 seconds to FI 40 seconds immediately produced this low-rate pattern. MK, however, starting on FI 40 seconds immediately following CRF, produced a broken, irregular pattern of responding for several sessions, until he began to respond at a relatively constant low-rate in session 8.

All the subjects in Group Three - the two and a half to four year olds - produced a transient scallop-like pattern similar to that of LC in Group 2 at some time during the development of their responding. In the case of TS in particular, and to a lesser extent in the case of JJ, this pattern persisted over a number of sessions. Such transient scalloping has been reported for young children by Long, Hammack and May (1958). However, this relatively stable pattern soon disappeared and all subsequent responding of all three subjects was irregular and broken, a result also reported by Long et al.

The development of fixed-interval responding in the infants Ann and Jon is shown in Figure 7. In the case of Ann, responding in the early sessions was characterised by bursts of responding immediately following reinforcement which were then followed by long periods of no responding; the next response was then reinforced. Long pauses, exceeding the FI values were also present in Jon´s earlier sessions. The response rates of both subjects increased in later sessions and the pause-respond pattern developed following reinforcement. A similar pattern of responding is

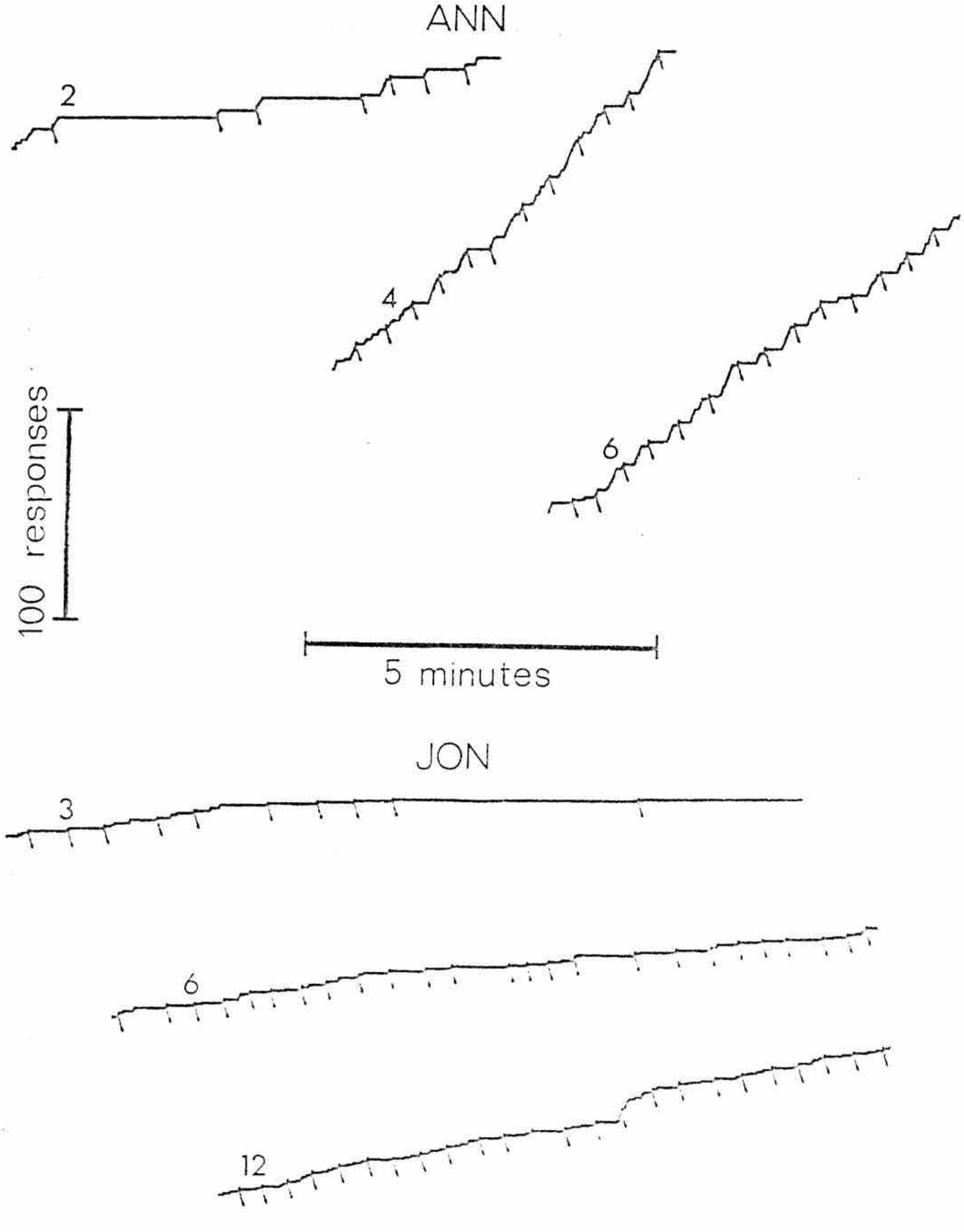

Figure 7: Cumulative records from initial sessions of infant subjects Ann and Jon, Experiment I.

shown for the two other infants, Pat and Will, in Figure 8. For Pat in particular, responding early in the interval during the early sessions can be seen. Although Will started to produce a scalloped pattern of responding at an early stage, he too produced occasional bursts of responding following reinforcement in the early sessions. Both the negatively accelerated responding in the course of the fixed interval observed in the early sessions and the gradual emergence of the final FI pattern over several sessions are characteristic of animal performance (Ferster and Skinner, 1957) but have not been reported in previous human studies. In the latter the final form of responding is often established very quickly in the first one or two sessions - results comparable to those obtained from the oldest children in this study.

The final form of the fixed interval performance of Group One, the oldest group, can be seen in Figure 9, which shows the cumulative records obtained from these subjects on the final session at each schedule value. The two familiar patterns of human fixed interval responding can be seen, reflecting the performance of the subjects in the early sessions. One subject, SR, responded at a constant high-rate, pausing only briefly after reinforcement in order

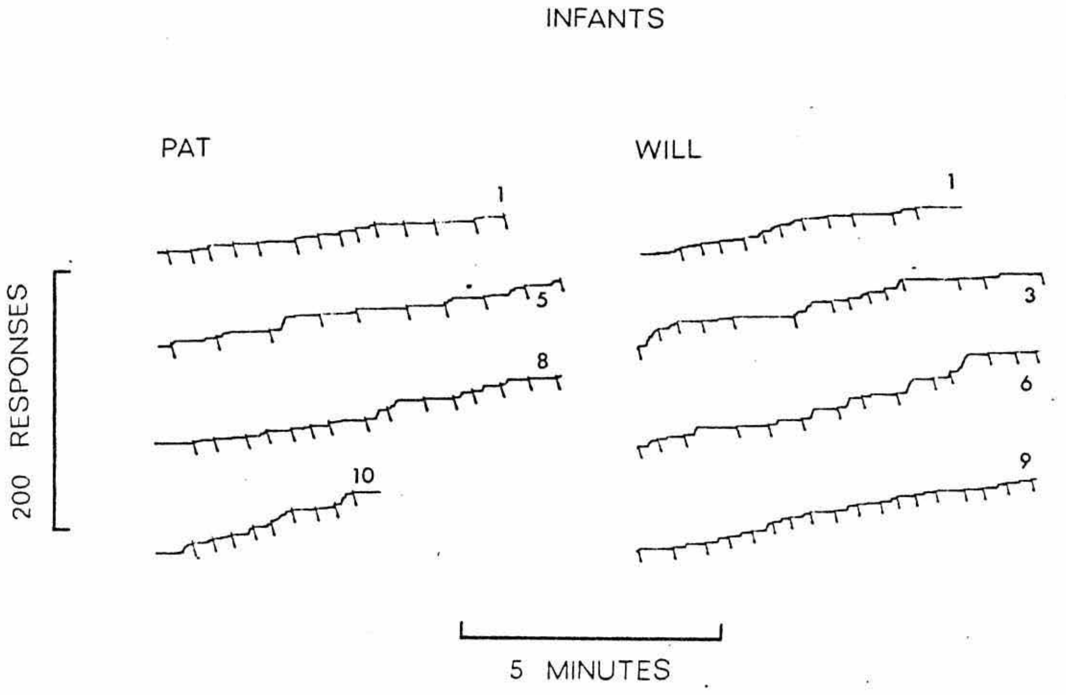

Figure 8: Cumulative records from initial sessions of infant subjects Pat and Will, Experiment I.

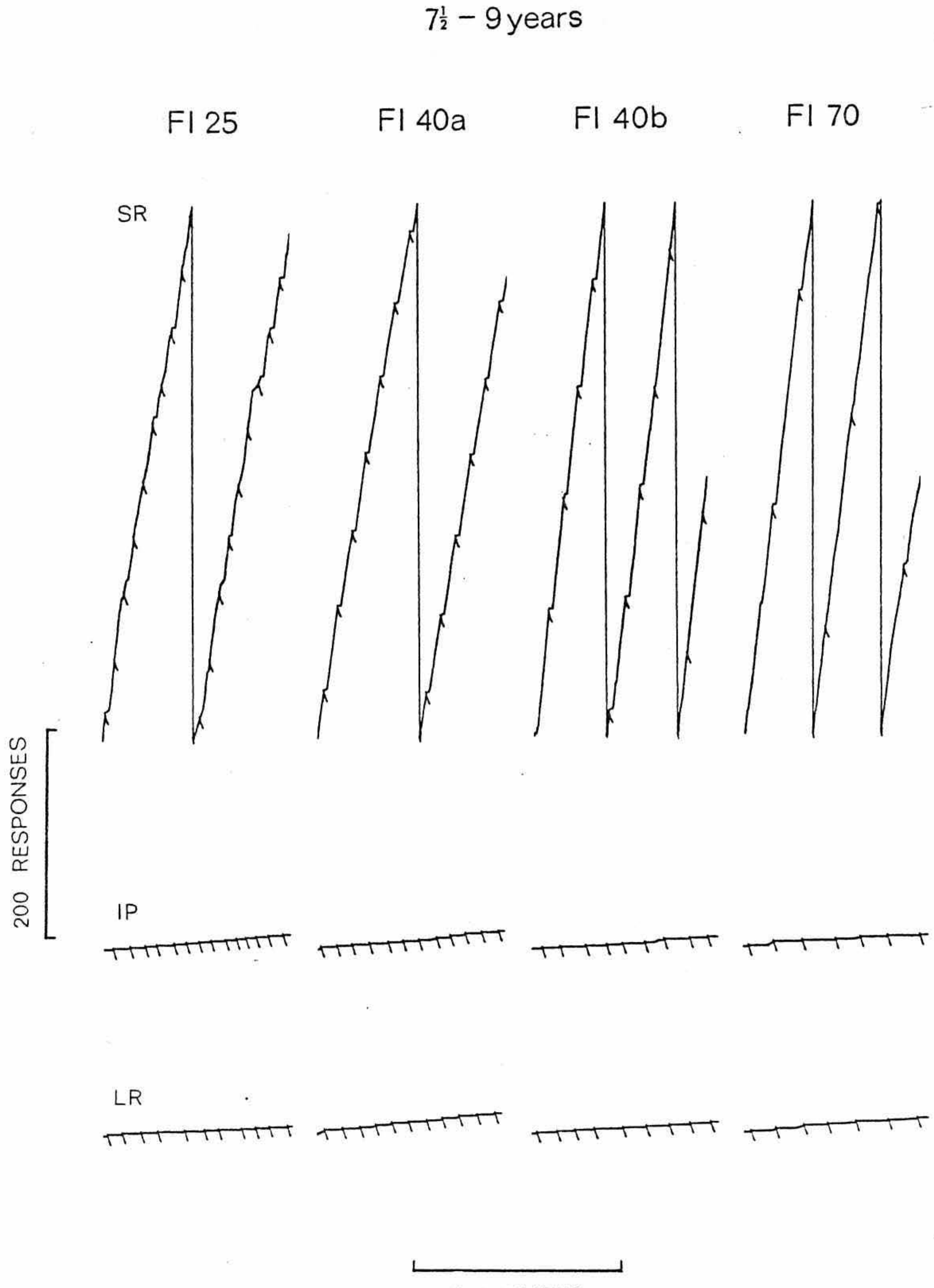

Figure 9: Final cumulative records of subjects in Group One, Experiment I.

to reach for and consume the food reinforcer. This high-rate pattern is identical to that found in adults who verbalise a response-based formulation of the contingency. The other two subjects, LR and IP, performed at a low rate, responding for the most part only once in each interval, just after reinforcement became available. This pattern of behaviour is the same as that previously found with human adults who verbalise an interval formulation of the contingency. These two patterns are reflected in the subjects´ verbal reports when questioned after finishing at each schedule value. Both IP and LR reported interval formulations, LR simply saying ´I wait for a bit and then press the lever´, IP suggesting that, ´After I´ve pressed the lever Sooty goes to sleep and then I wait for a little while before waking him´. SR, on the other hand, when pressed to describe how she responded, said, ´I press very hard.... as hard as possible,´ and would not be drawn further until, after the final session of the experiment, she said, ´I press as fast as possible.

Little verbal behaviour was recorded from these subjects during the sessions. SR was particularly silent, speaking only to ask questions of the Experimenter during the early sessions. LP occassionally hummed, hissed or blew very quietly and sometimes tapped his fingers on the table.

IP also said very little, but occassionally hummed and made ´animal noises´. Transient counting was also recorded from IP during one of his early sessions.

Figure 10 shows the final cumulative records of the subjects in Group Two, the five to six and a half year olds. All three of the subjects in this group were low-rate responders. It can be seen that the performance of these subjects was slightly less regular than that of the low-rate subjects in Group One. On occasion, all three of the subjects responded before the end of the interval. In a few instances the subjects even responded at an increasingly high-rate towards an interval´s end. This pattern, however, only persisted in the case of MK´s FI 70 performance and, with this one exception, the performance of all three of the subjects was similar to that produced by adult subjects and those children in Group One who verbalised an interval-based formulation of the contingencies.

All three of the subjects in this group also reported interval formulations of the contingencies. In the case of MK, this was quickly forthcoming:

E: ´How does Sooty work?´

MK:´I press the lever.´

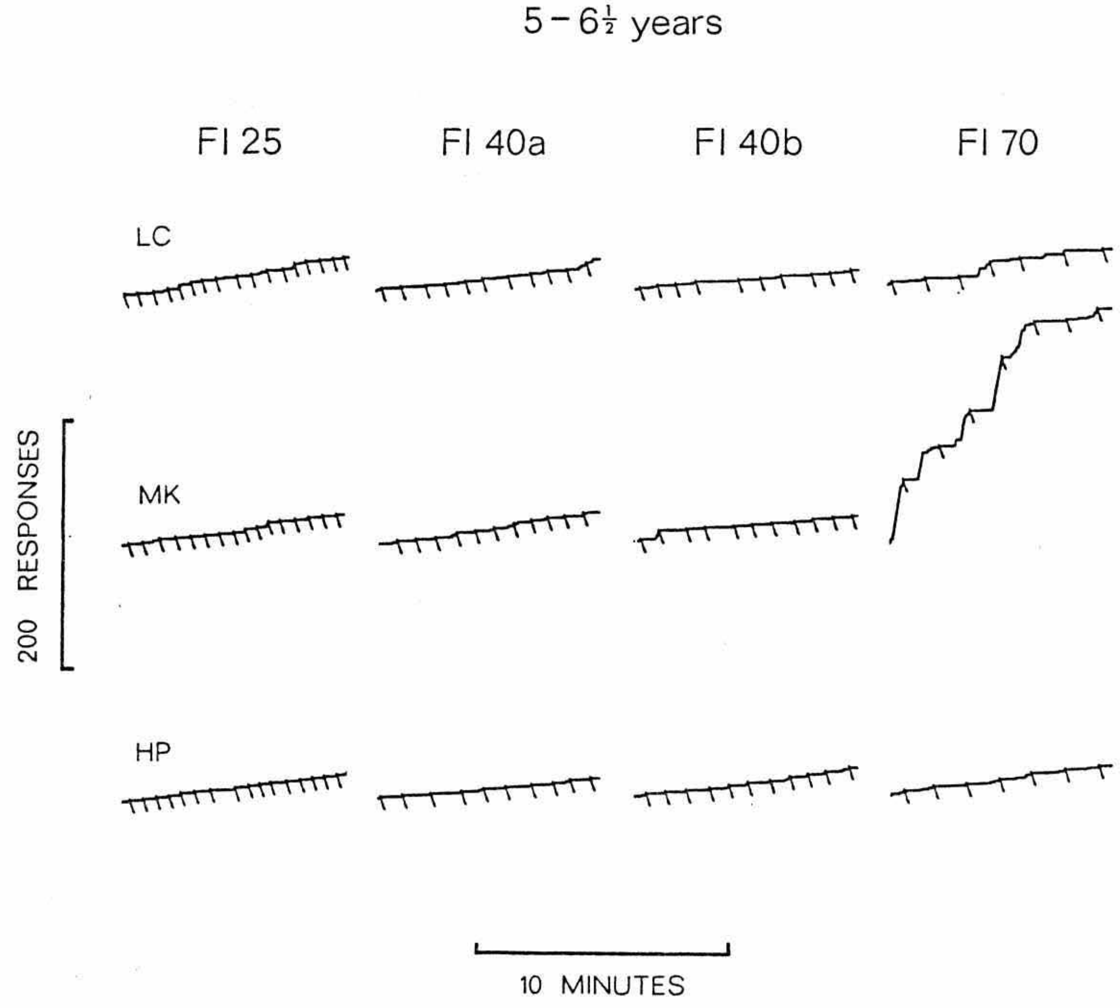

Figure 10: Final cumulative records of subjects in Group Two Experiment I.

E: ´Anything else?´

MK:´He doesn´t work everytime.´

E: ´So what do you do.´

MK:´Sometimes I wait.´

However, in the case of HP in particular, the verbal formulation was not easy to extract. Thus:

E: ´How does Sooty work?´

HP:´Ahh....´

E: ´Do you know?´

HP:´I´ve forgotten.´ (Looks around.)

E: ´Tell me what you think you have to do. It´s alright, tell me anything you think you have to do. Make a guess. If you´re not too sure it doesn´t matter.´

HP:´You press that typewriter thing.´ (Referring to the computer behind the screen, which HP had once observed E operating at the end of a session.)

E: ´No, what I mean is: What do YOU have to do to make the picture come on?´

HP:´Press the lever. Pull the lever.´

E: ´That´s right. Anything else? Do you have to press it in any special way or anything?´

HP:(Long pause.) ´I don´t know.´

E: ´Is there any special way you have to pull it?´

HP:´I just watch the picture.´

E: ´What do you do before you press the lever?´

HP:´Just wait.´

E: ´Just wait?´

HP:´Until I think it might come on.´

Obviously what happened in this case was that HP mistook the question to be one about how the experimental equipment functioned - a consequence, perhaps, of the indirect way in which questioning had to be carried out in order to avoid prompting.

It was also difficult to obtain a description of the contingencies from LC, who continued to answer, ´I don´t know,´ until pressed considerably. Eventually, when asked, ´Does Sooty come on every time you press the lever,´ she replied, ´No, you have to wait a while.´

During the sessions, MK made repeated attempts to converse with the experimenter (usually ignored), sang, whispered to himself and, on several occasions, bent close to the tape recorder and talked into it in an unintelligible growl. LC was generally very quiet but, during a number of sessions, whispered at length, apparently to the manipulandum. The speech took the form of ´magical mands´ (Skinner, 1957), for example, ´Work please. Work please,´ and on one occassion, ´Work when I say work. Why won´t you work? If you don´t work when I say work then I won´t take this way - try to get it over to Richard (E) to get it off.´ Transient counting was also recorded from LC in an early session, as it was from HP, who experimented to discover the amount of counting necessary between lever presses. Thus, after the first reinforcement of her third session, she waited and then counted to ten, then pulled the manipulandum and received a reinforcer. During the following interval she counted to nine very quickly, paused, said, ´Ten´ and pulled the manipulandum too early to receive a further reinforcer. She then said, ´Eleven, no ten, no fourteen!´, before counting from one to fourteen very quickly, after which she responded on the manipulandum and collected a reinforcer. During the next interval she counted to fourteen, pressed, failed to collect a reinforcer and then counted to fifteen before responding again, this time at the end of the interval. Later in the session, the counting faded. HP was also recorded singing (sometimes anticipating the jingle element of the reinforcer), talking to Sooty, and reciting rhymes.

The final cumulative records for Group Three are shown in Figure 11. Following the break-up of the scallop-like pattern (detailed above) the behaviour of these subjects was much less consistent than that of the older two groups. This type of responding bears little resemblance to that of either human adults or animals, although it does seem to exhibit elements of both in places. In all three subjects the pattern of responding in some intervals appears to be scalloped, in others almost low-rate, and in still others high-rate. In some intervals patterns can be seen which are not found in either animals or humans, for example a high-rate burst of responding after reinforcement followed by a long pause.

The verbal reports of these subjects did not indicate any knowledge of the contingencies. However, questioning of the subjects proved to be extremely difficult and caution should be addressed to young children´s answers to complex questions of causality (Donaldson, 1976), as the following conversation between the experimenter and TS illustrates:

E: ´Okay T, what makes Sooty work?´

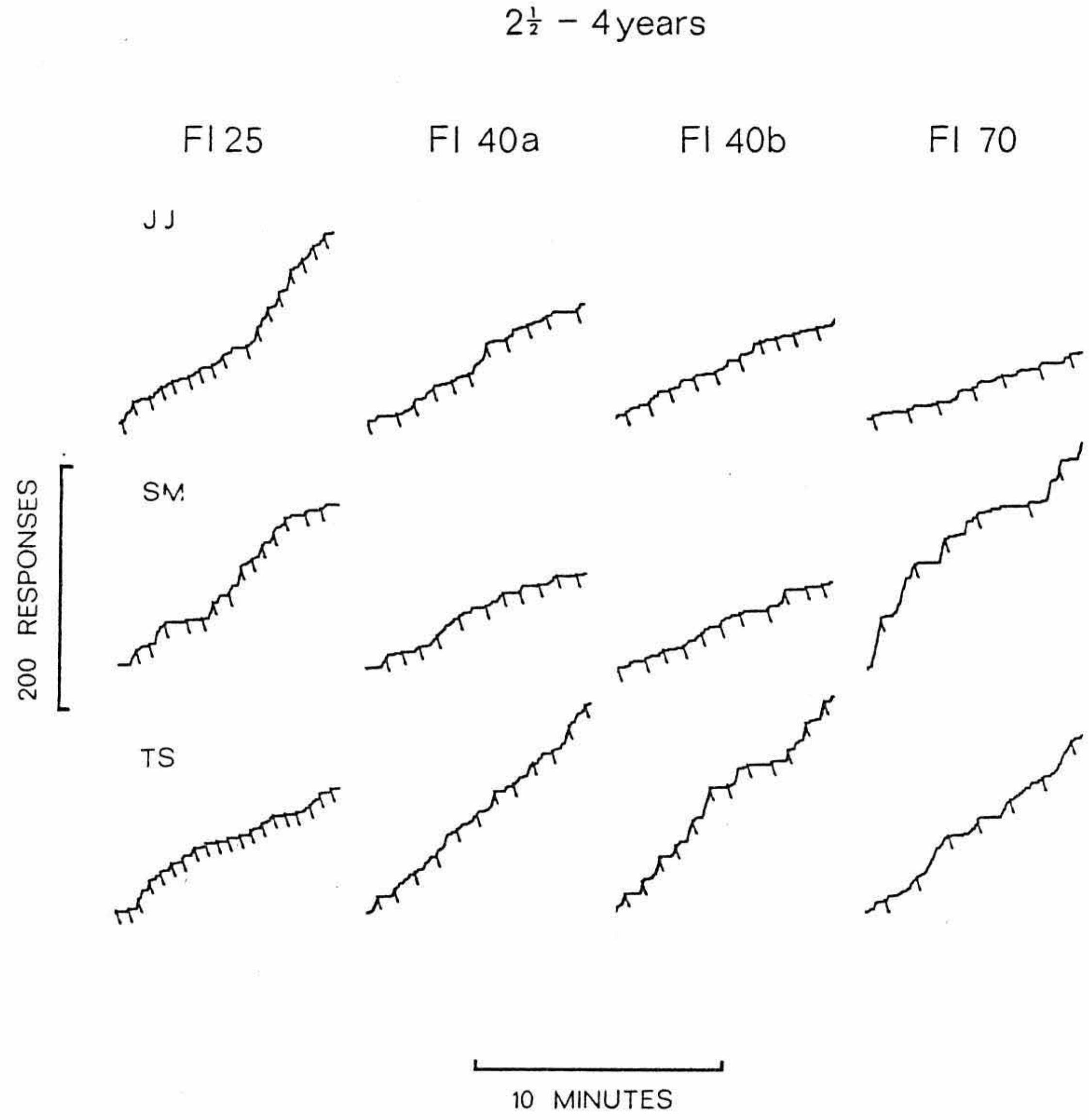

Figure 11: Final cumulative records of subjects in Group Three, Experiment I.

TS:´Apple....´

E: ´No, what do you have to do to make Sooty work?´

TS:´Sweets come down here....´ (Pointing to chute.)

E: ´I know sweets come down here but what do you have to do to make Sooty work?´

TS:´Pull this.´ (Pointing to lever.)

E: ´Anything else.... You just have to pull that? Can you tell me anything more about how you have to pull that? How do you have to pull that?´

TS:´With your hand.´

E: ´With your hand?´

TS:´Yes.´

E: ´Is that all?´

TS:(Indecipherable squeal.)

E: ´Do you know anything more about what makes Sooty work?´

TS:´No.´

E: ´Okay. Thanks.´

Similarly with JJ:

E: ´What do you have to do to make Sooty work?´

JJ:(Indecipherable squeal.)

E: ´What do you have to do to make Sooty work?´

JJ:´Press the lever.´

E: ´Anything else.... Do you have to do anything else.... To make Sooty work you have to press the lever. Do you have to do anything else?´

JJ:´No.´

And with SM:

E: ´Right Sarah, tell me something, what do you have to do to make Sooty work?´

SM:´I don´t know.... This!´ (Points to lever.)

E: ´What´s that? What is it?´

SM:´Make pictures.... Make pictures.´

E: ´Yes. What makes it work? How do you make the pictures work?´

SM:´Pull it!´

E: ´Anything else? Do you have to pull it in any special way, Sarah?´

SM:(Stunned silence.)

The recorded verbal activity of the two and a half to four year olds was much less orgainised than that of the older subjects. Most of TS´s verbal activity was very quiet and, therefore, impossible to discern. TS also grunted, gasped, and made ´motor car noises´. JJ similarly either laughed, hummed, ´clicked´ her tongue in her open mouth, or gasped. Extensive whispering was also recorded. Most was impossible to decipher, but some consisted of remarks addressed to Sooty and, during the interval, to some of the cartoon characters that decorated the walls of the experimental room (eg. ´Mr. Bump!´ ´Mickey Mouse!´). Verbalisations related to schedule performance were detected only for SM out of this group. Along with remarks addressed to Sooty (´Hello, Sooty!´) and singing, she repeatedly said, ´Can´t do it.... Can´t do it.... Can´t.... I can´t.... Can´t....´ after unsuccessful responses. SM also repeatedly wandered around the room during her sessions or played with her seat, rotating the cushion over and over again. Occasionally during her early sessions she wandered behind the screen and addressed the experimenter, only to be asked in a friendly but firm manner to return to her seat.

The cumulative records of the infants in Group Four are shown in Figure 12. Although the records produced by Pat were more broken than those produced by Ann, Jon and Will, in all cases the predominant response pattern consisted of a

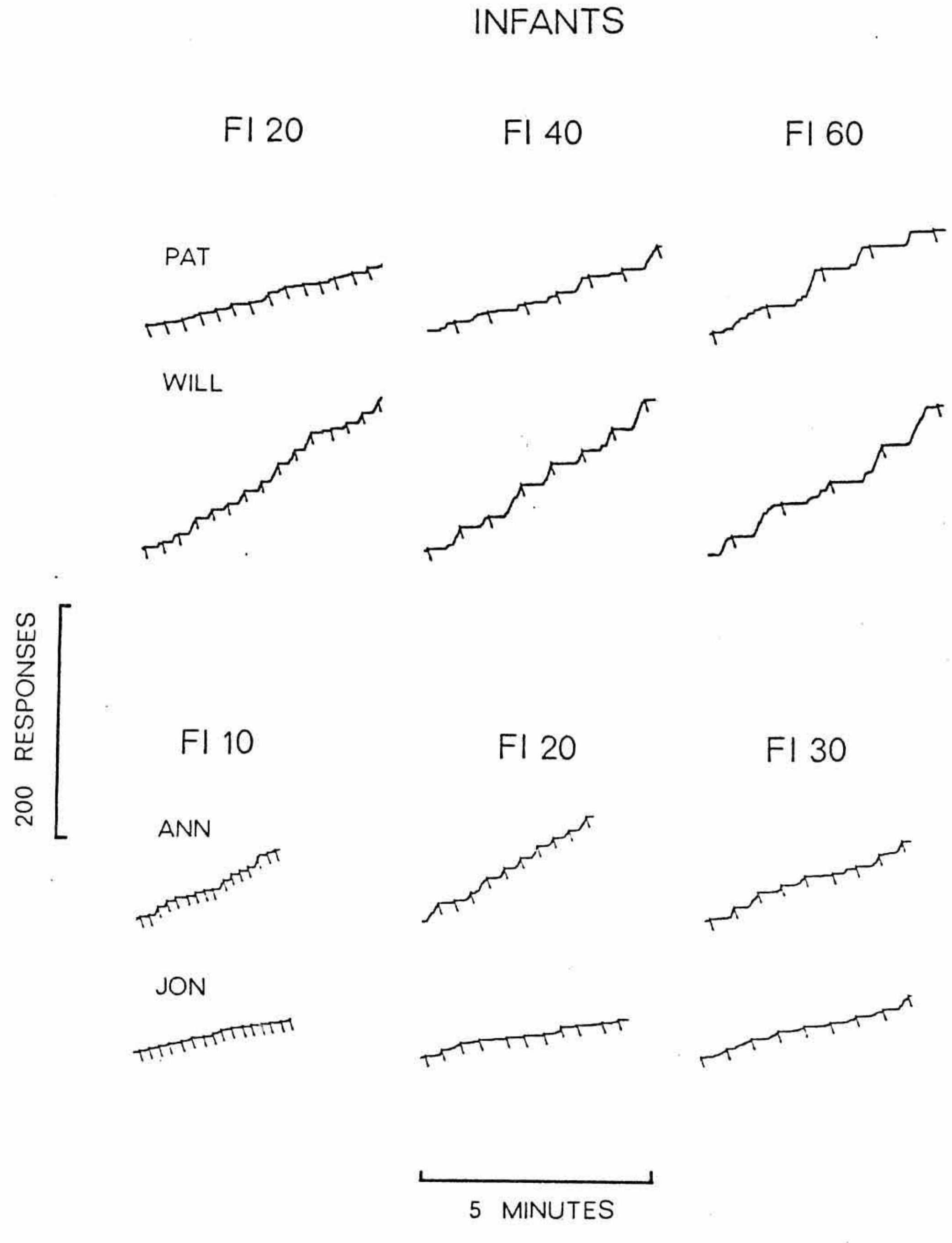

Figure 12: Final cumulative records of infant subjects Experiment I.

pause after reinforcement, followed by a gradual acceleration of responding terminated when the next reinforcement was delivered. This pattern is markedly different to those produced by the subjects in the three older groups. It is the familiar scalloped pattern found in animals but remarkably elusive in the literature on the performance of human adults and older children (Long et al., 1958; Weiner, 1969; Zeiler and Kelley, 1969; De Casper and Zeiler, 1972; Matthews et al., 1977; Leander et al., 1968; Lowe, 1979).

Sensitivity to the schedule parameter can be assessed by looking at the duration of the post-reinforcement pauses and the running rates, shown for all the subjects in the four groups in Figure 13. (The mean post-reinforcement pauses, together with standard deviations, are also given in Table 4.) In the case of animals, the post-reinforcement pause increases as a positively accelerated function of the schedule value, but the running rate, ie: the response rate calculated by excluding the post-reinforcement pause, declines.

For the low-rate responders in the two oldest groups the PRP increased with the schedule value. This was because the length of the pause was often close to that of the

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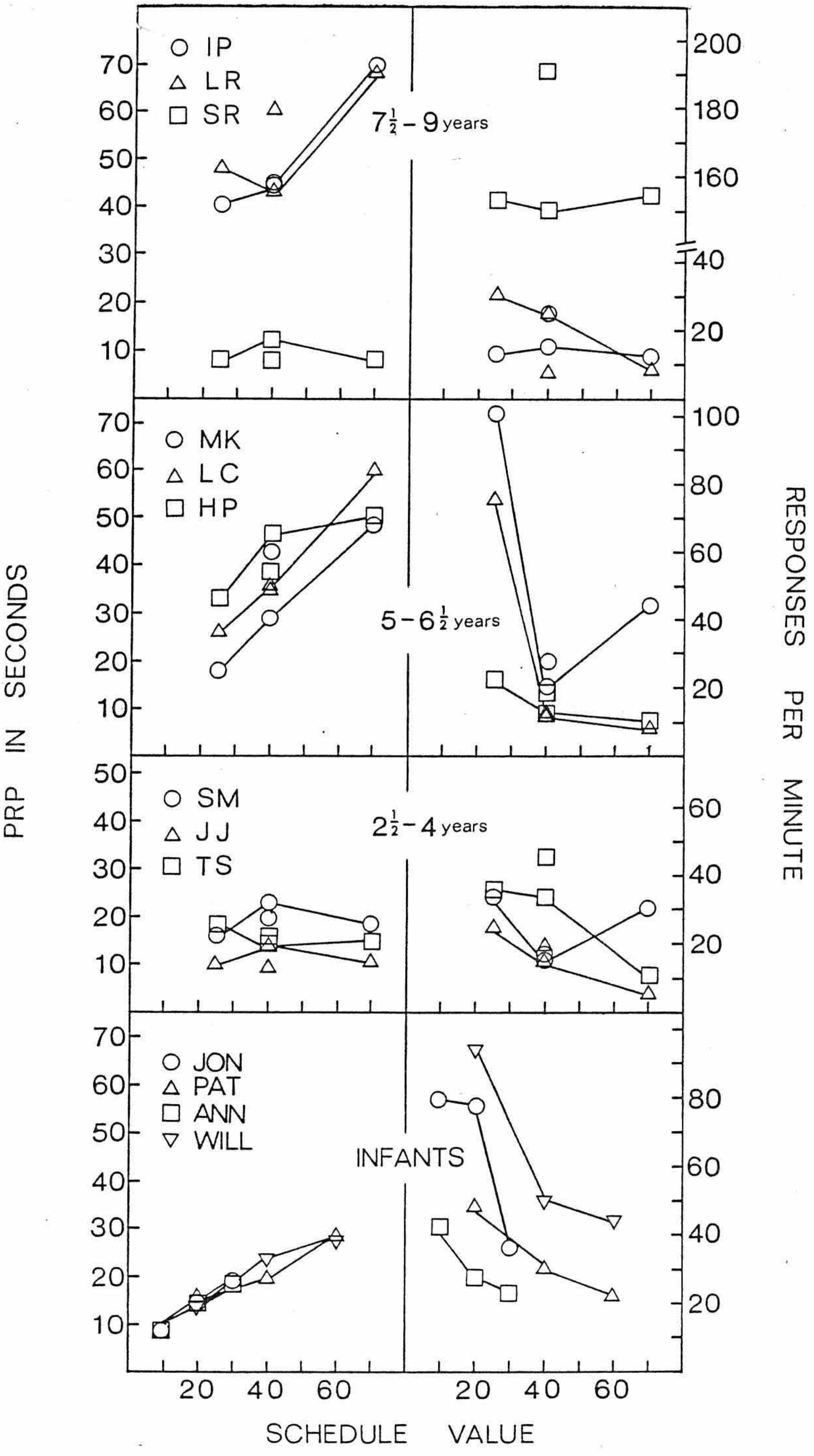
* All parametric data for this and all subsequent experiments is taken from the final three stable sessions on each schedule.

Figure 13: Mean post-reinforcement pauses and running rates of subjects in Experiment I. (Over)



IP
LR
SR
7½ - 9 years
MK
LC
HP
5 - 6½ years
SM
JJ
TS
2½ - 4 years
JON
PAT
ANN
WILL
INFANTS
PRP IN SECONDS
RESPONSES PER MINUTE
SCHEDULE VALUE
10
20
30
40
50
60
70
200
180
160
100
80

Table 4: Mean postreinforcement pauses and standard deviations for the subjects in Experiment I (Seconds).

Group	Subject	Sched. val.	Mean PRP	SD
1	SR	40	12.47	13.39
		70	9.95	3.19
		25	9.06	2.15
		40	7.51	1.54
	IP	40	45.61	9.81
		70	71.33	14.23
		25	40.28	12.44
		40	47.06	9.94
	LR	40	42.62	10.36
		70	66.73	12.38
		25	47.15	12.68
		40	59.39	7.46
2	LC	40	35.82	16.11
		70	60.86	35.93
		25	26.21	8.93
		40	35.08	15.72
	MK	40	29.79	12.45
		70	48.44	20.17
		25	18.37	8.11
		40	40.25	17.16
	HP	40	47.12	14.78
		70	42.89	21.83
		25	33.08	10.42
		40	38.71	9.54
3	JJ	40	10.02	7.24
		70	13.19	10.45
		25	9.48	3.63
		40	10.88	7.93
	SM	40	15.17	12.55
		70	23.00	18.59
		25	19.88	19.71
		40	18.85	14.34
	TS	40	18.40	13.03
		70	12.32	7.93
		25	15.70	13.57
		40	15.41	13.89

Table 4 (cont.).

Group	Subject	Sched. val.	Mean PRP	SD
4	JON	20	14.31	6.0
		30	17.57	7.0
		10	9.31	3.6
	ANN	20	13.91	4.8
		10	8.81	1.5
		30	19.29	5.4
	PAT	20	15.05	7.0
		60	28.20	14.8
		40	19.85	15.8
	WILL	60	26.80	16.2
		20	13.83	12.4
		40	23.47	13.5

interval. For SR, the high-rate responder in Group One, however, the PRP was little affected by the interval length, being the time taken to collect and consume the food reinforcer. In contrast, no consistent relationship between schedule value and the PRP can be observed for the subjects in Group Three. The performance of the infants, however, appears to have been sensitive to the schedule parameter in exactly the same way as that of animals, showing a negatively accelerating increase in PRP with respect to interval value.

With respect to running rates, data similar to those produced by animals were also produced by the infant group, the running rates of these subjects declining as a function of the interval value. It is difficult to detect any such consistent relationship among the results of the older children.

A detailed picture of both response patterning and schedule sensitivity can be found by examining the first nine successive mean inter-response times at each schedule value, shown for all the subjects in all the groups in Figure 14.

The uppermost panels of the figure show the mean IRTs of the subjects in Group One. As previously noted, the two

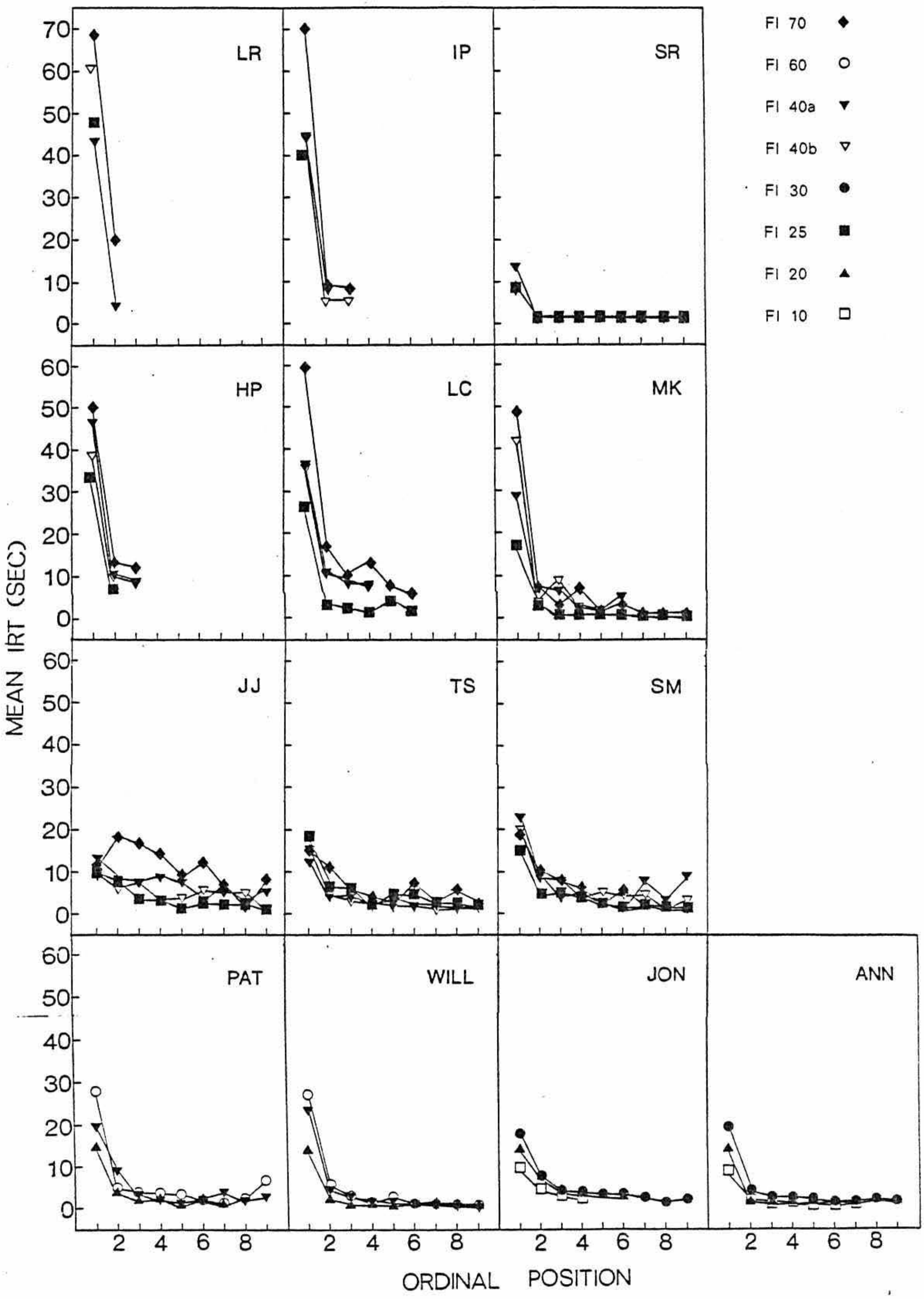


Figure 14: Successive mean IRTs plotted against their ordinal position in the interval for subjects in Experiment I.

low-rate responders, LR and IP, required few responses to obtain reinforcement, usually only one. For this reason there are few points on their respective functions and the first IRT value was, on average, often greater than the corresponding schedule value. In the case of the high-rate responder, SR, however, the first IRT was only slightly longer than the rest and all the subsequent IRTs were of the order of 0.4 seconds, whatever the schedule value.

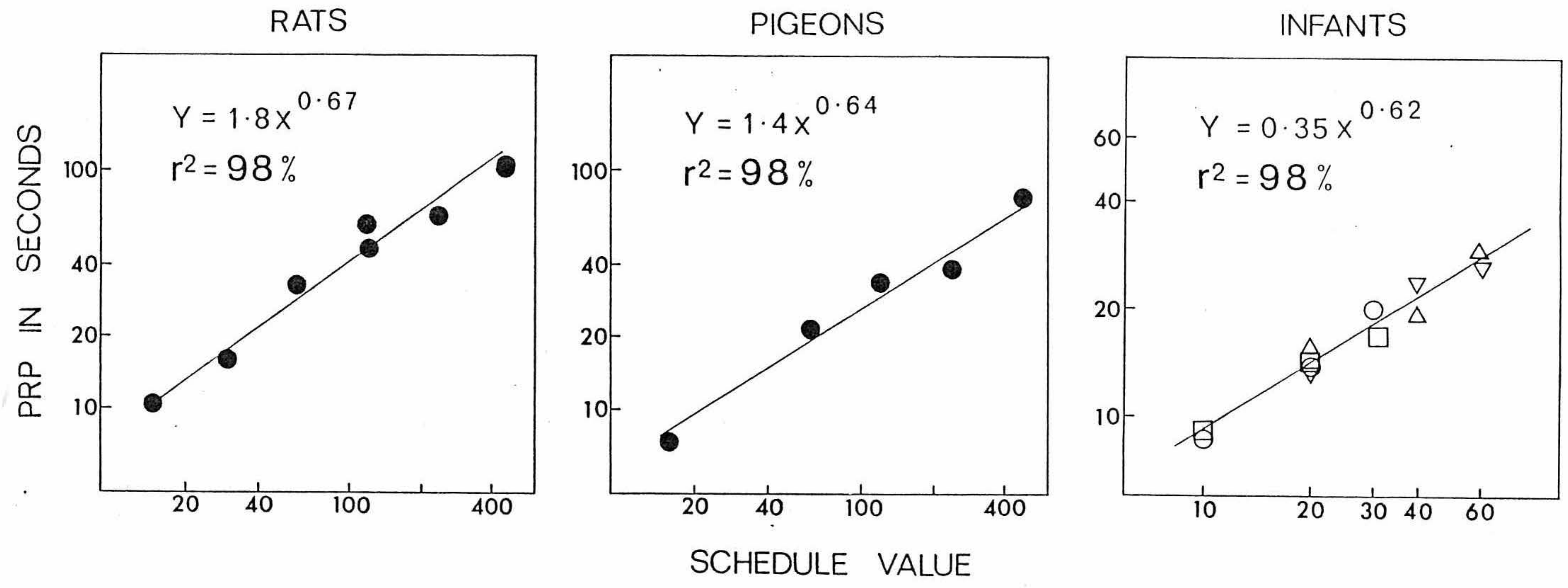
The IRT curves of Group Two, shown in the second row of panels of Figure 14, are all similar in many respects to those of Group One. The first IRT, however, was less consistently close to the interval value, with the result that more responses were made, on average, for each reinforcer. In the behaviour of MK and LC in particular we can see a degree of variability not found in Group One.

This irregularity is even more manifest in the mean IRT data for the two and a half to four year old group, particularly with respect to the ordering of the IRTs by schedule value. In one case, JJ, the mean first IRT was actually less than the mean second IRT. The IRT curves for the children in this group bear little resemblance to those obtained from either animals or human adults.

In marked contrast, the IRT data for the infant group, shown in the bottom-most panels, exhibit great regularity. With a very few exceptions, successive interresponse times decreased in the course of the fixed interval, showing that the gradual acceleration of responding observed in the cumulative records of these subjects was a consistent phenomenon present in all conditions. Considerable sensitivity to the schedule parameter is shown by the positive relationship between interresponse time at each ordinal position and the schedule value. In nearly all cases, the greater the schedule value the greater the mean IRT. Functions of this kind have been consistently observed with animal species but have not been reported with human subjects before now.

These results - the cumulative records, post-reinforcement pause data, rate data and finally, the analysis of each subject's IRT data are consistent in that they point to three distinct modes of responding. At the bottom of the developmental range, the behaviour of the infant subjects is indistinguishable from that of animals. The extent to which this is the case is indicated by Figure 15, which shows data from the infant subjects in this study and from rats and pigeons in a study by Lowe, Harzem and Spencer (1978).

Figure 15: Mean PRPs of the rats, pigeons and the infant subjects in Experiment I, shown as a function of schedule value and plotted on logarithmic co-ordinates. (Over.)


RATS
Y = 1·8 X^0·67
r² = 98 %
PIGEONS
Y = 1·4 X^0·64
r² = 98 %
INFANTS
Y = 0·35 X^0·62
r² = 98 %
PRP IN SECONDS
100
40
20
10
100
40
20
10
60
40
20
10
20
40
100
400
20
40
100
400
10
20
30
40
60
SCHEDULE VALUE

Animal FI responding can be described by the power function $Y=kX$, where Y is the duration of the post-reinforcement pause, X is the FI value and k and n are determined empirically, n being the slope when the PRP is plotted against the schedule value on a logarithmic scale and k being the intercept. For animal species n is less than unity. As can be seen in Figure 15, a significant linear function with a slope of less than 1, similar to that found for rats and pigeons, was found for the four infants. Note the goodness of fit.

In marked contrast to the infant subjects, subjects in the two and a half to four year group showed no consistent pattern of responding, although elements that might be described as animal-like can be discerned in some parts of the cumulative records, whereas elements that might be described as adult-like can perhaps be seen in others. These subjects seemed to exist in a ´transitional state´ between the animal and adult mode of responding. The extent to which their data differs from that of the infants can be seen in Table 5, which gives values of n, the power function described above, and the coefficients of linear regression, r, for log PRP against log interval value, for each subject in the experiment. Values of n and r for the transitional subjects indicate the absence of any relationship between PRP and schedule value.

The subjects in the two oldest groups, on the other hand, seem to be responding in a completely adult manner, particularly those in the seven and a half to nine years group. Thus, although the values of n and r indicated in Table 5 for the five low-rate subjects in these two groups seem to resemble those of the infant subjects, this is a result of the subjects matching their post-reinforcement pauses to the schedule value, and in most cases the value of n is therefore greater than normally found in animal species.

DISCUSSION

The present findings map out the development of human fixed-interval behaviour from infancy to late childhood. The infant subjects showed the same pattern of responding and sensitivity to the schedule parameter typically found in animals. This, together with other evidence of operant conditioning (cf. Lancioni, 1980) and animal-like classical conditioning in infants (cf. Fitzgerald and Brackbill, 1976) supports the view that infant learning obeys the same

Table 5: The slope n and the coefficient of linear regression r calculated from log PRPs and log schedule values for each of the subjects in Experiment I.

Subject	n	r
SR	0.0506	0.1125
LR	1.1480	0.7606
IP	0.5507	0.9375
LC	0.7925	0.9952
MK	0.9850	0.8944
HP	0.9711	0.9091
JJ	0.0775	0.2257
SM	0.1912	0.4689
TS	-0.1561	0.3966
JON	0.5826	0.9987
ANN	0.7071	0.9985
PAT	0.5530	0.9763
WILL	0.6195	0.9833

general laws that have been found to apply to infrahuman learning.

In marked contrast to the infant subjects, the subjects in the two oldest groups produced the same two patterns of FI responding found in adults (cf. Lowe, 1979) and in previous studies with children four years and older (Long et al., 1957; Zeiler and Kelley, 1969; De Casper and Zeiler, 1972). In the case of five of these subjects, a low-rate pattern was produced. In the case of one subject in the oldest group, a high-rate pattern was produced.

The behaviour of the children in the two and a half to four year old group, however, did not correspond either to the behaviour of the older subjects or to the behaviour of the infants. These subjects seemed to be between two stages, and their performance showed the irregularity and lack of patterning previously reported by Long et al. (1958) for some of his subjects.

These results, showing animal-like performance in infants, and an adult-like performance in older children following a transitional stage somewhere between two and five years of age, provide empirical support for the view, outlined in earlier chapters, that verbal self-regulation plays a major role in controlling the operant behaviour of human adults. Infants lack speech. However, developmental studies show that, normally, the ability to control motor behaviour by self-directed speech is substantially learnt by the fifth year of life (Vygotsky, 1962, 1978; Luria, 1959, 1961, 1982; Zivin, 1979).

Although much of the verbal activity recorded in the experiment was difficult to interpret that which could be understood supported the verbal regulatory hypothesis. Thus, the infants were capable of no truly linguistic activity. The two and a half to four year olds, on the other hand, produced a considerable amount of speech, most of which was irrelevant to the task. As with the similarly aged subjects of Luria´s (1959, 1960, 1982) experiments, their speech may have served only an impulsive, rather than a truly regulatory function. Only in the older subjects was speech organised with respect to the subjects´ responding on the schedule (as exemplified by counting). In the very oldest subjects virtually no overt speech, other than transient counting in the case of IP, was recorded and it may be hypothesised, after Vygotsky (1962), that, in their case, verbal regulatory activity had become internalised. These observations are comparable with the results of more systematic studies of self-directed speech (eg. Kohlberg et al., 1968; Deutsch and Stein, 1972).

Together with the results of other research, such as that outlined in chapters 2 to 5, therefore, the present evidence suggests that previous studies of human operant behaviour may have failed to produce animal-like responding because they used subjects with well-developed verbal repertoires. An obvious way of further exploring this hypothesis is to attempt to gain control of the self-regulatory verbalisations of children of different ages as they perform on operant tasks. That was the aim of the next experiment.

CHAPTER 8: THE EFFECTS OF INSTRUCTIONS AND SELF-INSTRUCTIONS ON CHILDREN'S OPERANT BEHAVIOR

In the last chapter, details were given of the development of fixed-interval responding in children between infancy and approximately nine years of age. It was observed that, while the behaviour of infants conformed to the laws formulated to account for animal responding, the behaviour of the older children exhibited those deviations from the animal laws that have come to be expected in adults. This result was in accordance with the verbal regulatory hypothesis of human operant behaviour. In Chapter 3 it was noted that a powerful determinant of human responding on schedules of reinforcement, absent in the case of animals, is the instructions given by the experimenter to the subject (Laties and Weiss, 1963; Baron, Kaufman and Kopp, 1966; Matthews et al., 1977; Lowe, 1979) The effect of such instructions on the behaviour of infants would seem a possibly fruitful object of study.

The concept of the zone of proximal development (ZPD) as advanced by Vygotsky was discussed in Chapter 4. Instructions about how to respond on a schedule of reinforcement, supplied by an adult, might prove a useful tool in the exploration of a child's ZPD on operant tasks. It may be expected that, if a child's self-regulatory skills have not yet developed but are in the process of development, then instructions given to the child might facilitate adult-like responding as opposed to the transitional style of responding found among two and a half to four year olds in Experiment I. This would seem especially likely if, as supposed by Vygotsky (1962) and Luria (1959, 1960, 1982), the child's ability to regulate his own behaviour by means of self-directed speech grows out of social speech in general and out of his ability to follow instructions from others in particular.

The obvious choice of instructions to use are those most likely to produce either the high-rate or the low-rate pattern of responding observed in human adults. Such instructions would, of necessity, have to be phrased in a way comprehensible to the child. A clue to how appropriate low-rate instructions might be phrased is given by the assertion by one of the subjects in the oldest group in Experiment I, that he paused after reinforcement while Sooty had "a little sleep". The work of Meichenbaum (1977) and his followers on the clinical application of self-instructional training suggests a further way in which a child's self-directed speech may be brought under the experimenter's control. Such self-instructional training was carried out as an additional condition in the following study, after the effects of instructions alone had been assessed.

Subjects

The subjects in the experiment were in three age groups corresponding to the three oldest age groups used in Experiment I. Group One consisted of three boys and and three girls aged between seven and a half and nine years of age, with a mean age of eight years and two months at the beginning of the experiment. Group Two consisted of three girls and three boys aged between five and six and a half years of age with a mean age of five years and eight months at the beginning of the experiment. Group Three, also consisting of three boys and three girls, was aged between two and a half and four years with a mean age of three years and four months at the experiment's beginning. The ages of the individual subjects at the beginning of the experiment are given in Table 6.

As in Experiment I, the subjects were recruited from a local school and a local nursery group.

Table 6: Ages of the individual subjects at the start of Experiment II (years and months).

Group	Subject	Age
1	BN	9,0
	CR	7,8
	GL	8,0
	HW	8,1
	MP	8,2
	LR	8,2
2	CH	5,7
	TH	6,2
	GD	5,6
	BP	6,1
	TL	5,7
	JS	5,4
3	GH	4,0
	JA	3,5
	KE	2,10
	ES	3,3
	CA	3,2
	AN	3,4

Half the children in each age group, either two boys and a girl or two girls and a boy, were assigned to the low-rate instruction and self-instruction conditions, and the remainder were assigned to the high-rate instruction and self-instruction conditions.

Apparatus

The apparatus used was identical to that used with the three oldest groups of children in Experiment I, and consisted of an Apple II Plus microcomputer, hidden behind a screen, which collected responses and scheduled reinforcement. As in the previous study, the response device was a manipulandum requiring approximately 670 grammes force response pressure, and each response produced an audible click. Reinforcement consisted of a series of coloured lights flashing in a vertical column, accompanied by pop music and a cartoon slide projected on the screen, both presented for ten seconds. In addition, a glove puppet, manipulated by the experimenter behind the screen, appeared above the screen and dropped a crisp or similar small snack item down a chute into a tray next to the manipulandum, from where it could be collected and eaten.

Procedure

The low-rate subjects were instructed as follows: After the subject had been introduced to the experimental room, the experimenter set up a FI 40 second schedule on the computer and sat by his or her side. E then said, "watch what happens when I press this lever", pressed the lever and received a reinforcement. When reinforcement next became available the experimenter suggested to the child that he press the lever and, when this happened delivered a reinforcer to the child. E then said, "Now try again". Because a reinforcer had just been delivered the child´s second response was invariably not reinforced. The experimenter explained this in the following manner:

"Look, nothing happened. Sooty didn´t come to see you. That´s because, after he came to see you before, he had a rest and went to sleep. You mustn´t press the lever when he´s asleep as that´ll wake him and he doesn´t like that. You must wait until he´s awake.... Do you think he´s awake yet.... I think he´s awake. You can press now."

The experimenter then talked the subject through the next response in a similar fashion. After this he told the subject that he would be going behind the screen and that the subject would have to do everything without the experimenter´s help.

For the next five intervals, the experimenter called, "Not yet, Sooty´s still asleep," if the subject pressed too early, and "Do you think Sooty´s awake now?" if the subject was more than ten seconds late. No further instructions were given.

The high-rate subjects were instructed in a similar manner except, in their case, the subjects were told that the reason why Sooty failed to come and see them was because he was lazy, and that they had to press the lever many times to make sure he woke up. During the first five intervals after the experimenter went behind the screen the subject was prompted to press faster if he stopped responding. This only happened on a couple of occasions.

All subsequent sessions were begun by reminding the subjects of their instructions. This was done by asking them, "What do you have to do to make Sooty come and see you?" In all cases the subjects were able to state some version of the contingency that had previously been described to them and it was never necessary to give any additional instructions.

The instructed subjects responded on the FI40 second schedule for five sessions during which, by inspection of the cumulative records, it was established that the performance of all subjects was stable.* The sessions lasted for about ten to fifteen minutes each, and were usually carried out on consecutive days, although in some cases two sessions were carried out with a subject on one day.

Having completed the instruction condition the subjects then performed on the self-instruction condition. At the beginning of the first session the high-rate subjects were all taught by modelling and explicit instruction to say "Faster, faster", while pressing the lever. In the case of all the subjects in the two oldest groups and one child in the youngest group, the subjects were then tested for three further sessions during which their behaviour was found to be stable. The other two children in the youngest group, however, seemed to find the self-instruction task aversive, and a certain amount of coaxing and several extra sessions were required before they would self-instruct consistently.

In the case of the low-rate self-instruction condition,

---

* over three consecutive sessions

the two oldest groups were simply told to count aloud to forty or fifty - depending on the individual subjects´ counting speed - before pressing the lever. This very straightforward procedure was sufficient to produce overt counting and a stable performance for three further sessions. Unfortunately, although Bem (1967) found that all of her three year old subjects could count none of the youngest children in this study had developed sufficient counting skills to allow them to perform the self-instructional task given to the older subjects, and when asked to count they all seemed to find the experience highly aversive. Having failed to teach these subjects the counting strategy, then, it was decided to teach them to ´sing-and-press´.

First of all, the subjects had a "sing-song" with the experimenter during which a medley of nursery rhymes was elicited from each. The subjects were then taught to sing a song before pressing the lever. This was done by a combination of shaping, modelling and explicit instruction. When the subjects seemed to have learned the strategy, which took a number of sessions, the experimenter faded himself out of the experimental situation and withdrew behind the screen, leaving the child to respond alone for the remaining sessions. Three further sessions of data were collected. Most sessions were tape recorded. The total number of sessions taken by each subject to complete each condition is shown in Table 7.

RESULTS

(i) Schedule Performance

When examining the performance of the children it is useful to look at (i) how their behaviour varied according to the type of instruction and self-instructional training given; (ii), how these differences related to the age of the subjects; and (iii), how the behaviour of the instructed and self-instructed children as a whole differed from that of non-instructed subjects of a similar age, such as those in Experiment I.

The cumulative records from the first session for each of the subjects in the two oldest groups are shown in Figure 16. As can be seen, from the first session, each of the high-rate instructed subjects responded at a consistent high-rate, whereas each of the low-rate instructed subjects responded at a low-rate, showing that the effect of the

Table 7: Design of Experiment II. (Session numbers shown in brackets.)

Group	Age range	Subjects	Sub-group	Conditions	
1	$7\frac{1}{2}$-9 yrs	BN	High-rate	FI40(5), instructed	FI40(3) self-instructed
		CR			
		GL			
		HW	Low-rate	FI40(5), instructed	FI40(3) self-instructed
		MP			
		LR			
2	5-$6\frac{1}{2}$ yrs	CH	High-rate	FI40(5), instructed	FI40(3) self-instructed
		TH			
		GD			
		BP	Low-rate	FI40(5), instructed	FI40(3) self-instructed
		TL			
		JS			
3	$2\frac{1}{2}$-4 yrs	GH	High-rate	FI40(5), instructed	FI40(3) self-instructed
		JA			
		KE			
		ES	Low-rate	FI40(5), instructed	FI40(3) self-instructed
		CA			
		AN			

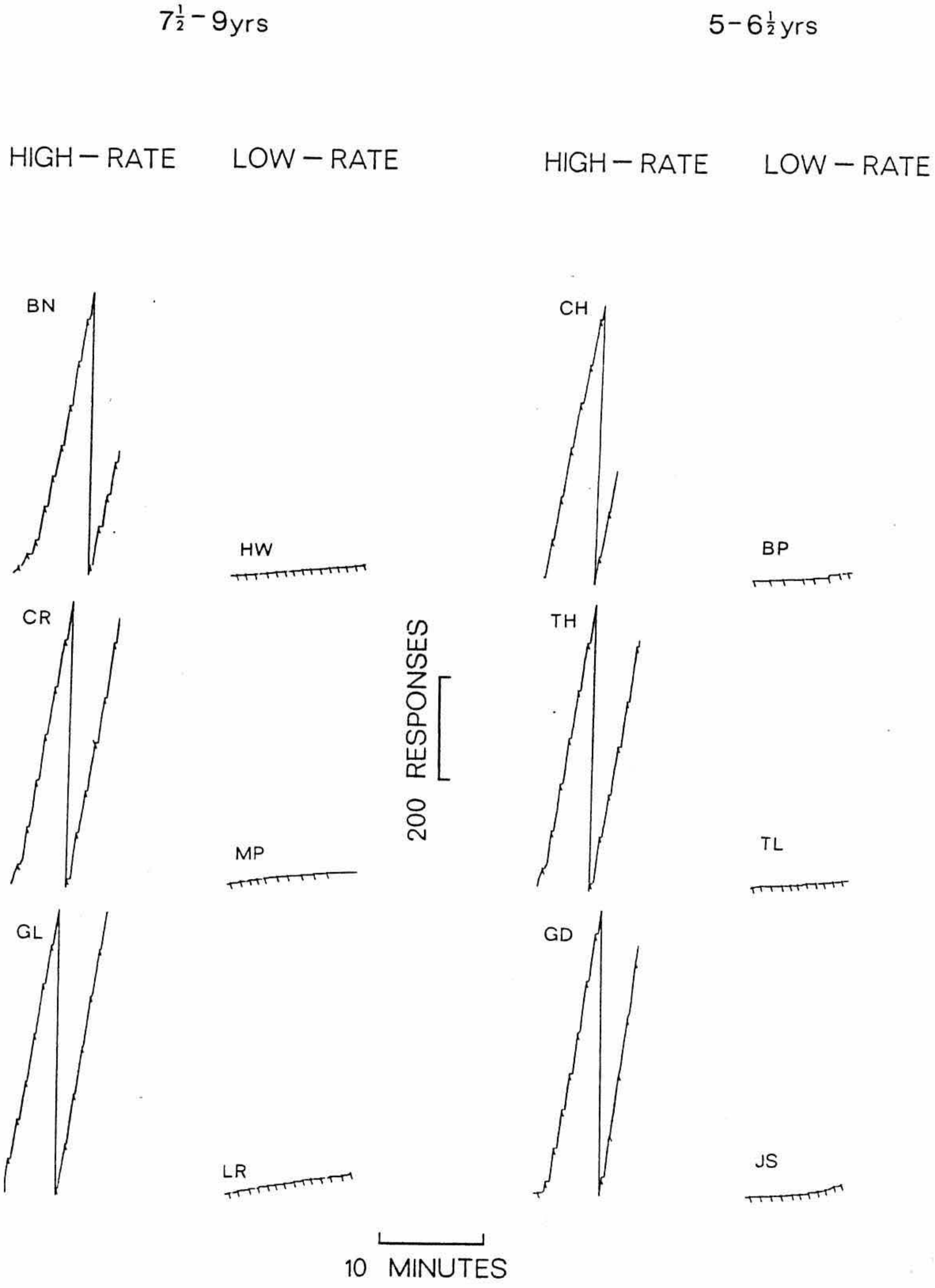


Figure 16: Initial cumulative records for high-rate and low-rate instructed subjects in Groups One and Two, Experiment II.

instructions was immediate. Although, in Experiment I, the one high-rate responder in the oldest group produced a stable pattern from her first session (see Figure 6), this was not found to be the norm for uninstructed subjects - uninstructed low-rate responders did not usually begin by pausing for the full interval length (although their rate of responding was low), and several sessions were normally required until a consistent low-rate pattern prevailed.

The first cumulative records of the youngest children are shown in Figure 17. For these children, marked differences existed between the initial effectiveness of the high-rate and the low-rate instructions. The high-rate instructed subjects responded immediately at a high-rate, although perhaps with not quite the same consistency as the older subjects. The low-rate subjects, on the other hand, despite some evidence of a reduced response rate compared to the non-instructed subjects in Experiment I, failed to maintain a consistent low-rate pattern (see Figure 119. Indeed, the rate of their responding seems to have increased slightly in subsequent sessions. This is despite clear indications that they were trying to wait after reinforcement, as revealed by their ability to describe the required behaviour when questioned at the beginning of each

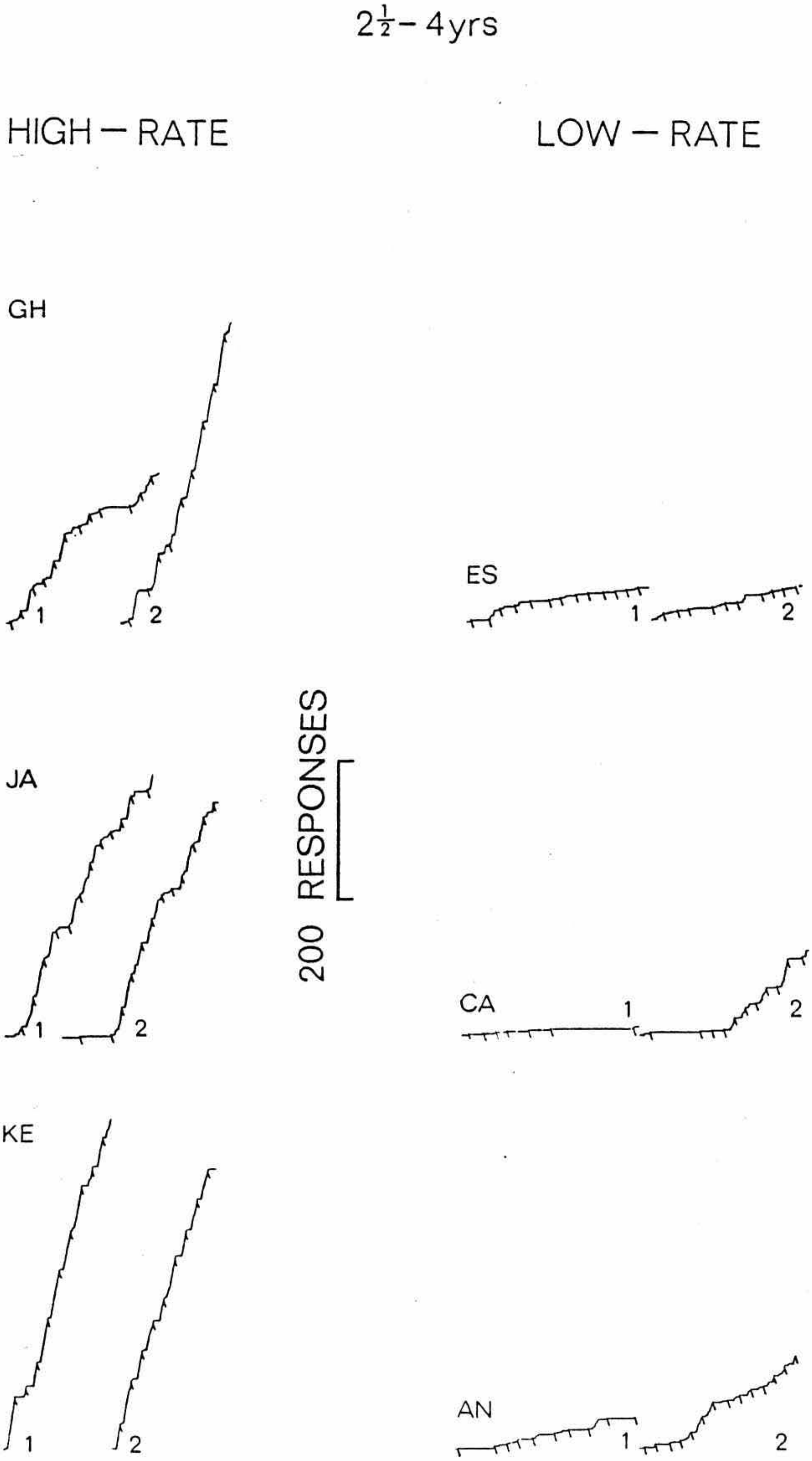


Figure 17: Initial cumulative records for high-rate and low-rate instructed subjects in Group Three, Experiment II.

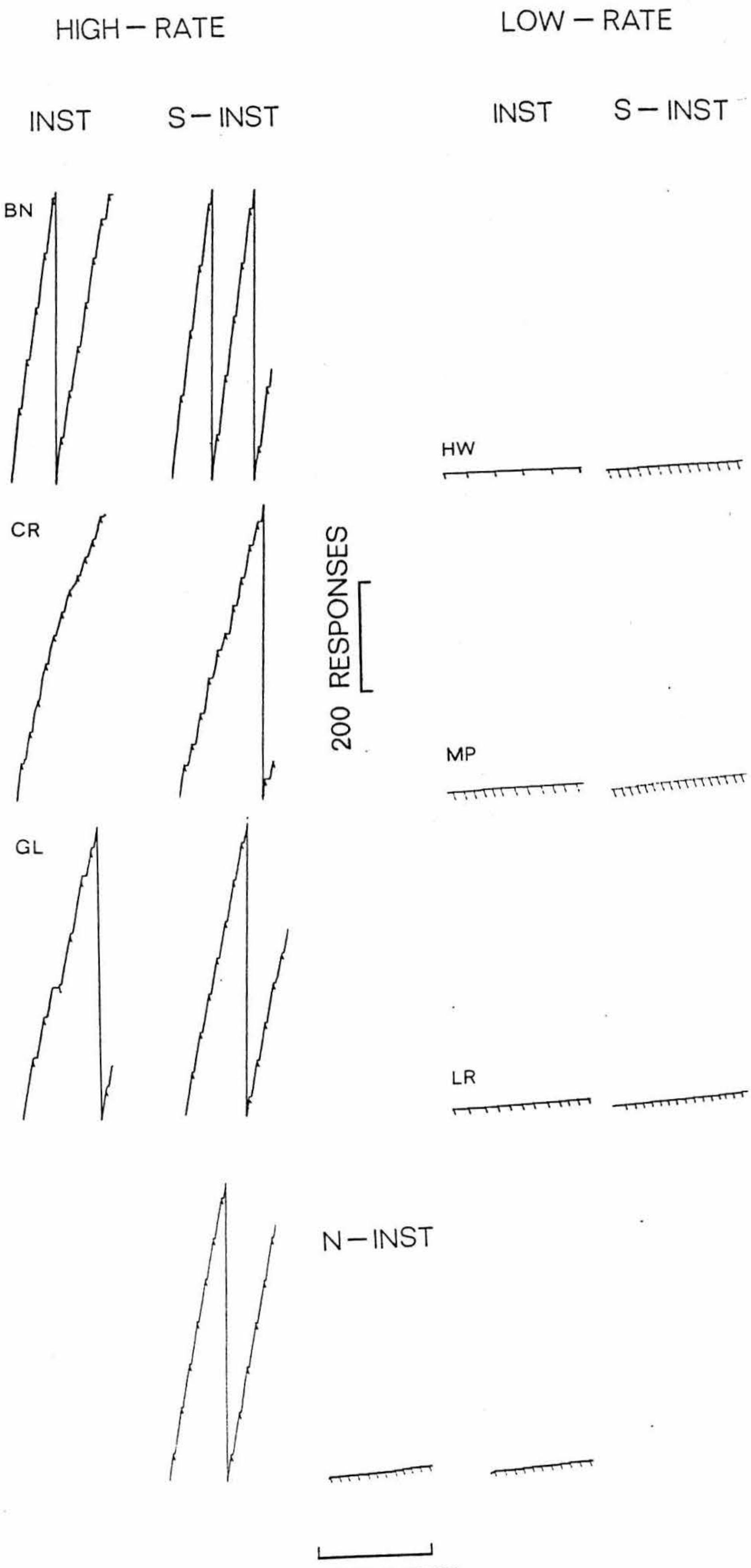
session, and by their concurrent verbal and motor behaviour during the session itself (discussed below).

The final forms of both the instructed and self-instructed responding of the subjects in Group One, the oldest group, are shown in Figure 18. For comparison purposes, the figure also contains the cumulative records, from Experiment I, of three non-instructed children of the same age, on the same schedule value. These data, as described in the last chapter, show the two familiar patterns of FI responding found in human adults.

The most striking aspect of the performance of the instructed and self-instructed children is the continued persistence of these two patterns in consistent relation to the instructions and self-instructional training given. Again, those given high-rate instructions and self-instructions have all produced the high-rate pattern, whereas the low-rate instructed and self-instructed children have all produced low-rate performances.

There appears to be little difference between the effects of the instructions and the self-instructions in both sub-groups. This is particularly so for the high-rate children, who were probably responding as fast as they could in the instruction condition. However, in the case of the

Figure 18: Final cumulative records of Group One subjects in both instruction and self-instruction conditions, Experiment II. (Comparison non-instruction data from Experiment I included.) (Over.)


HIGH – RATE
LOW – RATE
INST
S – INST
INST
S – INST
BN
HW
CR
200 RESPONSES
MP
GL
LR
N – INST
10 MINUTES

low-rate responders, all the subjects considerably overestimated the interval length in the instruction condition and the effect of the self-instructions was to increase the frequency of responding so that responses were typically made just after the end of the interval.

Figure 19 shows similar cumulative records for Group Two. In this case, the three comparison records taken from Experiment I were all low-rate. Again, the subjects in the low-rate sub-group produced generally low-rate performances, and the subjects in the high-rate sub-group responded at a constant high-rate. The records are slightly uneven compared with those produced by the older group but the overall picture remains essentially the same. The one important difference between these children and those in Group One can be found in the low-rate instruction condition, where the effect of the instructions has not been so great as to cause the children to overestimate the interval length. As a consequence, no difference between the effects of the instructions and the self-instructions can be found for either sub-group.

Similar cumulative records and comparison records for Group Three, the youngest group, can be seen in Figure 20. The comparison records for same age subjects on the same

Figure 19: Final cumulative records of Group Two subjects in both instruction and self-instruction conditions, Experiment II. (Comparison non-instruction data from Experiment I included.) (Over.)

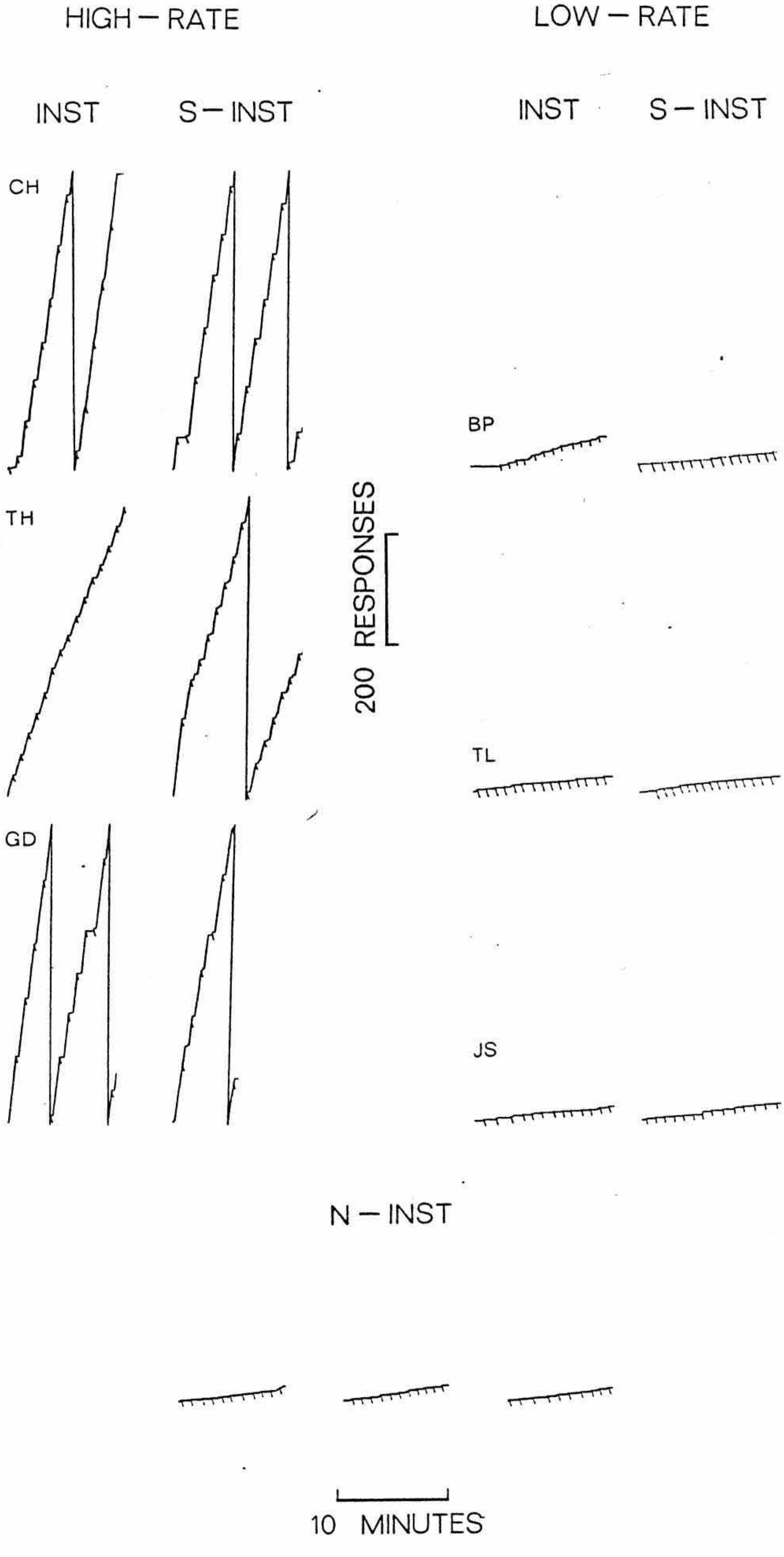
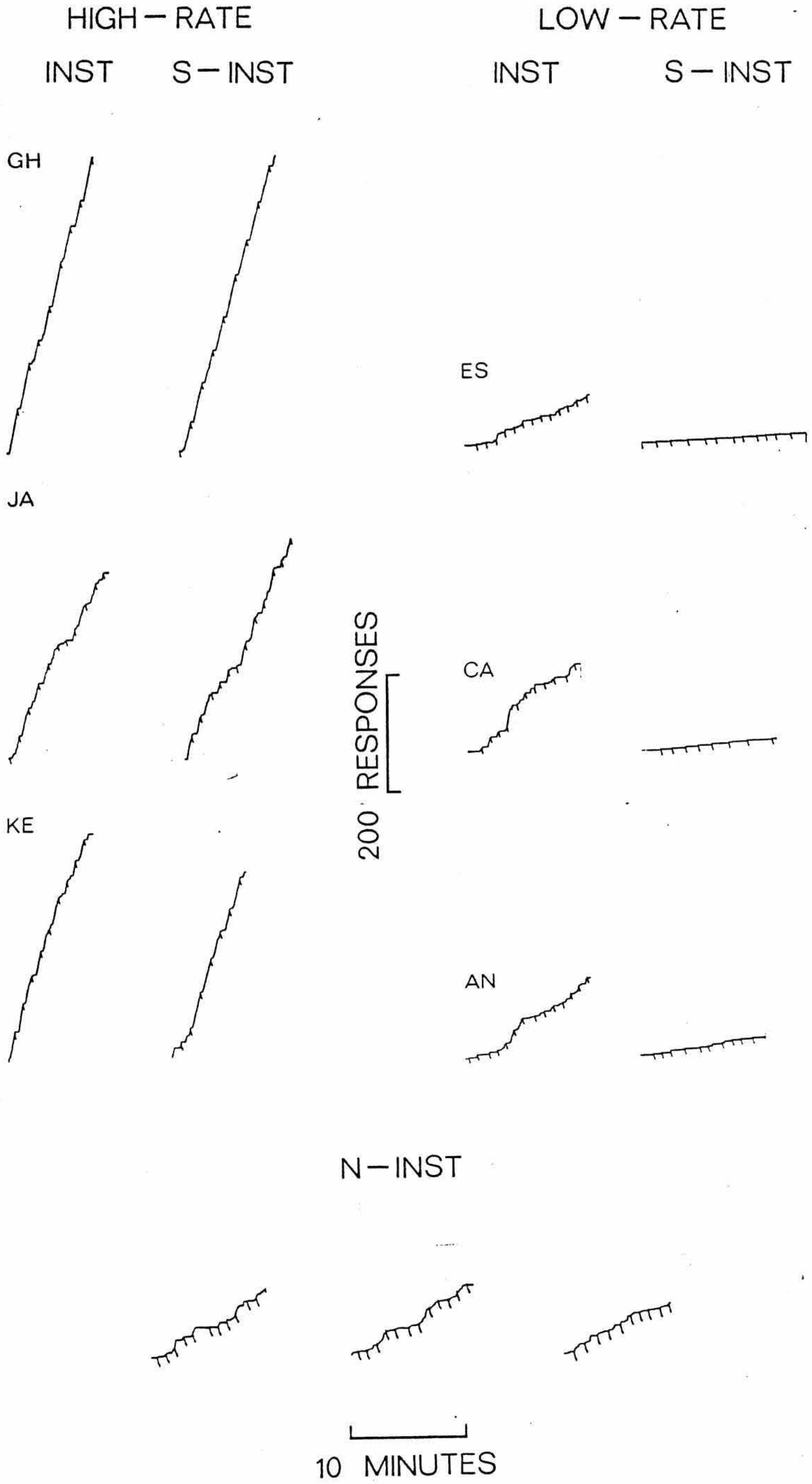

HIGH — RATE
LOW — RATE
INST
S — INST
INST
S — INST
CH
TH
GD
BP
TL
JS
200 RESPONSES
N — INST
10 MINUTES

Figure 20: Final cumulative records of Group Three subjects in both instruction and self-instruction conditions, Experiment II. (Comparison non-instruction data from Experiment I included.) (Over.)


HIGH — RATE
INST
S — INST
LOW — RATE
INST
S — INST
GH
ES
JA
200 RESPONSES
CA
KE
AN
N — INST
10 MINUTES

schedule value in Experiment I show the uneven, inconsistent pattern of responding reported for uninstructed subjects of this age. The high-rate instructed and self-instructed children of this age, on the other hand, again responded at a high-rate in both conditions. It can be seen therefore that, whereas the uninstructed children observed in Experiment I were not able to respond in an adult-like manner, the high-rate instructed and self-instructed children were able to do so.

The results obtained from the children in the low-rate sub-group are very different. There appears to be little if any difference between the behaviour of these children in the instruction condition and the uninstructed behaviour of the children of the same age reported in Experiment I. The addition of the special self-instruction strategy ("sing and press") developed specifically for these children, however, produced a constant low-rate performance similar to that of the older children. Thus, despite the ineffectiveness of instructions alone, the effect of self-instructions has been to produce behaviour that, on the surface at least, appears similar to that produced by adults but is markedly different from that produced by uninstructed children of the same age.

Other measures, taken from the last three sessions of each condition, support the general picture given by the cumulative records. Figure 21 shows the post-reinforcement pause data for the subjects in all three groups in both conditions. (The mean PRPs and their standard deviations are also given in Table 8.) Comparison data for the subjects in Experiment I is also included. In all cases the mean PRPs of the high-rate subjects are low in comparison to those of the low-rate subjects, about as low as that of the one non-instructed high-rate responder observed in Experiment I. Self-instructional training appears to have had little effect on the PRPs of the high-rate children, except for increasing them slightly in the oldest two groups. For the low-rate children, however, the PRPs were substantially increased by self-instructional training in the youngest group, increased slightly in the five to six and a half year group, and substantially decreased to just over the interval length in the oldest group, which had previously paused excessively.

A similar picture is revealed by the rate data, given in Figure 22. Again, there is the obvious difference between the high-rate and low-rate children. With the high-rate children there was no consistent change of rate brought

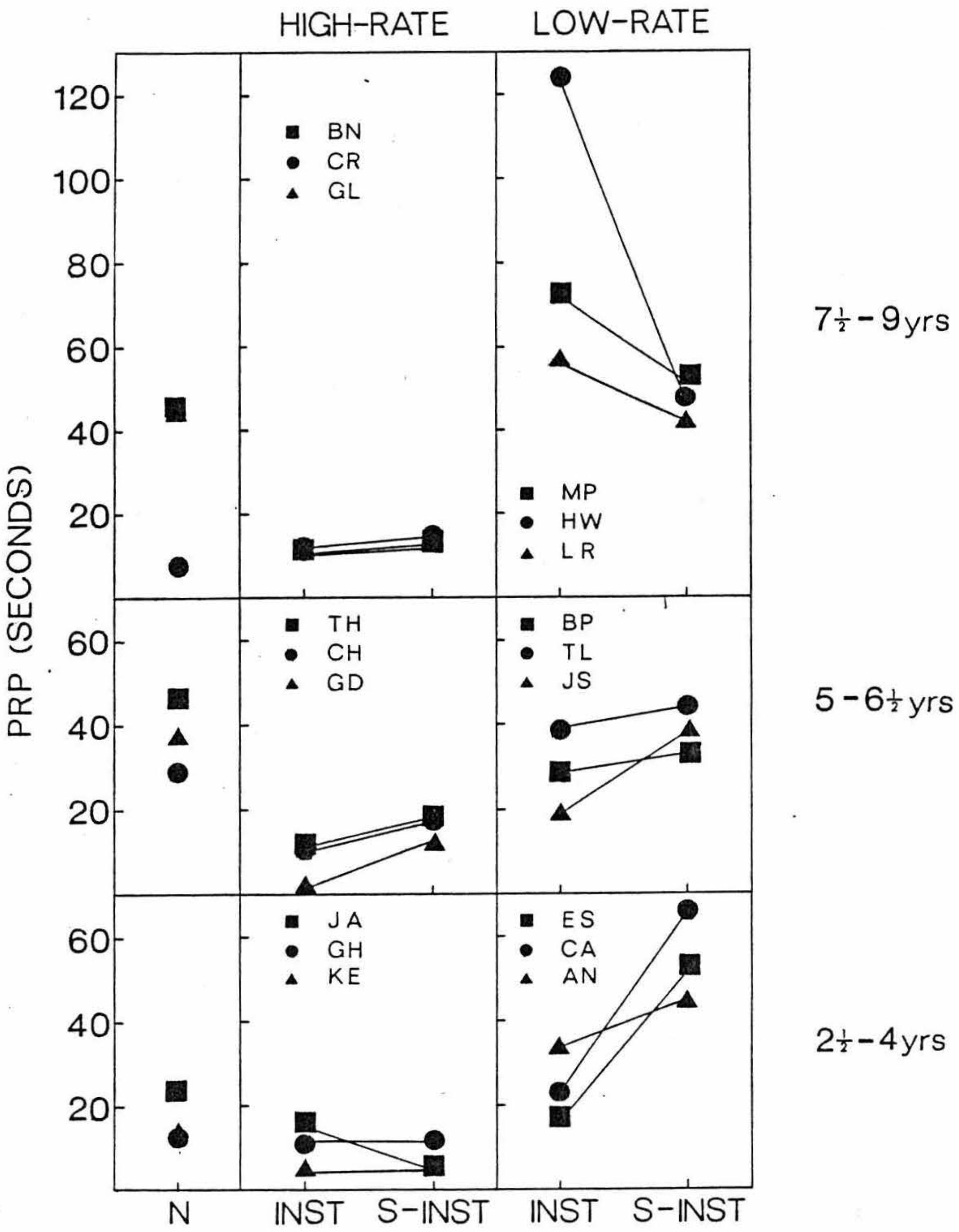


Figure 21: Mean post-reinforcement pauses of subjects in Experiment II in instruction and self-instruction conditions. (Comparison non-instruction data from Experiment I included.)

Table 8: Mean PRPs and standard deviations for the subjects in Experiment II (seconds).

Group	Sub-group	Subject	Condition	PRP	SD
1	High-rate	BN	Inst.	10.44	5.15
			S-Inst	12.11	2.67
		CR	Inst.	12.81	7.77
			S-Inst.	13.71	6.23
		GL	Inst.	10.16	2.27
			S-Inst.	11.49	1.65
	Low-rate	HW	Inst.	123.83	34.04
			S-Inst.	46.60	10.94
		MP	Inst.	72.08	43.46
			S-Inst.	51.21	13.46
		LR	Inst.	55.95	12.01
			S-Inst.	42.30	23.20
2	High-rate	CH	Inst.	10.83	1.65
			S-Inst.	18.35	8.74
		TH	Inst.	10.48	4.33
			S-Inst.	16.92	6.87
		GD	Inst.	1.45	1.65
			S-Inst.	12.67	4.87
	Low-rate	BP	Inst.	27.92	21.41
			S-Inst.	33,24	17.80
		TL	Inst.	38.63	12.32
			S-Inst.	43.87	12.17
		JS	Inst.	18.93	22.41
			S-Inst.	38.02	27.27
3	High-rate	GH	Inst.	11.25	5.55
			S-Inst.	12.72	3.07
		JA	Inst.	18.13	16.21
			S-Inst.	7.25	6.29
		KE	Inst.	5.80	2.16
			S-Inst.	7.69	5.23
	Low-rate	ES	Inst.	18.12	10.45
			S-Inst.	56.58	31.34
		CA	Inst.	24.91	13.65
			S-Inst.	66.59	14.44
		AN	Inst.	34.52	42.36
			S-Inst.	44.84	17.61.

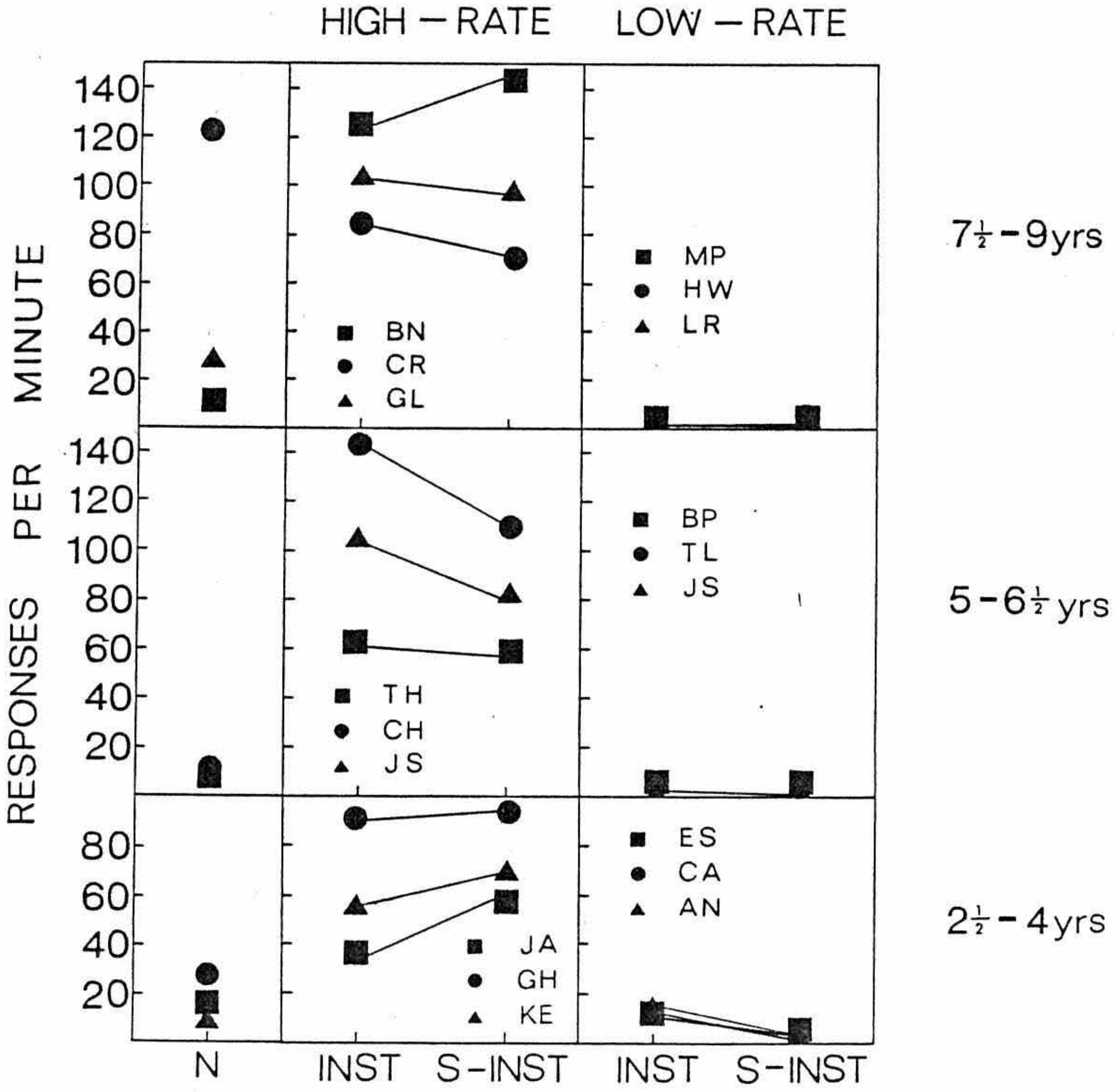


Figure 22: Rates of responding of subjects in Experiment II in instruction and self-instruction conditions. (Comparison non-instruction data from Experiment I included.)

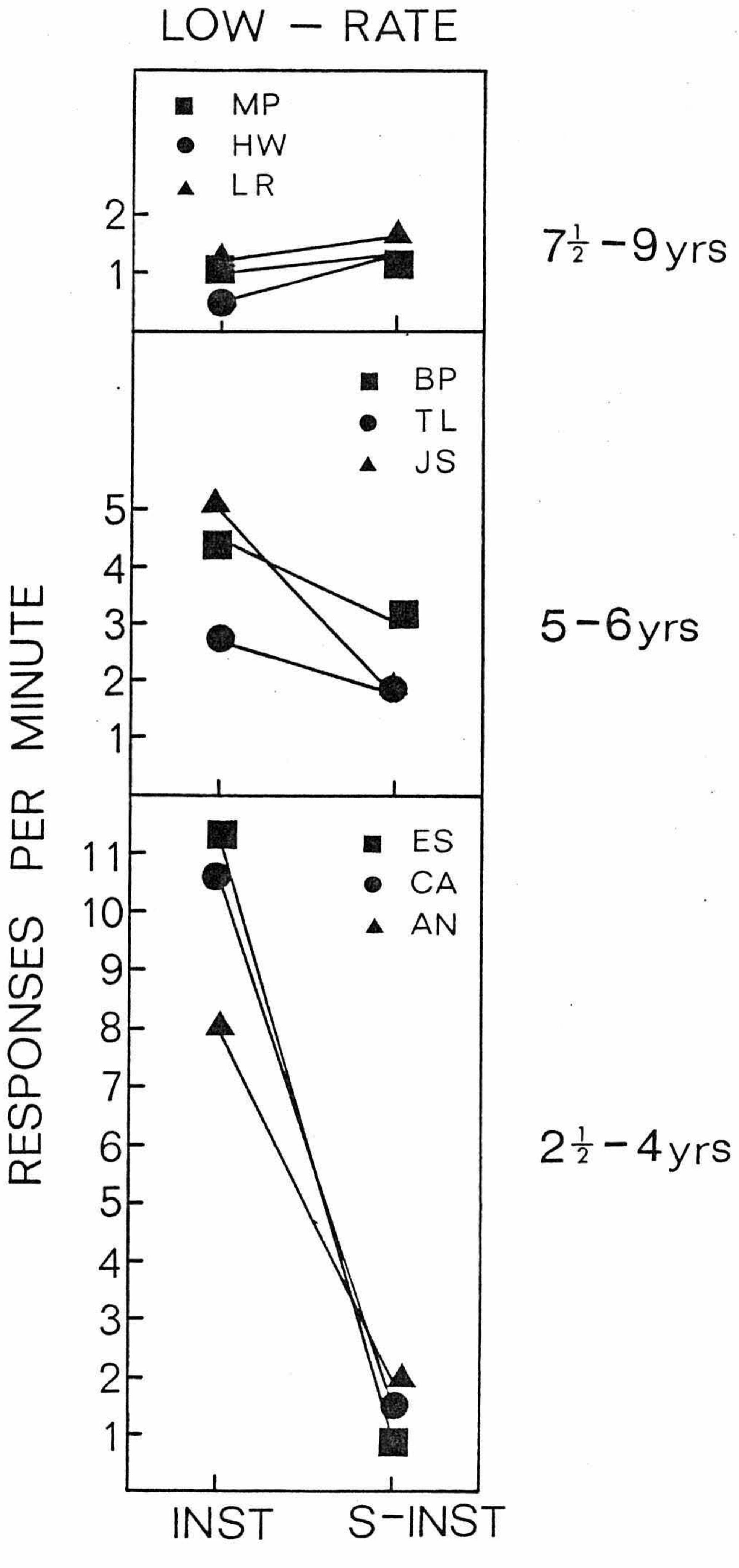
about by the addition of self-instructions. With the low-rate children it is useful to look at a further figure, Figure 23, on which their rate data is represented on a magnified scale. It can thus be seen that the effect of self-instruction in the oldest group has been to increase the rate of responding. This increase reflects the decrease in the PRPs already noted, and the reduced tendency for these children to overestimate the interval length. In contrast, some reduction of rate can be observed in the intermediate group but the greatest effect can be observed in the data for the youngest subjects. In their case, the effect of the self-instructions was to bring down the response rate to a level comparable to that of low-rate adult responders.

(ii) Other Behaviour During the Experiment

During the experiment, a certain amount of interesting verbal and non-verbal behaviour occurred.

In the two oldest groups, motor behaviour was confined exclusively to the experimental task. Although overt counting, as instructed, occurred throughout the self-instruction condition, little verbal behaviour, apart

Figure 23: Rates of responding of low-rate subjects in Experiment II in instruction and self-instruction conditions. (Over.)


LOW – RATE
MP
HW
LR
2
1
7½ – 9 yrs
BP
TL
JS
5
4
3
2
1
5 – 6 yrs
ES
CA
AN
11
10
9
8
7
6
5
4
3
2
1
2½ – 4 yrs
RESPONSES PER MINUTE
INST
S-INST

from the occasional indecipherable whisper or gurgle (the latter particularly in the case of JS), was recorded during the instruction condition.

In the youngest high-rate group, also, little verbal activity was recorded - the subjects were occupied with lever pressing and collecting reinforcement, almost to the total exclusion of everything else. It was from the youngest low-rate subjects that by far the greatest amount of verbal behaviour was recorded, particularly from subjects ES and CA. In addition, some interesting motor activity was observed.

This consisted, for the most part, of singing, laughing and the making of various loud squealing noises. Also, a number of remarks were addressed to Sooty such as, ´Here comes Sooty!´ and ´Hello! Hello!´. However, some verbal behaviour, together with motor behaviour, appeared to be emitted in an attempt to fulfil the requirements of the instructions. The most striking example of this was provided by ES, who, following reinforcement, occasionally placed her hands together (as if in prayer) and rocked from side to side, saying, ´Tick-tock. Tick-tock.´ in apparent imitation of a clock. This apparent attempt to regulate her own behaviour was, however, invariably unsuccessful as, before five or so seconds had passed she would break off and quickly pull the lever. Further verbal activity that perhaps had a regulatory function were shouts of ´Yes! Yes! Yes!´ and ´Now!´ in time with lever pressing. A similar verbal response repeatedly made by CA was ´Sooty´s awake!´ made typically before the end of an interval and accompanying lever presses. These acommunicative speech acts seemed to occur at the end of a short period of waiting and to initiate a burst of responding, as if the child could no longer tolerate the interval. In contrast to ES and CA, AN (a very shy child) produced very little speech that could be recorded, apart from the occasional squeal, sigh, or whisper. Even on the few occasions on which she said ´Hello´ to Sooty she did so very quietly. One response that was made by all three of the youngest low-rate instructed subjects, however, was running around and exploring the room - a form of collateral behaviour described by Pouthas (1981) for children on DRL schedules.

DISCUSSION

The results of this study can be best summarised in the following way: In children old enough to produce an adult-like pattern of responding when uninstructed, the effect of the instructions and self-instructions was to determine the type of adult-like performance - high-rate or low-rate - produced. This is consistent with the results found when adults are instructed and, together with the data on non-instructed children already outlined in Experiment I, confirms that by the time the child has reached the age of five or six years his or her behaviour on fixed-interval schedules of reinforcement no longer resembles that of animals.

At least as interesting are the results obtained by giving instructions and self-instructions to the younger children. It was with these subjects, the two and a half to four year olds, that it was possible to produce an adult-like performance which, so the comparison data indicates, would not otherwise have been produced.

This exploration of the zones of proximal development of children of different ages, providing help from adults in the form of verbal guidance and thus, in the case of younger children, transforming their behaviour, provides further evidence to support the view that it is the capacity for verbal self-regulation that marks off the older children from the younger, and by the same token, man from the animals.

One question that remains is that of why the high-rate instructions were so much more effective than the low-rate instructions at producing an adult-like performance in two and three year olds. Why was it necessary to give the low-rate group self-instructions before their behaviour took on adult form? The answer to this may be found by looking at the Soviet research on the development of verbal self-regulation.

As we have seen, Luria (1959, 1960, 1982; cf. Chapter 4) argued, on the basis of a number of experiments, that verbal regulatory skills do not develop fully until the third and fourth years of life. The effects of attempts to manipulate a child´s self-regulatory activity before that age, according to this line of reasoning, will depend upon the function of the word in the child´s understanding. For example, if a child was asked to press a bulb on seeing a red light and not on seeing a green light, Luria (1959) found that three year olds pressed to both lights and only children four and a half years old or older could master the task. This was because the semantic function of words was not fully developed in the three year old. For the three year old, meaning is ´sympractical´ - tied to particular acts - and words can only function in an impulsive manner (Luria, 1982). In other words, the verbal processes that govern the planning and witholding of behaviour develop later than the cue properties of words which, under most circumstances, can only initiate acts. In the case of the youngest children in this experiment, those told to respond at a high-rate found it easy to comply whereas those told to wait and to stop responding for a while, were unable to inhibit their behaviour until they were taught the ´sing-and-press´ self-instruction strategy.

Of couse, it would be wrong to conclude from this data that the singing emitted by the youngest low-rate children during the ´sing-and-press´ phase of the experiment was functionally identical to the self-directed speech induced in the more mature subjects. By the same argument outlined by Luria, this would only have been the case if it had been possible to alter the childrens´ use of words in general. Clearly this was not what happened and the verbal behaviour emitted by these subjects functioned as one component in a chain of responses determined initially by the experimenter.

Further evidence to support this interpretation can be found in the unsuccessful attempts made by these subjects to regulate their own responding in the instruction phase. The children tried to inhibit their own responding by both verbal and other means but were unable to do so effectively enough to produce low-rate responding. An interesting parallel can be found between these attempts at self-regulation and the results of other studies, particularly studies of delayed gratification such as that carried out by Mischel, Ebesen and Zeiss (1972). In Mischel et al.´s study children between the ages of three years and six months and five years and six months were required to wait fifteen minutes for a preferred reward or to ring a bell to fetch the experimenter, in which case they received a non-preferred reward. Although low-rate subjects in the schedule conditions of the present experiment were only required to wait for forty seconds, and although premature responding did not alter the nature of the reinforcer or the time of its occurrence, another way of looking at the low-rate instruction condition in this study is to regard it as a delayed gratification task. In their study, Mischel et al observed verbal and other behaviour similar to that observed in the present experiment (see Chapter 4 for an example).

Although Mischel did not interpret his results in terms of Vygotsky´s theory (preferring instead to regard the child´s activity in the waiting period as serving a distracting function) his children behaved in a manner strikingly similar to the children in this study, and his data is perfectly consistent with the data presented here.

The fact that the children in both this study and in Mischel´s understood that they were required to wait and (for the most part unsuccessfully) tried various strategies in order to prevent themselves from responding suggests that, in contradiction to Pouthas´ (1983) hypothesis, transitional responding is not the result of a lack of time concepts. Rather, what was involved was a real lack of self-regulatory skills as described by Luria - an inability to put the concept of ´wait´ into action.

CHAPTER 9: A STUDY OF THE RELATIONSHIP BETWEEN LEARNING HISTORY AND AGE

The evidence discussed so far has outlined the development of human operant responding from the animal-like mode of infant subjects to the verbally regulated mode of adults. It was observed that the effect of instructions on performance is dependent upon the developmental stage which a subject has reached. Obviously infants cannot be instructed. The performance of children in the two and a half to four year range (the transitional period) seems to depend on the exact nature of the instructions given - some instructions, particularly those requiring a witholding of responding, have little effect. In older children instructions appear to function in the same way as they do with adults.

In Chapter 3 it was observed that the effects of conditioning history in human adults differ from the effects of conditioning history in animals. In particular, human subjects continue to respond at a high-rate on FI schedules after exposure to FR schedules and at a low-rate after initial exposure to DRL schedules (Weiner, 1965a, 1969, 1970b). The effects of such histories on animal FI responding are much more transient (Skinner and Morse, 1958;

Urbain, Poling, Millam and Thompson, 1978). As we have seen in Chapter 3, one explanation of this difference is that human subjects may form verbal hypotheses on the basis of their experience of one schedule which continue to govern their behaviour when they transfer to another.

The verbal regulation hypothesis suggests that the conditioning history effects reported by Weiner should be attenuated in subjects with little or no language skills and that, therefore, the FI performances of infants previously exposed to either FR or DRL schedules should be less affected by such prior exposure than the FI performances of older children. The only evidence that has been collected on the effects of history in children has not been sampled from a wide enough age range to allow statements to be made about the developmental course of such effects (Long, 1962; Zeiler and Kelley, 1969; De Casper and Zeiler, 1972; cf. Chapter 5). This prediction was tested in the present experiment.

Subjects

For the present study subjects were recruited from three age ranges that had been shown to produce (a) adult-like, (b) transitional and (c) animal-like behaviour in Experiment I.

Group One consisted of one boy and five girls between the ages of seven and eight and a half years, with a mean age of seven years and eleven months. In the previous studies, children in this age range had produced adult-like responding. Subjects in Group Two consisted of one girl and five boys between the ages of two and a half and four years, with a mean age of three years and six months. Subjects in this age range had previously been observed to produce responding transitional between that of animals and that of adults. The final group consisted of four infants SARA, SION, GEM and LYNDON, aged respectively 1 year, 1 month, 11 months; 6 months and 7 months at the beginning of the study. (The ages of the individual subjects are shown in Table 9.)

Each group was divided into two sub-groups with three subjects each in the case of the two oldest groups, and with two subjects each, in the case of the infants. One sub-group in each age range received a fixed ratio history of reinforcement, the other a DRL history.

Apparatus

The apparatus used in this study was identical to that used in Experiments I and II. For the oldest two groups a

Table 9: Ages of the individual subjects at the start of Experiment III (years and months).

Group	Subject	Age
1	SU	7,10
	JL	8,1
	AL	8,3
	DV	8,4
	MR	7,9
	AJ	7,6
2	JU	3,9
	PE	3,3
	TM	3,8
	SA	4,0
	DN	2,11
	VC	3,3
3	SION	0,11
	SARA	1,1
	GEM	0,6
	LYNDON	0,7

manipulandum was placed on a table in front of a screen and reinforcement, consisting of coloured lights, a small snack item, the presentation of music, a glove puppet and a cartoon slide projected on the screen, was scheduled by a microcomputer. In contrast to Experiments I and II, however, reinforcement was presented for five, not ten seconds.

The apparatus used with the infant subjects consisted of a portable microcomputer and manipulandum. The manipulandum was a touch sensitive bar in the case of GEM and SION, and a small sphere, sensitive to a force of 100 grammes force in the case of SARA and LYNDON. For all four subjects, reinforcement consisted of a series of flashing lights, five seconds of music and the presentation of a small toy.

Procedure

The initial procedure was identical to that used in Experiment I.

The subjects in the two oldest groups were introduced to the experimental room, usually in the company of a familiar adult. As in Experiment I, the subjects were shown the operation of the manipulandum and were then told, "Now you have a try." Continuous reinforcement was delivered for the next ten responses.

Subjects in the FR-history sub-groups were then exposed to reinforcement on fixed-ratio schedules. Some were exposed initially to an FR 6 schedule, followed by an FR 12 schedule; some were placed immediately on the FR 12 schedule. After responding on FR 12 was determined to be stable* by inspection of the cumulative records, the subjects were exposed to an FI 15 schedule for eight sessions without being informed of the change in the contingencies.

Following CRF, subjects in the DRL-history condition were placed immediately on a DRL 15 second schedule and, when responding had stabilised, were similarly exposed to a FI 15 second schedule for a further eight sessions. This proved to be sufficient to produce stable responding.

Sessions lasted for approximately ten to fifteen minutes, were usually carried out on consecutive weekdays, and were tape recorded in most instances. Prior to the change to the FI schedule, the subjects were asked, "What do you have to do to make Sooty work?" The question was repeated after the final session.

The infant subjects were tested in their homes.

* over three consecutive sessions

Responding in all four subjects was initially shaped, prior to one or more sessions of CRF.

The infants in the DRL-history sub-group were exposed to DRL schedules of increasing value until responding stabilised at DRL 15 seconds. The infants in the FR-history sub-group were similarly exposed to FR schedules of increasing value until responding was determined to be stable at FR 6. All subjects were then placed on a FI 15 second schedule. Unfortunately, because of the time taken to achieve stable responding in the first condition, the parents of GEM and SION would allow only six further sessions. SARA and LYNDON, on the other hand, received 16 and 19 sessions on FI 15 respectively. Sessions lasted from five to ten minutes each and were usually carried out on consecutive weekdays.

The total number of sessions in each condition for each subject are shown in Table 10.

RESULTS

(i) Schedule Performance

It is important to examine two aspects of the subjects'

Table 10: Design of Experiment III. (Session numbers shown in brackets.)

Group	Age range	Subjects	Sub-group	Conditions
1	7-8$\frac{1}{2}$ yrs	SU	FR-HIST	FR12 (5), FI15 (8)
		JL		FR12 (6), FI15 (8)
		AL		FR12 (5), FI15 (8)
		DV	DRL-HIST	DRL15 (11), FI15 (8)
		MR		DRL15 (12), FI15 (8)
		AJ		DRL15 (13), FI15 (8)
2	2$\frac{1}{2}$-4 yrs	JU	FR-HIST	FR12 (9), FI15 (8)
		PE		FR12 (9), FI15 (8)
		TM		FR12 (9), FI15 (8)
		SA	DRL-HIST	DRL15 (10), FI15 (8)
		DN		DRL15 (11), FI15 (8)
		VC		DRL15 (10), FI15 (8)
3	Infants	SION	FR-HIST	FR6 (12), FI15 (6)
		SARA		FR6 (12), FI15 (16)
		GEM	DRL-HIST	DRL15 (12), FI15 (6)
		LYNDON		DRL15 (19), FI15(19)

performances: first, the development and the final form of the responding on the FR and DRL schedules; secondly, the way in which performance on the FI 15 second schedule interacted with the age of the subjects and their histories of reinforcement.

Cumulative records taken during the development of the fixed-ratio responding of all the FR subjects are shown in Figure 24. With animal subjects on low FR values immediately following CRF, responding usually increases in rate for a short period, then decreases to a low-rate before the familiar break and run pattern develops (Ferster and Skinner, 1957). A stable break and run pattern of responding was acquired almost immediately by the three oldest subjects in the present study, although there was some variability between subjects in the rate of responding after the first response. Despite this difference from animal performance, however, a significant pause after reinforcement has not been regularly reported in the literature on the fixed-ratio responding of young or adult humans, which has usually been found to be high-rate and constant (Weiner, 1964b, 1964c, 1965b, 1970b; Zeiler and Kelley, 1969). The pause found in the present study, although similar in appearance to that found in animal research, probably reflects the nature of

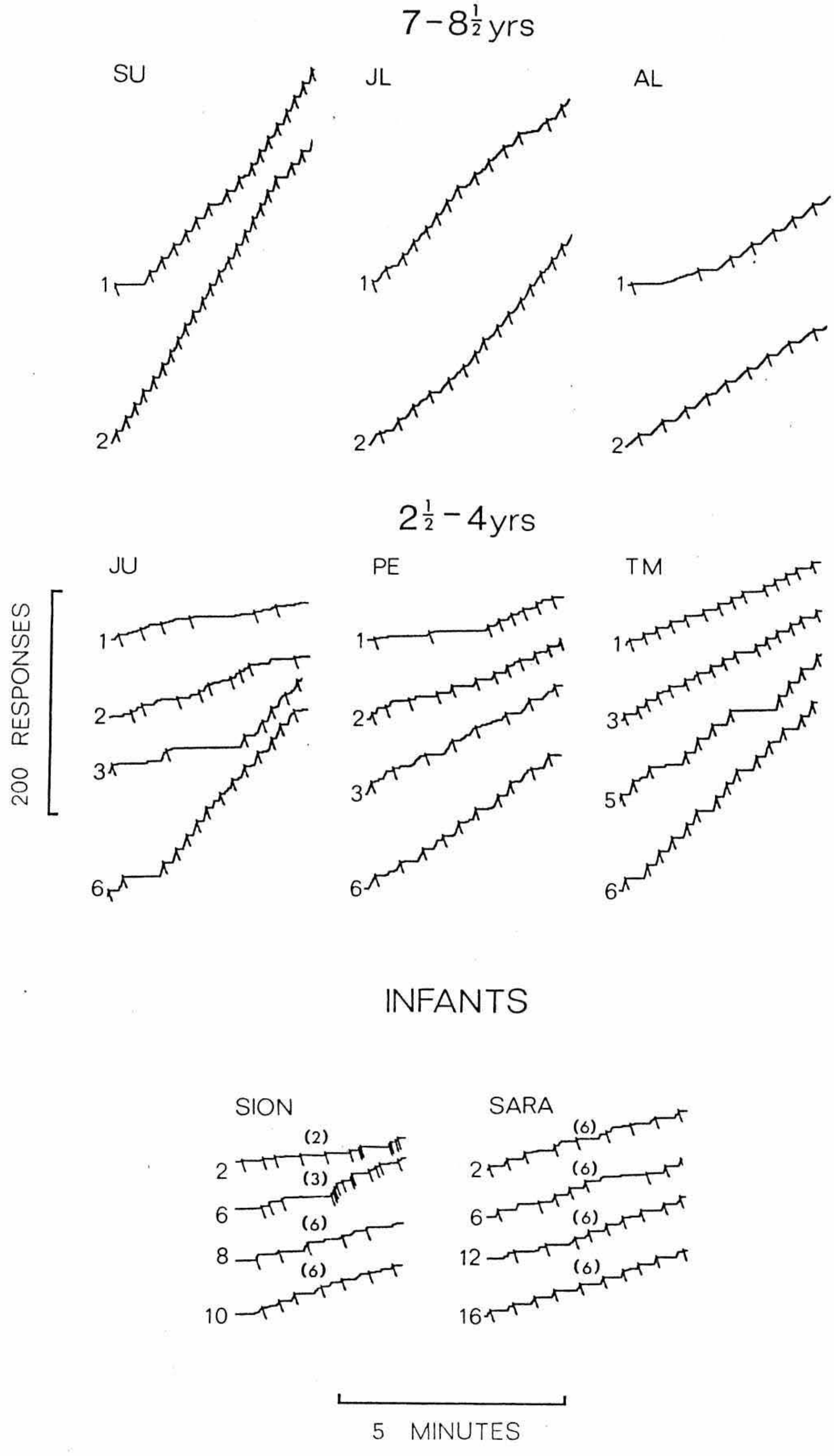


Figure 24: Cumulative records from initial sessions of FR-history subjects, Experiment III.

the reinforcer, which may have interupted responding. A similar short pause, previously unnoted in the human literature, was observed in the data from the one high-rate responder on FI in Experiment I, and from several of the high-rate instructed subjects in Experiment II. (Whether the duration of the pause would alter when the schedule value changed, as in animal studies, remains to be determined.)

A similar pattern of fixed-ratio development may be observed for Group Two, the two and a half to four year olds. Final performance consisted of a pause after reinforcement, followed by a relatively high and constant rate of responding until the next reinforcer became available. However, in the case of one of these subjects, JU, the development of the fixed-ratio pattern was less immediate and took place over several sessions.

The development of the fixed-ratio pattern of the two infant subjects, SION and SARA was more prolonged, particularly in the case of SION. Responding in the first FR sessions was generally at a higher rate than on CRF, but declined before the break and run pattern was established. In the case of SARA, however, the break and run pattern emerged earlier. In both cases the final patterns produced were comparable to those obtained in other studies of infant

FR responding (Weisberg and Fink, 1966; Sheppard, 1969).

Cumulative records from the development of the performance the DRL subjects are shown in Figure 25. In the case of the oldest subjects, a stable pattern of responding was slow to develop and the subjects persisted in responding relatively rapidly, despite a failure to obtain reinforcement. This was particularly the case for AJ, who, on one occasion, continued to press for more than ten minutes without receiving a reinforcer. Pauses in responding developed over a number of sessions, becoming more frequent. None the less, all three subjects continued to produce relatively high-rate bursts of responding after seven or more sessions.

The three subjects in Group Two acquired a relatively stable and low-rate pattern of responding more rapidly. This was particularly true for SA and VC. DN took longer to acquire a low-rate pattern. DRL responding has previously been obtained in this age range by Pouthas (Pouthas, 1981; Fraisse, 1982) although not all the subjects Pouthas tested obtained an efficient pattern of responding at DRL 15 seconds (cf. Chapter 5).

Cumulative records from the development of the DRL

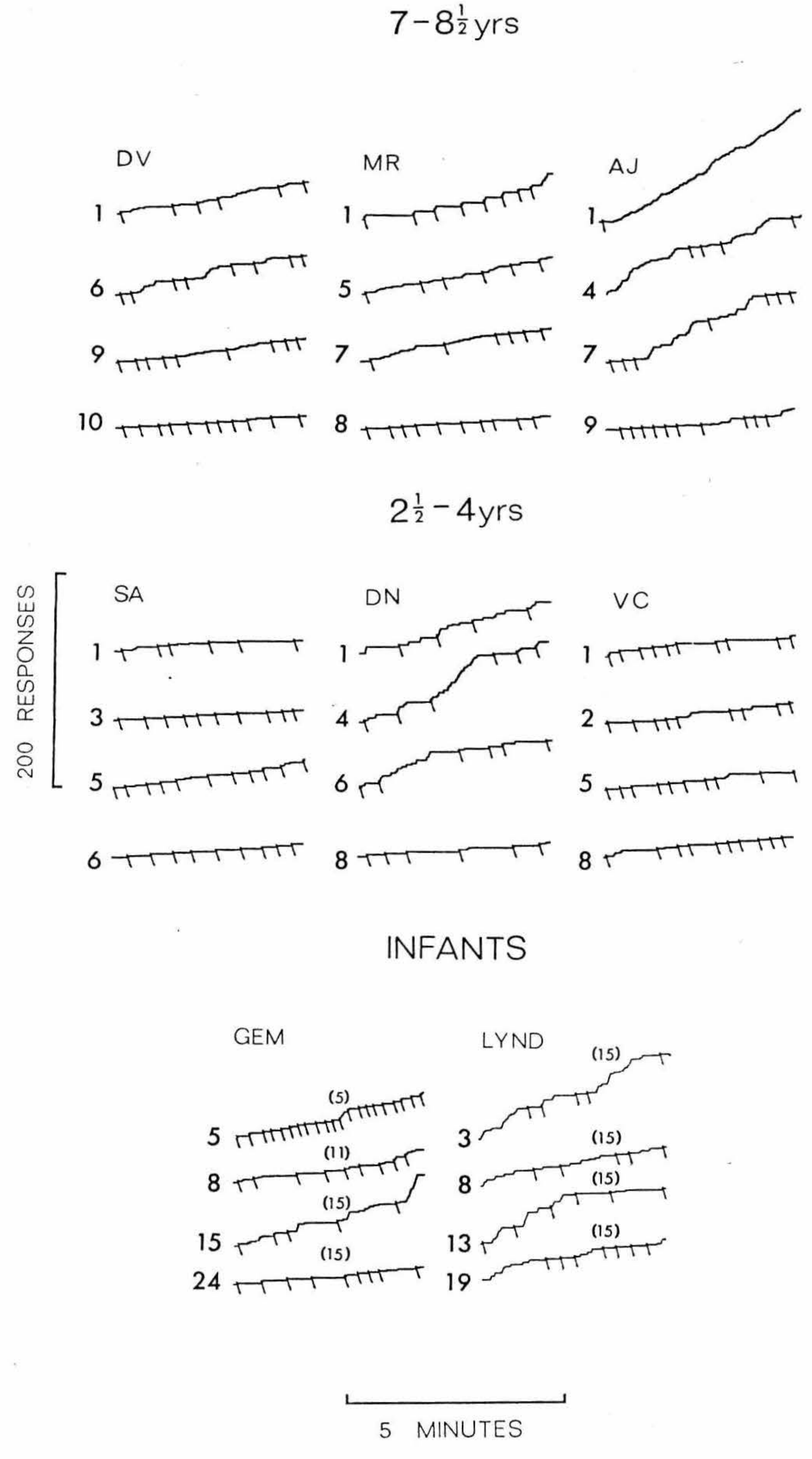


Figure 25: Cumulative records from initial sessions of DRL-history subjects, Experiment III.

responding of the two infant subjects are shown at the bottom of Figure 25. The development of a low-rate pattern in these subjects was slow and, in the case of GEM, particularly prolonged. In the records of both GEM and LYNDON short, rapid bursts or responding, just prior to the availability of reinforcement can be observed, a characteristic of the DRL responding of animals that has previously been observed with older infants by Weisberg and Tragakis (1967). This is in contrast to the bursts of responding that were observed in the development of the performances of the older children, which tended to occur after reinforcement.

Figure 26 shows cumulative records of the final performances on FR, DRL and FI schedules for all the subjects in the oldest group. The final FR performance of the FR history subjects shows the break and run pattern described above. In marked contrast, the final DRL performance of the DRL history subjects is low-rate, consisting, with an occasional exception, of a pause of greater length than the interval value, followed by just one response to obtain reinforcement, again as described above.

It is immediately obvious that the pattern of

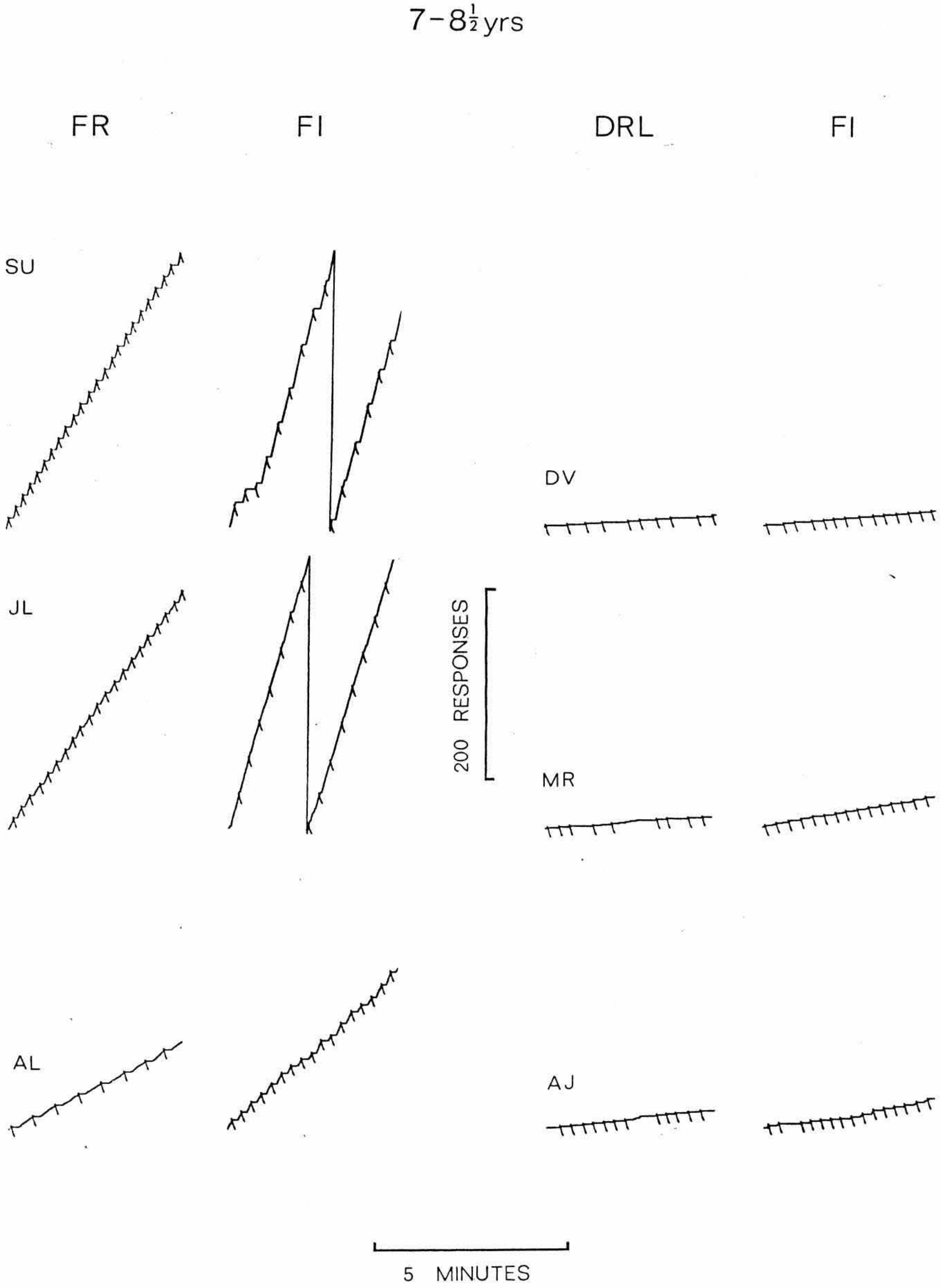


Figure 26: Final cumulative records on FR, DRL and FI schedules for FR-history and DRL-history subjects in Group One, Experiment III.

responding produced by each subject on the FI schedule reflects the pattern established during the initial phase of training on either the DRL or the FR schedule. Those subjects with experience on the FR schedule have produced a high-rate pattern of responding, comparable with the high-rate patterns produced by adult subjects on FI schedules after a FR history of reinforcement (Weiner, 1965a, 1969, 1970b) and unreported in animals. The rate of responding of each of the subjects also seems to reflect previous responding on the FR scedule, AL having responded at a noticably lower overall rate than either SU or JL. It is interesting to note that, in all three subjects the postreinforcement pause seems to have declined, especially in the case of JL, where it is hardly apparent at all.

The three DRL history subjects all responded at a low rate on the FI schedule. For the most part, only one response was emitted during each interval, although occasionally, and particularly in the case of AJ, a short, high-rate burst of responding occured just prior to the availability of reinforcement and continued until reinforcement was delivered. These results are comparable to the behaviour observed from adult subjects on FI schedules, after a history of DRL reinforcement (Weiner, 1965a, 1969, 1970b) and markedly different to that of animals in similar circumstances (Morse and Skinner, 1958; Urbain, Poling, Millam and Thompson, 1978) which produce a scalloped pattern of responding soon after being changed to FI.

All of the FR subjects in this group reported high-rate formulations of the contingencies, both prior to the change to the FI schedule and after the end of the experiment. Thus, when SU was questioned:

E: ´What do you have to do to make Sooty work?´
SU:´I press the lever.´
E: ´Do you press it in any particular way?´
SU:´You have to press it very fast.´

Similarly, JL reported that it was necessary to ´Press the lever quickly´. AL, on the other hand, was much more explicit, saying, ´I have to press twelve times,´ when first questioned. When questioned at the end of the experiment, AL said, ´I don´t know how many times I have to press now. I think it´s five or six.´

When questioned about their strategies, two of the DRL subjects, MR and AJ were unwilling to verbalise the contingency, despite considerable questioning. However, one of these subjects, AJ, was observed to count out the DRL interval on several occasions (see below). DV reported that he waited after reinforcement, a strategy he also reported when questioned after his last session on FI:

E: ´What else do you do besides pressing the lever, Dave?´
DV:´Well I wait a bit afterwards.´

The final patterns produced by Group Two on the three schedules are shown in Figure 27. The final FR performances of the FR history subjects are similar to those of the oldest group. However, the the cumulative records have a slightly more ´rough grain´ appearance. Similarly, the DRL records of the DRL history subjects are low-rate, but show greater variability in the spacing of responses than those of the comparable subjects in the older group.

It can be seen that, as for the older subjects, the final pattern produced by each subject on the FI schedule appears to reflect their initial schedule experience, but the effects of reinforcement history have been more variable. Although the performance of the FR history

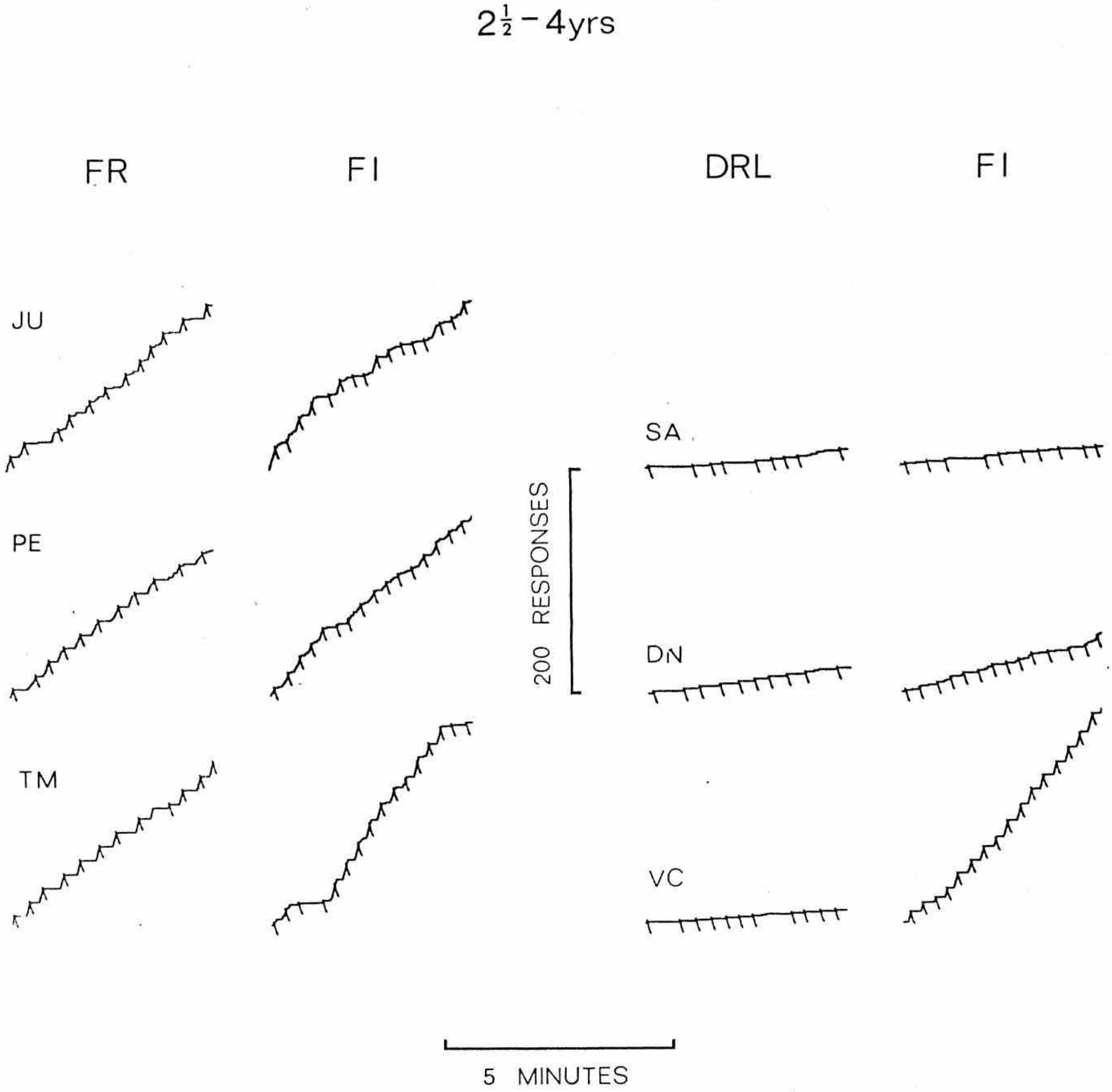


Figure 27: Final cumulative records on FR, DRL and FI schedules for FR-history and DRL-history subjects in Group Two, Experiment III.

subjects on FI was generally high-rate, there is some variability in their patterns of responding. These results are comparable to those of Zeiler and Kelley (1969) who worked with slightly older children (4-5 year olds) and found a maintenance of high-rate responding on changing to FI but some variability in response patterning. It may be recalled that the high-rate responding of instructed two and a half to four year old subjects in Experiment II showed similar variability.

Of the DRL history subjects in this age range, two produced a low-rate pattern of responding on FI. In both cases, despite continued variability in the spacing of responses, the final patterns were comparable to those of older DRL history subjects. For the final subject in this group, VC, there is no indication of persistence of the low-rate pattern established during the DRL phase. On the contrary, following a pause after reinforcement, responding occurred at a constant high-rate and the resultant pattern is similar to that produced by the FR history subjects in the oldest group but dissimilar to that produced by animal subjects on fixed-interval schedules.

In general, then, it can be seen that exposure to a previous schedule does seem to have had an effect on the performance of five out of six of the subjects in the two and a half to four years age range. In the case of both the FR history subjects and the DRL history subjects in this group, their final performance on FI was very different to the final performance of comparable subjects in Experiment I who had been exposed to fixed-interval reinforcement without prior experience of other schedules. However, as was the case with those subjects in Experiment I, none of the two and a half to four year olds in the present experiment produced a formulation of the contingencies.

The final performances of the infant group on the schedules of reinforcement are shown in Figure 28. In the case of both FR history subjects, SARA and SION, final performance on FR was in the form of a break and run pattern, similar to that found in animals and, as mentioned above, in previous studies of infant FR performance (Weisberg and Fink, 1966; Sheppard, 1969). The final DRL performance of the DRL history infants was also comparable to that of animals, showing long pauses and occasional bursts of responding, just prior to the availability of reinforcement. Although this data is comparable to that reported by Weisberg and Tragakis (1967), the subjects in

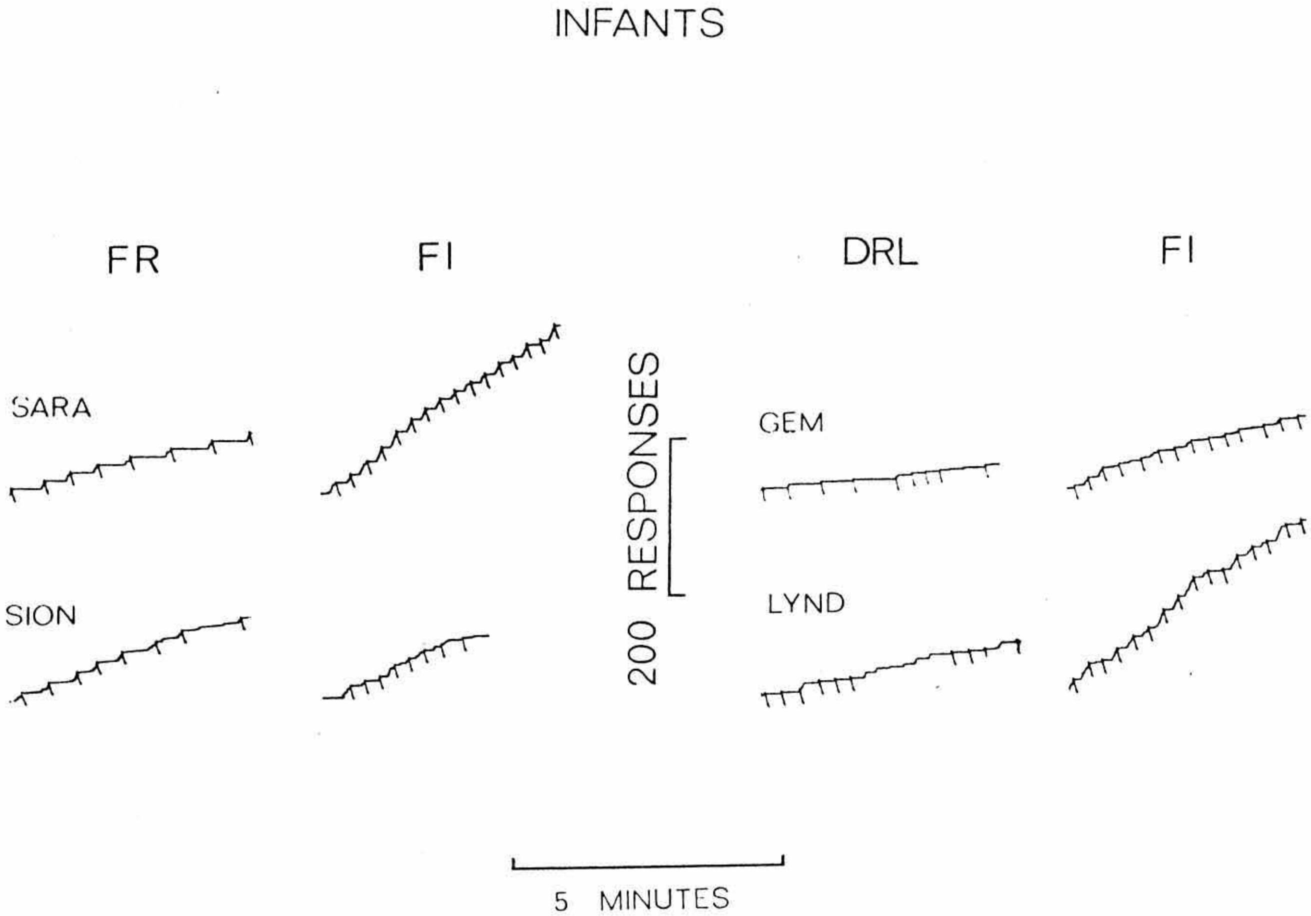


Figure 28: Final cumulative records on FR, DRL and FI schedules for FR-history and DRL-history infant subjects, Experiment III.

the present study are considerably younger than any previously studied on DRL schedules.

On changing to FI schedules, none of the subjects show a maintainance of the same pattern of responding. In the case of SARA, her pause after reinforcement declined, her terminal rate of responding decreased, producing an overall increase in rate, and her transition to a terminal rate of responding following the post-reinforcement pause became less abrupt. In the case of SION, despite the small number of sessions in which he was exposed to FI contingencies, the break and run pattern was no longer produced and scalloping began to develop. Similarly, the final performance on FI of both the DRL history infants shows the pause after reinforcement, followed by a positively accelerated rate of responding, as commonly found in animal subjects on FI schedules.

In general then, the cumulative records for the infant subjects indicate no evidence of a history effect due to previous schedule experience other than the transient effects on rate observed in animal subjects (Urbain, Poling, Millam and Thompson, 1978). In all cases there is a clear development of a scalloped FI performance, similar to that found in animals.

Further information about the presence or absence of behavioural rigidity in the subjects in the present experiment can be obtained from a number of measures. Figure 29 shows the mean postreinforcement pauses of all the subjects in all the conditions. Mean postreinforcement pauses and their standard deviations are also given in Table 11. It can be seen that, following the change to the FI schedule, the PRPs of all the the subjects dropped. However, a number of differences between the groups can be discerned. In the case of the older subjects, the mean postreinforcement pauses of the DRL history subjects on the DRL schedule all exceeded the mean pauses of the corresponding subjects on FR. Despite the drop in mean pause length on changing to FI, this difference persisted, the FI pauses of the DRL history subjects all exceeding those of the FR history subjects. In the case of two of the DRL history subjects, the mean postreinforcement pause on FI still exceeded the schedule value; in the case of the third, MR, it was just less than the schedule value. In contrast, the mean postreinforcement pauses on FI of all the FR history subjects were less than the schedule value.

For the subjects in the two and a half to four year age

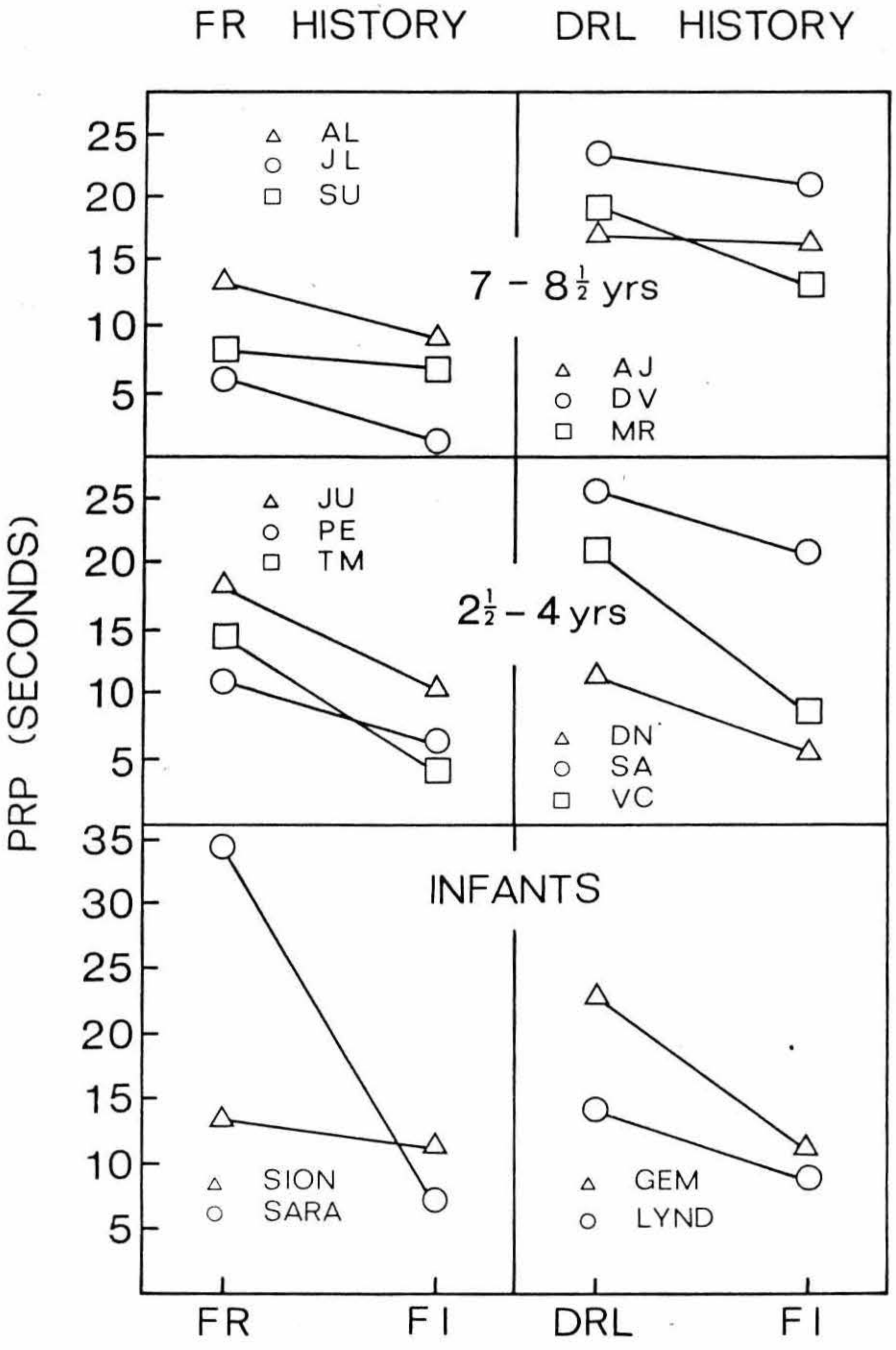


Figure 29: Mean PRPs on FR, DRL and FI schedules for FR-history and DRL-history subjects, Experiment III.

Table 11: Mean PRPs and standard deviations for subjects in Experiment III (seconds).

Group	Subject	Condition	PRP	SD
1	SU	FR12	8.15	3.95
		FI15	7.01	3.32
	JL	FR12	6.02	1.50
		FI15	1.39	0.46
	AL	FR12	13.25	5.23
		FI15	8.95	1.95
	DV	DRL15	23.47	9.78
		FI15	21.80	5.93
	MR	DRL15	19.21	11.23
		FI15	13.21	5.77
	AJ	DRL15	16.32	5.30
		FI15	17.64	4.62
2	JU	FR12	11.10	7.52
		FI15	6.02	10.46
	PE	FR12	14.15	7.04
		FI15	4.15	3.32
	TM	FR12	18.25	11.24
		FI15	10.28	8.50
	SA	DRL15	25.49	13.97
		FI15	20.85	14.12
	DN	DRL15	11.57	13.01
		FI15	5.60	7.09
	VC	DRL15	20.81	11.99
		FI15	8.36	5.02
3	SION	FR6	13.61	10.15
		FI15	11.78	13.69
	SARA	FR6	34.52	8.62
		FI15	7.22	4.78
	GEM	DRL15	23.03	19.68
		FI15	11.59	7.14
	LYNDON	DRL15	14.33	5.92
		FI15	8.52	4.07

range a different picture emerges and no clear distinction can be seen in the pause lengths on FI. Although two of the DRL history subjects, SA and VC, paused on DRL for a period exceeding fifteen seconds and the third, DN, paused for a mean length of 11.5 seconds, only in the case of SA was that length of pausing maintained on FI. In the case of VC, her mean postreinforcement pause dropped dramatically as she assumed her high-rate pattern of responding on FI. The mean pause length of all three subjects on FR was close to fifteen seconds, but dropped to a similarly low value on exposure to FI. In the case of these subjects, however, this decline in pausing was consistent with that of their counterparts in Group One. These results suggest that, particularly in the case of the DRL history subjects, the history effect observed in the two and a half to four year olds is weaker than the history effect observed in the older subjects.

All of the infant subjects paused for a mean length of less than fifteen seconds during the final three sessions of FI, irrespective of conditioning history. These data, then, suggest that conditioning history had little effect on the final pause duration of these subjects on FI.

Figure 30 shows the mean overall response rates of all

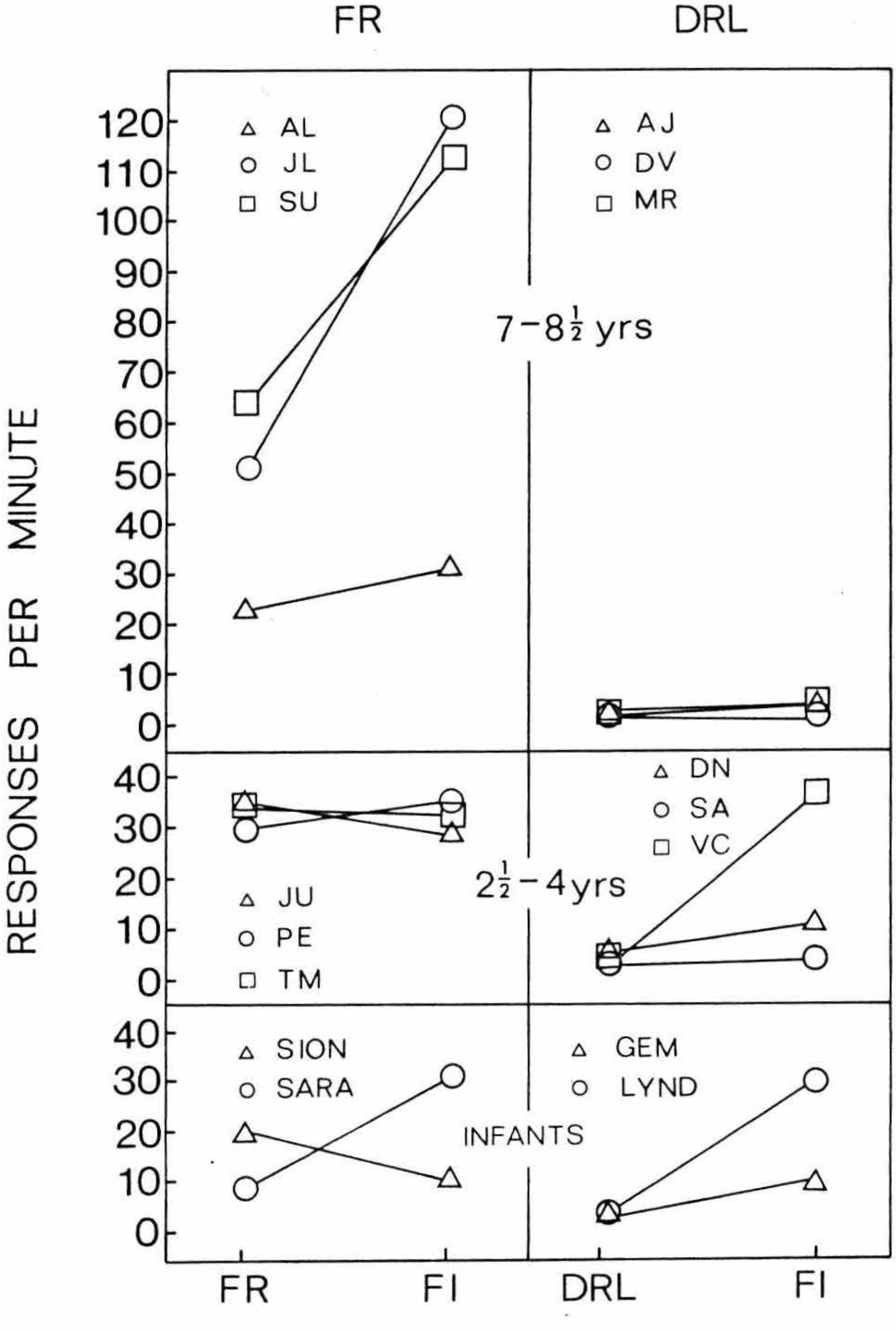


Figure 30: Mean response rates on FR, DRL and FI schedules for FR-history and DRL-history subjects, Experiment III.

the subjects in all the conditions. It can be seen that the overall rate of responding of the oldest FR history subjects actually increased on transfer to FI. In the case of the DRL history subjects in this age group, the overall rate of responding on FI was similar to that on DRL.

In the case of the FR history subjects in the two and a half to four year age group, rate of responding on FR and FI was almost identical. A similar carry over of rate from one condition to the next was observed for the two DRL history subjects who produced a low-rate pattern on FI. In the case of the third DRL history subject, VC, however, the overall rate on FI increased to a level comparable with that of the FR history subjects. These results support the view that a history effect exists for the subjects in this age range, with the exception of VC.

The overall rate data for the DRL history infants confirms the absence of behavioural rigidity already observed with respect to other measures. On DRL, these infants responded at a relatively low overall rate, but the rate increased on change to FI.[*]

Figure 31 shows the running rate data. It can be seen that, as was the case with overall rates, the running rates of the oldest FR history subjects increased on FI. However,

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[*] It is difficult to make specific predictions about overall rate for subjects changing from FR to FI

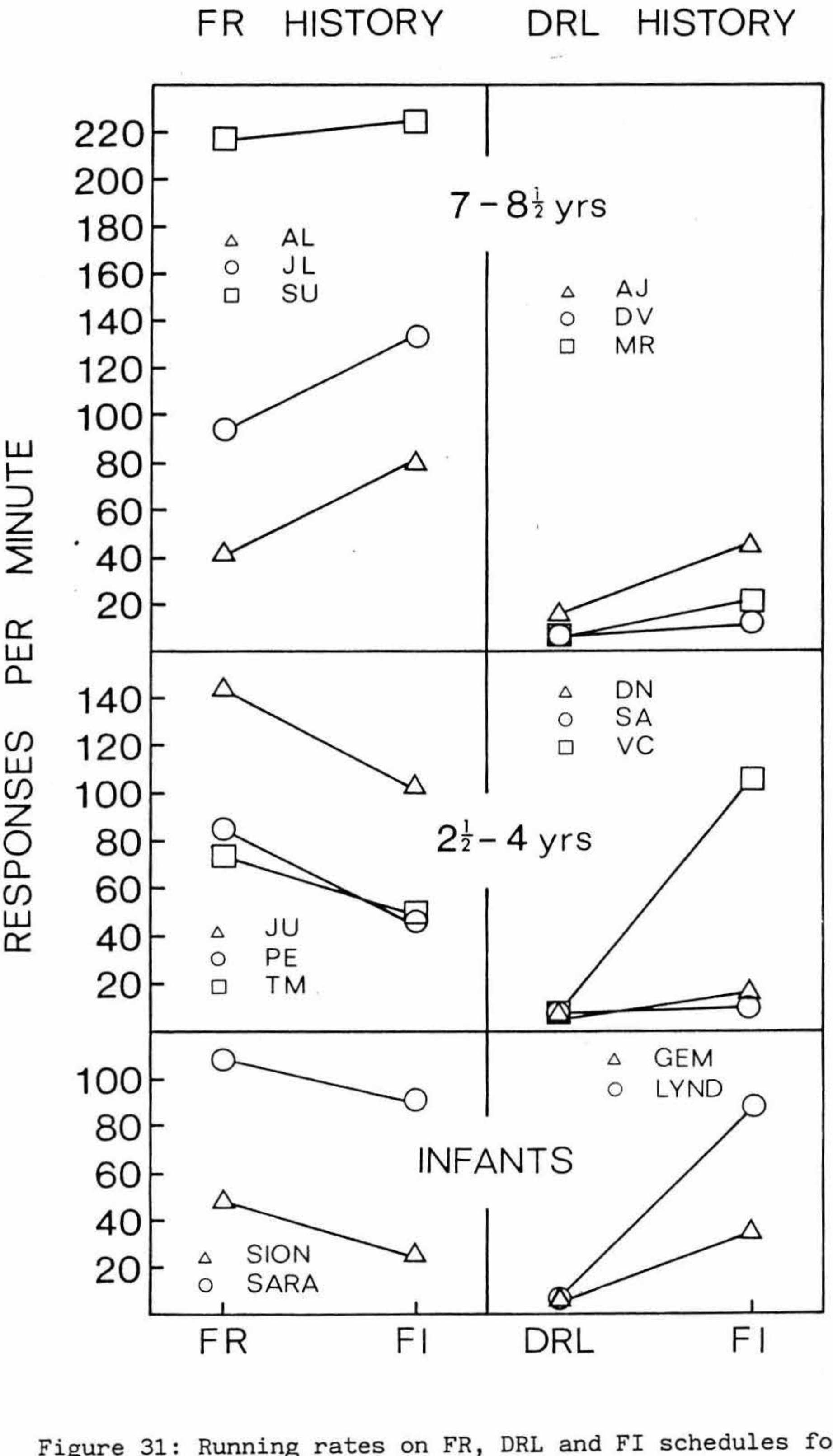

Figure 31: Running rates on FR, DRL and FI schedules for FR-history and DRL-history subjects, Experiment III.

the running rates of the DRL history subjects in the oldest group increased only slightly on changing to FI, confirming once again the strong behavioural rigidity observed in these subjects.

In the case of the two and a half to four year age group, the running rates of the FR history subjects dropped on transfer to FI, but still remained high in comparison with those of the two DRL history subjects who maintained a low-rate pattern on FI. Once again, the performance of VC on FI appears similar to that of the FR history subjects in this and the oldest group.

Finally, in the case of the infant subjects, the running rates changed in directions that indicate a lack of behavioural rigidity. Those of the two FR history subjects, SION and SARA, dropped on changing to FI whereas those of the two DRL subjects increased to a final comparable level.

Further information concerning response patterning can be obtained by looking at the mean length of successive IRTs, plotted against their ordinal position in the interval. Figure 32 gives such data for the oldest group. The FR history subjects are shown in the uppermost and the DRL history subjects on the lower panels. Looking at the FR

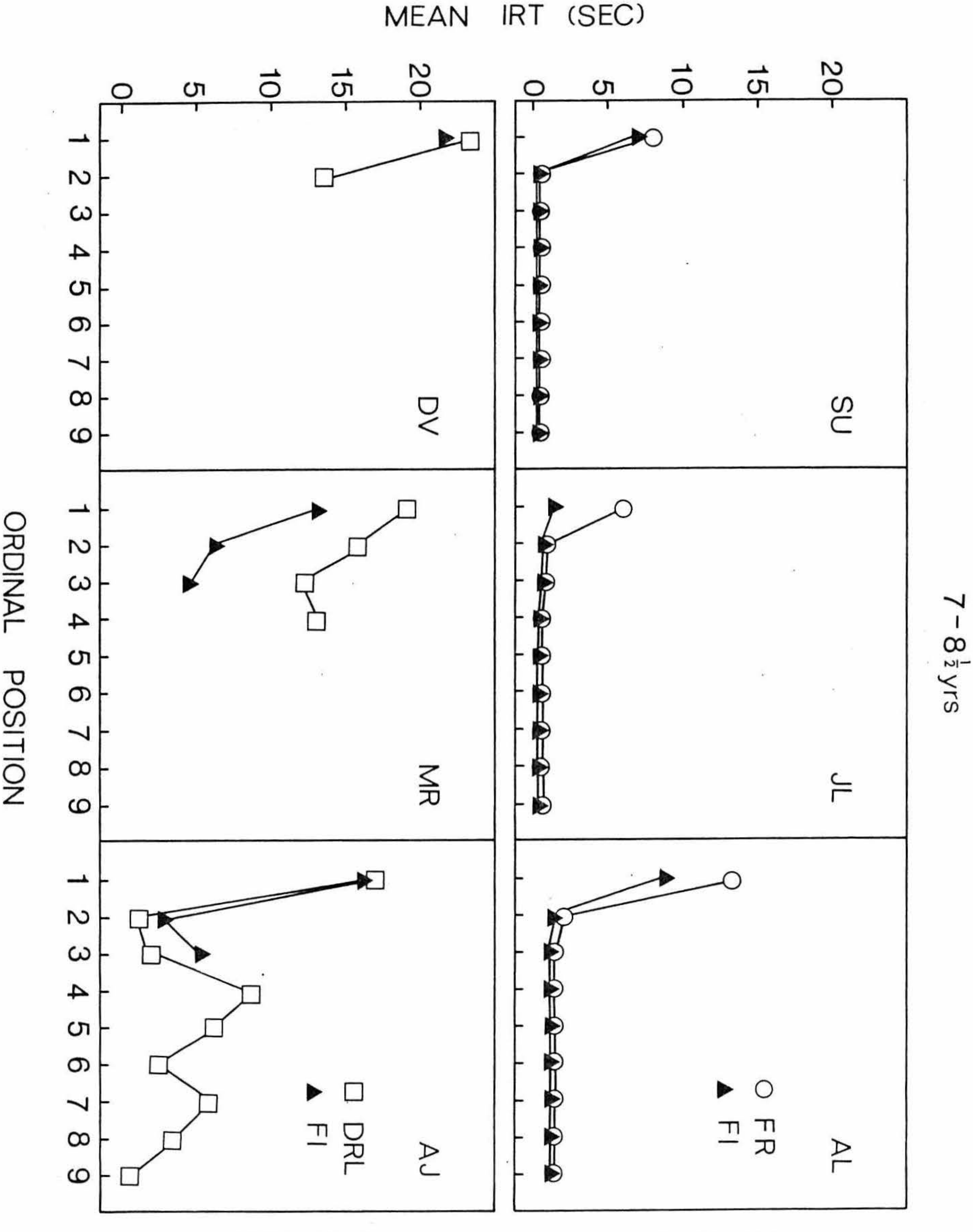

Figure 32: Successive mean IRTs plotted against their ordinal position in the interval for Group One subjects on FR, DRL and FI schedules, Experiment III.

history subjects first, there are very pronounced history effects. Successive IRTs are of very short duration after the initial post-reinforcement pause in both the FR and the FI conditions.

In the case of the DRL history subjects, two of the subjects, DV and MR, show few points, all of which are around the interval value, showing that most responses produced reinforcement on both schedules. The third subject, AJ, usually obtained reinforcement with her first response, but occasionally mistimed it, leading to further attempts of varying duration, or, as previously noted, the occasional response burst.

Figure 33 shows the mean ordinal IRTs of the two and a half to four year old group. The patterns of the FR history subjects are similar to those of the comparable subjects in the oldest group on both FR and FI, confirming again the presence of a history effect. In the case of the three DRL history subjects, there is evidence of spaced responding on DRL. In the case of both SA and DN, this pattern seems to persist on FI, although in the case of VC the final FI pattern is similar to that of the FR history subjects in this and the oldest groups.

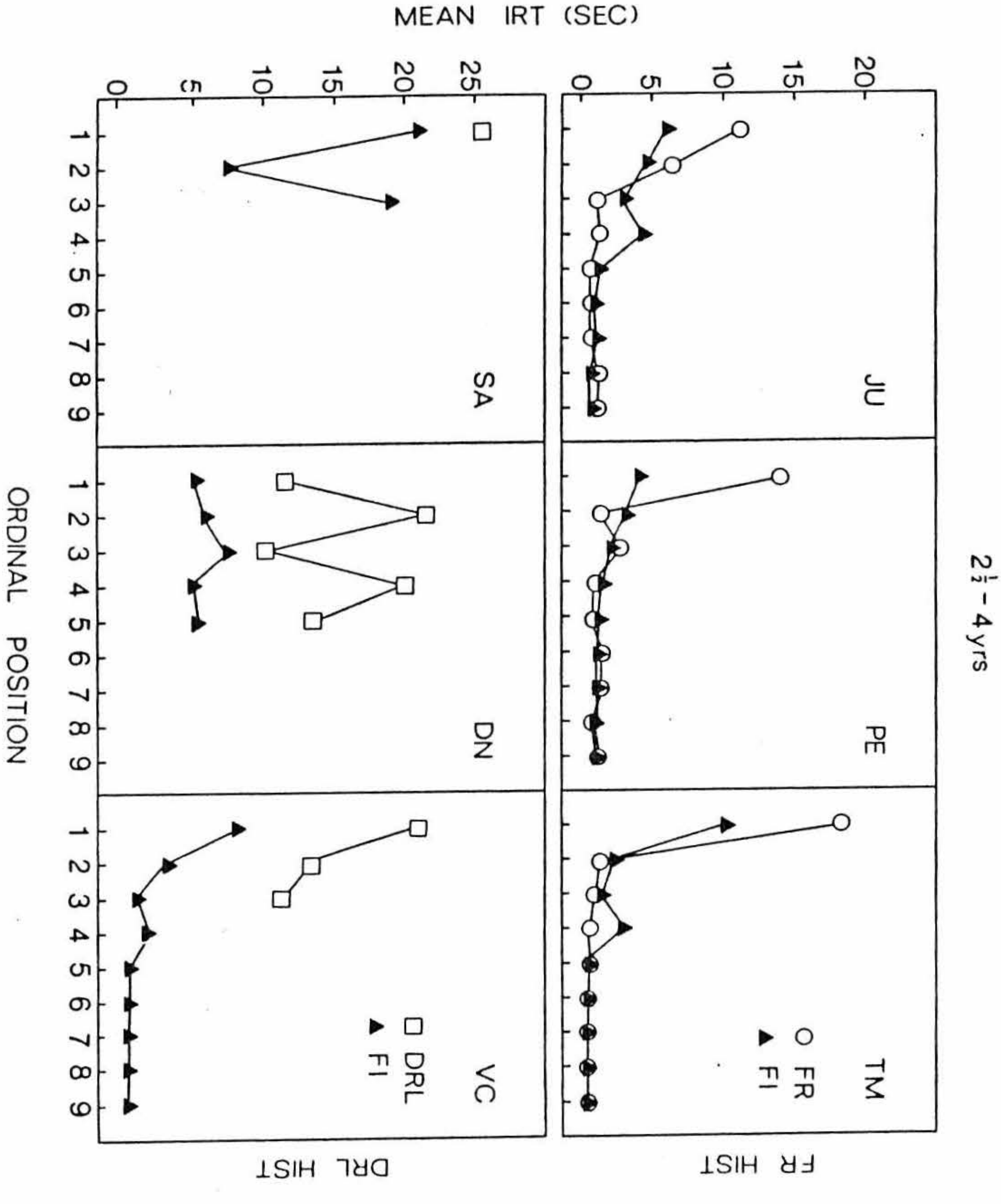

Figure 33: Successive mean IRTs plotted against their ordinal position in the interval for Group Two subjects on FR, DRL and FI schedules, Experiment III.

In figure 34 we can see the ordinal IRT data for the infant subjects. Both SION and SARA show evidence of break and run patterns on FR, and both GEMA and LYNDON show evidence of long pauses on DRL. On changing to FI schedules, all the subjects show a decelerating reduction in successive IRT lengths, as found in animal species.

A final indication of response patterning is given by plotting the proportion of IRTs falling within ranges of increasing duration. Figure 35 shows the proportion of IRTs falling in successive three second bins for all the subjects in the oldest group, and for all conditions. Thus, in any one panel, the first bar shows the proportion of IRTs of three seconds duration or less for that particular subject in that particular condition; the second bar shows the proportion of the total IRTs of between three and six seconds in duration, and so on. The data for the FR history subjects is on the left and the data for the FI history subjects is on the right. Bins longer than fifteen seconds are shaded.

Taking the FR history subjects first, in the FR condition nearly all the IRTs are less than three seconds, a result maintained in the FI condition. The results for the

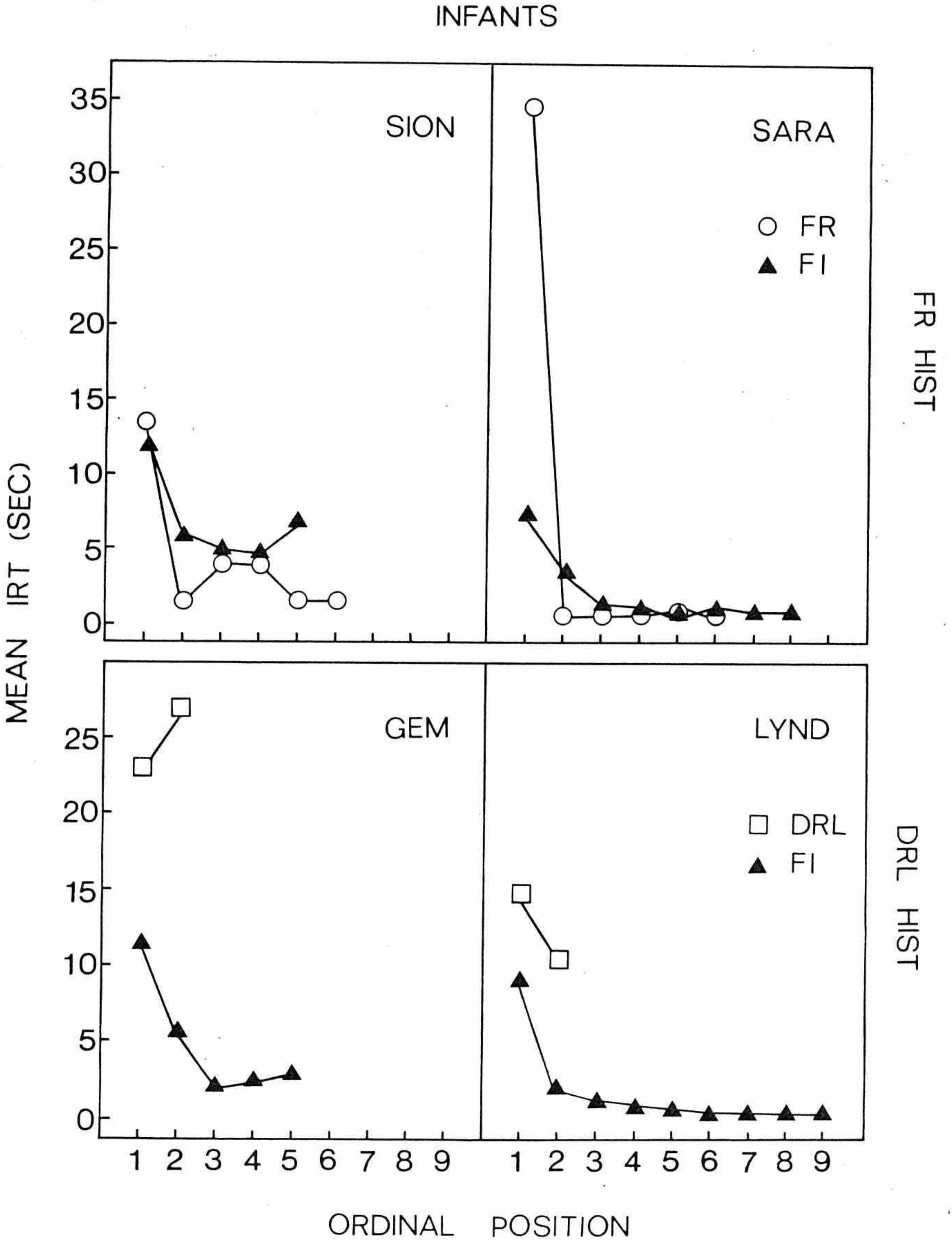

Figure 34: Successive mean IRTs plotted against their ordinal position in the interval for infant subjects on FR, DRL and FI schedules, Experiment III.

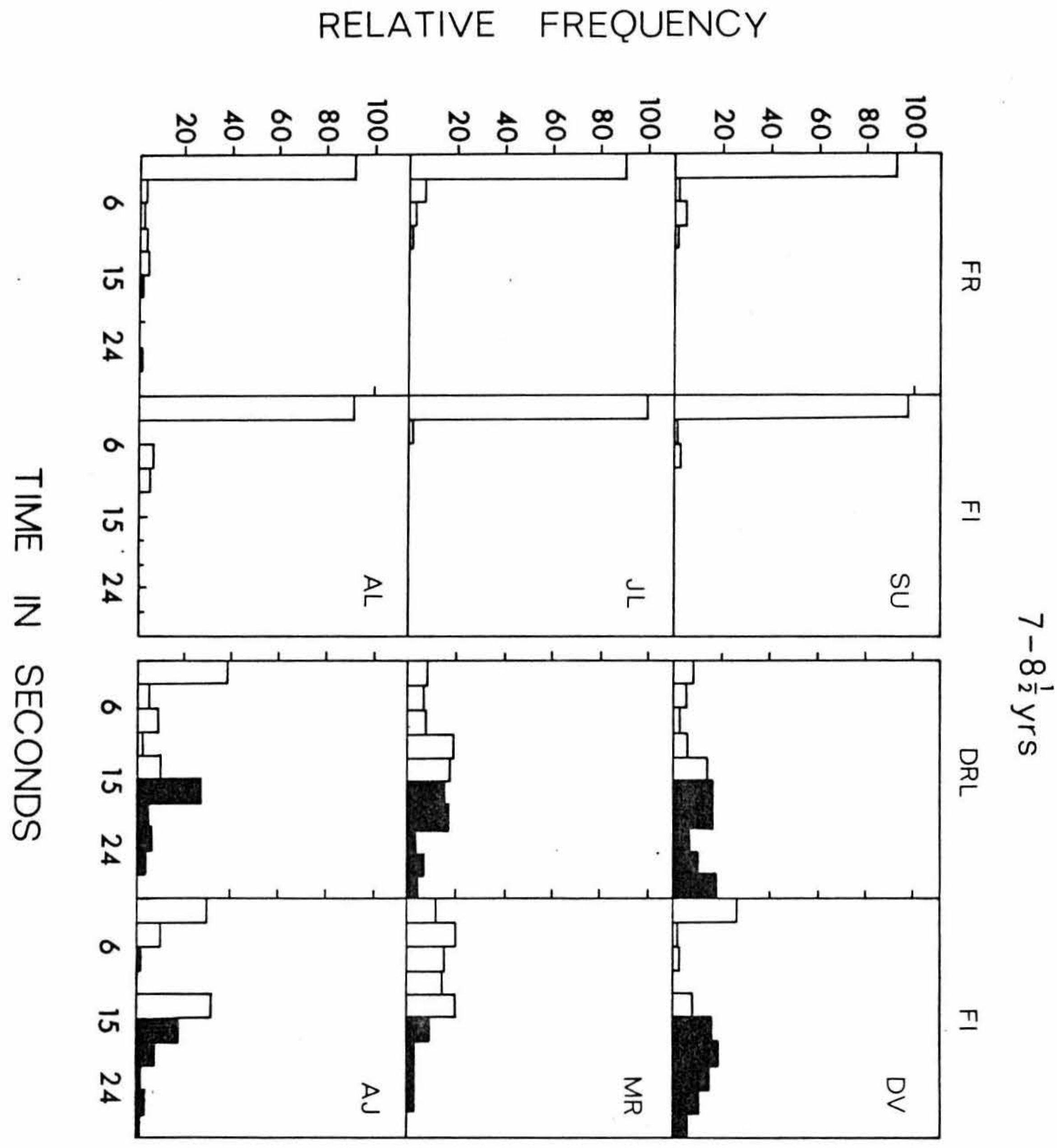

Figure 35: Proportion of IRTs of increasing duration for subjects in Group One on FR, DRL and FI schedules, Experiment III.

DRL history subjects are a little more complex. With animals on DRL, this kind of analysis typically produces a bimodal distribution, with peaks occuring for IRTs of short duration and for IRTs just greater than the schedule value. Looking at the DRL data for the DRL history subjects, we can see that only one subject, AJ, produced a bimodal distribution similar to that found in animals. The other two subjects, DV and MR, show distributions in which most IRTs are of long duration, and very few are of short duration. This data supports the view that the DRL performance of verbally able humans, although apparently similar to that of animals, is in fact under the control of different processes to those found in infrahuman species.

On changing to FI, we can see a general maintenance of a high proportion of long IRTs, but also, in the case of DV and MR, a slight increase in IRTs of short duration.

Figure 36 shows comparable data for the two and a half to four year old group. Again, the FR history subjects on FR produced IRTs of short duration and, for the most part, this is maintained in the FI condition. Of the DRL history subjects on DRL, two, SA and VC, show bimodal distributions, whereas the third, DV, shows less evidence of temporal discrimination. Turning to the FI data for these subjects,

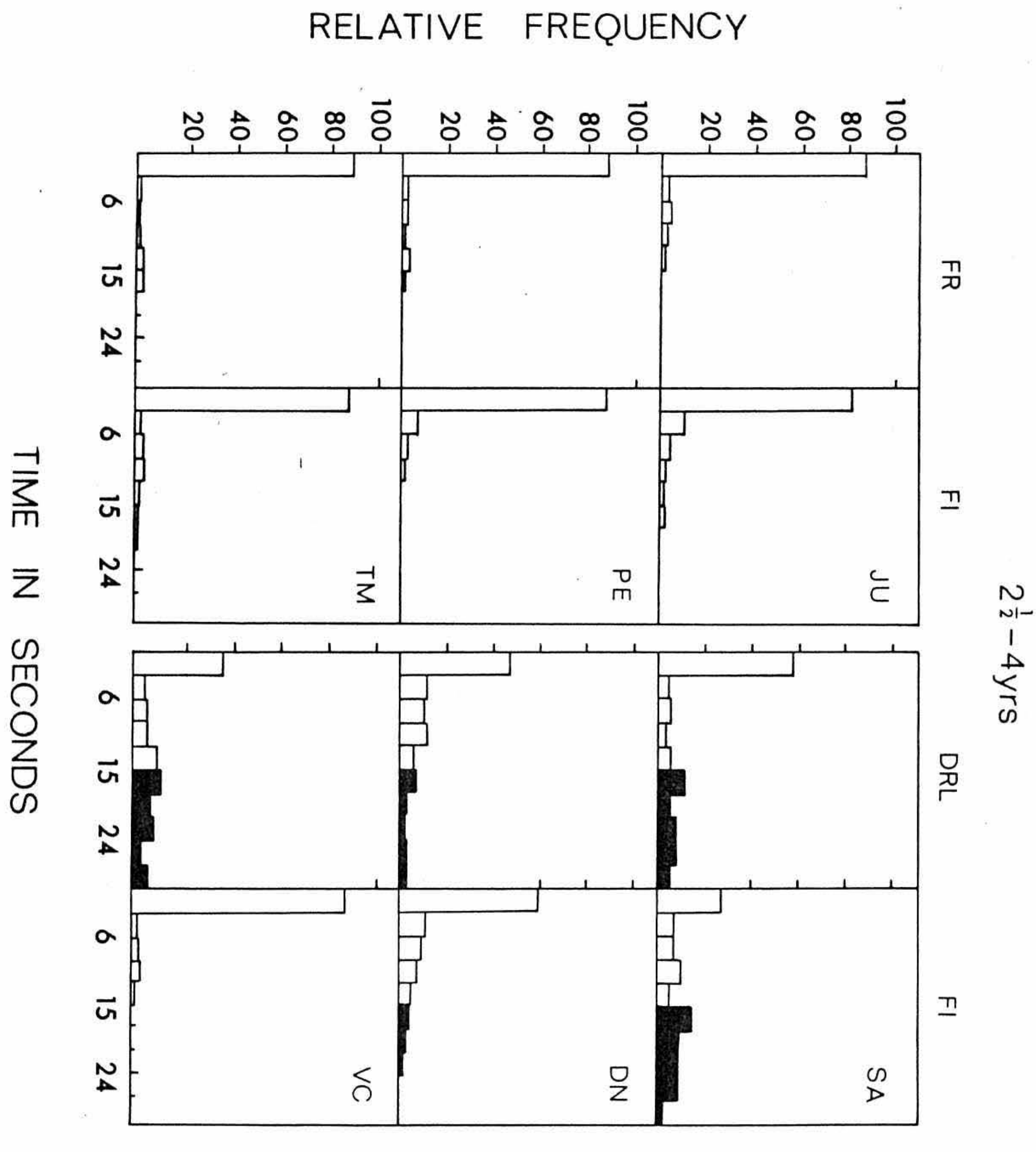

Figure 36: Proportion of IRTs of increasing duration for subjects in Group Two on FR, DRL and FI schedules, Experiment III.

we can see a maintenance of a bimodal distribution in SA´s case only. DN´s distribution, though not bimodal, remains similar to his distribution on DRL. In the case of VC, on the other hand, her behaviour clearly altered qualitatively on changing to FI. We have already noted that her final pause, rate and running rate data was similar to that produced by the FR history subjects in both her own and the oldest age groups. We may also note that such a sudden change of response patterning from low-rate to high-rate has not been observed in animal species in similar conditions. This therefore suggests that only a change in hypothesis about the contingencies can account for VC´s performance on FI.

The final figure, Figure 37, shows data for the infants. Both the FR infants produced most of their IRTs in the short range, as expected. On changing to FI, there is evidence of an increase in the proportion of longer range IRTs, particularly in the case of SION.

Both DRL babies produced bimodal distributions on DRL, similar in all respects to those of animals. To the writer´s knowledge, data of this sort has not been published for infants before. There is no evidence of these distributions

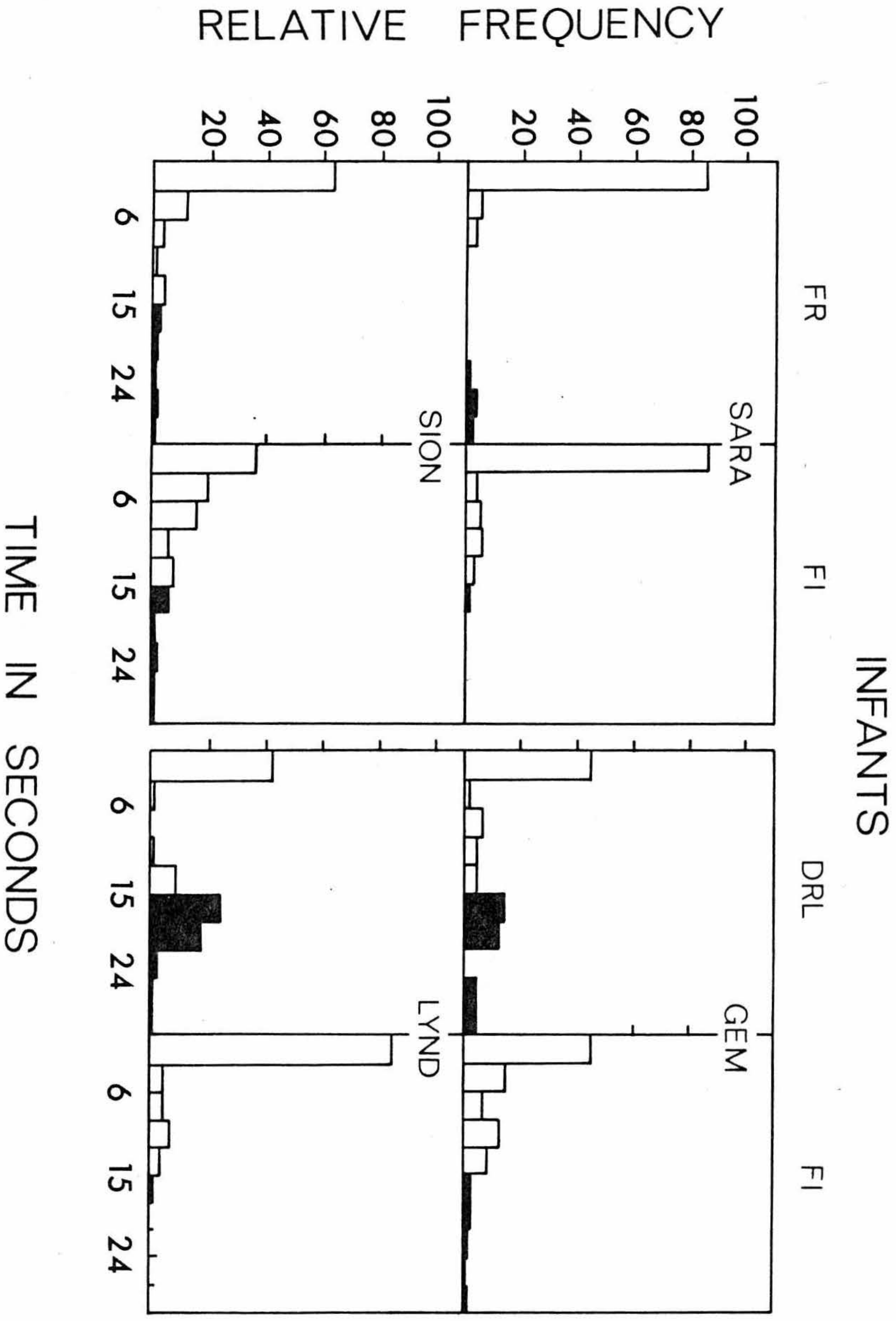

Figure 37: Proportion of IRTs of increasing duration for infant subjects on FR,DRL and FI schedules, Experiment III.

being maintained on FI, and thus no evidence of behavioural rigidity.

(ii) Other Behaviour During the Experiment

Very little verbal behaviour was recorded during the experiment. The only exception to this in the oldest group was AJ who, as already noted, counted out the interval on a number of occasions. Two of the subjects in the two and a half to four year group, VC and DN, occasionally squealed or emitted other noises during testing but otherwise the subjects in this age range also produced no speech.

A certain amount of interesting non-verbal behaviour was, however, observed in the case of the two and a half to four year old subjects on DRL. Two of these subjects, again VC and DN, developed stereotyped patterns of running around the experimental room between lever presses. Similar behaviour was observed by Pouthas (1981) with her youngest subjects on DRL.

DISCUSSION

The results of this study suggest that, in accordance with the verbal regulation hypothesis, behavioural rigidity is a feature of the operant behaviour of children above the age of two and a half years whereas the performance of infants on fixed-interval schedules reflects previous schedule experience only to an extent comparable to that observed in the animal literature. In addition, the FR and DRL responding of the infants was found to be similar to that of animal subjects. These findings complement the results of Experiments I and II, in which it was demonstrated that only children above the age of five years behave like adults on fixed-interval schedules of reinforcement when uninstructed but that, within certain conditions, children between two and a half and five years of age can be led to produce adult-like responding by the use of instructions and self-instructions.

The results of this experiment contrast with those of Pouthas (1983), who argued that young children may not be able to adapt to DRL schedules because they lack time concepts. The conceptual problems associated with Pouthas' position have already been discussed in earlier chapters. In addition to the present study, the results of Weisberg and Tragakis (1967) suggest that children too young to use time concepts may adapt to DRL schedules. Other evidence also provides support for the idea that young children may be able to learn to space their responding in time. Fitzgerald and Brackbill (1976), in a review of the classical conditioning literature, found that temporal conditioning could be successfully carried out with very young infants, and E. Friedman, in an unpublished study reported by W. Friedman (1978), found that two year olds could be taught to press a lever at fifteen second intervals by means of modelling and feedback.

The finding that behavioural rigidity increases with age may seem paradoxical as it suggests that, in some circumstances at least, infants can be more flexible than adults. Perhaps this is what Pavlov (1941) had in mind when he argued that: "On the one hand, numerous speech stimulations have removed us from reality, and we must always remember this in order not to distort our attitude to reality. On the other hand, it is precisely speech which has made us human...." (Cf. Chapter 3.)

Behavioural rigidity has previously been noted by psychologists working in traditions different from operant research. Luchins and Luchins (1950), for example, noted a ´deleterious effect´ due to the ´binding effects of habit´ in human problem solving. Wason (1960) subsequently observed that human adults, when solving complex logical problems, often failed to seek disconfirming evidence for their hypotheses, preferring instead to seek out information that confirmed their beliefs. This finding suggests a reason why a hypothesis formulated on one schedule of reinforcement will continue to be employed on a second schedule (unless a marked loss of reinforcement occurs as a result).

It may be that, in everyday life, persistence with tried and tested hypotheses is often adaptive in that it leads to a rapid solution of similar problems. However, within clinical psychology, the role of irrational beliefs in psychopathology has recently been emphasised by Ellis (1962) and Beck (1976). In the light of this it is perhaps important to note that persistence with maladaptive hypotheses need not always be a characteristic of human behaviour. Both Ellis and Beck have devised treatment techniques designed to teach people to question their own hypotheses and to identify when they are inappropriate. In schedule research with human adults, it has been found that the effects of behavioural rigidity can be decreased by giving subjects alternative response repertoires. Thus, Weiner (1969) found that, if subjects were exposed to DRL schedules followed by FR schedules, subsequent responding will be low-rate.

The extent to which behavioural rigidity affects other areas of human life, and the extent to which it can be overcome, remain subjects for further investigation.

CHAPTER 10: CONCLUSIONS

In the present thesis the development of human performance on schedules of reinforcement has been explored. It was found that the fixed-interval behaviour of subjects above five years of age was similar to the behaviour of adult subjects in respect to response patterning, insensitivity to the schedule parameter, and in terms of the effects of instructions or a history of responding on another schedule. The fixed-ratio and DRL responding of these subjects was also similar to that obtained from adult subjects.

Infant subjects behaved like animals in that their performance on fixed-interval schedules was found to be positively accelerated or ´scalloped´ throughout the interval and to be related to the schedule parameter in a systematic fashion. Similarly, the FR and DRL responding of the infant subjects was found to be comparable to that produced by animals. Infant subjects were also found to be sensitive to the change of contingencies when placed on a FI schedule following a history of reinforcement on either FR or DRL, unlike adult subjects who persist with the pattern of responding established on the first schedule. Like animal subjects, the infants rapidly developed a scalloped pattern of responding following transition to the FI schedule.

Subjects between approximately two and five years of age seemed to fall into an intermediate or ´transitional´ group. When uninstructed, their performance showed a lack of response patterning within the interval, and did not vary systematically with changes of the schedule value. None-the-less, when given high-rate instructions, subjects in this age range produced a high-rate pattern of responding similar to that found in adults. It was found that low-rate instructions had less effective control of the behaviour of the children in this group and it was necessary to teach a verbal response chain in order to produce low-rate behaviour. If a pattern of responding had been established by exposure to an FR or a DRL schedule, however, the same pattern of responding, whether low-rate or high-rate, was maintained on exposure to a subsequent fixed-interval schedule. The present evidence, therefore, together with the accompanying verbal reports and the verbal self-regulatory behaviour observed during the experiment, provides support for the verbal regulation account of human operant behaviour.

Further research needs to be carried out to confirm the conclusions of the present thesis. Already some supportive evidence is available from studies of the development of verbal self-regulation (cf. Chapter 4). As mentioned at the end of Chapter 5, the ontogenetic development of classically conditioned responding remains a relatively unexplored but potentially highly informative area.

Research which points to a relationship between mode of learning, behavioural development and language is, however, only a beginning. Future research could usefully be directed to determining how language comes to control motor behaviour. Philosophers such as Wittgenstein (1953) have emphasised that language is, above all, "a form of life". Yet, despite a number of promising attempts to develop a functional account of language (eg. Skinner, 1957; Halliday, 1974), until recently comparatively little research has been directed at understanding the consequences to the speaker of speaking. Should further research bear out the verbal regulation account of human behaviour, there are a number of important implications.

With respect to human learning, the present account suggests that there is a limit to what animal research can tell us. This is not to suggest that animal research should not be pursued, nor that it is entirely irrelevant to an understanding of human nature. For example, animal psychologists may have a great deal to offer developmental psychologists interested in the early, sensori-motor period of child development; clinical psychologists interested in the unconscious determinants of pathological behaviour; and social psychologists interested in the relatively automatic and unperceived aspects of interpersonal interaction.

With respect to animal learning theory, however, the present account suggests that psychologists should be wary of generalising in the opposite direction from man to animals (Blackman, 1983). Thus, while there is no reason why research into the mechanisms underlying animal conditioning should not proceed, it does not follow that those mechanisms will be identical to those involved in human thinking. Some ´cognitive´ animal psychologists have recently been moved to talk about an animal´s ´beliefs´ or ´hypotheses´ (eg. Dickinson, 1980). In the light of the present evidence such talk is likely, at best, to detract attention from the important differences between animals and man discussed in this thesis and, in particular, from the different ways in which human subjects can formulate and articulate rules which then govern their behaviour.

If this is indeed the case, it seems likely that any theories of human behaviour which focus exclusively on those factors known to be important in determining animal behaviour are likely to be misleading. This is so whether those theories stress genetic determinants (eg. Wilson, 1975) or environmental factors (eg. Skinner, 1948). In the past psychologists have often been tempted to overestimate their depth of understanding of human nature or their ability to bring forth ´the new age´, precisely because they have failed to recognise the importance of cultural factors in determining what we do. Given the importance of culture and the socially determined development of verbal self-regulatory skills (cf. Chapter 4), and our still limited understanding of the mechanisms by which culture is transmitted, it seems that, in future, psychologists should perhaps be more modest in what they claim.

Similarly, the theory of verbal self-regulation suggests that attempts to change human behaviour which focus purely on the contingencies and not upon the subject´s conceptualisation of those contingencies are likely to be missing an important source of control. In recent years, considerable evidence in support of this conclusion has been gathered independently by clinical psychologists. A number of researchers have suggested that ´irrational beliefs´ (Ellis, 1962; Beck, 1976) or inappropriate attributions

(Abramson, Seligman and Teasdale, 1978) may lie behind a range of emotional problems, particularly depression. As a result, there has been a growing trend towards ´cognitive´ modes of therapy in which an attempt is made to manipulate the patient´s understanding of his environment (Beck, 1974; Mahoney, 1974; Meichenbaum, 1977). Recent comparative outcome research has suggested that these techniques enjoy a modest superiority over traditional behavioural techniques formulated on the basis of animal learning theory (Shapiro and Shapiro, 1982). At the same time, some authors have attempted to reinterpret the mode of operation of traditional behavioural techniques of known efficaciousness and have suggested on the basis of a range of evidence that such treatments have their effect by manipulating the patient´s expectations of the consequences of his own behaviour and his evaluations of his own ability (Bandura, 1982).

Conversely, should a verbal regulation account of beliefs, expectations and evaluations be supported by further evidence, then animal learning theory may be a particularly helpful guide to the treatment of those individuals who fail to acquire language (for example, the profoundly mentally handicapped) or who have had verbal abilities but have lost them (for example, brain damaged individuals).

Finally, it is clear that the time has come for a rapprochement between radical behaviourism and other approaches to psychology. It is often assumed that behaviourists ignore altogether the role of private events. Although this is undoubtedly true in some cases, some behaviourists have attempted to provide a functional account of private events in terms of learning theory. Behaviourists have thus addressed the same issues (although in a different way) as information-processing theorists on the one hand and cognitive therapists on the other. Thus, although in some of Skinner´s utopian writing the emphasis is placed on contingency control, at other places, as we have seen, Skinner gives careful consideration to private events in general and verbal regulation in particular:

> "A science of behavior must consider the place of private stimuli..... The question then is this: What is inside the skin, and how do we know about it? The answer is, I believe, the heart of radical behaviorism." (Skinner, 1974.)

If an analysis of private events lies at the heart of radical behaviourism, then it may not be too much to hope that behaviourism may have something to offer other approaches to human psychology in which private events loom important but in which their nature remains relatively unanalysed.

Radical behaviourism should be understood as a philosophy of the science of behaviour, distinct from (but hopefully informing) the experimental analysis of behaviour (Skinner, 1974). In the past, behaviourists have been accused of ignoring the vast majority of psychological literature not couched in the language of learning theory. It is hoped that the present thesis has demonstrated that, as much as behaviourism can inform other approaches to psychology, it can be informed by the data collected by psychologists working within different traditions.

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