Copyright Statement

The digital copy of this thesis is protected by the Copyright Act 1994 (New Zealand).

This thesis may be consulted by you, provided you comply with the provisions of the Act and the following conditions of use:

- Any use you make of these documents or images must be for research or private study purposes only, and you may not make them available to any other person.
- Authors control the copyright of their thesis. You will recognise the author’s right to be identified as the author of this thesis, and due acknowledgement will be made to the author where appropriate.
- You will obtain the author's permission before publishing any material from their thesis.

To request permissions please use the Feedback form on our webpage. http://researchspace.auckland.ac.nz/feedback

General copyright and disclaimer

In addition to the above conditions, authors give their consent for the digital copy of their work to be used subject to the conditions specified on the Library Thesis Consent Form.
EXPERIMENTAL AND APPLIED BEHAVIOUR ANALYSIS

A thesis submitted to the University of Auckland
in partial fulfilment of the requirements
for the degree of Doctor of Philosophy

By

Grant Ronald Wardlaw M.A. (Hons)
1977
ABSTRACT

It is often claimed that applied behaviour analysis is founded on basic behavioural concepts involving the direct extrapolation of data from the experimental laboratory to the analysis of human behaviour. It is claimed that such an approach gives theoretical coherence to a set of procedures, and avoids the pitfalls associated with a collection of unrelated techniques. This presumed experimental base is said to make applied behaviour analysis more rigorous, more effective, more systematic, and easier to teach and learn than other approaches to the modification of human behaviour.

There is reason to believe, however, that the link between experimental and applied behaviour analysis is more one of commitment than reality. Previous authors have shown that research findings from the experimental analysis of behaviour are becoming increasingly isolated from other areas of psychology in general. The present study provides evidence that such findings are also isolated from applied behaviour analysis in particular. Experimental data are seldom cited in applied work and, when they are, it is seldom specific or current data to which references are made.

It is argued that this state of affairs is attributable to
the acceptance, by applied behaviour analysts, of a model of
behaviour whose complexity is insufficient to cope with the
complex interactive nature of human behaviour. The present
work provides an outline of the current applied behaviour analytic
model, and then proceeds to discuss some of the major types of
data of which the model takes no significant account. An
extensive examination of data from the experimental analysis of
choice behaviour is given, which reveals the level of complexity
to which application may be made. Suggestions as to the
practical utilization of these data are made, with particular
reference to the areas of self-control and commitment. A case
study involving the use of a commitment procedure in the treatment
of an exhibitionist and another concerning the application of
experimentally-derived procedures to a self-control problem,
provide clinical evidence of the usefulness of the direct
incorporation of experimental data.

Following these applied case studies, a number of other
areas of experimental research are examined with respect to their
relevance to applied behaviour analysis. Significant data from
the study of multiple-schedule interactions, stimulus control,
and two specific types of analyses of reinforcement phenomena are
outlined and suggestions made concerning their applied potential.
Further, data which could determine the limits of the applicability
of experimental data, in the context of biological constraints on
behaviour, are shown to be capable of further increasing the
utility of applied behaviour analytic techniques.
Finally, the implications of the data analyzed are discussed in terms of the future training of applied behaviour analysts.
# TABLE OF CONTENTS

## CHAPTER 1

Experimental and Applied Behaviour Analysis: A Relation Examined  \[1\]

## CHAPTER 2

The Experimental Context of Applied Behaviour Analysis  \[12\]

## CHAPTER 3

Applications of the Experimental Analysis of Choice Behaviour  \[41\]

## CHAPTER 4

The Clinical Application of Experimental Data - A Case Study  \[84\]

## CHAPTER 5

Premack Theory and Applied Behaviour Analysis  \[108\]

## CHAPTER 6

Behavioural Interactions  \[123\]

## CHAPTER 7

Conservation Theory and Applied Behaviour Analysis  \[152\]
TABLE OF CONTENTS CONT'D.

CHAPTER 8
  Stimulus Control .................. 170

CHAPTER 9
  Biological Constraints on
  Behaviour .......................... 194

CHAPTER 10
  Conclusions ....................... 202
ACKNOWLEDGEMENTS

During the course of this research, a number of people have made significant contributions, for which I offer my sincere thanks.

I owe a particular debt to Dr. Mike Davison, who has been my teacher and mentor since my earliest attempts at experimental behaviour analysis. It was primarily the challenging and stimulating discussions I have had with him over the years which made me aware of the issues discussed in this thesis.

Professor Hal Schaefer, who was a supervisor during the early part of this project, encouraged my interest in clinical work and provided many insights into the relation between experimental and applied behaviour analysis.

Special thanks are due to Mr. Fred Masters, Regional Senior Psychologist, Justice Department, Auckland, under whose supervision I gained three years of clinical experience during my research into applications of experimental data. His cheerful toleration of a researcher, and his

vi
frequent encouragement and critical reading of parts of the manuscript are gratefully acknowledged.

My wife, Carole, provided enthusiastic support for my work, for which I am most deeply indebted, and also contributed many insightful criticisms during numerous discussions.

Finally, a tribute is due to Mrs. Dorothy Zorichich for her patience and tolerance in typing various drafts, and whose technical expertise is evident in the final presentation of text and figures.
CHAPTER 1

Experimental and Applied Behaviour Analysis: A Relation Examined

Applied behaviour analysis may be chiefly distinguished from other approaches to the study of the human condition by its emphasis on behaviour, on the observable and measurable things people do. Since behaviour is observable and measurable, the behaviour analyst is able to define dependent variables which allow for replication of experiments. Ramp and Hopkins (1971) point out that research which claims to deal with unobservables characteristically deals with ill-specified dependent variables which make such replication difficult or impossible.

Innovative treatment techniques in clinical psychology are often based on opinion or appeals to authority. Yet if an objective evaluation of these techniques is to be made, we need precise definitions and measurements. The importance of emphasizing behaviour is that it possesses the necessary specificity to enable adequate evaluation.

An emphasis on experimentation and evaluation of empirical data follows from the priority of specifying and measuring behaviour. Little is known about what constitutes the therapeutically effective parts of many techniques and, when the components of a technique are ill-defined, it is difficult to train therapists in their use. Further, many techniques are
based on untested theory and accumulated experience. With this background, an empirical evaluation of therapeutic methods is the only way to ascertain their contribution to behaviour change and to make them explicitly communicable. The experimental determination of factors functionally related to specified behaviours allows for such an evaluation. In their discussion of applied behaviour analysis, Baer, Wolf, and Risley (1968) take analysis of behaviour to mean: "... a believable demonstration of the events that can be responsible for the occurrence or non-occurrence of that behaviour. An experimenter has achieved an analysis of a behaviour when he can exercise control over it" (1968, p.94).

Baer et al. (1968) outline two other important features of applied behaviour analysis: it is highly technological and it is based firmly upon established principles. It is technological in the sense that the techniques that constitute a particular application are comprehensively identified and described. The best way to judge the adequacy of a procedural description as technological is "to ask whether a typically trained reader could replicate the procedure well enough to produce the same results, given only a reading of the description" (Baer et al. 1968, p.95). An adequate procedural description outlines all possible contingencies of procedure. As well as specifying what happens when the subject makes the target response, it also specifies the events consequent upon alternative responses.
The second feature discussed by Baer et al. regarding firmly established basic principles, holds that while a good technological description is sufficient to allow successful replication by someone else in the same situation, the relating of this description to basic behavioural concepts facilitates the extension of the procedures to new situations or the derivation of new procedures from similar basic principles. Such an approach gives theoretical coherence to a set of procedures and avoids the pitfalls of being merely a collection of unrelated tricks. Baer et al. (1968) note that, historically, there has been considerable difficulty in systematically expanding collections of unrelated procedures, and that such collections are difficult to teach and to learn.

In view of the importance of relating procedure to principle, applied behaviour analysts claim that their techniques are based on the direct extrapolation of the principles of behaviour from the animal laboratory to human interaction. Thus, Neuringer (1970) claims that: "The therapeutic procedures and techniques that Behaviour Modifiers utilize come from the Experimental Psychology Laboratory .... Since its procedures with humans are based on controlled research, it represents that much called for rapprochment between experimental and clinical psychology" (1970, p.3).

Other authors, however, hold that although laboratory research has demonstrated that undesirable behaviour is learned
according to the same processes which govern desirable behaviour, the impact of these studies on clinical work has been minimal (Sandler and Davidson, 1971). Danaher (1974) has cautioned against the extrapolation from animal to human behaviour without ensuring that the essential parameters of the laboratory situation have been maintained. The debates over the adequacy of the experimental data base for a number of therapeutic techniques (for example, systematic desensitization, flooding, implosion) have illustrated some of the difficulties encountered when attempting to justify techniques on the basis of only parts of the available experimental data (Morganstern, 1973; Wilson and Davison, 1971).

If there is a strong link between experimental data and therapeutic practice, the references cited in clinical case studies should reflect it. Previous authors (for example, Krantz, 1971; Xhignesse and Osgood, 1967) have argued that the analysis of citation characteristics (the references given in articles in particular journals) is a valid measure of the inter-relationships between areas of research. Krantz (1971) calculated the mean percentage of self-citations per article in the primary "pure" operant outlet, the *Journal of the Experimental Analysis of Behavior* (JEAB) for the years 1958-1969 inclusive. His data showed an increasing rate of self-citation by JEAB and a decreasing rate of citation of JEAB articles by other journals, thus leading to a mutual isolation of research findings from the experimental analysis of behaviour.
A recent review (Kazdin, 1975) looked at the relationship between applied behaviour analysis and the general area of behaviour modification. Kazdin defined applied behaviour analysis as embracing Sidman's (1960) intrasubject methodology, focusing on socially significant behaviours in applied (rather than laboratory settings), and as an attempt to effect a socially significant change in behaviour. The Journal of Applied Behavior Analysis (JABA) was taken as representative of this area. No attempt was made to define "behaviour modification", but three journals, Behavior Therapy (BT), Behaviour Research and Therapy (BRT), and the Journal of Behavior Therapy and Experimental Psychiatry (JBTEP), were designated as representative of this area. Data for the years 1968-1974 inclusive showed that JABA had a high self-citation rate (a mean of 21.1%) as compared with the behaviour modification journals (means of 15.3%, 4.1%, and 4.0% for BRT, BT, and JBTEP respectively). As Krantz (1971) noted, a high self-citation rate on its own does not indicate that any area is becoming isolated from other related areas. Such isolation becomes evident, however, when self-citation is high and when related journals fail to cite that journal. Thus, an examination of reciprocal citation rates can provide useful information. Kazdin (1975) analyzed the extent to which JABA cited other behaviour modification journals and the extent to which those journals cited JABA. Taken as a whole, the data showed that JABA is neither being cited less by other related journals, nor is it citing those journals less frequently. Thus, on the basis of citation rates, JABA is not becoming increasingly isolated from other behaviour
modification journals in the way that JEAB has become isolated from other experimental psychology journals.

Kazdin (1975) also analyzed the applied operant studies in each of the four journals above to see how many met the minimal standards of applied behaviour analysis studies. The criteria used were based upon the research methodology guidelines set out by Sidman (1960) and discussed in JABA (for example, Baer et al. 1968), and also specific guidelines published in JABA for the information of authors (for example, JABA, 1969, p.1). Across all years, a mean percentage of 78.9% of JABA articles met these criteria. For the other behaviour modification journals the mean percentages were 40.3% for BT, 27.5% for JBTEP, and 16.7% for BRT. The data also showed a trend for the percentage of articles meeting the design and assessment criteria to increase over the years, with the exception of JBTEP which showed a decrease.

Taking the findings of the Kazdin and Krantz studies together, some disquieting implications arise: behavioural treatments (whether rigidly defined as applied behaviour analysis or more widely inclusive as behaviour modification) are rapidly increasing in popularity and use. They are primarily "marketed" as being scientific, methodologically sound, and based on firmly established experimental evidence. Kazdin's study indicates that a disturbingly low (although increasing) proportion of published case studies meet the minimum criteria of applied behaviour analysis investigation.
Krantz's study shows that the work on which applied behaviour analysis is founded (the experimental analysis of behaviour, Skinner, 1957, 1966a, 1966b) is becoming isolated from allied experimental fields. Given the increasingly wide use of applied behaviour analysis and its extension to new problems, new situations, and more complex areas, it is more vital than ever that application be grounded in established laboratory principles. A crucial question now is: to what extent are findings in the experimental analysis of behaviour currently being incorporated into applied behaviour analysis?

**TABLE 1.1:**

<table>
<thead>
<tr>
<th>Journal</th>
<th>No. of articles studied</th>
<th>Percentage of articles referring to experimental literature</th>
</tr>
</thead>
<tbody>
<tr>
<td>JBTEP</td>
<td>62</td>
<td>8 (N = 5)</td>
</tr>
<tr>
<td>BT</td>
<td>43</td>
<td>9 (N = 4)</td>
</tr>
<tr>
<td>BRT</td>
<td>18</td>
<td>11 (N = 2)</td>
</tr>
<tr>
<td>JABA</td>
<td>56</td>
<td>13 (N = 7)</td>
</tr>
</tbody>
</table>

**Table 1.1:**

Number of case studies published in four applied behaviour analysis or behaviour modification journals in 1974 and the percentage of these papers which explicitly referred to the literature on the experimental analysis of behaviour. The percentages have been rounded up to the nearest whole number.

To provide some data on this question I analyzed all of the case-study type reports published in 1974 in JABA, BT, BRT, and JBTEP. Articles included were those with a clearly defined target behaviour to be modified and the use of at least one
recognized therapeutic technique presumed to be based on either established operant or respondent principles (that is, the criteria were very lenient). Discussion articles which did not report actual data but summarized and criticized other case studies were included. Technical notes, book reviews, letters to the editor, and articles dealing with normative data, assessment methodology or instruments, and professional issues were not included. To evaluate the extent to which research in the experimental analysis of behaviour has an impact on applied behaviour analytic studies, I read the reference section of each case study and counted the number of studies referring to basic animal experiments. Table 1.1 summarizes the results and shows that for JBTEP 8% of case studies referred at least once to basic experimental work with animals as a source upon which the therapeutic technique was based. For BT the figure was 9%; for BRT, 11%; and for JABA it was 13%. For a discipline which claims to be based firmly on experimental foundations, these percentages are very low.

In order to examine further these data, each reference to the experimental literature was examined to see how up-to-date it was. For BRT the 11% consisted of only 2 articles. One referred to Premack's early (1959) paper on reinforcement and the other to Azrin and Holz's (1966) well-cited chapter on punishment. Studies in JBTEP referred to experiments all published in the 1960's (for example, Azrin and Holz, 1966; Miller, 1969; Premack, 1965). BT was also represented mostly
by 1960's references (for example, Premack, 1963, 1965; Sidman, 1966; Terrace, 1966a) and three papers in the early 1970's (Herrnstein, 1970; Wikler and Pescor, 1970; Premack, 1971). JABA, the journal which referred most to experimental studies, cited almost exclusively early work (for example, Premack, 1959; Reynolds, 1961a; Terrace, 1966a). The latest experiment referred to was that of Touchette (1971). Thus, even when experimental animal studies are cited as a base for the procedure in question, the evidence is typically 5 to 10 years old. It is also interesting to note that the majority of references were either to chapters in Honig's (1966) compendium of operant research (chapters by Azrin and Holz, Sidman, and Terrace) or to papers by Premack on his approach to the problem of the nature of reinforcement.

The present analysis reveals a number of disturbing trends. Firstly, on average, somewhere around 90% of studies published in 1974 in the four leading journals devoted to applied behaviour analysis gave no indication of any basic experimental research base for the therapeutic technique they employed. Secondly, those studies which did refer to the experimental literature cited reports which are years old (and in many cases whose findings have been questioned by more recent research). And finally, the majority of references were to only a small common core of papers.

Recent research in a number of areas of the experimental
analysis of behaviour has resulted in new data and new analyses of past data (particularly such analyses as Ainslie, 1974; Allison, 1976a, 1976b; Baum, 1973, 1974; Catania, 1973a; Davison and Hunter, 1976; de Villiers and Herrnstein, 1976; Herrnstein and Loveland, 1975; Rachlin and Green, 1972; Schoenfeld and Cole, 1972; Seligman and Hager, 1972; Staddon, 1972). New data and new interpretations call into question the particular applications which may have been suggested by now outdated references. If applied behaviour analysis is to have a credible claim to an empirical foundation it must first make much more effort to tie applied work to experimental underpinnings; and second, it must keep abreast of current animal experimentation and the theoretical developments stemming from it.

We are currently witnessing a proliferation of treatment techniques claimed to be behaviour analytic, and there is much pressure to press forward with their development. What is urgently needed now, however, is not more techniques, but rather a critical analysis of present procedures. A number of approaches are open. Firstly, we should experimentally isolate those components of treatment "packages" which are necessary for behaviour change (cf., Barlow, Leitenberg, and Agras, 1969; Paul, 1969; Barlow, Leintenberg, Agras, Callahan, and Moore, 1972). We should also compare the effectiveness of applied behaviour analytic techniques with each other and with procedures derived from other theoretical backgrounds. Such comparisons could be based on the speed with which the behaviour
change is effected, the magnitude of the change, its durability, and the extent to which it generalizes to other relevant situations. We could also include evaluation on the basis of the ease with which the techniques can be taught, the types of people who could use them, and so forth.

Important as these sorts of analyses are, the most pressing need is for an evaluation of the adequacy of the experimental foundations of applied behaviour analytic techniques. The rapid expansion in the number of individual techniques combined with the discovery that little recent experimental work is quoted in applied behaviour analysis journals, implies that much so-called applied behaviour analysis is not entirely based on findings from the experimental analysis of behaviour. Furthermore, the applied work which is founded on basic principles is not assimilating current experimental data and the revised theoretical explanations of behaviour based upon them. The purpose of this thesis is to provide part of the missing link between experimental and applied behaviour analysis. I shall outline the current experimental framework upon which applied behaviour analysis rests, and then show how recent experimental data and the theoretical formulations evolved from them can contribute to increasing both the functional utility and scientific power of the analysis.

.....
CHAPTER 2

The Experimental Context of Applied Behaviour Analysis

A severe limitation in applied behaviour analysis, and a major factor in its failure to produce long-term and generalizable changes in behaviour (Kelley, Shemberg, and Carbonell, 1976), is its reliance on a simple model of behaviour which cannot cope with the complexity of its subject matter. This model is implicitly stated in outlines of behavioural "diagnosis" or assessment (Peterson, 1968; Kanfer and Saslow, 1969; Gardner, 1972; Mash and Terdal, 1974) which focus on identifying critical variables which are functionally related to behaviours of concern. The assumption is that having identified these relations, the controlling variables may be manipulated and the behaviour consequently modified in a predictable manner. The purpose of this chapter is to elucidate the model of behaviour implicit in behavioural assessment schemes and to examine the extent to which experimentally-derived data are explicitly used in the execution of applied behaviour analysis. This examination points to a major weakness in applied behaviour analysis, which subsequent chapters will seek to redress.

The Behaviour Analysis Model

Although there is no definitive explication of the elements of the model upon which applied behaviour analysis is based, there is broad agreement as to its basic components. Generally,
the two major concepts are related to behaviour and the environment. Behaviour is divided into units called responses, and the environment is divided into units called stimuli (Skinner, 1938).

Units of the Model

Responses are further classified as being either respondents or operants. Respondent behaviour is said to be reflexive or involuntary, and is behaviour which is reliably produced or elicited by specific stimulus changes in the environment. Respondents (such as eye-blink, blood vessel constriction, salivation) are characterized as being unlearned (that is, present from birth), not under the control of the individual, and elicited by specific antecedent stimuli (Pavlov, 1927).

Operant behaviour, in contrast, is said to be emitted by the individual and is under the individual's control. No obvious stimulus evokes this type of behaviour, and there is no invariant relation between a particular stimulus and a particular response. Operant behaviour is so called because it operates upon the environment and, in so doing, produces consequences in the environment (Skinner, 1937). It is operant behaviour which is said to be learned, and which constitutes the majority of human behaviour.

The environment is divided into several classes of stimuli. The first class, eliciting stimuli, consists of events which regularly precede responses. These events cause relatively
fixed and stereotyped responses (respondents) to occur immediately following their presentation. If the same eliciting stimulus (e.g., bright light on the eye) is presented a number of times in a row, it will elicit virtually the same response (constriction of the pupil) each time. The second class of stimuli consists of environmental events which follow responses. These events, called reinforcing stimuli or reinforcers, increase the future probability of occurrence of the responses they follow (Skinner, 1953). The class of responses whose probability of occurrence is increased by being followed by reinforcers consists of operant responses. A third class of stimuli, discriminative stimuli, consists of events that precede and accompany operants but do not perform an eliciting function. Rather, the presence of a discriminative stimulus merely increases or decreases the probability of occurrence of responses which have previously been reinforced or punished in the presence of the same discriminative stimulus. The final class of stimuli consists of environmental events which, whether they precede, accompany, or follow responses, bring about no change in the probability of occurrence of those responses. Such events are called neutral stimuli.

In order to study the relations between behaviour and environment, we must present stimuli to an organism. In so doing we must distinguish between behavioural operations and behavioural processes (Catania, 1973b). The experimental or clinical procedures imposed upon the behaviour are operations; the behavioural effects of these operations are processes.
Behavioural Operations

Behavioural operations are usually divided into three classes.

The elicitation operation is the simplest and involves presenting stimuli to an organism so that we may observe changes in its responses. As noted earlier, the presentation of an eliciting stimulus produces a respondent behaviour. Reynolds (1968) notes two characteristics which distinguish respondents from operants. First, the frequency of occurrence of the eliciting stimulus determines the frequency of occurrence of the respondent. The absence of the eliciting stimulus generally means the absence of the respondent. Thus, if we require a change in the frequency of a respondent, we need only make a corresponding change in the frequency of presentation of the eliciting stimulus. Second, generally the environmental events following respondents do not change their frequency of occurrence.

Respondents are relatively invariant over an organism's life, but new, previously neutral, stimuli may come to elicit respondent behaviours when they are paired with prevailing eliciting stimuli (Pavlov, 1927).

Elicited behaviour, however, accounts for only a small proportion of behaviour. Much of the behaviour of organisms in general, and most of human behaviour, is controlled by the effect of behaviour on the environment, as well as that of the
environment on behaviour. For example, we may pay attention every time a child behaves appropriately, or we can slap it following inappropriate behaviour, and these consequences are likely to alter the frequency of these behaviours. These are consequential operations. Here, behaviour changes because of the relationship between stimuli and behaviour, rather than simply because stimuli are presented to the organism.

There are three basic consequential operations:

- reinforcement
- punishment
- extinction

Historically, it was E.L. Thorndike who first systematically studied the effects of these operations on subsequent responding. He formalized the results of his experiments in terms of the Law of Effect, which stated that:

"Of several responses made to the same situation, those which are accompanied or closely followed by satisfaction to the animal will, other things being equal, be more firmly connected with the situation, so that when it occurs, they will be more likely to recur; those which are accompanied or closely followed by discomfort to the animal will, other things being equal, have their connection with that situation weakened, so that, when it recurs, they will be less likely to occur" (Thorndike, 1911, p.244).

In other words, behaviour may be made more probable if followed by
some consequences and less probable if followed by others.

The work of B.F. Skinner has led to a reformulation of the positive part of Thorndike's law of effect in terms of the reinforcement operation. Skinner's reformulation states that the future probability of occurrence of a response will increase if a member of that operant class is followed by a reinforcing stimulus (or reinforcer) (Skinner, 1938). Put simply, responding increases when followed by reinforcers.¹

¹ A note on vocabulary is in order here. The term 'reinforcer' or 'reinforcing stimulus' denotes a stimulus which increases the probability of occurrence of the behaviour it follows. The term 'reinforcement' refers to the operation of following a response with a reinforcer. Thus, food may be a reinforcer to a food-deprived organism, and the presentation of food to an organism contingent upon a specified response is an instance of reinforcement. The meaning of the term 'reinforcement' is confused in the literature by the presence of another usage. 'Reinforcement' is often used to refer to the process that follows the operation. Thus, it sometimes refers to the increase in probability of responding. This means that the phrase "a response was reinforced" may refer either to the fact that the response was followed by a reinforcer, or that an increase in responding was a consequence of the delivery of a reinforcer. The convention adopted in this thesis follows the policy of the Journal of the Experimental Analysis of Behavior (Preparation of manuscripts for JEAB, January 1975), that is, reinforcement is an operation. Catania (1973b) points out that:

"... the process that follows from this operation is concretely described in terms of changes in frequency of a response, and thus there is little justification for substituting other terminology for a direct description in terms of changes in frequency" (1973, p.40).
(cf. Catania, 1968, in which the two definitions of reinforcement are given equal status).
Reinforcement

The environmental consequences which increase the probability of occurrence of the behaviours they follow may involve either the presentation of an additional environmental event or the withdrawal of a current one. A stimulus whose presentation following a response increases the future probability of occurrence of that response is called a positive reinforcer. The operation of presenting a positive reinforcer is that of positive reinforcement. A stimulus whose withdrawal following a response increases the future probability of occurrence of that response is called a negative reinforcer and the operation of withdrawing a negative reinforcer is that of negative reinforcement. It is important to remember that both positive and negative reinforcement result in an increase in responding and that the adjectives positive and negative refer, respectively, to the presentation and withdrawal of stimuli, and not to changes in behaviour (see Micheal, 1975 for a discussion of the confusions over positive and negative reinforcement).

Schedules of Reinforcement

It is not necessary for every instance of a response to be followed by a reinforcer for the rate of occurrence of responses in that class to be maintained or increased. In fact, in human behaviour, continuous reinforcement is the exception rather than the rule. Whenever only some of the organism's behaviours are reinforced by the environment, an intermittent schedule of
reinforcement is operative. It has been widely accepted by applied behaviour analysts (in their theoretical writings but not their case studies, as I shall show) that "schedules of reinforcement have regular, orderly and profound effects on the organism's rate of responding" (Reynolds, 1968, p.60). (Emphasis mine). Reynolds (1968) stresses the importance of schedule control when he writes:

"No description, account, or explanation of any operant behaviour of any organism is complete unless the schedule of reinforcement is specified. Schedules are the mainsprings of behavioural control, and thus the study of schedules is central to the study of behaviour" (p.60).

Intermittent reinforcers are delivered according to schedules that specify which responses will be reinforced. Such schedules may specify that reinforcers will be delivered according to the number of responses emitted, how the responses are temporally spaced, or the time elapsed since some previous event. These requirements may be combined in a variety of ways to determine more complex scheduling arrangements.

Most textbooks on applied behaviour analysis include a description of schedules specifying reinforcer delivery contingent upon number of responses emitted or time elapsed since some previous event (usually the previous reinforcer) - that is, fixed-ratio (FR), fixed-interval (FI), variable-ratio (VR), and variable-interval (VI) schedules. The early description of these
schedules and their behavioural effects was made by Ferster and Skinner (1957).

A ratio schedule is one in which reinforcers are delivered according to the number of responses that have been emitted. Such a schedule specifies the ratio of responses to reinforcers. In a fixed-ratio (FR) schedule, the reinforcer is contingent upon the emission of a fixed number of responses, counted from the last reinforced response. For example, in an FR 20 schedule, every twentieth response is reinforced. In a variable-ratio (VR) schedule, the reinforcer is contingent upon the emission of the last of a variable number of responses, counted from the last reinforced response. The number of responses required for the delivery of a reinforcer changes from one presentation to the next, but the variable requirement has a fixed mean. Thus, a VR 5 schedule specifies an average of five responses per reinforcer (but on any particular occasion the requirement may be less than or greater than five responses per reinforcer. Applied behaviour analysts are usually aware only of the general information that the typical behaviour of an organism on an FR schedule consists of a pause after each reinforcer delivery followed by a high rate of responding that continues, usually at a constant rate, until the next reinforcer is delivered. VR schedules are also characterized by a high constant rate of responding, but there is generally no pause after each reinforcer is delivered (Ferster and Skinner, 1957).
In an interval schedule, a reinforcer is delivered following the first response after a specified time has passed since some event (usually the last reinforcer delivery). If the period of time is constant from one interval to the next, this defines a fixed-interval (FI) schedule. Thus, an FI 2-min schedule specifies that a reinforcer will be delivered for the first response emitted when two minutes have elapsed since the delivery of the last reinforcer. In a variable-interval (VI) schedule the amount of time that must elapse before a response can be reinforced varies between one delivery of a reinforcer and the next, but the intervals average out to some mean value. Thus, in a VI 2-min schedule, a reinforcer will be delivered on the average of once every 2-min (provided the appropriate response is made). Most applied behaviour analysis textbooks supply only simple information about patterns of responding generated by these schedules. They usually state that FI schedules generate a pattern called a scallop in which responding is at a low rate after reinforcement and gradually increases as the end of the interval approaches. VI schedules produce almost constant rates of responding, similar to the pattern produced by VR schedules, but at a lower rate. Thus, VI performance is often characterized as being a slow, even rate of responding (Ferster and Skinner, 1957). It is usually noted that the longer the average interval in a VI schedule, the slower the response rate will be.

Of all the other schedules of reinforcement for which
experimental data exist, only two are frequently mentioned in applied behaviour analysis textbooks. The first of these is differential reinforcement of low rates of responding (DRL) in which reinforcement is contingent upon only those responses that are preceded by a period of non-responding of at least t seconds. Thus, if a DRL 2-min schedule is in effect, only those responses which follow other responses by more than 2-min will be reinforced. Any responses made before 2-min after the previous response reset the interval to 2-min. Once behaviour comes under the control of a DRL schedule, the majority of intervals between responses (inter-response times or IRTs) are close to, or greater than, the length of the prescribed interval (Malott and Cumming, 1964). A less common method of scheduling DRL is to reinforce a response only if fewer than a specified number of responses have been emitted during the preceding time period, but experimental data on DRL schedules have come almost exclusively from studies in which one response is the numerical limitation set by the experimenters (see Kramer and Rilling, 1970, for a review). (It is pertinent that the Journal of Applied Behavior Analysis whose stated purpose is "the original publication of reports of experimental research involving applications of the experimental analysis of behavior to problems of social importance" (inside front cover, each issue), has only published one study in its eight years of operation (1968-1975) which deals specifically with the applied use of DRL schedules (Dietz and Repp, 1973). Even this study reported the use of the type of DRL schedules for which little experimental data exist - a
reinforcer was delivered "if the number of responses in a specified period of time (was) less than, or equal to, a prescribed limit ..." (1973, p.451) (my emphasis).

The other schedule sometimes referred to by applied behaviour analysts is the differential reinforcement of other behaviour (DRO) schedule, in which a reinforcer is delivered following the non-occurrence of a specified response for some fixed time interval. Thus, under a 20-sec DRO schedule, a reinforcer is delivered every 20-sec unless a response occurs. Each response postpones the onset of the reinforcer 20-sec from that response. Such schedules lead to a rapid decrease in the rate of the defined response (Reynolds, 1961a).

Two further scheduling arrangements - multiple and concurrent schedules - are infrequently used in applied behaviour analysis. In a multiple schedule, two or more reinforcement schedules operate alternatively and each is associated with a distinctive stimulus (Reynolds, 1961b). Thus a mult FI 30-sec FR 20 schedule may be arranged with a blue light associated with the FI 30-sec component and a yellow light with the FR 20. Alternation may be arranged either following reinforcement or according to fixed or variable periods of time. Following training on multiple schedules, response patterns in the presence of each stimulus are appropriate to the schedule with which the stimulus is associated. Thus, in the above example, in the presence of a blue light the
organism would respond with a scalloped pattern and in the presence of the yellow light with a pause followed by a high, constant rate.

While in multiple schedules the two schedules occur successively and are associated with different stimuli, in concurrent schedules, the two responses are available simultaneously, and each is associated with its own schedule of reinforcement (Catania, 1966). Thus, in a conc VI 60-sec VI 30-sec schedule there are two responses available, one maintained by a VI 60-sec schedule and the other by a VI 30-sec schedule. A considerable body of experimental data exists on concurrent scheduling phenomena, but the generalized law acknowledged by applied behaviour analysts is simply that organisms roughly match the proportion of responses to one alternative to the proportion of reinforcers obtained for that alternative (Herrnstein, 1970). Thus, in the above example, approximately two-thirds of the responses would be emitted to the alternative maintained by the VI 30-sec schedule.

**Punishment**

The second basic consequential operation is that of punishment. This involves arranging a consequence for a response which reduces the future probability of occurrence of members of that operant class. A punisher is the event or stimulus that is arranged as the consequence. The vocabulary
of punishment parallels that of reinforcement. Thus, if an electric shock is presented whenever a certain response is made, and the frequency of that response is reduced, the response is said to be punished and the shock is said to be a punisher.

As is the case with reinforcement, the term 'punishment' has been used to refer to a process as well as an operation, so that to say a response has been punished may, to some authors, mean that a response produces a punisher and, to others, that as a consequence of producing a punisher, the response decreased in frequency. In this thesis, punishment refers to the operation. (Note, however, that the definition most frequently quoted in applied behaviour analysis is expressed in terms of a process: "... punishment is a reduction of the future probability of a specific response as a result of the immediate delivery of a stimulus for that response" (Azrin and Holz, 1966, p. 381)).

As with the reinforcement operation, punishment may involve either presentation or removal of stimuli, and the events may be either positive or negative. Thus, punishment can be the presentation of an aversive stimulus contingent upon a response. This operation is sometimes referred to as positive punishment - positive because a stimulus is added. Another punishment operation is the removal of a reinforcing stimulus contingent upon a response (sometimes referred to as negative punishment). This latter operation may be further subdivided into timeout from
positive reinforcement (Ferster, 1957) and response cost (Weiner, 1962). Timeout from positive reinforcement (TO) involves removal of the opportunity for a behaviour to be followed by a positive reinforcer for a certain period of time. This may be achieved by:

(i) removing the organism from the environment in which reinforcers are delivered;

(ii) removing the environment in which reinforcers are delivered from the organism; or

(iii) removing the particular stimuli associated with the delivery of reinforcers.

Response cost involves the loss of a reinforcer already in the organism's possession contingent upon the emission of a particular behaviour. A fine for a traffic offence is an example of response cost. Response cost should not be confused with time out which prevents the organism from obtaining reinforcers not presently in its possession.

Apart from citing research on the undesirable side-effects of positive punishment (for example, escape, aggression, anxiety) little or no further background information on punishment is given in applied behaviour analysis textbooks. Frequently Azrin and Holz's (1966) list of what constitutes an ideal punisher is outlined, but discussion of such areas as scheduling punishers is extremely rare.
Extinction

Extinction, the discontinuation of reinforcement of a previously reinforced response, is the final consequential operation. Thus, if a behaviour has been maintained at a certain frequency because it has been followed by food, we describe the situation of no longer following that behaviour by food as extinction. Once again extinction refers to an operation and not a process. The usual effect of extinction is eventually to decrease the frequency of the behaviour in question. When a previously reinforced behaviour is no longer reinforced it generally occurs initially at a high frequency and then decreases until its rate is near its original operant level (that is, its freely occurring rate before the reinforcement operation was instituted).

The pattern of responding following the institution of extinction depends on a number of variables which, taken as a whole, constitute the resistance to extinction shown by an organism. The schedule of reinforcement which was maintaining the extinguished response is of primary importance. Each type of schedule when terminated leads to different response patterns in extinction. Most textbooks merely point out that performances maintained by continuous reinforcement schedules decrease rapidly during extinction, whereas those maintained on intermittent schedules are more resistant to extinction.
Few applied behaviour analysis textbooks mention the other variables important in extinction (cf. MacMillan, 1973). Resistance to extinction is lessened if the response has been extinguished in the past. In general, resistance is greater the greater the magnitude of the reinforcer and the greater the frequency with which members of that operant class have been reinforced in the past. And, finally, a response will generally decrease more slowly in frequency during extinction, the greater the state of the organism's deprivation with respect to the reinforcer (Catania, 1973b).

The Stimulus-Control Operation

We can superimpose yet another operation onto either the elicitation or consequential operations. This is the stimulus-control operation, which arranges that the simpler operations are in effect only in the presence of an additional stimulus. Thus, we can superimpose this operation on a consequential operation so that only in the presence of a particular stimulus will a response have programmed consequences. For example, we could provide a reinforcer for a pigeon's keypecks in the presence of a light of a certain wavelength, or we could arrange a punisher contingent upon responses in the presence of a specified tone. If, as a result of the stimulus-control operation, the keypecks came to occur frequently in the presence of the specified light and infrequently in the presence of the tone, these outcomes would be referred to as instances of the behavioural process of
discrimination. The particular stimuli used (light and tone) are termed discriminative stimuli and are said to set the occasion for responses (the light sets the occasion for an increase in responding and the tone for a decrease). An important corollary of discrimination is the process of generalization. An organism exhibits generalization if having learned to emit a response in the presence of one stimulus, it emits the same response in the presence of another stimulus (or other stimuli). To the extent that responding is similar in the presence of two or more stimuli, behaviour is said to generalize across the stimuli. To the extent that responding is different in the presence of two or more stimuli, the organism is said to discriminate between the stimuli. Applied behaviour analysts hold that many behavioural problems are failures to learn appropriate discriminations and generalizations and hence data from stimulus control should be of some importance in applied settings.

Just as the stimulus-control operation can be superimposed on consequential operations, so it can be on the elicitation operation. Pavlov's (1927) respondent conditioning or conditioned-reflex experiments provide the best-known example. In these experiments, a harnessed dog had food (which elicits salivation) placed in its mouth only following the ringing of a bell. In other words, the elicitation operation, the delivery of food, was arranged only in the presence of a particular stimulus, the bell. The food is an unconditioned stimulus, the bell a conditioned stimulus, and salivation an unconditioned
response. Following a number of presentations in this manner, Pavlov found that the dog began to salivate at the sound of the bell (presented in the absence of food) and this was called a conditioned response.

The Model Related to Behavioural Assessment

Behavioural assessment outlines are typically organized into a few simple categories. First, both problematic and non-problematic behaviours are identified. Problematic behaviours are classified as either excesses or deficits. A behaviour may be excessive in terms of:

(i) frequency of occurrence, e.g., compulsive handwashing;
(ii) intensity, e.g., aggression;
(iii) duration, e.g., staying angry for days over a minor disagreement; or
(iv) occurrence under conditions in which society deems its frequency should be near zero, e.g., sexual exhibitionism.

Similarly, behaviour may be characterized as deficit because of a failure to occur:

(i) with sufficient frequency;
(ii) with sufficient intensity;
(iii) for an adequate duration; or
(iv) under socially expected conditions.
Non-problematic behaviours or "behavioural assets" are those adaptive behaviours in the person's current repertoire. They may be used either as a base upon which to build further behaviours or as reinforcers for behaviour change. Having identified relevant behaviours, assessment attempts to pinpoint the stimulus conditions in the presence of which they occur. The hope is that if discriminative stimuli which "set the occasion for" behaviours can be identified they may be manipulated and target behaviours may be brought under the control of different stimuli. Finally, the environmental consequences of the relevant behaviours are enumerated. Events which currently maintain behaviours and those which decrease their probability of occurrence are identified, as are those which may potentially fulfil these functions.

The *impression* given by most published outlines of behavioural assessment is that the environment may be conveniently divided into a small number of relatively discrete events and that the relations between these events are similarly uncomplicated. The amount of space devoted to behavioural assessment in chapters and textbooks on applied behaviour analysis indicates that it is considered either unimportant or uncomplicated. As assessment is vital to any modification programme, we must assume that it is seen as a simple procedure not worthy of prolonged discussion. However, the lack of explicit reference to the complexity both of assessment and the relations between environmental events is
a major weakness in much applied behaviour analysis. This lack of emphasis has two implications. Primarily, it mitigates against the success of applied behaviour analysis when its techniques are used to modify complex socially-relevant behaviours. Witness, for example, the paucity of studies which attempt to modify behaviours involved in crime, pollution, urban transport, government. Further, it restricts the scientific evolution of applied behaviour analysis. Notwithstanding the significant gains in remediating countless "simple" behaviour problems achieved by working within the current framework, further growth will be stunted unless applied work incorporates data from current experimental behaviour analysis. A significant part of experimental work examines complex interactions between behaviours, and between behaviour and environment, which have to be grasped if we are to understand the even more complex interactions involved in human behaviour. Without such an understanding applied behaviour analysis is in danger of becoming trivial behaviour analysis.

The outline of the model of behaviour presented above is, I believe, representative of that given in most textbooks on applied behaviour analysis as the empirical background to applied techniques. While most authors provide numerous examples of each of the areas discussed, the model I have outlined indicates the depth of data typically discussed. The style of writing in many texts gives the impression that it is but a simple matter to match
this "empirical" outline to the outline for behavioural assessment. Assessment indicates whether behaviour is deficit, or excessive, or under faulty stimulus control. Further, it indicates what events are potential reinforcers and punishers for the person. With these data, the two outlines may be meshed. If the behaviour is excessive, the operations which may be used to decrease it are positive and negative punishment, time-out from positive reinforcement, response cost, or extinction. If the behaviour is deficit, the operations of positive or negative reinforcement may be used to increase it. If faulty stimulus control is the problem we may either remove the discriminative stimuli for inappropriate behaviour or establish discriminative stimuli for appropriate behaviour.

Now this description of how intervention follows from assessment in applied behaviour analysis is a gross oversimplification and, in many ways, is more caricature than characterization. Clinicians are mostly well aware of the complexity of the behaviour they are modifying and they expend time and energy far in excess of that which would be indicated by the above description. Within the limits of the model of behaviour outlined here, a vast literature of applied behaviour analysis case studies has grown, seemingly attesting to the validity of this approach. However, it is these limits which fail to confront us with the complexity of behaviour. The field of the experimental analysis of behaviour is characterized by new data forcing new conclusions. It is an area of dissent over
basic principles, with questioning of the boundaries set by earlier work.

By contrast the field of applied behaviour analysis is amazingly unaffected by the ferment in experimental analysis. To be sure, there is vociferous debate about many things of importance but, surprisingly, not about the experimental basis for its techniques. It seems that applied behaviour analysts have been so enamoured with the deceptive simplicity of their model of behaviour that they have failed to incorporate even very early data from experimental analysis into their applications. Some examples from recent applied behaviour analysis journals will serve to illustrate this point.

The Use of Data from Experimental Behaviour Analysis in Applied Behaviour Analysis

The control of behaviour can be broken into two vital areas - reinforcement schedule control and stimulus control. Applied behaviour analysts invariably acknowledge that effective behaviour modification involves an adequate knowledge of and control over both. Therefore, we should expect to find that maintaining schedules and relevant stimuli are particularly well specified in applied behaviour analytic studies.

In order to assess the importance accorded to schedule control by applied behaviour analysts I analyzed all issues for 1975 of each of the major applied behaviour analysis Journals (Journal of
Applied Behavior Analysis (JABA), Behaviour Research and Therapy (BRT), Journal of Behavior Therapy and Experimental Psychiatry (JBTEP) and Behavior Therapy (BT) in the following manner: each case study was read and scored positive if it:

(i) in the introduction, cited a study from the field of experimental behaviour analysis which detailed the effects of a specified schedule of reinforcement, and/or

(ii) in the procedure, specified a schedule of reinforcement other than CRF, and/or

(iii) in the discussion, analyzed the results or any part thereof in terms of the known behavioural effects of a specified schedule or reinforcement.

Papers were designated as case studies if they reported the use of a behavioural technique to modify a target behaviour either in an individual or a group. Technical notes, letters to the editor, papers on data collection, follow-ups, ethics, design of environments, and theoretical position papers were not included in the analysis. Table 2.1 summarizes the findings of the survey.

Not surprisingly, in view of its stated commitment to publishing applications of the experimental analysis of behaviour, JABA contained the most case studies (23%) specifying schedule effects. Of the seven papers which constituted this percentage, four cited references to experimental literature which either described the schedule in more detail or gave
TABLE 2.1:

<table>
<thead>
<tr>
<th>Journal</th>
<th>% of Case Studies providing schedule information</th>
<th>Number of Case Studies analyzed</th>
</tr>
</thead>
<tbody>
<tr>
<td>JABA</td>
<td>23 (N = 7)</td>
<td>31</td>
</tr>
<tr>
<td>BRT</td>
<td>7 (N = 2)</td>
<td>27</td>
</tr>
<tr>
<td>JBTEP</td>
<td>9 (N = 5)</td>
<td>56</td>
</tr>
<tr>
<td>BT</td>
<td>12 (N = 5)</td>
<td>42</td>
</tr>
<tr>
<td><strong>COMBINED:</strong></td>
<td><strong>13 (N = 19)</strong></td>
<td><strong>156</strong></td>
</tr>
</tbody>
</table>

Table 2.1:

Percentage of case studies in applied behaviour analysis journals for 1975 detailing reinforcement schedule information. Percentages have been rounded to the nearest whole number.

Information on behavioural effects of these schedules. Zlutnick, Mayville and Moffat (1975) examined the use of FR punishment and DRO schedules on the incidence of seizure disorders in children. Of their 24 references, 14 were to papers published in the area of experimental behaviour analysis and seven of these dealt predominantly with schedule control. Van Houten and Sullivan (1975) used a variable-time constant-probability schedule to cue teachers to praise children's academic behaviour. Of the thirteen studies referenced in this paper two provided more information on what a variable-time constant-probability schedule is, but the authors did not explain why such a schedule was used. Stephens, Pear, Wray and Jackson (1975) carried out the only study which actually examined the effects of various schedules - comparisons between FR and CRF, different FRs, and interlocking and FR
schedules - on teaching picture names to retarded children. Of their 24 references, 13 were to experimental behaviour analysis. Their data provide a valuable extension of schedule effects from animal studies to human behaviour. Goetz, Holmberg and Le Blanc (1975) examined the effect of a DRO schedule on a pre-schooler's compliance. Of the twenty-one references, three were to the experimental behaviour analysis literature. One provided the definition of DRO and the other two gave information on the behavioural effects of the schedule. The remaining three JABA papers utilized specified reinforcement schedules (VR punishment, VR positive reinforcement, DRO, DRH) but gave no reason for the choice of those particular schedules or any indication of their expected effects as shown by reference to the experimental literature.

Of the papers reviewed in BRT, 7% specified particular reinforcement schedules (two papers accounted for this percentage). One study (Myers, 1975) examined the relative efficacy of extinction, DRO, and response-cost procedures for eliminating self-injurious behaviour in a twenty-year-old retardate. Of the twelve references, five were to experimental analysis studies which gave more detailed data about the schedules, but no specific background data were actually reported in the paper itself. Taylor and Turner (1975) in the other study, utilized a 50% random reinforcement schedule to control a "bell and pad" treatment of enuresis. This schedule was not defined, no reason was given
for the choice of that particular schedule, and there were no references to experimental studies.

In JBTEP, 9% (N = 5) of studies included mention of specific types of schedules. Of the five studies, two utilized schedules (an unspecified VI and an FR 8), but gave no description, no reason, and no source reference. A third, (Hollander and Homer, 1975) referred briefly to the resistance to extinction advantages of delayed reinforcement schedules, but did not cite specific data. The fourth study used specific VI schedules to facilitate extinction of infant crying (Glavin and Moyer, 1975), but was referenced only generally to Reynolds' (1968) primer. The final study (Calhoun and Matherne, 1975), investigated the effects of varying schedules of time-out on aggressive behaviour, and was extensively referenced to the experimental literature.

Finally, 12% (N = 5) of BT articles contained schedule information. Of the five studies, four provided detailed reference to experimental data to either fully describe the schedules or to provide information about their effects on behaviour.

In summary, over all four journals, only 13% of case studies contained pertinent data on schedule factors. (However, even this percentage is inflated by JABA's contribution of 23%. Over the other three journals, only 9% of cases provided detailed reference to schedules of reinforcement). The assertion that
applied behaviour analysis recognizes the importance of schedule effects is certainly not borne out by these data.

If schedule effects are not taken into account in much applied behaviour analysis, one would still expect that prominence would be given to stimulus control. An analysis of all issues of JEAB for the years 1970-75 shows that an average of 32% of articles dealt specifically with stimulus control. By contrast, for the same years, only 7% of JABA case studies specifically mentioned stimulus control factors and provided references to experimental studies to explain the effect of these factors. Once again, these figures show how divorced applied behaviour analysis is from its presumed experimental base.

A comprehensive and unified applied behaviour analysis is more hope and promise than current reality. The premise that applied behaviour analysis utilizes experimental behaviour analytic data in the prediction and control of human behaviour problems is not borne out by the data. The model of behaviour upon which applied studies are based is a much simplified version of that which is indicated by experimental data and cannot hope to deal with the complex interactive nature of behaviour. Further, analysis of case studies reveals that applied work makes scant reference even to this model. For applied behaviour analysis to be essentially different from any other approach to behaviour control it must first come to terms with the complex nature of its
subject matter and then must systematically and specifically apply experimentally-based data to behaviour control problems. The remainder of this thesis constitutes an attempt to point to the important data from the experimental analysis of behaviour which have particular significance for applied behaviour analysis.

Note:

The model of behaviour described in this chapter is based in the main on an analysis of descriptions in the following books:


The basic procedural approach in applied behaviour analysis is to select a specific behaviour and to introduce consequences contingent upon its occurrence or non-occurrence such that its future probability of occurrence is either increased or decreased. Very often this procedure is adopted without explicit consideration of the interactions between behaviours. For example, analysis of all case studies published in JABA in 1975 (Volume 8) shows that 71% (N = 15) merely specified the behaviour of interest and the particular consequences arranged for it, while the remaining 29% (N = 6) described, in addition, structural or functional relations between the target behaviours and other behaviours and "side effects" either within or across subjects. I suggest that while relations between the target behaviour and other aspects of the subject's or others' behaviour may not always be of interest (in a practical sense) they are very often vital if we wish to modify complex behaviour or ensure generalizable and enduring behaviour change. Where applied behaviour analytic procedures have been found to be ineffective, one reason may well be the failure to appreciate, isolate, and manipulate important behavioural interactions. The current revived interest in ethical concerns would also argue for an increased understanding of the effects intervention may have on behaviours other than the behaviour of interest.
That behavioural interactions are widely ignored in applied work is surprising in view of the amount of data on interactions available from the experimental analysis of behaviour. In fact, much experimental work is characterized by its specific concern for interactions between behaviours. Experimental data from the areas of choice behaviour, schedule-induced phenomena, self-control and commitment, behavioural contrast and peak shift, and biological boundaries of behaviour exemplify a concern for the interactive nature of behaviour. I will examine, in this chapter, some of the more important data from one of these areas, that of choice behaviour, and illustrate, with a case study, how the assimilation of these data can increase the usefulness and extent of applied behaviour analysis.

Paralleling the failure to utilize data on interactions between behaviours is the continuing (and even more surprising) neglect of basic data on the effects of schedules of reinforcement and punishment, extinction phenomena, stimulus control procedures, etc. In view of the established reliability and generality of these data (much of the original work having been done over twenty years ago now) there can be no question of the value of applying the principles to the modification of practical human problems. Accordingly, in the following chapters, I shall outline some potential areas of application which illustrate the usefulness of basic (but as yet essentially untried in the applied field) behavioural data from a number of experimental areas.
The Experimental Analysis of Choice Behaviour and its Relevance to
Applied Behaviour Analysis

In human, as in any behaviour, an individual can generally
engage in one of several alternatives. Since, at any particular
moment, one alternative is only available to the exclusion of
others, behaviour generally implies choice. Thus, Herrnstein
(1970) was able to say that all behaviour is choice behaviour in
the sense that "at every moment of possible action, a set of
alternatives confronts an animal" (p.254). In view of this, many
of the clinical problems presented to applied behaviour
analysts may be viewed as involving (a) the investigation of the
factors which led the individual to indulge in one alternative
(presumably, the undesirable behaviour) to the partial or complete
exclusion of other (desirable) alternatives, and (b) the manipula-
tion of environmental events in such a way that the exercise of
the desirable alternatives becomes more probable. The
experimental analysis of behaviour has provided many studies
which specifically examine the variables which enter into an
organism's preference for one alternative over another. In
particular, studies of concurrent and concurrent-chains schedules
of reinforcement have provided data which demonstrate the poten-
tial which exists for the environmental manipulation of choice
behaviour. If many clinical problems can be conceptualized as
the failure to make socially appropriate choice, then the
experimental data point to ways in which we might re-arrange
environments to alter choice ratios. At the very least, they
suggest clinical experiments aimed at gathering parametric data
which are much needed to test the generality of the experimentally established relations governing choice among alternatives. I shall examine some representative experiments in this area and discuss their applicability to applied behaviour analysis.

Experimental studies of choice seek to determine what controls an organism's distribution of activities. Frequently, concurrent schedules are used to study the variables which control an organism's preference for one over another alternative. Such a schedule arrangement provides for two or more schedules operating simultaneously and independently, each for a different response. Thus, an organism may have available two response keys, each associated with a different schedule of reinforcement, and responses according to one schedule have no programmed consequences for the alternative schedule. An early study by Herrnstein (1961) examined the performance of pigeons under concurrent VI VI schedules and reported a simple relation between responses and reinforcements. He found that the equation:

\[
\frac{P_1}{P_2} = \frac{r_1}{r_2}
\]  

(1)

described this relation, where \( P_1 \) and \( P_2 \) are the rates of responding at alternatives 1 and 2, and \( r_1 \) and \( r_2 \) are the rates of reinforcement obtained from alternatives 1 and 2. The relation is, therefore, stated as: the ratio of response rates to the two alternatives equals the ratio of reinforcement rates obtained from
the two alternatives. Subsequent confirmation of this relation (e.g., Staddon, 1968; Baum and Rachlin, 1969) has been made by numerous investigators and it has been formalized as the matching law (Herrnstein, 1970).

The currently accepted form of the matching law (Baum, 1974) is expressed as:

$$\frac{P_1}{P_2} = c\left(\frac{r_1}{r_2}\right)^a$$

where $a$ the slope and $c$ the intercept, are empirically obtained. In the simplest form of matching (Equation 1) both $a$ and $c$ equal one. Equation 2 allows us to evaluate the influence of various types of factors entering into preference relations. As far as applied behaviour analysis is concerned it is important to learn what are the variables that account for the values of $a$ and $c$. The parameter $a$ describes the sensitivity with which preference changes as independent variables are changed (Lander and Irwin, 1968).

**Bias**

The constant, $c$, is called bias (Baum and Rachlin, 1969; Baum, 1974) and indicates the amount of preference for one alternative when equality of reinforcement would predict indifference. Where $c = 1$, bias does not play a part. However, in all situations, there are variables affecting preference which
are unaccounted for by the experimenter, and \( c \) measures their contribution. Fig. 3.1 (from Baum, 1974) illustrates bias. In this study, pigeons' standing on a particular side of a chamber was reinforced with food according to concurrent VI schedules. During the experiment, the frequencies of reinforcement were manipulated. Resultant data from one bird are given in the Figure and show that, when plotted according to Equation 2, all the data points fall below the simple matching line, with \( a = 0.98 \) and \( c = 0.54 \) showing a constant proportional bias towards the right-hand side.

In the context of animal studies, Baum (1974) identified four major sources of bias:

(i) response bias;
(ii) discrepancy between scheduled and obtained reinforcement;
(iii) qualitatively different reinforcers; and
(iv) qualitatively different schedules.

Response bias refers to asymmetries in the response situation. If an organism is confronted with two manipulanda, the first of which requires more force to operate than the second, the latter will be preferred even when the reinforcement rates for each are equal (see also Beautrais and Davison, 1977). Similarly, any other factors which introduce an element of difficulty or effort into performing one as opposed to another response will tend to
FIGURE 3.1:

Proportion of time spent on the left, of the total spent on the left and right sides of a chamber, as a function of the proportion of reinforcement obtained on the left. The data are from one bird in an experiment by Baum and Rachlin (1969). When graphed as the logarithm of the ratio of time on the left to time on the right versus the logarithm of the ratio of reinforcement on the left to reinforcement on the right, these data conform closely to a line with equation: $y = 0.98x - 0.27$ (see Baum and Rachlin, 1969, Figure 2). There is a constant proportional bias in favor of the right side.

[From Baum, 1974]
make responding biased in favour of that requiring least effort. Such influences as colour preferences or muscle or nervous system asymmetries also contribute to response bias. Assessment of response bias, where possible, in applied behaviour analysis may provide data on some treatment goals or priorities. Imagine that we are asked to treat someone who is very aggressive (assaultive) when making requests of a complex nature. We can, at one level, conceptualize the situation as one involving a preference for aggressive assault to try to achieve some end, as opposed to reasoned discourse with someone in authority. Suppose further that it seems that our client finds it difficult to express himself verbally and is embarrassed by this deficiency. In such a situation it may be much more effortful for him to reason than to fight. While other factors may cause his aggression at least part of the cause may lie in the bias towards the response requiring less effort. If this was our conclusion, our first treatment priority might be to make verbal requests less of an effort by teaching the client how to make such requests. We may relegate procedures designed to decrease his aggressive behaviour itself to a secondary position. The point is that knowledge of response bias helps us to make decisions on priorities and may suggest ways of altering response requirements.

Another source of bias arises because of discrepancies between scheduled and obtained reinforcement. In any experiment, the scheduled parameters of reinforcement will never be those experienced by the organism. Pausing during responding,
inappropriate responding, and less than full utilization of reinforcement periods when available, ensure that obtained reinforcement rates are always somewhat less than those scheduled. Since the matching law applies only to obtained reinforcement (Herrnstein, 1970), bias will be present unless the actual values occur in the same proportion as the scheduled values. This type of bias is cited more as a warning to applied behaviour analysts than as a prescription for remedy. Because of the much more complex and less (experimenter-) controlled nature of human behaviour, there is often likely to be some significant difference between scheduled and obtained reinforcement. In most cases, it is difficult to see how this can be overcome, but it does indicate the necessity for adequate observation and control if the full potential of applications of experimentally-derived principles is to be realize. Further, such differences between scheduled and obtained reinforcement may account for some of the differences between patterns of human- and lower-organism responding on concurrent schedules.

Hollard and Davison (1971) examined another potential cause of bias, the availability of qualitatively different reinforcers. The obtained reinforcing values of two qualitatively different reinforcers are not known in advance of empirical test, but it might be expected that n-sec access to food might differ in value from n-sec access to a receptive sexual partner. Under constant conditions of deprivation, however, the relative values of the two reinforcers should remain constant. Examining choice between
food and brain stimulation, Hollard and Davison found that the ratio of the values of the two types of reinforcers was constant when the rates of the two reinforcers were varied. Thus, qualitative differences between reinforcers enter into the matching relation in the same way as differences in amount of reinforcement. Once again, these data suggest to applied behaviour analysts a variable which may be altered to change preference. Obviously, the trading off of an amount of one reinforcer with a amount of a qualitatively different one will be an empirical matter to be decided in each individual case. But the search for reinforcers for an individual's behaviour which are qualitatively superior to those maintaining an undesirable behaviour may pay dividends when applied to a desirable alternative.

The final source of bias discussed by Baum (1974) arises from the qualitative differences between concurrent schedules. In concurrent arrangements where both schedules are qualitatively the same, for example conc VI VI (Baum and Rachlin, 1969), or conc Fl Fl (White and Davison, 1973), response patterns maintained by both schedules are typical of those maintained by each in isolation, and the values of both c and a in Equation 2 are close to 1.0. Using conc Fl VI schedules, however, Trevett, Davison, and Williams (1972) found that c was less than 1.0 when schedule 1 was the Fl schedule. They showed that when VI and Fl schedules with the same mean interval are arranged concurrently, there is a constant proportional preference for the former over the latter schedule. A replication of the Trevett et al. study and an
earlier experiment by Nevin (1971), provided further evidence for the preference for VI over FI schedules when programmed concurrently (Lobb and Davison, 1975). Lobb and Davison also showed that the value of $a$ is larger for conc VI VI performance than for conc FI VI performance.

La Bounty and Reynolds (1973) studied performance under conc FI FR schedules and found that, on ratio measures (Baum, 1974), all of their six pigeons showed a preference for the FI schedule as opposed to the FR schedule. As the FR requirement was increased, FR response rates decreased and FI response rates increased. If this relation holds true for the human case, it would suggest that given a performance maintained on an FI schedule which we want to increase and another maintained on an FR schedule which we want to decrease, one way of achieving our ends would merely be to increase the size of the ratio until FR response rates began to decrease.

Type of Reinforcer

If data such as these are to be useful in applied settings, it will be necessary to know how the use of qualitatively different reinforcers interacts with choice between concurrent schedules. Catania (1966) has suggested that concurrent performances may be controlled partially by the type of reinforcers used. Wood, Martinez, and Willis (1965) have examined this possibility by comparing performance on conc FI food FR food
schedules with performance on conc FI food FR water schedules. They found a direct relation between the ratio requirement and interval response rates when both responses were reinforced with food, but no explicit relation when different reinforcers were in effect. As Wood et al. observe, considerable data are now required on schedule, stimulus, schedule value, and deprivation effects on this phenomenon, but their data do point to the importance of evaluating interactions between reinforcement parameters used in concurrent programming. Since the reinforcers available for human performance in the free environment are often likely to be qualitatively different, such data are of importance before information on qualitatively different schedules can be effectively utilized in applied behaviour analysis.

Amount of Reinforcer

Numerous other quantitative relations have been well documented in research with concurrent schedules, and their consequences for applied behaviour analysis must be evaluated. Using equal VI VI concurrent schedules, Catania (1963) investigated the effects on performance of scheduling differing amounts (3- vs 6-sec) of a reinforcer provided by each schedule. He found that the response ratio matched the ratio of reinforcement magnitudes. The same study illustrated another important relation. Catania found that when responding on a single key was reinforced by a VI schedule, response rate was not affected by reinforcement duration. It was only when a second alternative
was added to the situation that reinforcement duration became a relevant variable. It is data on such effects which, if utilized in executing applied behaviour analysis programmes, will vastly increase our understanding of the potential effects of clinical procedures. A treatment variable may well be ineffective unless we offer a concurrent, alternative response. Knowledge of such interactions provided by data from experimental analysis allows us to programme such an arrangement deliberately.

Temporal Distribution of Reinforcers

Within a specified period it is possible to deliver a given quantity of a reinforcer in frequent, small amounts or in less frequent, large ones. When reinforcement is contingent upon a particular response, fewer responses lead to reinforcement in the latter than in the former situation, but each reinforcer is of greater magnitude. In deciding how to apportion and deliver reinforcers in the applied situation, it is of some importance to know how the level of responding will be related to the quantity of the reinforcer. For example, it may be easier to arrange for large, infrequent deliveries of a reinforcer because of difficulties in having continuous access to the patient, but we would want to know whether convenience will serve the interests of response maintenance. Schneider (1973) has examined the problem using pigeons in a concurrent situation in which a single VI schedule allotted successive reinforcements to one or the other of two keys. Both the number of reinforcers assigned and the number of pellets
delivered during each reinforcement, differed from each key. The results showed that the temporal distribution of the quantity of reinforcer associated with each alternative determined the distribution of responses. Responding to a particular key was maintained at a higher rate when the allotted quantity of reinforcer was delivered in frequent, small amounts than when it was delivered in less frequent, larger amounts. It remains to be demonstrated that this relation holds for human behaviour, but these data suggest that, in applied behaviour analysis, a particular amount of a reinforcer will be more effective if delivered in frequent, small amounts.

Most studies of concurrent-schedule choice have used a fixed duration of reinforcer and have not investigated the effects of providing variable durations or amounts of reinforcer. Essock and Reese (1974) found that higher rates of responding were maintained by variable durations of reinforcement than by fixed durations in both multiple and concurrent schedule arrangements (although the data were clearer for the multiple case). This study indicates another variable which may be potentially manipulated by the applied behaviour analyst to increase behaviour rates in clinical situations. Again, although parametric data are required for the human case, the data suggest that delivering reinforcers of variable durations (and possibly of variable amounts) may help maintain a high response rate.
Deprivation

Another factor which can influence the distribution of responding across alternatives is deprivation. If an organism has available to it two responses, both of which will produce reinforcer A, its distribution of responses will conform to the matching law, other things being equal. As it receives more of reinforcer A, the organism will become satiated with respect to it and it will, accordingly, diminish in reinforcing value. This change will affect both alternatives equally and the choice ratio will remain unchanged, although the absolute level of responding will decrease. (Baum, 1972) has demonstrated that this holds true by having a pigeon living continuously in an experimental chamber and obtaining all its food by pecking at two keys). If the reinforcers are qualitatively different, however, satiation could become a crucial variable. If an organism satiates more rapidly to reinforcer A than to reinforcer B, preference will progressively move towards the alternative producing reinforcer B. Information, where obtainable, on the speed of satiation with respect to reinforcing events is potentially an important part of selecting reinforcers for applied behaviour analysis programmes. If the aim of a programme is to increase an individual's preference for a (desirable) alternative at the expense of his present (undesirable) behaviour it is important to ensure that the reinforcer chosen for the target behaviour will not satiate more quickly than the reinforcer for the ongoing behaviour.
Immediacy

Another variable which may be manipulated to change preference is the immediacy of reinforcement. Using conc VI VI schedules, Chung and Herrnstein (1967) arranged for a delay (blackout) to occur between the response to the schedule which had timed the interval (making a reinforcer available) and the delivery of the reinforcer. They found that the greater the immediacy of reinforcement (defined as the reciprocal of the delay), the more probable the choice for that alternative. Thus, in applied settings, it is suggested that preference may be changed, or other preference-changing devices enhanced, by arranging for a delay to be interposed between the undesirable response and its reinforcer or by arranging for increased immediacy of reinforcement for the desirable response.

Complex Situations

The application of concurrent-schedule performance data to applied behaviour analysis may appear to be relatively limited in view of the fact that only two simple alternatives are used as the basis for these extrapolations. Extensions of the matching law to more complex situations, however, have increased the confidence with which we may make predictions for complex human behaviour. Catania (1966) argued that the matching law also applies to preference relations between more-than-two alternatives. This contention is based on data reported by Reynolds (1963) using three-key concurrent-chain schedules, with VI 3-min initial links, and
terminal links of either 30-sec of time-out or an FR 25 schedule. (In concurrent-chains schedules, initial responding does not lead to reinforcement itself, but rather to further situations in which reinforcers are available. In a typical arrangement (Autor, 1960) an organism responds on two concurrently available keys (each of which is illuminated by the stimulus associated with the initial link of one of the chains) until a response-contingent stimulus change on one of the keys signals the beginning of the terminal-link schedule on that key. Responses in the presence of either of the mutually exclusive terminal-link stimuli produce access to a primary reinforcer according to some schedule. Preference for the terminal-link schedules is measured by the ratio of responses emitted to the initial-link schedules). In part of Reynolds' experiment, one key was programmed to deliver twice the reinforcement rate of a second key, with a third key delivering reinforcers at a rate equal to the sum of that on the other two keys. Later, the third key was covered and provided no reinforcement. Even in the absence of this key, the first key continued to maintain double the responding on the second key. These data were taken by Catania (1966) as evidence for the principle of indifference from irrelevant alternatives (Luce, 1959), which states that "the probability that one of two alternatives will be chosen over another is not affected by the introduction or elimination of additional alternatives" (Catania, 1966, p.240). Thus, we could expect that changing the rate of reinforcement for one alternative would not change the response ratios between two other alternatives.
with constant reinforcement rates. It is important to know how generally this principle holds if we are concerned about the effects on other concurrent behaviours of introducing contingencies for maladaptive behaviours in applied settings. Imagine, for example, a clinical setting in which three behaviours are the alternatives in question. Alternatives A and B are maintained by constant reinforcement rates and are desirable behaviours. Alternative C is undesirable and we intend to reduce its frequency by removing its reinforcers. It is essential for the person's social adjustment that A and B continue to occur in the same ratios. Will reducing responses to alternative C affect these ratios? If, as the principle of indifference from irrelevant alternatives suggests, that ratio of responses to A and B will remain constant we may perhaps (other considerations having already been taken into account) have some justification in simply removing the favourable consequences for alternative C. If, however, changing the frequency of responses to C will change the response ratios for A and B, we will either have to seek other treatment avenues or programme consequences for A and B which will work to maintain equilibrium in their response ratios. The important point being made here is that only with the sort of data provided by Reynolds (1963) are we in a position to base such treatment decisions in an empirical framework. If, because we lack such data and thus do not consider the problem, our programmes either fail to produce beneficial changes or, worse, produce harmful changes, it may often be because treatment priorities and areas to be programmed have not been properly evaluated in this
manner.

What is the generality, then, of the law of indifference from irrelevant alternatives? Davison and Temple (1974) studied preference using a three-key concurrent-chains schedule, and varied both initial- and terminal-link values. In the first part of the experiment, they held all initial links constant at VI 60-sec, the FI terminal-link schedules on keys 2 and 3 were always FI 25-sec and FI 15-sec, respectively, and the terminal link on key 1 was varied between 5- and 40-sec. In part two, the initial links on keys 2 and 3 were kept at VI 60-sec and the initial link on key 1 was varied from VI 30-sec to VI 180-sec. They found that relations in the three-key procedure were not those found in a two-key concurrent-chain using the same type of schedules. In other words, the addition of another alternative changed preference between the original two alternatives, thus questioning the generality of Catania's (1966) conclusions.

However, two recent studies provide additional strong support for the principle of indifference from irrelevant alternatives. In the first of these, Davison and Hunter (1976) studied pigeons' responding on VI schedules arranged on one, two, and three response keys. They found that the response ratios between two, constant reinforcement rate, keys were not changed by varying the reinforcement rate on the third key. Response allocation measures (a and c in Equation 2) were independent of the presence or absence of a third concurrent schedule. Thus, the simultaneous
or concurrent context in which a performance occurred did not affect response allocation in pairs of concurrent VI schedules.

**Context Effects**

This type of analysis has also been applied to multiple VI VI schedules with another VI schedule arranged concurrently with both multiple schedule components. A re-analysis of Pliskoff, Shull, and Gollub's (1968) data by Lobb (1975) indicates that the successive context in which it occurs does not affect response allocation in concurrent schedules (cf. Davison and Hunter, 1976). Response allocation in a multiple-schedule was affected, however, by its concurrent context. Increasing the reinforcement rate of the common schedule resulted in relatively more responses being allocated to the component with the higher reinforcement rate.

Lobb and Davison (1977) have extended this analysis further in an experiment designed to study context effects in multiple and concurrent schedule performance, and we shall consider their data in detail soon. They assert that "in the experimental analysis of behaviour an important question is the generality of a relation between variables - that is, how far is a specified relation independent of contextual or other environmental manipulations" (Lobb and Davison, 1977). If this question is important to experimental analysis, how much more important it must be in determining the effects of applied behaviour analysis. If experimental data demonstrate a simple relation between
reinforcement rate and response rate it is important to know whether or how reinforcement delivered at other places or other times will affect that relation. It is unlikely in an extra-laboratory setting that we will encounter a situation which exemplifies either a simple concurrent or multiple schedule arrangement. All behaviour occurs in some context and it is vital that we appreciate how different context will affect the nature of our behaviour change efforts. If we were to base the main part of a behaviour change programme on applications of the matching law we must know what are the consequences for this programme of both successive and concurrent schedule contexts. If the relation is independent of such contexts, we have empirical justification for initiating the programme without taking steps to counter any such interactions.

(Note the importance of such empirical justification. We may already successfully apply certain techniques without such data and may thus seem little further advanced by giving approval to the status quo. However, there are two important reasons for seeking such information. First, we have an ethical responsibility to know why we are doing what we do and to be aware (as much as possible) of all likely consequences. Second, where applied behaviour analysis is at present of limited applicability or has negative results, it is quite possibly because of our neglect to appreciate effects of consequences not directly involved in the programme. It is only by taking note of experimental data and assessing how they may be integrated with common clinical
situations that we will be making rational and responsible decisions).

If the relation is not independent of context, we are at least aware of the fact and can take it into consideration when assessing the benefits and costs of one manipulation over another. Further, it is now possible, given that a situation involves manipulations which are responsive to successive or concurrent contexts, that we can take steps to enhance, maintain, or negate the interactive effects. This area is a fertile one for illustrating the ideal interplay between experimental and applied behaviour analysis. Basic experimental work should suggest ways of dealing with behaviours in practical settings. Conversely, clinical problems should give rise to queries which may be investigated at the animal-experimental level.

It is important to analyze the effects of context on both response ratios and response rates. It is often of interest to know whether either or both of these measures change with changes in conditions at other times and places, and the extent of such changes if they occur. Davison and Temple (1974) provided such data on changes in response rates and ratios in a three-key concurrent-chain situation. As noted earlier, they reported some changes in preference (response rate ratios) when a two alternative situation is extended by the addition of another response. Their data also illustrated how response rates are
sensitive to changes in such a situation. In Part 1 of their study, initial-link responding decreased on Key 1 and increased on Keys 2 and 3 as the terminal-link schedule on Key 1 was lengthened. When the chain on Key 1 was absent, initial-link response rates on Keys 2 and 3 were higher than when it was present. (Reynolds, 1963, also reported that absolute response rates increased with the removal of the third alternative). In the terminal links, the response rate on Key 1 decreased as the value of the FI schedule on that key increased. Terminal-link response rates on Keys 2 and 3, however, were not significantly affected by the presence or absence of the chain on Key 1. In Part 2 of this experiment, the initial-link schedule on Key 1 was varied while the terminal-link schedules remained constant. Under these conditions, initial-link response rates on Key 1 decreased as the VI schedule increased in value, while initial-link response rates to Keys 2 and 3 (both VI 60-sec schedules) increased. These interactions illustrate how aware we must be of changes in related behaviours caused by attempts to modify a specific behaviour. It will be necessary to collect much more data over different schedule values and data on human performance under controlled conditions before we can justifiably extrapolate from these data to clinical situations. However, the techniques are available and should urgently be used to increase the power of applied behaviour analysis.

Further data from the Lobb and Davison (1977) study extend our knowledge of similar effects. Their experiment specifically
examined context effects in multiple and concurrent schedule performances. Pigeons were trained on mult VI VI schedules and their performance measured in the presence of absence of another (common) VI schedule arranged concurrently with both components. Part 1 of the study investigated the effects of varying the common schedule when the multiple-schedule components were set at VI 45-sec and VI 15-sec. As the common schedule reinforcement rate was increased, response rates increased in the common schedule components, and decreased in the multiple-schedule components (thus showing concurrent interactions, i.e., the interactive effect of reinforcement at other places). A bias towards multiple-schedule responding was evident from the fact that, even when the common schedule provided a higher reinforcement rate than one of the multiple-schedule components (VI 30-sec vs VI 45-sec) the response rates in the multiple schedule were always higher than those in the common schedule components.

In Part II, the common schedule was held at VI 60-sec, while the multiple components were varied. As the multiple-schedule reinforcement rates were increased, the response rates in that multiple-schedule component increased, while the response rates in the concurrent common components decreased, once again demonstrating concurrent interactions. However, the response-rate decrease on the common schedule caused by concurrent interactions was less than the response-rate increase on the multiple schedule itself (see also Davison and Hunter, 1976).
In the third part of the study, the common schedule was not included and the multiple-schedule reinforcement rates were varied as for Part II. It was found that response rates in the multiple-schedule components varied in a similar manner whether or not the common schedule was present.

Finally, in another part of the experiment, the reinforcement rate in only one component of the multiple schedule was varied, resulting in an increase in response rate in that component as the reinforcement rate increased, and a corresponding (though smaller) decrease in response rate in the alternated multiple-schedule component. In the common schedule component concurrent with the varied multiple-schedule component, response rate decreased when the multiple reinforcement rate increased. The response rate in the successive common component did not differ significantly with changes in the multiple reinforcement rate.

Taken together, these data clearly showed both multiple and concurrent interactions occurring at the same time and supports Herrnstein's (1970) contention that the concurrent interactive effect is the larger of the two. However, response rate in the component both successive and concurrent to a changed component was unaffected by the reinforcement rate change. Lobb and Davison (1977) suggested that this could well apply to other situations in which more than one component response rate might remain unchanged, e.g., in two-component multiple three-key concurrent schedules.
Equation 2 may be assessed as it applies to the two concurrent schedules comprising the multiple concurrent schedule. Davison and Hunter (1976) found the rules of response allocation (a and c in Equation 2) were unaffected by the presence or absence of a third concurrent schedule. Lobb and Davison (1977) showed that the same rules apply when other schedules occur successively to, rather than simultaneously with, concurrent VI schedules.

On the basis of these data we can at present conclude that, apart from bias, concurrent schedule response allocation is unaffected by both the successive context in which it occurs and the concurrent context in which it occurs. The slopes of the lines fitted to response allocation in multiple schedules were similar whether or not a common schedule was available, and were less steep than those fitted to the concurrent-schedule data. We can conclude that multiple-schedule response allocation is unaffected by the provision of a common reinforcement schedule. Further, multiple-schedule performance is less sensitive to reinforcement rate-changes than is concurrent-schedule performance.

**Concurrent-Chains Schedules**

Concurrent schedules are a convenient way to study preference for many pairs of alternatives. However, they are limited by two factors. The first problem is methodological. It is possible that the data from examination of preference for two widely different concurrent schedules may be confounded by the rates of
responding generated by the schedules themselves. For example, schedule-generated response rates may well confound measures of preference for VI vs differential reinforcement of low rates (DRL) schedules. (However, note here that there is evidence that rates both do (Schuster, 1969; Arnett, 1972) and do not (Killeen, 1968; Neuringer, 1969) affect choice). Second, in clinical settings, it is possible that we may also wish to extend the concurrent situation to something more complex. While simple, concurrent alternatives often face humans, frequently the situation is not so straightforward. The use of concurrent-chains schedules goes some way towards meeting both of these difficulties. Response rates generated by schedules arranging primary reinforcement are not a contaminating factor in choice because preference is assessed on the basis of initial-link responding, and is thus more isolated from the effects of particular terminal-link schedules. Further, such an arrangement adds another dimension (responding to enter a situation in whose presence further responding leads to access to a reinforcer) which may more closely approximate some human choice situations.

Early experiments using concurrent-chains schedules (Autor, 1960; Herrnstein, 1964) found that the matching relation applied to performance under these schedules, i.e., that the ratio of initial-link response rates matches terminal-link reinforcement rates. However, later studies showed that the adequacy of the fit of the data to the matching relation was largely a function of the limited range of schedule values used. Fantina and his
co-workers (Fantino, 1969a, b; Squires and Fantino, 1971) suggested that preference in the concurrent-chains situation was controlled by the relative reduction in the expected time to reinforcement signified by each of the stimuli (and their associated schedules of reinforcement) being chosen. Thus, if equal VI schedules with short average inter-reinforcement intervals were arranged in the initial links, Fantino would predict preference for the preferred alternative to be great; but if the initial links were equal VI schedules with long average inter-reinforcement intervals, the model would predict relative indifference between the alternatives. Wardlaw and Davison (1974) confirmed these predictions with data from an experiment in which the values of both the equal initial-link VI schedules were varied. They showed that preference for a particular terminal-link schedule combination was greater, the shorter the initial-link schedules. Once again, a power relation of the form of Equation 2 (Wardlaw and Davison, 1974) best describes data from concurrent-chains experiments. Further data (Wardlaw and Davison, unpublished) indicate that as the size of the initial-link schedules is reduced until only a single response is required, preference becomes more and more sensitive (shown by a large value of a in Equation 2) until it becomes almost exclusive to the alternative with the shorter time to reinforcement. This finding may help explain how, in a clinical problem, an undesirable behaviour, which appears to have only a very marginally larger reinforcer than a desirable one, is almost exclusively preferred. It does not necessarily have to be the magnitude of the reinforcers which is the important
factor in choice. It may simply by the type of choice situation which determines exclusivity. This indicates that a full knowledge of choice factors must include some measure of the sensitivity (i.e., the value of $a$).

Of course, these data are not only useful in suggesting variables to manipulate to avoid exclusive choice for an alternative. They also suggest how, when desirable, we may engineer such preference. If we do not have access to powerful or different reinforcers we are still able to change preference by manipulating the conditions of choice rather than altering reinforcers directly.

How will preference between terminal-link schedules be affected by having unequal initial-link schedules? Davison (1976) has studied the effect of unequal VI initial links on FI terminal-link performance. He showed that the rate of change of preference is greater when a shorter initial link leads to the longer terminal-link schedule, than when the shorter initial link leads to the shorter terminal links. Thus, manipulating the relative size of the initial-link schedule is another way of manipulating preference for an alternative (as opposed to manipulating the absolute size of the initial links).

Thus, the value of initial-link schedules should always be assessed in attempting to change preference. Any manipulation which results in preference for one of two terminal-link schedules
(for example, by increasing the duration of primary reinforcement on one terminal link or punishing behaviour on the other) should be more effective the smaller the size of the initial-link schedules. Therefore, as either a preference-changing mechanism itself or as an adjunct to other measures, the importance of the initial links should not be overlooked.

Another variable which enters into choice in concurrent-chains schedules involves the number of links in the chains (Duncan and Fantino, 1972). Early work by Findley (1962) showed that with simple chain schedules (i.e., a schedule in which reinforcement is contingent upon the successive completion of the requirements of two or more component schedules, each of which is correlated with a different discriminative stimulus) performance in the earlier links of the chain becomes harder to maintain the larger the number of links in the chain. With too many links it is not possible to maintain any behaviour at all. It seems logical, then, that (other factors being equal) in a concurrent-chains situation, where the number of links on each chain is different, an organism would prefer that having fewer links. Duncan and Fantino (1972) found that, indeed, when the inter-reinforcement intervals were equal in the terminal links, pigeons preferred a simple FI terminal link vs a two-component chained FI schedule, and a two- vs a three-component chained FI schedule. The preference could be reversed, however, by ensuring that the simple FI was sufficiently longer than the chained schedule. Thus, both number of links in the chain and time to reinforcement
interact to produce preference in this situation. Once again, these data indicate:

(i) reasons for choice of one alternative over another; and
(ii) ideas for changing a problem preference.

Thus, in an applied setting, we may look at the number of links in a chain of behaviours when assessing how to make a desirable behaviour more likely. A number of options are open to us. We may be able to increase the number of links on the chain involving a problem behaviour in order to decrease its probability of occurrence. We may decrease the number of links on the chain involving a desirable behaviour to increase its frequency. Or we may increase the length of the simple schedule involving an unwanted behaviour (in a simple schedule vs chain situation) in order to decrease its occurrence.

There may sometimes be situations confronting the applied behaviour analyst in which it is undesirable or impractical to alter both the values of the initial-link schedules, and the ratios of the terminal-link schedules. How can we change preference under such conditions? MacEwen (1972) has published data which suggest one answer: he found that choice proportions for a shorter VI terminal link increased monotonically as the absolute size of the terminal links was increased. He also found analogous results for FI terminal links.
The Application of Choice Data

I have shown that data from experiments on the matching law enhance our understanding of the variables entering preference relations and thereby suggest operations by which preferences may be altered away from undesirable alternatives towards desirable ones. An area of application which immediately arises in the context of choice is that of self-control (Rachlin, 1974). Recent attempts (Rachlin and Green, 1972; Ainslie, 1974) have shown that the principles of the matching relation can be used to predict and manipulate commitment to a course of action, and thus self-control. Since many of the goals of applied behaviour analysis may be conceptualized as self-control, such an application of experimental data is of universal appeal. In this section, the experimental background to the analysis of self-control will be outlined and then a clinical example will be discussed in detail which shows how these experiments lend themselves to clinical application.

A major problem with all therapies is frequently that of the patient's commitment to partake in therapy or to engage in activities prescribed by the therapist. Suppose, for example, that the long-term consequences of a particular therapy are beneficial (for example, a problem behaviour will be removed and adaptive behaviour substituted), but occasionally (or even often) the value of not keeping appointments or not carrying out therapeutic exercises rises above that of keeping appointments and doing the exercises. Unfortunately, it is at those times when it is
possible to treat the problem (e.g., an arranged therapy period) that the short-term aversive consequences outweigh the long-term positive consequences (e.g., going to see the therapist may interrupt a social outing), and the value of not continuing in treatment or not carrying out assignments rises above doing those things. At times other than this (that is, in the presence of stimuli which signal the occurrence of the problem behaviour), the value of treatment again rises to a high level. In such situations, a commitment procedure may be used to change the influence of short-term aversive consequences. The patient is offered a commitment at the time the value of treatment is high, i.e., in close temporal association with the occurrence or negative consequences of the problem behaviour.

Fig. 3.2 (adapted from Rachlin, 1974) diagrams such a commitment decision process.

There are two possible decisions diagrammed in Fig. 3.2. Decision X might represent a time when a problem behaviour is not occurring, when the patient is able, say, to go to an enjoyable social function, and he has to decide whether or not to keep his appointment with the therapist. Decision Y is more divorced from immediate consequences and may take place, for example, in a problem situation where the patient desires treatment, or at the end of a fruitful therapy session where the patient's expectations of success are high and he is highly probable to report he wants to continue treatment to speed his "recovery". Choice at point X
FIGURE 3.2:

Flow diagram of commitment to be positively involved in therapy. Choice at X is between keeping a therapy appointment or carrying out therapeutic instructions and not doing those things. It is assumed that a patient would take the negative course at X. Choice at Y is between having a choice later (top arm) and being forced to keep appointments or carry out instructions later (bottom arm). A patient who would choose not to keep an appointment at X might nevertheless commit himself to keep appointments by choosing the lower arm at Y.

[Adapted from Rachlin, 1974]
FIGURE 3.2

CONTINUE IN THERAPY
KEEP APPOINTMENT
CARRY OUT THERAPEUTIC
INSTRUCTIONS

DON'T CONTINUE IN
THERAPY
DON'T KEEP APPOINTMENT
DON'T CARRY OUT
INSTRUCTIONS

CONTINUE IN THERAPY
KEEP APPOINTMENT
CARRY OUT INSTRUCTIONS
represents a decision about doing and not doing. Here it is more probable that the short-term consequences will control behaviour and not keeping the appointment will result. Choice at point Y, however, removes the later decision. The decision now is whether to agree to commitment. Having committed himself, the patient essentially removes the option of not doing. As discussion of Rachlin and Green's (1972) and Ainslie's (1974) experiments will show, it is possible to manipulate the temporal structure of the situation so that at the time Y the value of treatment will be higher than that of no treatment and the patient is highly likely to agree to commit himself to treatment.

It is easily demonstrated that preference changes as a function of the time the choice is made. Chung and Harrnstein (1967), as mentioned earlier, showed that reinforcers are preferred in inverse proportion to their delay. It follows, therefore, that if a reinforcer were available at time T and an alternative five times as great at T + 5-sec., a subject choosing at T-5-sec would choose the larger, later reinforcer, while a choice at T-1-sec would result in a preference for the smaller, earlier one.

The problem we face with keeping a person in therapy, then, may often be that preference changes with time. Any method which leads to the larger, later reinforcer (in our example this might be treatment success) must involve some way of either preventing preference from responding to the increasing proximity of the
smaller, earlier reward, or must keep the person from acting on the change in preference. Commitment is one possibility for achieving this end.

Rachlin and Green's (1972) study closely paralleled the situation diagrammed in Fig. 3.2. There, pigeons chose at X between a small, immediate reinforcer and a larger reinforcer delayed by several seconds. The small, immediate reinforcer at X was almost exclusively preferred. However, when they could choose to restrict future alternatives (at Y, several seconds before X) the pigeons chose not to have a choice. That is, they preferred the bottom branch of Fig. 3.2 and, as a consequence, received the larger, but delayed, reinforcer. In other words, "commitment restricts choice so that behaviour will automatically conform to long-term consequences" (Rachlin, 1974, p. 104).

In the Rachlin and Green (1972) arrangement, the pigeons were forced to make a choice at X. The procedure did not allow the subject to not make a commitment and still proceed. This, of course, is not analogous to many human situations in which we have the opportunity to make a commitment, but must not necessarily do so. The person with a problem may make a commitment to seek professional help and may then be more likely to do so. But if he neglects to make the commitment he is not thereby prevented from reaping the consequences of his decision.

Ainslie's (1974) experiment added the condition which more
realistically parallels the human situation. In this study, pigeons were trained to peck at a key which could be illuminated either red or green. The procedure utilized discrete trials, beginning with the key being lit green for 7.5-sec. If no responses were made in this period, the key was darkened for 4.5-sec, and then illuminated red for 3-sec. If still no response had been made, the keylight was turned off and the pigeon was given 4-sec access to grain. If a response was made during the green stimulus, the key was darkened and the pigeon prevented from entering the red phase later in the trial. During the last 4-sec of the trial, the pigeon was again allowed access to grain for 4-sec. A peck to the red key immediately produced 2-sec access to the reinforcer. The key then remained darkened and no further access to the reinforcer was possible for the remainder of that trial. In other words, the appearance of the red key presented the pigeon with the choice between a small, immediate reinforcer and a larger, delayed one. The pigeons were more often than not unable to resist the temptation of the immediate reinforcer. However, a response to the green key prevented the temptation of the red key being faced and some of the pigeons learned to peck during green on up to 90% of the trials. Ainslie's arrangement provides an option not to take the opportunity of commitment when it occurs. Some pigeons did ignore it, the red stimulus arrived as programmed, and they invariably responded to get the immediate reinforcer. Rachlin and Green's (1972) data shows every pigeon would ignore the commitment procedure if the green occurred immediately before the
red stimulus. But the longer the period between the opportunity to commit oneself and the choice situation, the more likely is commitment (cf. Wardlaw and Davison's 1974, conclusion that choice is less extreme the longer the size of initial-link schedules on concurrent chains) and, hence, the better is self-control. It seems, then, that commitment may increase the probability of self-control because of the dynamics of reinforcement (Brown and Herrnstein, 1975).

Obviously, we must investigate the effects of many parameters before we can widely apply the results of these sorts of data. As Rachlin and Green (1972) point out, their procedure is analogous to a payroll savings plan in which, because a savings commitment makes cash not available immediately, a person may prefer the long-term benefits of saving over the short-term one of spending ready cash. In the Rachlin and Green experiment, and in the savings plan, once the commitment is made, the organism is removed from further temptation. However, it is not in all choice situations that a prior commitment inevitably leads to self-control. The search, then, must be for those parameters (for example, combinations of delay and amount of reinforcement) which will produce self-control through the mere dynamics of reinforcement. These factors may be utilized as treatment itself or, probably more importantly, to bring behaviour under the control of long-term consequences which, once experienced, will maintain behaviour themselves (Rachlin, 1974).
A practical application of a commitment procedure of the latter type is detailed in the following case study. 'B' had been arrested on numerous occasions for indecent exposure and had served terms of probation and imprisonment for these offences. A treatment programme involving "controlled" exposures (as described by Wardlaw, 1975) was being employed with only marginal success. The major obstacle to treatment was that 'B' would invariably consider that merely coming to see the therapist was sufficient and successful "treatment". He felt that he was making progress during therapy sessions and could not be persuaded of the necessity to complete the whole programme of practice exhibitions at home (which he was assigned to carry out every hour, on the hour during his normal waking hours). The assistance of Mrs. 'B' was solicited to record the frequency of 'B's compliance with instructions. 'B' was unemployed at the time of treatment and was at home much of the time, thus facilitating data collection. The data (shown in Table 3-1) showed that 'B' was not carrying out the assignments very often, but was still exhibiting and still had frequent erotic urges to exhibit. (These data are 'B's own records. The frequency of practice exhibitions are also 'B's, but his wife agreed with these observations on 98% of the occasions). It was reported by both 'B' and his wife that immediately following an incident of indecent exposure, 'B' remorsefully claimed that he wished he had followed instructions and vowed to do so in future. He typically kept to the programme for only three or four successive, hourly practice exhibitions before he again decided it was unnecessary to
continue.

Table 3.1 shows 'B's recordings for the various measures (indecent exposure, "practice exposure", urges to expose) over the course of treatment. The frequencies are daily averages for each week. The first two weeks were baseline records, before treatment commenced. They reveal a high number of urges to exhibit and two-three actual exhibitions each day. ('B' reported that he had exhibited hundreds of times for every time he was apprehended). Following baseline, the practice exhibition technique as outlined by Wardlaw (1975) was instituted for three weeks. Table 3.1 shows that 'B' carried out few of the practice assignments, and that both urges and exhibitions decreased only insignificantly. At this point it was decided to make the choice more salient by introducing a commitment procedure. As noted earlier, 'B' habitually told his wife after each incident of exhibiting, and was full of remorse about not following instructions, and good intentions for the future. It is at this point that commitment is most likely. 'B' was instructed to read a prepared statement to his wife as soon as possible after each exposure incident. This statement stressed the importance of regular practice sessions and the consequences of not carrying out instructions, and ended with a promise to begin practice exhibitions on the next hour and to continue hourly thereafter (until told to stop by the therapist). He was also asked to sign a contract specifying that he would follow instructions. Furthermore, 'B' was told to read the statement to himself
<table>
<thead>
<tr>
<th>Treatment Phase</th>
<th>Week</th>
<th>Indecent Exposures</th>
<th>Urges to Expose</th>
<th>Controlled Exposures</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baseline</td>
<td>(1)</td>
<td>2</td>
<td>13</td>
<td>n/a</td>
</tr>
<tr>
<td></td>
<td>(2)</td>
<td>3</td>
<td>16</td>
<td>n/a</td>
</tr>
<tr>
<td>Treatment</td>
<td>(3)</td>
<td>2</td>
<td>12</td>
<td>3</td>
</tr>
<tr>
<td></td>
<td>(4)</td>
<td>1</td>
<td>12</td>
<td>5</td>
</tr>
<tr>
<td></td>
<td>(5)</td>
<td>2</td>
<td>9</td>
<td>2</td>
</tr>
<tr>
<td>Treatment plus commitment procedure</td>
<td>(6)</td>
<td>1</td>
<td>12</td>
<td>10</td>
</tr>
<tr>
<td></td>
<td>(7)</td>
<td>0</td>
<td>6</td>
<td>12</td>
</tr>
<tr>
<td></td>
<td>(8)</td>
<td>0</td>
<td>5</td>
<td>12</td>
</tr>
<tr>
<td>Baseline</td>
<td>(9)</td>
<td>0</td>
<td>3</td>
<td>n/a</td>
</tr>
<tr>
<td></td>
<td>(10)</td>
<td>0</td>
<td>6</td>
<td>n/a</td>
</tr>
<tr>
<td></td>
<td>(11)</td>
<td>0</td>
<td>5</td>
<td>n/a</td>
</tr>
<tr>
<td>Follow-up</td>
<td>(3 mths)</td>
<td>0</td>
<td>0</td>
<td>n/a</td>
</tr>
<tr>
<td></td>
<td>(6 mths)</td>
<td>0</td>
<td>0</td>
<td>n/a</td>
</tr>
<tr>
<td></td>
<td>(9 mths)</td>
<td>0</td>
<td>0</td>
<td>n/a</td>
</tr>
</tbody>
</table>

**TABLE 3.1:**

Numbers of daily indecent exposures, urges to expose, and controlled exposures for Subject 'B'. The data are daily averages for each week.
immediately before each hourly practice. The effect of the addition of this commitment procedure is shown in Table 3.1 (Weeks 6-8). Practice exhibitions increased over the next three weeks until in the third week 'B' was practicing every hour on the hour for most of his waking hours (an average of 12 times a day). At the same time urges decreased to what he considered to be an acceptable level. (Mainly, he thought the residual urges acceptable because he felt they were qualitatively different. He considered that they were now merely normal sexual responses and had none of the compulsive qualities of his previous urges). During the treatment plus commitment phase, actual exhibitions decreased to zero. In Week 9, both treatment and commitment were discontinued because 'B' had secured employment and felt he would be unable to continue the practice exhibitions any longer. He was asked, however, to continue recording exposures and urges and to report weekly. Table 3.1 (Weeks 9-11) shows that exposures did not occur during this period and urges remained at a low level (and were reported to be qualitatively satisfactory). Follow-up interviews at three, six and nine months revealed no further incidents of indecent exposure.

The foregoing case provides preliminary evidence that a commitment procedure of the type suggested by Rachlin and Green (1972) and Rachlin (1974) does work to increase "self-control". In this case, the primary object was to increase commitment to a treatment procedure. The commitment phase was eliminated after the behaviour had been brought under the control of long-term
contingencies (for example, the clinet's sexual and emotional relationship with his wife was greatly improved, he felt more confident in general social relationships). While more clinical data must be collected to confirm the generality of this effect, the use of a similar commitment procedure to ensure that clients continue in therapy has successfully been used by the present author with three other clients (another exhibitionist, and with two weight control programmes). These encouraging results suggest that the commitment procedure has value and should be more widely tested and utilized. Its obvious value as an adjunct to the treatment of behaviours such as drug and alcohol abuse, overeating, aggression, and any other problems of self-control, indicates the commitment procedure is potentially an important tool in applied behaviour analysis. While much applied testing must be carried out, it is also essential for experimental work such as that of Rachlin and Green (1972) and Ainslie (1974) to continue to tease out the variables which enter into the commitment relation so that these may be used, in turn, to enhance the power of the procedure in applied behaviour analysis. A constant interplay should be in force such that the experimental analysis of behaviour suggests practical applications and applied behaviour analysis suggests areas for experimental investigation. At present, only lip-service is paid to this principle.
CHAPTER 4

The Clinical Application of Experimental Data - A Case Study

A major problem experienced by people who report they lack "self-control" is that of ensuring that, when an individual has access to a reinforcing event, they meet some response requirement before the delivery of a reinforcer. For example, someone who wants to reduce alcohol intake may require themselves to wait a certain interval before taking another sip of a drink. The problem lies in training a person to wait for the interval to pass and not to drink without waiting. Mahoney and Bandura (1972) have experimented with pigeons to determine the factors influencing the adoption of performance standards. In this study, reinforcers were always available but the pigeons were trained to ignore them until they had fulfilled a particular behavioural requirement. During training, the pigeons pecked a key to produce access to the reinforcer. As trials progressed, access was allowed progressively earlier in the sequence. Eventually, access to the reinforcer was available before the pigeon pecked the key. However, if a peck did not precede eating, access was immediately terminated. In this way, the pigeons learned to peck before eating.

Following the establishment of a stable pattern of responding under these conditions, each subject was tested to measure the persistence of this pattern. Under these conditions, transgressions (i.e., eating before or without pecking) were no longer
punished. Access to the reinforcer was available continuously, regardless of the pigeons' behaviour. Mahoney and Bandura reported that their subjects maintained faultless performances in the test conditions (i.e., they never ate without responding, even though food was freely available) for hundreds of trials. In a later phase of the study, Mahoney and Bandura trained the pigeons to adopt increasing response requirements before eating. Using the same training procedure as above, the pigeons were first taught to gain access to the reinforcers after one response (FRI). When performance had stabilized, the ratio requirement was progressively raised until five responses were required for access to the reinforcer (FR5). Following stable performance on FR5, test conditions were instituted (i.e., transgressions were not punished). The results showed that requiring progressively larger response requirements and punishing unmerited access to the reinforcer, trained the pigeon to adhere to more onerous response requirements long after the punishment contingency had been discontinued.

As it is often difficult to encourage humans to perform a response when reinforcers are freely available without such behaviour, Mahoney and Bandura's (1972) procedure is potentially useful in the modification of human performance. The case study outlined below paralleled closely the problem behaviour studied by Mahoney and Bandura with pigeons. It was the major object of the present case to examine the possibility that a similar procedure could produce similar results in a clinical situation, thus
indicating the practical implications experimental data have for applied behaviour analysis.

The client was a 26-year-old housewife who found herself unable to complete household chores or indulge in recreation outside the house because of her excessive reading. Upon arising each day, she would say to herself: "I will just read for half-an-hour before I do the dishes". At the end of this time she was often half-way through a chapter, and would promise herself only to read to the end of that. Typically, she would then put a further time restriction on herself ("I may as well just read until half-past the hour"), followed by another chapter restriction, and so on. Finally, Mrs. A either did not have enough time to get through all her work when she did stop reading or, realizing this fact, became depressed, gave up all hope of getting any work done, and read on. The problem had reached such proportions that there were sometimes two weeks' washing, a number of days' dishes, and almost all general homework undone. Mr. A was rapidly becoming estranged from his wife because of the state of their home and felt unable to handle the situation because neither comforting nor admonition succeeded in changing his wife's behaviour. At the same time, Mrs. A claimed that she wanted to get on with her work, both to please her husband and to allow her to get out of the house and engage in other activities. The couple had tried to stop Mrs. A's excessive reading by locking all available books in one room. However, under these circumstances she would either buy more when getting the weekly groceries or merely sit most of the
day thinking about what a problem she had - and still not complete any tasks.

In an initial attempt to increase the proportion of work completed each day, we began a Premack-type reinforcement programme. A pool of highly probable behaviours was established by observation over two days (reading, smoking, drinking coffee, sitting in a chair relaxing) and Mrs. A was then told that access to 10-min reading, one cigarette, one cup of coffee, or 10-min sitting in a chair was contingent upon the emission of 10-min of any of a number of low probability behaviours (e.g., washing dishes, clearing table, picking up clothing, dusting furniture, washing clothes etc.). Mr. A (whose employment involved rotating shifts which frequently allowed him to be at home during the day) agreed to administer the contingencies. Marginal success was obtained after one week of this programme, but when Mr. A was not present to ensure that access to reinforcers was only contingent upon target behaviours, the system broke down. It was apparent that the programme's first goal had to be to shape up ignoring freely available reinforcers until a response requirement had been met. A training procedure based on the Mahoney and Bandura (1972) study was devised to achieve this end.

2 The dangers of this type of application of "quasi-Premack" theory are discussed in Chapter 5.
During the course of the new programme the husband was always available to monitor performance and administer contingencies (he was on six weeks leave and then worked night shift and was thus available for some of the day). For the purposes of data collection each 10-min segment of Mrs. A's reading was designated as one unit of reinforcer. All household chores required to be completed by Mrs. A were rated by her and equated to unitary value (see Table 4.1) - for example, 30-min of dusting was equivalent to 15-min of washing dishes. Mr. and Mrs. A both recorded the number of chores attempted and time spent so engaged each day. Mr. A recorded the number of reinforcers appropriately self-administered by Mrs. A, - i.e., the number of reinforcers which followed response requirements. Concurrently, both completed a modified version of the Marital Happiness Scale (Azrin, Naster, and Jones 1973) (see Table 4.2) each day, and Mrs. A kept a diary detailing activities outside the house. Mrs. A defined the goals of therapy as:

(i) being able to complete normal household chores in a reasonable time (defined by her as being able to complete an average of any ten of the specified tasks each day;

(ii) being able to engage-in more social activities;

(iii) a better relationship with her husband; and

(iv) still enjoying reading books.
<table>
<thead>
<tr>
<th>Specified Household Tasks</th>
<th>Time for 1 unit (min)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Prepare breakfast</td>
<td>20</td>
</tr>
<tr>
<td>2. Prepare lunch</td>
<td>20</td>
</tr>
<tr>
<td>3. Prepare dinner</td>
<td>30</td>
</tr>
<tr>
<td>4. Wash up breakfast dishes</td>
<td>15</td>
</tr>
<tr>
<td>5. Wash up lunch dishes</td>
<td>15</td>
</tr>
<tr>
<td>6. Wash up dinner dishes</td>
<td>15</td>
</tr>
<tr>
<td>7. Wash clothes</td>
<td>20</td>
</tr>
<tr>
<td>8. Hang clothes out to dry</td>
<td>20</td>
</tr>
<tr>
<td>9. Iron clothes</td>
<td>10</td>
</tr>
<tr>
<td>10. Tidy house</td>
<td>30</td>
</tr>
<tr>
<td>11. Dust furniture</td>
<td>30</td>
</tr>
<tr>
<td>12. Do grocery shopping</td>
<td>45</td>
</tr>
<tr>
<td>13. Clean windows</td>
<td>10</td>
</tr>
<tr>
<td>14. Clean bath, shower, toilet, etc.</td>
<td>10</td>
</tr>
<tr>
<td>15. Vacuum carpets</td>
<td>25</td>
</tr>
<tr>
<td>16. Wash car</td>
<td>30</td>
</tr>
<tr>
<td>17. Light gardening</td>
<td>60</td>
</tr>
<tr>
<td>18. Do sewing and mending</td>
<td>40</td>
</tr>
</tbody>
</table>

Table 4.1:

A list of household chores required to be completed by Mrs. A.
The number of minutes in the right-hand column designate one unit of each chore.
TABLE 4.2:

Marital Happiness Scale

This scale is intended to estimate your current happiness with your marriage on each of the dimensions listed. You are to circle one of the numbers (1-10) beside each marriage area. Numbers toward the left end of the ten-unit scale indicate some degree of unhappiness and those toward the right end of the scale reflect varying degrees of happiness. Ask yourself this question as you rate each marriage area: "If my partner continues to act in the future as he/she is acting today with respect to this marriage area, how happy will I be with this area of our marriage?" In other words, state according to the numerical scale (1-10) exactly how you feel today. Try to exclude all feelings of yesterday and concentrate only on the feelings of today in each of the marital areas. Also try not to allow one category to influence the results of the other categories.

<table>
<thead>
<tr>
<th>Household responsibilities</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
<th>10</th>
</tr>
</thead>
<tbody>
<tr>
<td>Social activities</td>
<td></td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
<td>7</td>
<td>8</td>
<td>9 10</td>
</tr>
<tr>
<td>Money</td>
<td></td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
<td>7</td>
<td>8</td>
<td>9 10</td>
</tr>
<tr>
<td>Communication</td>
<td></td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
<td>7</td>
<td>8</td>
<td>9 10</td>
</tr>
<tr>
<td>Sex</td>
<td></td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
<td>7</td>
<td>8</td>
<td>9 10</td>
</tr>
<tr>
<td>Personal independence</td>
<td></td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
<td>7</td>
<td>8</td>
<td>9 10</td>
</tr>
<tr>
<td>Spouse's independence</td>
<td></td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
<td>7</td>
<td>8</td>
<td>9 10</td>
</tr>
</tbody>
</table>

[A shortened version of Azrin, Masten, and Jones, 1973]

Table 4.2:

Both Mr. and Mrs. A completed this scale daily during treatment, and on one day each month during the six-month follow-up.
TREATMENT PROCEDURE

1. Baseline recording: For one week Mr. A recorded the number of 10-min intervals during which reading followed one of the specified household tasks. In other words, if Mrs. A completed a chore and then sat down to read for 10-min, this was scored as appropriate. If she continued to read past that 10-min interval, each succeeding 10-min interval, or part thereof, was scored as an inappropriate response. Books were freely available during this time and Mr. A was told to act (as much as possible) as he had in the past, i.e., at different times he pleaded with his wife to complete chores, became angry with her lack of action, ignored her behaviour, or provided sympathy for her problem. Mrs. A was merely told that she should try to read only if she had completed a unit of household duties.

2. Training procedures: Following baseline, a training procedure based on Mahoney and Bandura (1972) was instituted. Mr. A controlled all access to books. Initially, Mrs. A was required to complete a unit of a defined chore before being allowed to read for 10-min. However, over a two-day period, Mr. A gradually left the book available earlier in the sequence, so that, eventually, the book was continuously available. During the training phase, any attempt by Mrs. A to read without having first completed a chore was immediately punished by the removal of the book. Training
was continued until Mrs. A had exhibited appropriate behaviour (i.e., reinforcer only following defined behaviour) for seven consecutive days.

3. **Test procedures:** After faultless performance had been established, the punishment contingency was removed completely and Mr. A merely recorded behaviour. Now, books were freely available at all times, and no programmed consequences followed transgressions.

4. **Further training:** Following twelve days of test conditions, it was decided to reintroduce the original training procedure with increasing response requirements. (While there had been an improvement on pre-treatment levels, reading between each completed chore was still rather disruptive). Thus, for two days, access to books was allowed only following completion of two chores (FR2). Then response requirements were gradually increased as follows:

- FR4 - (two days)
- FR6 - (three days)
- FR8 - (two days)
- FR10 - (four days)

During this period, Mr. A again terminated unmerited access to books.
5. **Further test conditions**: Following the establishment of faultless performance on FR, test conditions were again instituted to assess the stability of the behaviour in the absence of a punishment contingency. This continued for nine days, at which time Mr. A's working hours were changed and he was unable to continue monitoring his wife's behaviour.

6. **Follow-up**: Mr. & Mrs. A were contacted at monthly intervals for six months to assess the long-term generalization of the pattern established by this procedure. Data collected during the follow-up were Mrs. A's *estimates* of performance.

**RESULTS**

Fig. 4.1 shows the percentage of appropriately self-administered reinforcers for Mrs. A for all phases of the treatment. On five of the seven baseline days, she spent all day reading, without completing any housework. On the remaining two days, only a very low percentage of time spend reading actually followed specified behaviours. No data were recorded for days eight and nine because this period was used for gradually making the reinforcer available earlier in each response sequence. However, Fig. 4.1 shows that during the remainder of the training procedure (when transgressions were punished) perfect performance was maintained on most days. Following seven days of perfect performance, the test conditions were instituted, and Fig. 4.1 shows that, except for two consecutive days, perfect performance
FIGURE 4.1:
The percentage of reinforcers appropriately self-administered by Mrs. A during baseline, treatment, and follow-up. Follow-up data are estimates by Mrs. A.
was maintained in the absence of a punishment contingency. The re-introduction of the training procedure with the addition of increased response requirements resulted in continued perfect performance. Only on the first day of FR6 did the pattern break down, but the following day performance returned to 100% again and was maintained at this level until training ceased. Following training with increased response requirements, a second test situation was introduced (with the response requirement still at FR10). Fig. 4.1 shows that perfect performance was again attained on all but one day. A follow-up probe one month after treatment cessation revealed an estimated 90% level of appropriately self-administered reinforcers, and follow-ups over each of the succeeding five months showed an estimated 100% performance.

Fig. 4.2 shows the changes in the number of specified tasks completed by Mrs. A each day. Baseline data show that only on two of the seven days did she complete any tasks (two on Day 1 and three on Day 5, respectively). However, during the first training phase, Mrs. A's rate of work steadily increased until about six tasks per day were being completed. This rate was maintained early in the first test period, but dropped away slightly over the last few days. However, rate of task completion was still above baseline. The introduction of the increased response requirements in the second training phase resulted in significantly increased rate of task completion. For the last four days of this period, Mrs. A completed ten household tasks each day. This was the level which she herself set as the treatment
FIGURE 4.2:

The number of specified tasks completed each day by Mrs. A during baseline, treatment, and follow-up. Follow-up data are estimates by Mrs. A.
goal. The criterion level was maintained on all but one day of the second test phase. At follow-ups monthly, for six months, Mrs. A reported that she was still maintaining this rate.

Fig. 4.3 shows the number of hours Mrs. A spent doing housework each day. During baseline, housework only occupied minutes on most days and usually the tasks were not completed. For example, Mrs. A would start to wash the dishes, become depressed, and go and sit down and read or stare fixedly for the remainder of the day. The first training procedure resulted in an increase in the proportion of the day spent working, so that on the last four days of this phase three hours or more each day were spent working. As indicated in Fig. 4.2, this time was not sufficient to allow completion of the criterion number of tasks. This was because the procedure allowed frequent access to reading, of which Mrs. A almost always availed herself. Thus, continuity of tasks was lost, and each job took longer to complete than would normally be the case. However, the increased response requirements of the second training period resulted in Mrs. A completing more tasks, more speedily - a trend which continued during the second test phase. Follow-ups indicated that an average of about 2.5-hr of housework per day were maintained up to six months after treatment. Mrs. A considered this to be highly satisfactory.

The changes in reported overall marital happiness averaged from both Mr. and Mrs. A's self-reports over each week are shown in Fig. 4.4. This reveals a steadily increasing reported
FIGURE 4.3:

Hours per day engaged on specified tasks by Mrs. A during baseline, treatment, and follow-up. Follow-up data are estimates by Mrs. A.
FIGURE 4.4: Changes in Marital Happiness Scale ratings during treatment and follow-up. These data are weekly averages for both Mr. & Mrs. A.
satisfaction with the relationship by both partners as treatment progressed. These gains were maintained during the six-month follow-up period.

Mrs. A's ratings on the Marital Happiness Scale at Week 1 (Baseline) and Week 9 (end of treatment) are shown in Fig. 4.5. In the area of household responsibilities, she expressed herself as extremely dissatisfied at Week 1 and very satisfied at Week 9. A similar result is shown for the social activities area (a change from 2 to 8). There was no change in Mrs. A's rating of satisfaction with money matters (she was reasonably satisfied) and an increase in both her communication (5 to 8) and sex (5 to 7) ratings of satisfaction. Mrs. A felt that her personal independence had increased markedly (a rating increase from 1 to 7) but did not change her opinion of her husband's independence (she rated him 8 at both times).

Similar data from Mr. A are shown in Fig. 4.6, and they reveal some interesting changes. His ratings of Mrs. A's behaviour with respect to responsibilities show the same change as his wife's evaluation of her one behaviour in this category (a change from 1 to 9). Mr. A. was originally happier with their social life than was his wife (4 vs 2), but his satisfaction

---

3 Note that neither ever rated anything 10. This was because they agreed that nobody could ever be completely happy about anything.
FIGURE 4.5:

Mrs. A's self-ratings on the Marital Happiness Scale during Weeks 1 and 9 for each of seven problem areas. Data are weekly averages.
Figure 4.5

Marital Happiness Scale (MHS, A)

Problems Areas

1. Household
2. (MHS, A's)
3. Money
4. Communication
5. Sex
6. Personal Independence
7. Spouse's Independence

Week 1

Week 9
FIGURE 4.6:

Mr. A's self-ratings on the Marital Happiness Scale during Weeks 1 and 9 for each of seven problem areas. Data are weekly averages.
with this area increased over the treatment period (from 4 to 7). Ratings for money affairs were identical to Mrs. A's. In the area of communication, Mr. A showed a considerably larger change in satisfaction (from 3 to 9) than did his wife (from 5 to 8). Mr. A also rated their sex life improved over the treatment period (from 6 to 8). He considered that the changes in his wife's behaviour gave him a good deal more personal independence (an increase in rating from 4 to 8) because he no longer had to spend excessive time either cajoling Mrs. A to do housework or having to do it himself. Similarly, he noted that treatment led to much greater independence on her part (from 2 to 8).

Fig. 4.7 records changes in Mrs. A's social activities, both those she engaged in alone (e.g., visiting friends during the day, going to town with a friend) and those involving her husband also. The graph shows a marked increase in the number of times each week that Mrs. A was able to get out of the house as treatment progressed. The improvement in Mr. and Mrs. A's relationship, as evidenced by Figs. 4.4, 4.5, and 4.6, also appears to have led to an increase in the social outings they made as a couple.

DISCUSSION

This case study demonstrates that procedures identical to those utilized in an animal laboratory may be beneficially used in an applied setting. In this case, procedures reported by Mahoney and Bandura (1972) were used to train a woman to cease a
FIGURE 4.7: Number of Mrs. A's social activities, and of activities involving both Mr. and Mrs. A. during Week 1 (baseline) and Week 9 (final days of treatment).
debilitating pattern of almost never completing necessary household chores because of her indulgence in excessive reading. Phasing in a response-requirement contingency combined with reinforcer withdrawal for transgressive behaviour resulted in the establishment of adequate work behaviour. This pattern was maintained when external contingencies were withdrawn. Further improvements in the target behaviour were attained by gradually increasing the response requirement so that progressively more work was required before the woman was allowed (or, eventually, allowed herself) to read. Once established, the self-regulatory behaviour persisted even though the reinforcer was freely available.

Mahoney and Bandura (1972) reported that pigeons, after adhering to self-regulation contingencies for a long period, suddenly abandoned them and self-fed without prior performance. Such was not the case here (six-month follow-up data showed the pattern maintained at a high level) and this illustrates a major difference between the animal and human situation. In the present case, changes in the target behaviour (housework completion) led to unprogrammed changes in other reinforcers (husband's approval, more time for social activities, better marital relationship, etc.) which "trapped" the target behaviour and made its future probability of occurrence high. In the animal study, there were apparently no such unprogrammed consequences to trap behaviour. (For a discussion of "behavioural trapping" see Baer, D.M. and Wolf, M.M. The entry into natural communities of reinforcement. In R. Ulrich, T. Stachnik and J. Mabry (Eds.), Control of Human Behavior, Vol. 2, Glenview, Ill.: Scott-Foresman, 1970, 319-324.)
This study illustrates also the importance of monitoring behaviours other than the target behaviour. Figs. 4.1 to 4.7 show changes in a range of behaviours which are all of clinical interest. In the present case, these changes are positive but we should be aware of them so that either we can use these changes as reinforcers or, if the change is negative, can change our strategy. The data collected here showed changes in amount of housework done, time to complete housework, frequency of social activities, and increased satisfaction with a number of facets of a marital relationship. Monitoring of the spouse's behaviour (in this case, by self-report inventory) allowed me to assess the effects of changes in the client's behaviour on another significant person in her environment. Finally, Mrs. A reports that she still enjoys reading.

In their discussion, Mahoney and Bandura note that:

"The present experimental paradigm lends itself readily to investigation of the variables and processes governing acquisition and stability of self-reinforcement patterns. The form, severity, and frequency of punishment for transgressive behaviour have already been mentioned as possible important determinants. It would also be of interest to examine the conditions under which self-reinforcement can be brought under stimulus control such that animals dutifully work for self-rewards in certain situations, but reward themselves without performing any behaviour in other stimulus contexts" (1972, p.301).

These are questions of some clinical relevance which could
well be studied in the experimental laboratory and tested in an applied setting. It is such cross-fertilization which should characterize behaviour analysis.

Note:

There are two other comments which should be made with respect to the case study reported in this chapter.

1. There is the possibility that the failure of the Premack-type programme was caused by the absence of Mr A. as a contingency manager during parts of the programme. This could invalidate comparisons between the Premack programme and the Mahoney and Bandura type programmes. However, my reason for thinking that the latter programme was superior was the rapidity with which it ensured appropriate behaviour. Neither major nor sustained behaviour change occurred in the Premack programme even when the husband was home for one full week and acting as a contingency manager.

2. It could also be argued that Mr A. provided contingencies other than those specified. This was in fact the case but he did so both before treatment and during the two types of programme. However, no empirical information was collected as to the frequency with which he offered these reinforcers. He was reminded though, at various times during the treatment to try and keep his behaviour towards his wife as constant as possible - see p.91. An interesting observation was that there was a qualitative change in the use of reinforcers in that originally Mr A. offered them as an inducement but later offered them more naturally as part of a normal interaction - i.e., the difference was one of "How would you like to go out?" instead of "If you do the housework I will take you out".
CHAPTER 5

Premack Theory and Applied Behaviour Analysis

Application of the experimental work of Premack (1959, 1963, 1965, 1971) provides a striking example of both the lack of incorporation and misuse of data from experimental behaviour analysis. Premack's approach is to analyze reinforcement as a relation between responses, rather than one between a stimulus and a response. A typical experiment in this paradigm involves placing an organism in an environment which provides the opportunity to engage in a number of activities without time or response restrictions. The organism is allowed freely to engage in these activities and the proportion of time allocated to each is recorded and taken as a measure of response probability. From these data, a hierarchy from most to least probable behaviours for that organism and those activities can be drawn up. Subsequently, if two of the original test activities are made available, the experimenter can make the opportunity for engagement in one, contingent upon the performance of the other, response. The typical finding is that if a response of high probability is made contingent upon a response of low probability, then the latter response will increase in frequency. On the basis of these data, Premack has proposed that the principle of reinforcement should be stated as: for any two behaviours, the more probable one will reinforce the less probable one. Thus:
"Reinforcement is a relative property. The most probable response of a set of responses will reinforce all members of the set; the least probable will reinforce no member of the set. However, responses of intermediate probability will reinforce those less probable than themselves but not those more probable than themselves. Intermediate members of the set thus both are and are not reinforcers, depending upon the relative probability of the base response .... The reinforcement relation is reversible. If the probability of occurrence of two responses can be reversed in order, so can the reinforcement relation between the two responses" (Premack, 1965, pp.132-133).

That the reinforcement relation is reversible was shown by Premack (1962) in an experiment in which rats had the opportunity to run in a wheel and drink from a tube introduced through a stationary wall on one side of the wheel. Running in the wheel was controlled by engaging or releasing a brake on the wheel, and drinking by introducing and removing the tube into the wheel. When running was restricted but water freely available, running increased until it was more probable than drinking. However, when free running was allowed, but drinking restricted, drinking became more probable than running. Premack was then able to show that the opportunity to engage in the more probable response functioned to increase the probability of the less probable response. When drinking was more probable than running, the latter became more probable if it introduced the drinking tube. Similarly, when running was more probable than drinking, drinking became more probable if it released the brake and allowed the rat to run. This experiment is important because it shows that a
reinforcer cannot be defined independently of the response that is reinforced and the situation in which it occurs. Catania (1968) makes the point that:

"Ordinarily, in the presence of a reinforcer, reinforced responses are less probable than the responses for which the reinforcer sets the occasion. A hungry pigeon, for example, is more likely to eat in the presence of food than to peck a key - which is why an opportunity to eat is used to reinforce pecking. Premack's example illustrates that these relationships may be reversed, and that a reinforcer must therefore be defined relative to the response that is to be reinforced. An opportunity to engage in one response may serve as a reinforcer for another, less probable, response, but it may also be itself reinforced by an opportunity to engage in a third, even more probable, response" (1968, p.64).

Premack (1965, pp.170-171) admits that the results of his experiments are dependent on the "rate of exchange" of any two behaviours. There will be no increase in the low probability behaviour if only a small unit of it is required to produce access to a large unit of a high probability behaviour. Premack (1965) found that "an invariant though unrecognized component of the contingency is a decrement in the amount of responding that occurs to the contingent stimulus, relative to what would occur were the stimulus free" (p.172). Similarly, Eisenberger, Karpman, and Trattner (1967) showed that the less probable behaviour increases only if the high probability behaviour was suppressed beneath its free operant level. In fact, their data indicated that the relationship can be reversed if the organism has to emit a large unit of the highly probable behaviour to gain
access to a small unit of the low probability response.
Obviously, we need to have some way of comparing behaviours, and
this is why time is accepted as a measure upon which we can base
a "rate of exchange".

Premack (1965, 1971) used independent response duration as
his measure of response probability. The measure is obtained
by one of two methods:

(a) a number of stimuli are made simultaneously
available and the amount of time spent
interacting with each alternative is
recorded; or

(b) each of the stimuli is presented separately
in discrete trials of uniform length and
the time spent interacting with each
alternative is measured.

The amount of time spent interacting with each of the alternatives
in either situation may then be ranked in relation to the other
alternatives. The chief criticism of the use of duration as a
measure of probability is that the "hedonic" value of some
behaviours does not appear to be accurately reflected by the
duration of the response. For example, 15-min of sexual
intercourse may be preferred considerably more than 15-min of
washing dishes.

Premack argues, however, that subject to some important
constraints on the assessment situation, duration is an accurate
measure. First, the assessment must be conducted in a free-operant environment and involve only intrinsically-maintained responses (Premack, 1965). Thus, the assessment setting must allow the organism to respond freely and continuously and there must be no contingency arrangements in operation. The latter is particularly important since duration measures taken under contingency conditions would not represent relative value. Danaher (1974) points to the rather extreme situation of an avoidance schedule situation as an example of the relevance of this condition. In such a situation we would expect the frequency and duration of the avoidance response to be high. However, we would not label the response a high probability behaviour on the basis of this measure because the situation is largely controlled by the programmed contingencies. We could only make an assessment of the response in a situation where it is freely available and not subject to external reinforcement.

This requirement may appear to contradict Premack’s (1965) Indifference Principle which holds that the "prediction of reinforcement can be made without regard to the history of the response probability, for the outcome is indifferent to either the parameters used to produce the probability or the responses that manifest it" (p.143). The accuracy of this position has been established, however, by the finding that the same response can function both as a reinforcer and a punisher. All the Principle states is that, although deprivation and satiation (for example) may influence the duration of intrinsically-maintained
responses, these responses may still be accurately assessed as long as Premack's constraints are observed.

A second constraint mentioned by Premack is the avoidance of averaged data. There are two problems here:

(a) if the average probability value differs significantly from the momentary value we may seriously mispredict the effect of a contingency arrangement; and

(b) we must be careful not to generalize probability data from one situation to another or similar mispredictions may be made (because probability data are situation-specific).

Premack (1965) suggests that these difficulties can be largely overcome by keeping constant both the length of time an organism is allowed to respond and the inter-trial interval. Baum (1973) argues, however, that the value of any activity must be thought of as extending through time because it cannot be assessed at any particular point in time, but must be averaged over a certain period. "In other words, since value depends on integrated feedback, an activity has value and changes value only over extended periods of time, and behaviour varies with changes in value only over extended periods of time. A concept like momentary value could be meaningful only as the temporary derivative of value expressed as a function of time (just as momentary velocity is the derivative of distance with respect to time)" (Baum, 1973, p.150). [Italics in original]
If we examine moment to moment behaviour, however, we see that the organism engages in a number of alternatives and changes from one to another. Such changes do not in themselves reflect value. At any specified point in time an organism may be emitting a behaviour of any value, but over time it will emit highly-valued behaviours more often (Baum, 1973). Momentary changes in variables such as deprivation, which have a constant effect over time, are the causes of moment-to-moment changes in an organism's behaviour. Premack (1971) claims that it is these momentary fluctuations which are often of interest and that averaging the measures fail to record these changes. Averaging would not, for example, show how, over the course of a session, satiation reduces the frequency of behaviour A from an originally high level to below a low, but constant, level of behaviour B. Baum (1973) argues convincingly however, that the study of average relations complements the study of momentary relations, and is not incompatible with it.

Another constraint on probability assessment is that it is limited to those situations in which an organism presents a stimulus to itself. This is simple because Premack's model, as presently postulated, only accounts for a single organism's preference between certain stimuli under a singly programmed schedule.

Premack's formulation has obvious appeal to applied behaviour
analysts because it provides the impetus to find and use a wider range of reinforcers than have hitherto been used. Rather than concentrate on a small range of "obvious" reinforcers, Premack's work indicates that any activity is a potential reinforcer. Because a behaviour of a certain value can reinforce any behaviour having less value, an unlimited range of behaviours can be used to train and maintain other behaviour. In view of this potential, it is very surprising that only relatively few reports of applications of the Premack Principle have appeared in the ten years since it was clearly articulated. These studies are almost exclusively limited to classroom and self-control applications. Concern, similar to that expressed earlier in this work, over the manner in which experimental data are claimed to form the basis for applied behaviour analysis in general, apply particularly to the Premack Principle (see especially Danaher's, 1974, critique of these studies). Homme, de Baca, Devine, Steinhorst, and Rickert (1963) used the contingent application of high probability behaviours such as running around the room, screaming, and pushing chairs as reinforcers for desired behaviours in three-year-old nursery-school children. Homme, Csanyi, Gonzales, and Rechs (1970) proposed the use of "reinforcement menus" (lists of potential high probability behaviours from which the individual selects activities of self-reported high value to himself to be used as reinforcers for specified target behaviours) to discover reinforcers for appropriate classroom behaviours. Ayllon and Azrin (1968) instructed staff in a psychiatric hospital token economy setting to observe patients' behaviour at any time during the day to
discover what were the high probability behaviours for them. These behaviours were then used as reinforcers for desirable ward behaviours. Studies such as these represent a step forward due to their conceptual acknowledgement of Premack's work - they are a move away from the limitation placed on applied behaviour analysis by the emphasis on tangible objects as reinforcers. The concept of using valued behaviours as reinforcers widens considerably the options open to the behaviour analyst. However, the assessment of probability in these studies uniformly illustrates the spirit rather than the strict observance of the Premack procedure.

Many clinical applications of the Premack Principle equate subjective preference with empirical probability so that, for example, children select activities from a list rather than have their preferences directly assessed by duration measures. Similarly, those studies which employ observation of a person's behaviours and then select as reinforcers those which occur most frequently, do not involve controlled, measured observations but, rather, the coincidental noting of behaviours during the course of other duties (e.g., Ayllon and Azrin, 1968). Numerous applications of the Premack Principle in the area of self-management and self-control further emphasize this point (Kanfer and Karoly, 1972; Mahoney and Thoresen, 1974; Watson and Tharp, 1972). In these studies, reinforcers are typically identified as those responses which are the most frequent per unit time. Thus, typical reinforcers in self-control studies are such activities
as closing drawers (Homme, 1965), rising from and sitting at a
desk (Homme, 1966), speaking over the telephone (Todd, 1972),
urination (Mahoney, 1970), and cigarette smoking (Mahoney, 1971).
The divergence from a Premackian assessment is immediately
apparent in these studies. The first difference is the use of
frequency as an index of probability, rather than duration.
Second, many of the behaviours used as high-probability
behaviours (reinforcers) are under the control of external
contingencies (that is, they violate the constraint that such
behaviours must be intrinsically maintained) and so their
frequency is probably higher than would be expected in a measure
of their relative value. Finally, none of the assessments have
attempted to provide a free-operant environment in which to
measure the behaviours.

Homme (1965) recognizes the difficulty of extrapolating from
empirical assessment to that used by clinicians, but claims that
the approach is valid. He notes that " ... the probability
referred to here, of course, is not based on formal frequency or
duration data; it is a kind of phenomenological probability
estimate. The assumption is that S can predict what he is
likely to do next" (footnote 3, p. 504). In other words,
prediction of behaviour replaces empirical probability and,
therefore, self-report becomes the method of assessment - with
all its attendant difficulties. The use of the Premack Principle
thus appears to reflect the idea rather than the detailed
procedures of the original experimental work, and provides an
example of how experimental procedures can become a conceptual rather than an empirical basis for clinical techniques. This, of course, says nothing about the effectiveness of the latter. The point at issue is the undesirability of inaccurate transfer of data from one situation to another, with its potential dangers. As Danaher (1974) concludes in his critique: "While not necessarily unique to the generalization of animal learning theory to the human situation, the uncritical citation of theory and prior data can only lead to perpetuated misuse and paradoxical findings" (p.321).

The only published study known to the present author which attempted to observe some of Premack's constraints is that by Mitchell and Stoffelmayr (1973). In addition, their study is also an elegant example of how the Principle extends the limits of the usual reinforcers used by applied behaviour analysts. The subjects in this case were extremely inactive schizophrenics who consistently refused to accept any tangible reinforcers offered to them during shaping sessions. In the assessment phase, recordings were made of the work behaviour of patients for six 30-min sessions, during which any instance of work that occurred in each 30-sec period was recorded. Work was defined as holding a coil of wire in one hand while using the other to pull wire off the coil. The dependent measure was the number of 30-sec intervals per 30-min session, during which the patients exhibited work behaviour. Two patients refused all tangible reinforcers and the authors decided to use as a reinforcer any
response that occurred frequently when freely allowed. Sitting was the very high frequency behaviour shown by the observation data, so this was used as a reinforcer. Patients were required to stand until they removed a few inches of wire, following which they were allowed to sit for 90-sec. This procedure was repeated for the remainder of the session and the response requirements were gradually increased to removing three coil wrappings of wire. During the baseline sessions, Subject 1 worked for 4.5% of the intervals, and Subject 2 for 2.5%, whereas work frequency increased under the reinforcement contingency (to 80% and 79%, respectively) and was maintained throughout the experimental period. These results showed that even severely inactive patients with long hospitalization (a range of 15 to 32 years) will respond to a reinforcement programme if the reinforcers are carefully selected. It is unlikely that many would have thought of using the one thing that defined these patients as problems (being inactive) to motivate their behaviour had the Premack Principle not been considered appropriate. Significantly, Mitchell and Stoffelmayr (1973) were concerned about "the strict application of Premack's principle" (p.423, emphasis mine). The activity chosen as a high probability behaviour (sitting) was intrinsically maintained and self-applied. The assessment phase was conducted in a free-operant environment and responses were timed every 30-sec. Such attention to the detail of Premack's procedure is what is urgently needed to test the adequacy of his model in the realm of human behaviour.
In view of the widened scope which a Premack-type analysis can give applied behaviour analysts, it is interesting to speculate on the reasons for the relative dearth of such applications in the literature. I content that, once again, a major obstacle to implementation is the lack of contact between experimental and applied behaviour analysis, which results in many applied behaviour analysts being simply unaware of Premack's data. Certainly, critical analyses of studies of applications of Premack Theory indicate that many authors' understanding of Premack comes from its explication in the applied literature, rather than the original experimental work. Such citing of experimental data at second-(or more) remove can only compound any errors of omission or interpretation made in an original applied setting. This suggests two important areas of emphasis for the training of applied behaviour analysts. First, that extensive knowledge of the experimental analysis of behaviour should be a pre-requisite for training in applied behaviour analysis (cf. the scientist-clinician debate at the Boulder Conference, Raimy, 1950) - and should be a necessary condition for continued practice in this field - and, second, that emphasis should be placed on clinicians taking the trouble to read the original work if they are to cite it as justification for their procedures. Uncritical acceptance of original source experimentation will obviously do nothing to extend the clinician's knowledge of important data. This may appear to be an obvious point to make, but apparently the practice is widespread. Therefore, training

---

4 My informal conversations with a number of "applied behaviour analysts" indicates that they frequently obtain references to experimental work from published case studies without consulting the original data.
programmes could usefully require trainees to demonstrate their understanding of experimental data.

Another possible reason for the lack of application of the Premack Principle lies in the seemingly complicated assessment of response probability (particularly if the aforementioned constraints are observed). However, while there are undoubtedly many situations in the applied field where meeting the requirements is problematic, Mitchell and Stoffelmayr (1973) have demonstrated that it is possible, given the proper regard for Premack's experimental work.

Finally, a major barrier to application is simply the method of training applied behaviour analysts. The emphasis on identifying tangible reinforcers is very strong in many university courses, so that trainees are simply shaped away from thinking in wider Premackian terms. As a consequence, many potentially reinforcing activities are overlooked (e.g., sitting; may be viewed merely as a problem behaviour, not a reinforcer) Goldiamond (1975) has made a similar point in his contention that applied behaviour analysts often view behaviour from a pathological rather than a constructional point of view. I suggest that if we followed the example set by Mitchell and Stoffelmayr (1973) and closely observed Premack's strictures, applied behaviour analysis would potentially be able to cope with a wider range of problem behaviours. Such an analysis would be particularly helpful in the treatment of "delinquent" behaviours, where the opinion is
frequently expressed that delinquents have such a narrow range of reinforcers that it is difficult to construct programmes to modify their behaviour. However, our assessment efforts to date have only focused on a narrow range of possible reinforcers. The application of the Premack Principle to assessment of this class of behaviours would undoubtedly reveal as yet untouched sources of reinforcing events. It is in this manner that the scope of applied behaviour analysis can be increased by a comprehensive knowledge of data from the experimental analysis of behaviour.
CHAPTER 6

Behavioural Interactions

Lanyon (1971) has characterized the development of applied behaviour analysis as a three-stage process. Early applications were to simple, specific problem behaviours and were not generally regarded as the main thrust of a therapeutic enterprise. Procedures were aimed at behaviours which inconvenienced staff and kept them away from their therapeutic role (for example, Ayllon and Haughton, 1962, modified coming late to dinner in mental hospital patients). The success of early programmes led to the wider application of these techniques to the task of getting patients to perform all routine tasks expected of them - and these changes in behaviour came to be seen by some as therapeutic in themselves. Finally, success with system-wide programmes resulted in highly individualized behaviour modification programmes seen as the sole therapeutic approach. Unfortunately, the sophistication of technique has not paralleled the increased levels of complication of problems, so that the therapeutic failure is more common at level three than at level one. Such failures are often attributed to the therapist's lack of practical control over the patient's environment (that is, he does not have access to or is not able to manipulate sufficiently reinforcing or punishing events) or to problems of response generalization from therapeutic to "real-life" environments. I suggest, however, that one of the major reasons for these failures
is that the behaviour analysis of level three problems is usually
that which is more appropriate to level one. Such analysis has
frequently been successful with level one behaviours but cannot
cope with the complexity of the controlling relations of level
three behaviours. Thus, an attempt to analyze a "delinquent
lifestyle" in terms of discrete behaviours emitted in the presence
of discrete stimulus events and having easily identified
reinforcing or punishing consequences is unlikely to yield
fruitful results. What are involved here are a complex series
of behavioural interactions, such that modification of one part
of the series has consequences for another part. Until we
are aware of the nature of these interactions and can either
harness them or take steps to circumvent them, applied behaviour
analysis will, for the most part, only be successful with "simple",
discrete behaviours.

Once again, it seems that the separation of experimental and
applied behaviour analysis has contributed to a weakness in the
latter, with the result that the wrong level of analysis is being
conducted with many problem behaviours. It is probably true to
say that much early experimental behaviour analysis was
characterized by its discrete nature. This was appropriate to
this stage in its development - as it was to early applied
behaviour analysis. The major thrust in contemporary
experimental work, however, is towards the recognition that the
world cannot be divided into seemingly simple categories. Of
course few applied behaviour analysts would argue that the
boundaries of their model of behaviour are as finite and certain as a simple exposition would have us believe - but the actual application of such a simple analysis in almost all published case studies belies their assertion. The fact is that the separation of experimental from applied work, coupled with the practicalities of teaching applied behaviour analysis to a wide (and often non-professional) audience, has resulted in only a simplistic outline being widely promulgated. The consequences of repeated exposition of the limited model is that it has come to be seen as a sufficient framework for most applied endeavours, even (in terms of published case studies, at least) by those with knowledge that extends its scope.

Stein (1975) has discussed the ethical problems associated with teaching applied behaviour analysis in short-term workshops. He asks, in view of their potential for misuse, whether we can teach these principles properly and responsibly in courses of short duration. I contend further, that our desire to teach these principles has resulted in an ever-refined "simplified" version of the behavioural model being presented widely in journals, books, and lectures. This simplification process, part of the early "missionary fervour" phase of spreading the good word about applied behaviour analysis, is now seriously weakening the link even with the experimental data of days bygone, and certainly totally ignores current experimental work. While this approach has undoubtedly been successful in stimulating the acceptance and use of applied behaviour analytic techniques, it
is currently in danger of leading to oversimplification and triviality which will stifle the growth of applied behaviour analysis, and indeed, contribute to its abuse. We therefore come again to the point I will make repeatedly - we must return to the experimental data as the basis for applied behaviour analysis. The focus of this return is on two levels. First, we must gain a broad appreciation of the compass of current experimental analysis of behaviour, which will provide a context in which broad formulations of behaviour change and maintenance can be embedded. Second, we must strive for the direct application of the principles elucidated by particular experimental data to specific applied situations.

The Continuity of Behaviour

The discrete units of the applied behaviour analysis model have obscured the most important lesson taught by the experimental analysis of behaviour - that behaviour is a continuous stream (Baum, 1973; Catania, 1971; Goldiamond, 1975; Schoenfeld, Cole, Lang, and Mankoff, 1973; Schoenfeld and Farmer, 1970). The understanding that behaviour is fundamentally a continuous series of events in time eliminates many of the artificial boundaries set by the simple model and has particularly important implications for the ideas of contingency and reinforcement schedules.

Both in the experimental analysis of behaviour and applied behaviour analysis, the first problem is the definition of the response to be measured. The behaviour analyst influences both
the interpretation and the usefulness of a measurement the moment he specifies the boundaries of the response class (by stipulating the physical properties a behaviour must have to fall within the class). Since the definition of a response class is necessarily an arbitrary matter, the behaviour analyst should be aware of the consequences of different decisions regarding the boundaries.

The definition of the response class also determines the definition of another class - those responses not to be reinforced (Goldiamond, 1975; Schoenfeld and Farmer, 1970). In any situation, the class of responses not be reinforced provides a background or context for the class to be reinforced. Thus, the two classes may be of equal importance. In most of the early work in the experimental analysis of behaviour, response definitions were in the form of descriptions of behaviour as discrete events, and this trend has been universally followed in applied behaviour analysis. As such, the importance of the behaviour stream and a behaviour context have been overlooked entirely by applied behaviour analysts. In the experimental analysis of behaviour, on the other hand, an increasing number of experimenters (Baum, 1973; Catania, 1973c; Herrnstein, 1970) have pointed to the limitations of such a definition and the contiguity-based law of effect entailed by it. This criticism has led to alternative formulations taking the behaviour stream as axiomatic and viewing the relation as the result of time-based *correlations* between behaviours and
environmental consequences. This change of emphasis vitally changes the nature of response definition. The use of a model based on the collection of data in the form of discrete behavioural events is inappropriate in at least some (and probably most) of the contingencies relating behaviours and their consequences. This is especially true for applied behaviour analysis which often seeks to clarify the nature of interactions between two or more human beings. In such situations, the definitions of behaviours effective in producing consequences are often not stable over time and are not based on criteria of the simple occurrence or not of a particular form of behaviour.

Because not everything in the clinical situation can be controlled, or even observed, there is always a large class of responses not to be reinforced (not-R) which is free to vary. (This, of course, is also true of the experimental situation, but to a lesser extent because more artificial constraints may be imposed). This class has traditionally been ignored in applied behaviour analysis, but is now receiving attention in the experimental analysis of behaviour.

Schoenfeld's research group (Schoenfeld and Cole, 1972; Schoenfeld, Cole, and associates, 1972; Schoenfeld, Cole, Lang, and Mankoff, 1973; Schoenfeld and Farmer, 1970) has extensively investigated the importance of not-R as a context for R. As noted above (see also Goldiamond, 1975), much behaviour analysis is unilinear in the sense that there is only one well-defined set,
the behavioural element of which is termed the response (R). Any alternatives are defined by exclusion from that set. Schoenfeld et al.'s work investigates the situation of at least two well-defined sets which may be designated (using Goldiamond's, 1975, terms) as target behaviour (TB) and alternative behaviour (AB). A third class, designated neither behaviour (NB), is defined by the exclusion of both TB and AB. The NB class can also be subdivided into other well-defined behaviours.

Schoenfeld and Farmer (1970) conducted three experiments which investigated the relation between well-defined TB and ABs. TB was a key-peck reinforced on the average of every twentieth (p = 0.05) occurrence. AB was defined as a repeating time period which was reset by each TB. The duration of AB was determined for each subject individually and, when the value was established, AB was reinforced according to schedules between p = 0.0125 and p = 0.20. TB continued to be reinforced on the p = 0.05 schedule. The results showed that as the probability of reinforcement for AB increased, the proportion of reinforcers earned by AB increased. In other words, AB, like TB, is controlled by schedule effects. Having established this, Schoenfeld and Farmer went on to demonstrate, with FI and DRL schedules, that the duration of AB controlled the rate of TB under certain circumstances, demonstrating that:

"... how the behavioral stream is manipulated will help determine what is observed of that segment of the stream that is chosen as the sample R in any experiment" (p.244).
Accepting behaviour as a continuous stream forces us to study the relations between behaviour and environmental events in an extended time perspective, rather than as a series of discrete behaviours contiguous with discrete environmental events. An experiment by Herrnstein and Hineline (1966) (noted by Rachlin, 1974) emphasizes the importance of the extended view. In this study, rats were subjected to an avoidance procedure in which shocks were distributed randomly in time, but in which responses reduced the overall shock rate. Bar presses occurred, although no single response avoided any single shock, and it was concluded that shock-frequency reduction alone was sufficient to maintain avoidance responding. Fig. 6.1 shows a hypothetical distribution of shocks and responses from this experiment. It is evident that, generally, the more bar presses there are, the fewer are the shocks. But if the distribution is analyzed in discrete areas instead of as a continuous whole, that is, if the observer takes samples between M and N, between 0 and P, and between Q and R, he could conclude that shocks caused bar presses (M-N), bar pressed caused shocks (0-P), or that there was no causal relation between the two. As Rachlin (1974) points out:

"Only an extended view of the temporal properties of bar presses and shocks allows us to see the true relationship ... the cause of the bar presses is the relationship between bar pressing and shocks as it is experienced by the rat" (p.97) (italics in original).

When we consider environmental events extended in time, we
FIGURE 6.1: A hypothetical pattern of shocks and responses in an experiment by Herrnstein and Hineline (1966). The pips on the top line represent shocks and the pips on the bottom line represent responses. The restricted periods of observation M-N, O-P, and Q-R referred to in the text are delineated by the vertical dotted lines.

[From Rachlin, 1974]
need no longer view a specific unit of an environmental change as causing a specific response unit. We are concerned only with the causal relation between the entire environmental event and the entire response. Thus, in the Herrnstein and Hineline experiment, the rate of bar pressing is caused by the relation between responses and shocks, but no individual cause may be ascribed to any individual response.

One of the major consequences of accepting that behaviour is a continuous stream has been the focus, in experimental work, on the interactive nature of behaviour. In fact, it could be said that the experimental analysis of behaviour is now almost exclusively concerned with behavioural interactions. By this I mean that experimenters, realizing that it is not usually possible to modify one behaviour in isolation, seek to understand the relations between changes in one behaviour and those in others. Their procedures are accordingly characterized by measures of more than one behaviour. Applied studies, on the other hand, typically measure only the behaviour of interest and thereby are unable to monitor important concomitant changes.

Only a few lone voices in applied behaviour analysis have pointed out the dangers of a unitary approach. Arguing that we should be as aware of behavioural inter-dependencies as we have become about ecological relations, Willems (1974) points out the limitations of viewing behaviour from a simple, short-term perspective:
"Applied behavior modification is an astonishingly simple and successful technology of behavior change. However, its precision and objectivity depend, in large part, upon its application to single dimensions of behavior, one at a time. The questions of larger and unintended effects within interpersonal and environment contexts and over long periods of time beg for evaluation and research, because lessons learned in other areas suggest that we should always be sensitive to "other" effects of single-dimensional intrusions" (p.155).

Willems notes a number of examples of possible inter-relations among behaviours. Two unpublished studies are noteworthy. In the first, an attempt was made to modify the behaviour of a mother who nagged at rates of up to 100 times or more per hour, and whose child's rate of compliance was very low. Following intervention, nagging was reduced to 15 per hour and the child's compliance increased significantly. However, as the therapy became more "successful", the mother gained weight and reported frequent tension and anxiety. Finally, she left town, abandoning her child. The second example concerned an attempt by the Los Angeles Probation Department to use behavioural techniques to modify the delinquent behaviours of adolescent boys. The programme resulted in less petty offences, but was accompanied by a corresponding increase in more serious offending. As Willems (1974) points out, no definite statements about causal relations can be made in these cases on the basis of these data. However, that is precisely his point:

"The phenomena beg for research ... (a) that admits the possibility of unanticipated
complexities, (b) that uses models that lead us to look for them and define them as real phenomena, and (c) that adds procedures that allow their detection and measurement when they occur" (p.157) (emphasis in original).

Only relatively few studies have examined the effect on other behaviours of modifying the target behaviour. Most that have been conducted have merely recorded the side-effects (both desirable and undesirable) of a procedure (e.g., Koegel, Firestone, Kramme, and Dunlap, 1974; Sajwaj, Twardosz, and Burke, 1974; and Wahler, Sperling, Thomas, Teeter, and Luper, 1970). Almost no researchers have deliberately searched for behavioural interactions and used them to their advantage. A notable exception was Nordquist's (1971) treatment of an enuretic child. Starting from the knowledge that some undesirable behaviours typically resistant to change by altering reinforcement contingencies could be altered by effectively controlling another functionally-related response class, Nordquist studied three things. First, his task was to identify a response class functionally related to nocturnal enuresis; second, he modified this response class by changing social reinforcement contingencies; and finally, he was interested to study the short- and long-term effects of these operations on enuresis. In this case, oppositional behaviour was identified as the functionally-related response class and time-out and differential attention were manipulated as behavioural consequences. Contingencies were put into operation, removed, and re-instated and the frequency of oppositional behaviour decreased, increased, and decreased again as a consequence. Fluctuations in enuretic episodes were also
correlated with the presence or absence of the contingencies. Suppression of both oppositional behaviour and enuretic activity persisted over an eighteen-month follow-up period. However, even Nordquist did not base any of his assumptions or procedures on specific experimental data. His approach was more one of distilling and utilizing ideas from previous applied work. Surely an awareness of relevant data on interactions from the experimental analysis of behaviour would make an approach more compelling and far-reaching. Once again, if applied behaviour analysts were aware of the types of data on behavioural interactions which have been gained in experimental behaviour analysis they would be better equipped to:

(a) decide on appropriate behaviours to observe and measure;
(b) take steps to minimize or maximize the effects of these interactions; and
(c) as a consequence of these studies, formulate more basic questions to seek initial experimental answers (thereby facilitating communication both from experimental to clinical and from clinical to experimental enterprises).


The following section surveys some recent studies on behavioural interactions which could potentially contribute to this cross-fertilization.
Experimental Studies of Behavioural Interactions

One of the most obvious places to start when examining interactions is with the phenomena produced by varying the absolute frequency of reinforcement in one component of a multiple schedule and leaving absolute reinforcement frequency in the other component(s) unchanged. Reynolds (1961a) classified the interactions in two-component multiple schedules. Following stabilization, he changed the reinforcement schedule in one component and recorded changes in responding in both components. The possible interactions are diagrammed in Fig. 6.2. The interaction is classified as positive if the rate of response during the constant component increases, and as negative if it decreases. The interaction is further classified as induction if the change in the rate of response in the constant component is toward the rate during the varied component, and as behavioural contrast if the change is opposite to the rate in the varied component. The most widely studied of these four interactions is positive behavioural contrast. In the usual simple experiment this is obtained by changing schedules from a multiple VI VI baseline to a multiple VI EXT. The response rate during the extinction component decreases and the rate during the other (unchanged) component increases significantly.

The essential conditions for the production of contrast have been hotly debated and centre around whether reduction in responding or reduction in reinforcement rate are of major
FIGURE 6.2:

Changes in response rates as a function of different types of changes in the schedule in the second component of multiple schedules. This figure illustrates the four basic behavioural interactions in multiple schedules: positive and negative contrast and positive and negative induction.
importance. Many experiments producing contrast have involved a reduction in both. However, Weisman (1969) has shown that it is possible to obtain contrast under conditions in which the rate of responding, but not the rate of reinforcement, was reduced. Such experiments support Terrace's (1966a) contention that contrast is a result of a reduction in the relative rate of responding in one of the components. Nevin (1968), however, demonstrated that contrast is not necessarily a consequence of reduced responding. This issue is not finally resolved. What is important is that contrast is a remarkably robust effect which is frequently reported in the animal literature. It is strikingly obvious, though, that the phenomenon is never mentioned in the applied literature. This is odd because it is an interaction of which we should be particularly aware. But there is no reference whatsoever, for example, in any issue of the Journal of Applied Behavior Analysis (as revealed by reference to the Cumulative Index of Volumes 1-8, 1968-1975). It is possible that this is because contrast and induction do not occur in complex human behaviour. It is more likely, however, that it occurs but is not recorded as an effect of intervention. The stimulus control literature has been assimilated into applied behaviour analysis to an even smaller extent than schedule control data.

The date from multiple-schedule interactions may well have unexplored consequences for the important issue of generalization of treatment effects from therapy to the usual environment and may explain some treatment failures. Consider, for example, a situation
in which it is decided to apply extinction procedures to a problem behaviour in a therapeutic setting. Records show that, over sessions, the occurrence of that behaviour decreases in the therapist's presence. However, it is reported that the same behaviour has increased in frequency outside the therapeutic situation. If we view the therapy situation and the "real World" as two components of a multiple schedule we could conceptualize the changes in behaviour as an instance of positive contrast. In the situation above, generalization of therapeutic effects requires the production of negative induction. Taking the examples shown in Fig. 6.2 we could decide that a DRO procedure, applied to the undesirable behaviour may result in generalization.\(^5\) Similarly, it may be possible to choose schedules and schedule values to produce any of the other interactions. What are urgently needed are parametric data on human performance under multiple schedules with simple response requirements to test the relevance of this suggestion.

Recent studies in the experimental analysis of behaviour point to areas which may be important to investigate. Boakes

---

\(^5\) This raises an important point about combining information from different sources. We could, in the above example, decide to use a DRO procedure because we think it more likely to lead to negative induction. However, were we aware of more of the experimental literature we may also choose DRO because it may more permanently reduce behaviour than does extinction (Uhl and Sherman, 1971; Zeiler, 1971). If we combine these two reasons we have an even more powerful justification for our choice of procedure. Once again, the plea is for extensive knowledge of experimental data so that different sources pertaining to the same problem may be effectively integrated.
(1973) has shown, for example, that in a situation in which behavioural contrast was obtained under multiple VI EXT schedules, the use of multiple VI free VI conditions resulted in a slight negative induction. Wilkie (1973) found that introducing a signal indicating the availability of reinforcement in one component of a multiple schedule produced (in most subjects) an increased response rate in the unchanged component (i.e., showed behavioural contrast). His results also replicated Reynolds and Limpio's (1968) finding that even when the frequency of reinforcement in the signalled component is greater than that in the unaltered component, contrast still occurs. Wilkie's (1973) findings have subsequently been replicated by Thompson and Corr (1974).

Interactions in multiple avoidance schedules have been shown by de Villiers (1972) to be comparable to those in multiple schedules of positive reinforcement. When shock-rate reduction was decreased in one component, there was an increase in responding in the other component even though shock-rate reduction in that component remained constant. Conversely, when shock-rate reduction was increased in one component, response rate in the other component decreased. Once again, these data indicate a need to be aware of, and monitor, patient behaviours where avoidance schedules are being utilized as part of therapy. A number of similar studies attest to the ubiquity of behavioural interactions in both positive and negative procedures. Wertheim (1965) reported that changes in the response-shock interval in
one component of a multiple schedule lead to changes in the rate of avoidance behaviour in the second component (in which the response-shock interval was held constant). Brethower and Reynolds (1962) showed that the rate of occurrence of unpunished behaviour was also affected by manipulation of punishment contingencies for another response in a multiple schedule. In their study, mult VI3-min VI3-min schedules were correlated with a red and green light, respectively. Responses in the presence of green led to a brief unavoidable shock. As a result, response rate in the presence of the green stimulus decreased and, at the same time, response rate increased in the presence of the red light, thus showing a facilitative effect of punishing an alternative response. A later experiment by Rachlin (1966) also reported that the introduction of punishment in one component of a multiple VI VI schedule resulted in an increase in the rate of punished behaviour in a second component. These data indicate that, in a multiple-schedule situation, the use of aversive stimulation in one component will produce changes in behaviour in the other component, regardless of whether the other response is reinforced or punished. This may well raise some important questions for the programming of generalization in applied behaviour analysis. Again looking at the therapy session and the outside environment as two components of a multiple schedule, we can see how these data might explain how a response can be punished in a therapeutic situation, but increase in frequency outside the therapist's office. What we must do is be aware of the conditions under which such effects are experimentally induced
and look for these same conditions in the clinical setting.

A study by Lattal (1970), which extended the Rachlin (1966) experiment, is further indicative of the interactive nature of behaviour. Pigeons were trained on multiple VI VI reinforcement schedules with each response in one component being followed by a brief shock. Following stabilization of rate of punished responding, reinforcement frequency in the second component was varied. The results again showed "that variables removed from the stimulus conditions in which punishment occurs may exert control over the punishment rate of response" (1970, p.323). Higher relative frequency of reinforcement in the punishment component lead to increased rate of punished responses, while rate of punished responses decreased with lower relative frequency of reinforcement in that component. The interactions here underline the necessity to measure behaviours both inside and outside the therapeutic situation if the therapist is to understand the causes of behaviour change. For example, it could be that a decrease in responding correlated with punishment of that response in therapy is more a result of increased reinforcement of that response by the wider environment than of the intervention. We will be unaware of this possible complication if we do not monitor behaviours adequately.

In discussing his results, Lattal (1970) covers a number of points which should be considered by applied behaviour analysts. He notes that the facilitation of the unpunished response rates
reported by Brethower and Reynolds (1962) did not occur following the initial introduction of punishment in one component of the multiple schedule. A possible explanation is offered in terms of different previous histories of punishment (cf. Terrace, 1966a, Exp. 1, who found that behavioural contrast in positive reinforcement schedules is weakened by a history of extinction). Lattal makes a plea for parametric research into punishment-produced contrast effects in multiple schedules. His call is taken up here in the wider context of the expansion and cross-fertilization of experimental and applied behaviour analysis. The urgent need from the applied viewpoint is to have as much data as possible on the conditions leading to interaction effects. If experimental data are to be usefully applied to human behaviour, much parametric data of the sort wanted by Lattal must be collected. Only with such data will the application of the principles alluded to above be practical. It is at this interface that communication between pure and applied analysis will be most beneficial.

Another effect noted by Lattal (1970) was the tendency for the rate of punished behaviour to increase with successive returns to the baseline multiple schedule. This confirms earlier studies (e.g., Azrin and Holz, 1966) indicating the recovery of punishment response rates following long-term exposure to punishment and has obvious implications for extended use of punishment with human behaviour. If such extended use is necessary, however, this study indicates that the manipulation of
reinforcement-related variables (rather than a direct change in punishment variables) may still suppress behaviour to a lower level.

Apart from pointing out the direct applications or areas of potential investigation suggested by this study, the important point being made here is just the simple fact that one small study can provide so much information of direct relevance to applied behaviour analysis. How much more powerful would applied behaviour analysis be with a comprehensive and continuous assimilation of data from the experimental laboratory. Conversely, how much more relevant would experimental behaviour analysis be if some of its questions were those asked by clinicians.

Lattal and Griffin (1972), investigating punishment contrast during free-operant avoidance, found some transient effects which should also be of interest to applied researchers. Punishment of bar presses (of monkeys) with shock in one component of a multiple free-operant avoidance schedule resulted in suppressed responding in that component. At the same time, response rates in the unpunished component increased. When punishment was removed altogether, response rates in both components increased, but later decreased with successive sessions. Azrin and Holz (1966) have reported that when punishment of positively reinforced responses was discontinued, response rates transiently exceeded those measured before punishment was introduced. These two sets of results should serve to warn applied clinicians that
there may be a temporary increase in the rate of response of a previously punished behaviour when a punishment contingency is removed.

We should also, in the applied setting, be concerned about the effects on qualitatively different responses of altering the rate of a particular response. A number of studies have potential implications in this area. Scull and Westbrook (1973) have specifically examined interactions in multiple schedules with different responses in each of the components. The studies examined so far have concentrated on the effects on response rate in a constant component of changing the schedule of reinforcement in another component. Premack (1969), however, has suggested that there may be limits to these interactions, and one possibility is that interactions involving different responses in each component may differ from those involving the same response. Scull and Westbrook (1973) tested this possibility by having pigeons perform on multiple schedules with a key lit in one component and a bar present in the other. Some subjects were trained to peck the key and to press the bar with their feet, while others were required to peck both the bar and the key. Following exposure to mult VI VI schedules, the pigeons were exposed to extinction in one component. Subjects in the key-peck/bar-press condition generally showed a decrease in response rate in the constant component (negative induction). However, subjects in the key-peck/bar-peck condition generally showed positive behavioural contrast (an increase in response rate in the
constant component). The possibility exists, then, that for contrast to occur, the behaviours in both components should be topographically similar. However, the generality of this conclusion has been questioned by Beninger and Kendall (1975) who found it is possible to obtain contrast with topographically different operants. It is important that such confounding results should not deter applied behaviour analysts from coming to grips with experimental studies. Such contradictions merely point to new factors which must be investigated and it is important that applied behaviour analysts maintain an interest in such experimentation, for only in this way will very specific data be able to be applied to specific situations. This is indeed what the inter-relationship between experimental and applied behaviour analysis is aimed at. In the Beninger and Kendall (1975) case it is possible, for example, that the disagreement with Scull and Westbrook's (1973) findings may be accounted for by the use of different species or of differences in topographical overlaps. It is these limiting conditions, however, which will allow the application of experimental findings to the fine grain analysis of human behaviour.

Beninger and Kendall's (1975) study also investigated another aspect of contrast which is of interest to the applied researcher. Rats were trained on a multiple random-interval random-interval (mult RI RI) schedule with sweetened condensed milk as the reinforcer in one component and solid pellets in the other. They were interested to see if contrast occurred following the
withdrawal of a reinforcer (that is, if one component was changed to extinction) and to measure to what extent the magnitude of the effect was dependent on the removal of a particular reinforcer. Contrast effects were recorded following the removal of both reinforcers, respectively, in different phases of the experiment. However, the effect was larger and long-term when extinction was introduced into the milk component than into the pellet component (where one rat displayed induction and the contrast effects for the other two were only transient). In a second experiment, Beninger and Kendall (1975) showed that, in time measures, milk was preferred to pellets. Using this as confirmatory data, they suggested that the results of the first experiment can be understood with reference to Herrnstein's (1970) analysis of responding in concurrent and multiple schedules. As we saw earlier, this formulation states that rate of response in one component is a function of rate of reinforcement in that component. If there are different reinforcement rates in two components of a multiple schedule, a change to extinction in the higher reinforcement-rate component will result in a larger contrast effect than a change to extinction in the lower reinforcement-rate component. Further, Hollard and Davison (1971) have shown that quality and rate of reinforcement are functionally interchangeable. Therefore, removal of the preferred reinforcer should lead to a greater contrast effect than removal of the non-preferred one. The larger contrast effect with the removal of milk in Beninger and Kendall's (1975) study is consistent with this expectation. Again, these results
suggest the need for parametric data for the human case. With such information we may be in a position to predict if the removal of particular reinforcers in clinical situations is likely to have effects on other behaviours. Conversely, if contrast effects are desirable these data indicate the sort of manipulations which may enhance them.

The issue of stimulus control is also important in studying behavioural interactions. A number of authors (e.g., Wertheim, 1965; Pear and Wilkie, 1971) have suggested that the degree of stimulus control present in a situation will partly determine the type of interaction that occurs in multiple schedules. Powell (1973) studied discriminative responding in pigeons under multiple VI EXT schedules in which either a white keylight or a tone were correlated with extinction. When keylight colours were correlated with reinforcement and extinction, a high degree of stimulus control over responding was recorded. When a tone was correlated with extinction, discriminative responding was much less accurate. Powell found that there was no interaction between schedule components in the strong stimulus-control situation when extinction was introduced. However, in the weak stimulus-control situation there was marked induction between components. If this finding is a general one it could account for the relative paucity of obvious instances of interactions such as contrast in the human situation. If we can assume that often the therapy situation and the outside world are easily distinguished from each other, and that the patient's behaviour
is under strong stimulus control, we might not expect changes in therapy to interact to cause changes in behaviour outside. This poses a problem for some attempts to programme generalization and is strong argument for treatment in the problem environment. (The whole issue of stimulus control is one which has been largely neglected by applied behaviour analysts and will be discussed in Chapter 8.)

The final factor we will consider as a contributory factor to contrast effects is deprivation. Herrnstein and Loveland (1974) have shown that as deprivation decreases, so does the amount of contrast, until eventually multiple-schedule performance becomes almost identical to concurrent-schedule performance. Specifically, Herrnstein and Loveland varied the body weights of pigeons working for food on a multiple VI1-min VI4-min schedule. As body weight increased, the ratio of responses in each component approached, and eventually reached, the ratio of reinforcements. While it is still a question for extensive investigation, this could suggest that programmes which utilize deprivation to make reinforcers more potent could also be more likely to show contrast effects.

I have tried, by briefly examining some multiple-schedule data, to indicate the breadth and complexity of interactions known already from one small area of experimental research. I have not touched on the many other areas of equal relevance such as behavioural drift (Keehn, 1972), schedule-induced phenomena such
as adjunctive behaviours (Falk, 1972), or the side effects of specific control procedures (Ulrich, Dulaney, Kucera, Colasaco, 1972), to name but a few. Similarly, while I have discussed the concurrent-schedule literature in another context I have not extended the analysis here to the area of interactions. Neither have I provided experimental/clinical tests of the suggestions made - each would occupy the entire content of a separate research project. Rather the object of the present work is to point to the richness of data which already exist and which should be investigated in applied settings. I have suggested many potential applications in order to foster the applied research which is needed and which suggests, in turn, more experimental studies, particularly of a parametric nature.

The data presented on multiple-schedule interactions do serve, however, to point up the complexity of analysis which the experimental analysts have achieved. Current applied behaviour analysis obviously fails to confront this complexity and will continue to err on the side of extreme simplicity until these

---

6 But it should be considered, particularly in relation to ethics. Following the Herrnstein (1970) formulation, it is obvious that an increase in a response following an increase in reinforcement rate must be accompanied by a decrease in concurrent responses if their reinforcement rates are relatively different as a consequence. Obviously, we should be aware of such potential changes before intervention so that we are able to weigh up the social/personal costs of the decreases in these behaviours.
and similar data from the broad spectrum of experimental behaviour analysis are incorporated into everyday behaviour analysis and modification.
CHAPTER 7

Conservation Theory and Applied Behaviour Analysis

The advent of a recent model of behaviour, conservation theory (Allison, 1976a,b), may help to make more coherent some of the implications of the data presented thus far. I consider Allison's work to be of potentially far-reaching importance to applied behaviour analysis and will, therefore, consider it in some detail.

The experimental situation used by Allison involves two kinds of daily sessions of constant duration, in which rats get all of their daily water intake. The first type of session is the baseline session in which both a lever and a water tube are freely available. The second is a contingency session in which a fixed amount of lever pressing is required for each access to water, and a fixed amount of drinking is necessary to reinstate the lever-pressing conditions.

Under these conditions, some interesting facts emerge. If, during the contingency session, the organism is unable to perform its baseline amount of drinking except by exceeding its baseline amount of lever-pressing, the contingency will result in more lever-pressing relative to its baseline, and less drinking relative to its baseline. The size of the effect depends on a number of factors. The amount of lever-pressing required for
each access to water and the amount of drinking required to return to pressing, partly determine how much the schedule will increase lever-pressing and decrease drinking. If a higher lever-press requirement is operative, the rat will increase lever-pressing, but not to the extent required to keep drinking constant. If the requirement for drinking is increased, the rat will increase total drinking but will decrease total lever-pressing.

Fig. 7.1 (from Allison, 1976b) illustrates these findings with data from a study by Miller and Allison (1975). The top panel shows time spent lever-pressing, while time drinking is shown in the lower panel. The baseline levels before and after contingency training are shown by the isolated dots at the extreme left and right of each panel. The rats were run on nine schedules formed by combining the three level-press requirements shown on the abscissa (L = 10-, 20-, or 30-sec) with the three drink requirements shown as a parameter (C = 20-, 30-, or 40-sec of drinking). Note that in no case was a rat able to perform its baseline amount of drinking without performing more than its baseline amount of lever-pressing. Fig. 7.1 shows that, generally, lever-pressing was facilitated relative to baseline lever-pressing, and drinking was suppressed relative to baseline drinking. As the amount of pressing required for each access to water (L) increased, the total amount of pressing increased, but the total amount of drinking decreased. As the amount of drinking required by C increased, total drinking increased, but total pressing decreased.
FIGURE 7.1:

Time spent lever pressing and drinking as a function of lever-press requirements for each of the three drink requirements (C = 20, 30, and 40). Baseline levels before and after contingency training are indicated by the isolated points at the extreme left and right, respectively. The data illustrated are the group data for six rats from a study by Miller and Allison (1975).

[From Allison, 1976b]
FIGURE 7.1

LEVER PRESSING

SEC

C = 20

C = 30

C = 40

1200

1000

1000

900

800

700

600

BASE  I=10  I=20  I=30  BASE

LEVER PRESS REQUIREMENTS

C = 20

C = 30

C = 40
Allison (1976b) interprets these data in the following manner:

"... the rat conserves between baseline and contingency conditions the total amount of a dimension apportioned to lever-pressing and drinking, each of these responses entailing a positive amount of the dimension" (p.4).

The mathematical description of this model is given by Allison (1976a). A unit-free parameter, $k_{ij}$ is the amount of the dimension entailed in performing one unit of response $i$ (in this case, lever-pressing), relative to the amount entailed in performing one unit of response $c$ (drinking). The total amount of response $i$ is represented by $0_i$, and the total amount of response $c$ by $0_c$. In the schedule specified earlier, each component requires a fixed amount of each response ($1$ for response $i$ and $C$ for response $c$). The model thus states:

$$ N(kl + C) = k_{0i} + 0_c \quad \quad (7-1) $$

With $k$ defined as above, Equation 7-1 gives the total amount of the dimension apportioned to responses $i$ and $c$. The total for the baseline session is specified by the right side of the equation, and that for the contingency session by the left. The equality shows that the organism conserves this total between baseline and contingency sessions.

Fig. 7.2 shows the fit of obtained values, for six individual
FIGURE 7.2:

Time spent lever pressing and drinking as a function of the times predicted by the conservation model for the six individual rats in the Miller and Allison (1975) study.

[From Allison, 1976b.]
FIGURE 7.2

R-1  R-2

R-3  R-4

R-5  R-6

O = D
D = LP

OBTAINED

SEC

SEC PREDICTED
rats in Miller and Allison's (1975) study, to the values predicted by the conservation model. Closed circles represent level-pressing, and open circles represent drinking. It is apparent that the model provides a good fit. Other models do not fare so well. Allison (1976b) demonstrates this with figures showing that a regulatory model which assumes that the rat will continue to perform the baseline amount of drinking, predicts far too much responding. Conversely, far too little responding is predicted by a development of Herrnstein's matching law suggested by Mazur (1975).

Fig. 7.3 shows the sorts of predictions made by the conservation model. This hypothetical example shows five schedules involving two responses (Responses i and c). Response i is illustrated by the top half of the figure and Response c by the bottom. The baselines of these responses are shown by the broken lines. In this case, Response c has a higher baseline than Response i (200 vs 100). The example assumes that one unit of Response i entails the same amount of the dimension (say, time) as one unit of Response c. It is also assumed that the schedule parameter c remains constant at 20 units for all schedules (i.e., each schedule requires a certain amount of Response i for 20 units of Response c).

With reference to the right side of Fig. 7.3, we see that the model predicts facilitation of Response i and suppression of Response c if a schedule requires 15 or 20 units of Response i for
FIGURE 7.3:

Predicted times engaged in two alternatives as a function of the schedule parameter $I$ for five hypothetical contingency schedules, each involving the same two responses referred to as Response $I$ (e.g., running in an activity wheel) and Response $C$ (e.g., drinking). The top half of the figure refers to Response $I$, and the bottom to Response $C$. The two broken lines show the baselines of these responses.

[From Allison, 1976b.]
FIGURE 7.3

PARAMETERS
\[
\begin{align*}
0_i &= 100 \\
0_c &= 200 \\
C &= 20 \\
k &= 1
\end{align*}
\]

MODEL
\[
N(ki + C) = k0_i + 0_c
\]
20 units of Response c. If the schedule parameter l is reduced slightly so that only 10 units of Response i are required, the model predicts equilibrium (see centre of Fig. 7.3). The left of the figure shows that Response i will be suppressed and Response c facilitated if l is reduced still further (to 5 or 1).

It is important here to relate the conservation model to the predictions made by Premack theory, to which prominence was given in Chapter 5. Premack states that if, under paired baseline conditions, Response c is more probable than Response i, then a contingency between the two will facilitate Response i relative to baseline (0i) and suppress Response c relative to baseline (0c). The conservation model predicts the same outcome, but because 10c was greater than 0c, not because Response c was more probable than Response i. This difference is important in cases for which Premack theory predicts no facilitation of Response i because it is more probable than Response c. However, conservation theory would predict facilitation where 10c is greater than 0c. Similarly, where 10c = 0c, conservation theory would predict no effect, whereas Premack theory would predict facilitation of Response i because Response c is more probable. In these crucial areas, the experimental data support the conservation model (Allison and Timberlake, 1974; Timberlake and Allison, 1974).

An important influence on the model's prediction is the amount of the dimension entailed in performing one unit of
Response \_ \_ relative to that entailed in performing one unit of Response \_ \_. Fig. 7.4 shows the effect where the contingency schedules require 1, 5, or 10 units of Response \_ \_ for 10 units of Response \_ \_. The top curve in the upper panel shows that if (unit-for-unit) Response \_ \_ entails only 1% of the amount of the dimension Response \_ \_ entails, Response \_ \_ will be highly sensitive to changes in L (the schedule parameter). As L increases, the total amount of Response \_ \_ will increase markedly. However, the top curve in the bottom panel shows that Response \_ \_ will be almost unaffected by changes in L. Although all the schedules should suppress \_ \_, it is unlikely that the change will be large enough to be detected.

The bottom curve on the upper panel shows the effect at the other extreme - if Response \_ \_ entails 100 times as much of the dimension as Response \_ \_. Now there is a similarly minute facilitation of Response \_ \_ but a clear and marked suppression of Response \_ \_ (shown in the bottom curve of the bottom panel). In other words, where the dimensional parameter k in Equation 7-1 is small, Response \_ \_ will be greatly facilitated and Response \_ \_ will be minutely suppressed; where k is large, Response \_ \_ will be minutely facilitated and Response \_ \_ greatly suppressed.

Allison (1976b) points out that these predictions made by the conservation model are able to interpret some of the so-called "biological constraints" on behaviour in a practically important
FIGURE 7.4:

Predicted times engaged in two alternatives as a function of the schedule parameter $I$ for three hypothetical contingency schedules requiring 1, 5, or 10 units of Response $i$ for 10 units of Response $c$. The dotted lines represent the baselines of these responses.

[From Allison, 1976b.]
FIGURE 7.4

PARAMETERS
\[
\begin{align*}
O_i &= 10 \\
O_c &= 1000 \\
C &= 10
\end{align*}
\]

MODEL
\[
\frac{N(kI + C)}{k} = kO_i + O_c
\]

\(k = 0.01\)
\(k = 0.1\)
\(k = 1\)
\(k = 10\)
\(k = 100\)
\(k = 0.01\)
\(k = 0.1\)
\(k = 1\)
\(k = 10\)
\(k = 100\)
fashion. He cites Shettleworth's (1975) work with golden hamsters in which eating was the contingent Response $c$ (in food-deprived subjects) and the instrumental Response $i$ was either digging, scrabbling at a wall, open rearing, scratching the body, or scent marking. The first three of these responses were all facilitated, indicating a small to moderate $k$ for pairs involving scrabbling, digging, or open rearing as Response $i$, and eating as Response $c$. If this interpretation is correct, and the size of the dimensional parameter $k$ is vital, then we should be able to facilitate eating while suppressing face-washing, scratching, or scent marking. This remains to be experimentally tested, but obviously if the predictions are borne out we can view "biological constraints" in a new light. "If it could be demonstrated empirically, we would seem to be dealing with constraints on performance rather than constraints on learning" (Allison, 1976b, p.8).

The importance of conservation theory lies in its ability to predict the outcomes of contingency manipulations. This has been strikingly demonstrated by Allison (1976a) in an experiment designed to predict contrast and induction and is of particular relevance in the light of comments made in the preceding chapter. In this study, a mixed schedule composed of two alternating components was used. Response $i$ was pressing and holding a lever and Response $c$ drinking from a water spout. Rats were required to press and hold the lever for a fixed amount of time ($1_1$ sec) in the Component 1, for access to the water spout.
Following the appearance of the spout, a cumulative total of (C₁ sec) of drinking was required for the spout to retract and the lever to re-appear. This signified the start of Component 2, during which (I₂ sec) of pressing and holding was required for (C₂ sec) of drinking, after which the spout retracts, the lever is reinstated, and Component 1 begins again. The dependent variable, N, is the number of completions of this sequence in a session of fixed duration.

Table 7.1 shows a hypothetical set of data from such schedule arrangements. Schedule A shows that each of four schedule requirements is 10-sec. If an organism completed the sequence 30 times (N = 30) it would spend 300-sec in each component. The remainder of Table 7.1 illustrates the effects of varying Schedule A by changing only Component 2. Schedule B₁ (Rows 2-6) involves a change of I₂ and C₂ from 10-sec to 5-sec. Row 2 shows that if the organism completed the sequence 40 times, the total time spent lever-pressing would increase in the unchanged Component 1 (relative to Schedule A), and decrease in the changed Component 2, thus demonstrating positive contrast. Rows 3-6 show that the direction of the effect is vitally dependent upon N. Row 3 shows that N = 70 leads to positive induction; Row 4 shows that N = 20 results in negative induction; and Rows 5 and 6 show that other values of N can lead to no contrast and no induction.

It is also apparent from Table 7.1 that particular schedules exclude particular effects. For example, negative contrast is not
TABLE 7.1:
Hypothetical responding to schedules requiring $I_1$ sec of lever pressing for $C_1$ sec of drinking in Component 1, and $I_2$ sec of lever pressing and $C_2$ sec of drinking in Component 2.

[From Allison, 1976a.]
<table>
<thead>
<tr>
<th>Rule</th>
<th>Schedule</th>
<th>C1</th>
<th>C2</th>
<th>N</th>
<th>Component 1</th>
<th>Component 2</th>
<th>Effect Relative to Schedule A</th>
<th>Component 1</th>
<th>Component 2</th>
<th>Effect Relative to Schedule A</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>A</td>
<td>10</td>
<td>10</td>
<td>10</td>
<td>10</td>
<td>30</td>
<td>300</td>
<td>300</td>
<td>300</td>
<td>Positive contrast</td>
</tr>
<tr>
<td>2</td>
<td>B1</td>
<td>10</td>
<td>10</td>
<td>10</td>
<td>5</td>
<td>40</td>
<td>400</td>
<td>400</td>
<td>200</td>
<td>Positive contrast</td>
</tr>
<tr>
<td>3</td>
<td></td>
<td>70</td>
<td>70</td>
<td>350</td>
<td>Positive induction</td>
<td>700</td>
<td>Positive induction</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4</td>
<td></td>
<td>20</td>
<td>20</td>
<td>100</td>
<td>Negative induction</td>
<td>200</td>
<td>Negative induction</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>5</td>
<td></td>
<td>30</td>
<td>30</td>
<td>150</td>
<td>-</td>
<td>300</td>
<td>150</td>
<td>-</td>
<td>-</td>
<td></td>
</tr>
<tr>
<td>6</td>
<td></td>
<td>60</td>
<td>60</td>
<td>300</td>
<td>-</td>
<td>600</td>
<td>300</td>
<td>-</td>
<td>-</td>
<td></td>
</tr>
<tr>
<td>7</td>
<td>B2</td>
<td>10</td>
<td>10</td>
<td>15</td>
<td>15</td>
<td>25</td>
<td>250</td>
<td>250</td>
<td>375</td>
<td>Negative contrast</td>
</tr>
<tr>
<td>8</td>
<td></td>
<td>10</td>
<td>10</td>
<td>150</td>
<td>Negative induction</td>
<td>100</td>
<td>Negative induction</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>9</td>
<td></td>
<td>40</td>
<td>40</td>
<td>600</td>
<td>Positive induction</td>
<td>400</td>
<td>Positive induction</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>10</td>
<td></td>
<td>30</td>
<td>30</td>
<td>450</td>
<td>-</td>
<td>300</td>
<td>450</td>
<td>-</td>
<td>-</td>
<td></td>
</tr>
<tr>
<td>11</td>
<td></td>
<td>20</td>
<td>20</td>
<td>300</td>
<td>-</td>
<td>200</td>
<td>300</td>
<td>-</td>
<td>-</td>
<td></td>
</tr>
<tr>
<td>12</td>
<td>B3</td>
<td>10</td>
<td>10</td>
<td>15</td>
<td>10</td>
<td>25</td>
<td>250</td>
<td>250</td>
<td>250</td>
<td>Negative induction</td>
</tr>
<tr>
<td>13</td>
<td></td>
<td>20</td>
<td>20</td>
<td>300</td>
<td>-</td>
<td>200</td>
<td>200</td>
<td>-</td>
<td>-</td>
<td></td>
</tr>
</tbody>
</table>
possible under Schedule B₁, because an N large enough to produce an increase in time spent lever-pressing in Component 2 necessarily produces an increase in time spent lever-pressing in Component 1 (positive induction).

Rows 7-13 illustrate similar effects for other changes in contingency schedules. In Schedule B₂, l₂ = 15 and C₂ = 15, and in Schedule B₃, l₂ = 15 and C₂ = 10.

The other response, in this case drinking, is subject to the same effects. Note that positive contrast cannot occur here unless C₂ (the drinking requirement) is decreased relative to Schedule A, and negative contrast cannot occur unless C₂ is increased. It is also interesting to note that the same effect need not apply to both responses. For example, Row 12 shows negative contrast with respect to lever-pressing, but negative induction with respect to drinking.

If earlier experiments (e.g., Premack, Schaeffer, and Hundt, 1964; Barofsky and Hurwitz, 1968; Premack, 1971, 1972; Bernstein, 1973; Allison and Timberlake, 1974) are evaluated in the context of the conservation model, their data all conform to its predictions. If further extensive empirical test of the model's predictions confirms its utility we would appear to have an excellent framework for predicting behavioural interactions and, therefore, for manipulating contingencies to produce specified interactions. Taking the simple situation as an example, we
know that if we manipulate contingencies so that a patient is unable to perform his baseline amount of an undesirable behaviour without exceeding his baseline amount of a desirable behaviour, the contingency will result in facilitation of the desirable behaviour and suppression of the undesirable behaviour.

As a practical example, we could analyze the problem presented by the woman in Chapter 4 according to the conservation model. We have two types of session, a baseline and a contingency. In the baseline session, both doing the household chores and reading books are freely available. In the contingency session, Mrs. A must complete a household chore before being allowed to read.

Conservation theory tells us that if the chore requirement and the reading requirement specified by the schedule meet a certain antecedent condition, then Mrs. A will work more, but read less, than she did in the baseline session when both activities were freely available. We can specify the antecedent condition as: if Mrs. A could not perform her baseline amount of reading except by performing more than her baseline of chores, then the contingency will facilitate the carrying out of chores relative to its baseline, and suppress reading relative to its baseline. Allison's work shows that the size of the effects is partially dependent on the amount of chore-completion required for each access to reading, and on the amount of reading required for each access to chores. However, Mrs. A should
respond to a higher chore-completion requirement by increasing the total amount of chores completed in a session, but not enough to hold reading constant. Thus, an increase in chore-completion should be accompanied by a decrease in reading.

We can more clearly see the model in operation if we return to Fig. 7.3 and suppose that the five hypothetical contingency schedules shown involve doing chores as Response 1 and reading as Response C. The baselines (shown by the dotted lines) indicate that reading has a higher baseline than doing chores (200 vs 100). The example illustrated assumes that the schedule parameter C is always fixed at 20 units for all schedules. The figure shows (right-hand side) that if a schedule requires 15 or 20 units of chores for 20 units of reading, the schedule will facilitate chore-completion and suppress reading. The outcomes of other manipulations are also shown. In other words, the model gives us an empirical basis upon which to make decisions as to the size of specific manipulation of variables.

Fig. 7.3 suggests that given that we can measure baseline rates of clinically significant behaviours and that we can control the schedule parameter C (for example, hold it constant), the conservation model allows us to predict which values of the schedule parameter 1 will be needed to facilitate or suppress the target behaviour. Thus, instead of judging or guessing what adjustments need to be made we will be able to predict accurately the effects of specified adjustments.
This model also allows us to predict how sensitive a behaviour will be to changes in contingencies. Suppose we wish to increase Behaviour A without affecting the rate of Behaviour B. Observation has shown that Behaviour A entails only a small amount of the dimension Behaviour B entails. We know that under these conditions Behaviour A will be extremely sensitive to changes in the schedule parameter controlling it and that we need not vary this parameter much in order to alter the rate of Behaviour A. At the same time, although Behaviour B should be suppressed by this manipulation, the model allows us to predict that this effect will be so small as to be insignificant. To take the other extreme, if we know that Response A entails much more of the dimension than Response B, attempts to increase the former will be difficult and gains made will be at the expense of marked reduction in the rate of Response B - which may raise practical or ethical difficulties. Further, as pointed out in discussing Shettleworth's (1975) experiment, having established the value of \( k \) for a number of behaviours, we are able to predict which contingency arrangements should facilitate one behaviour while suppressing another.

Finally, when applied to more complex situations, (e.g., the mixed schedules in Allison's, 1976a, experiment) the conservation model allows us to predict the occurrence of behavioural interactions such as contrast and induction. If we wish to produce any of these effects, we now have a framework within which to plan their occurrence. Obviously, wide-ranging experimentation with
more schedules is necessary to test the generality of the model, but on the present evidence I suggest that its adoption as a basis for much applied behaviour analysis would significantly advance that discipline's predictive power. However, conservation theory must be widely articulated to achieve this end or it is likely to suffer the same fate as Premack theory - to be generally overlooked and unused.
I noted earlier that the area of stimulus control is even more under-represented than that of schedule control in the rate of incorporation of experimental data into applied behaviour analysis. While all general accounts of applied behaviour analytic procedures dwell on the necessity for bringing behaviour under the control of appropriate discriminative stimuli, few cite actual procedures directly derived from laboratory data. Those that do so, frequently quote only general review material (usually Terrace, 1966a) as the basis for their assumptions. There has been, over recent years, a growing awareness of the importance of stimulus control as it affects the transfer of behaviour change from a therapeutic to a "real-life" environment (Baer, Wolf, and Risley, 1968; Walker and Buckley, 1972; Rincover and Koege!, 1975). Kazdin and Bootzin (1972) have noted, in a survey of the token economy literature, the importance of planning the transfer of stimulus control from one environment to another. In few instances, however, have clinicians attempted to use the specific techniques extensively reported in experimental behaviour analysis to modify stimulus control of behaviour. Again we are confronted with procedures being based more on the "idea" than on specific data. The commitment is philosophical rather than experimental. The purpose of this chapter is briefly to indicate some areas of the stimulus control literature which should be routinely incorporated.
into applied behaviour analysis in the hope that this important area will cease to be the neglected wasteland it presently seems to be.

Staddon (1972) makes a distinction between two types of stimulus control which appears useful to adopt in applied behaviour analysis. He notes that Skinner's description of a discriminative stimulus as "setting the occasion" for a response, acknowledges that there is no fixed relationship between the precise occurrence of the controlling stimulus and the precise occurrence of each response. Thus, the behaviour may occur with a lesser probability in the absence of the stimulus, but there is no absolute probability that this will be true in each instance. Staddon proposes the term situational control for this type of relationship between a discriminative stimulus and responding. He means by this that while we can establish a controlling relationship between a discriminative stimulus and behaviour, we cannot predict the actual temporal occurrence of the behaviour.

The second type of stimulus control proposed is temporal control. This describes those cases in which the onset of a stimulus at a particular point in time can be shown to determine the time of occurrence of a response. Schneider (1969) has shown that the pause after reinforcement on fixed-interval schedules is approximately proportional to the interval duration. This is an example of temporal control, because the onset of pecking is controlled by the time since the offset of reinforcement.
Responding is, therefore, always under both situational and temporal control. In the context suggested by Staddon, situational control may be said to act on the relation between responses and an antecedent stimulus (rather than directly on responses). Further, situational control now may be said to "set the occasion" not for responses, but for temporal control to operate. The state of the organism is therefore controlled by a stimulus which exerts situational control. Thus, "situational control is whatever must be added to temporal control to account for the occurrence of a response. This distinction is proposed as an aid to experimental analysis, because it is often useful to separate the factors which determine whether or not a response can occur at all (situational control, contextual factors) from those that determine exactly when it will occur (temporal control, "causal" factors)" (Staddon, 1972, p. 215). I suggest that this same distinction could well be an important factor in deciding which modification procedures to use or which problem behaviour to modify in applied behaviour analysis.

Staddon points out that the intervening variable "behavioural state" may not be necessary when considering the simple, steady-state behaviour of lower organisms. However, when the analysis is extended to complex performances where there may well be no single identifiable stimulus configuration controlling a specified response, the notion is extremely useful. Human behaviour under verbal control is a case in point. In applied behaviour analysis we may often want to identify such behavioural
states. The analysis of sexual offending is an example. A man may be induced to commit rape by a number of experiences, but just the presence of an attractive female will not typically generate this behaviour.

The multiple-schedule research described in Chapter 6 provides an explicit example of the two types of stimulus control. Here the stimulus correlated with each component of a mult VI EXT schedule indicates the probability of responding to be expected. The stimulus is exerting situational control. However, the actual occurrence of the first response within each FI component is mainly determined by the time since reinforcement - an example of temporal control.

The distinction between situational and temporal control may allow us to make a fine grain analysis of human responding leading to more precise methods of changing problem behaviours. The term situational control refers to the type of control normally referred to as "stimulus control" in operant textbooks (e.g., Terrace, 1966a) and the methods described in the following paragraphs are largely relevant to its establishment. However, a knowledge in depth of schedule-related temporal controlling factors will eventually allow applied behaviour analysis to both set the context and predict the temporal occurrence of behaviour. This is the contribution that the application of stimulus control data should make to applied behaviour analysis.
Another distinction that is important to make is between the stimulus functions of the organism's own behaviour and those of exteroceptive environment. If the organism is responding in a stable environment, discriminative stimuli need not be specified for a contingency to control behaviour in a predictable fashion. Behaviour on fixed-interval schedules provides an example. Here we can suppose that stimuli are provided by the organism's own behaviour. These stimuli will come to gain control over typical response patterns as long as a fairly stable external environment is maintained. The pattern is likely to be changed however, with sudden changes in the environment. Since the possibility of such changes is fairly high in human environments it seems important to learn about such phenomena. When we can identify the stimuli which control behaviour in this manner, we will be able to predict such changes in behaviour.

Ferster and Skinner (1957) have pointed to the stimulus control established by the organism's own behaviour:

"When the more or less stable performance has been well established under a given schedule, the organism is being reinforced under certain stimulus conditions... among the physical events occurring in the experimental chamber are the activities of the organism itself... we determine sensitivity to color by demonstrating a differential reaction to colored stimuli, and we demonstrate a sensitivity to some aspect of the organism's behaviour by demonstrating a differential reaction to that behavior" (p.10).
Ray and Sidman (1970) point out that once an organism comes under the control of its own prior behaviour, the control may be maintained indefinitely by the contingency of reinforcement even where reinforcement is not contingent upon the controlling relation. With a repetitive behaviour pattern, one stage of the pattern may become dependent on a preceding stage. "Once a controlling relation occurs, it is available to be reinforced and maintained" (1970, p.188) and it is important that applied behaviour analysts should be aware of such dependencies for a comprehensive approach to behaviour prediction and control. Of course, it is often impossible to separate stimulus control due to the outside environment. This confounding may not be important where the effects of a schedule are reliable. However, as Ray and Sidman (1970) note, where schedule effects are not reliable, changes in behaviour may be accounted for by inadvertently established stimulus control and it is important for applied behaviour analysts to know about such potential sources of control.

There are a number of laboratory techniques commonly used to bring behaviour under the control of environmental stimuli. Applied behaviour analysts should particularly be aware of how reinforcement establishes control if they wish to use the most efficient techniques for establishing and maintaining stimulus control. The first point we should recognize is that merely reinforcing a response in the presence of a stimulus is not a sufficient condition for the establishment of stimulus control. In basic research, the first requirement is that the stimulus
impinge on the appropriate receptor organ. With pigeons, for example, this is achieved by placing the stimulus (say, a coloured light) in the response key. Similarly, in applied work, efforts must be made to ensure that the relevant stimulus is establishing an input to the system. Instructions are often useful in the human case, but attempts to make stimuli physically salient are also relevant.

Having reached a receptor, there are several ways reinforcement can operate which lead to control by that stimulus. Ferster and Skinner (1957) outlined the simplest possibility:

"The effect of reinforcement is maximally felt when precisely the same conditions prevail. Thus, if a response is reinforced in the presence of stimulus A, any increase in responding will be maximal in the presence of stimulus A" (p.8).

The question of whether such single-stimulus training is sufficient for the establishment of stimulus control has been widely debated. Peterson (1962) suggested that such training is not enough. He raised ducklings in monochromatic light and then, using a key illuminated by the same light, reinforced their key-pecks. In a generalization test, Peterson varied the colour of the key. The ducklings did not respond differentially to key-colour, thus demonstrating a failure to develop stimulus control. Control ducklings reared under normal illumination did show generalization gradients (responded differently to the several
colours), showing the establishment of stimulus control. A number of subsequent studies (Ganz and Reisen, 1962; Rudolf, Honig, and Gerry, 1969; Riley and Leuín, 1971) have, however, thrown doubt on the generality of Peterson's results. The issue is yet to be resolved, but for our purposes, it seems that it is easier to establish stimulus control with differential reinforcement in the presence of more than one stimulus.

However, even this latter technique does not guarantee control. Experiments with compound stimuli (Reynolds, 1961b; Newman and Baron, 1965) have shown that control was established only by some stimulus elements correlated with reinforcement and not by others (equally correlated with reinforcement). Maki and Leith (1973) tested the effect on stimulus control by elements of a compound stimulus of reinforcing responses in the presence of each (of two) elements. The task was a three-key matching-to-sample. The comparison stimuli (either solid colours or white lines) were arrayed on a side key. The sample stimuli were either compounds (white lines on coloured grounds) or elements (white lines on black grounds or merely solid colours) and were arrayed on the centre key. Sample stimuli were presented for nine sample stimulus durations. Within each daily session, and according to a random sequence, both compound and element samples were presented at each sample duration. The results showed that compound samples were much less effective than element samples at controlling matching responses (at all sample durations). This suggests that when attention is shared among elements of a compound
stimulus, the result will be a reduction of control by those elements. A general rule describing these phenomena is given by Maki and Leith (1973): "... the degree of control exerted by one element of a compound stimulus is inversely related to the stimulus control exerted by other elements of the compound" (p.348). This has important ramifications for the applied behaviour analyst because it implies that the presence of an element that exerts strong stimulus control over a behaviour will probably reduce the control which can be exerted by other stimulus elements. It is important, then, to assess, where possible, the degree of control exerted by different elements so that contingencies may be arranged to bring the appropriate element into predominance. Similarly, if one element does not exert strong stimulus control, other elements may come to do so. This too, is useful information upon which an intervention plan may be partly based. In either case, if we desire to create unequal stimulus control by elements of a compound, we should give extra training with one element alone (Johnson and Cumming, 1968).

Much of the experimental analysis of stimulus control has centered around attempts to elucidate the nature of "attention" to particular stimuli. In view of the infinite range of possible stimulation to which an organism could respond, it is of interest to know by what process organisms selectively attend to only certain parts of stimulus arrays. In the case of human behaviour it is necessary for a full account of behaviour to be able to identify those elements which control behaviour. It is
further necessary to be able to teach attention to relevant stimuli (bring behaviour under the control of relevant stimuli). The study of selective attention is, therefore, of importance to applied behaviour analysis.

Honig (1970) views selective attention as the modulation of stimulus control. Stimulus control refers to the relation between a set of stimuli and a measure of responding. Terrace (1966a) defines stimulus control as "the extent to which the value of an antecedent stimulus determines the probability measured as a change in response probability that results from a change in stimulus value" (p.271). The generalization gradient has been found to be the most useful measure of stimulus control. The study of stimulus control requires the acquisition of a response in the presence of one or more stimuli on the dimension of generalization. This training is called dimensional acquisition. Even when the conditions of dimensional acquisition are kept constant, the stimulus control measured after such training still shows wide variation among the experimental subjects. Attention can be defined in terms of such variability.

Honig (1970) suggests that we separate systematic variations in stimulus control from the residual variation observed in the data. His aim is to define attentional effects in a way that such variations are brought into relation with experimental procedures that are deliberately manipulated. His definition requires dimensional acquisition to be the same for all
conditions in which systematic variations in stimulus control are observed, so that the experimental source of such variations must lie outside the acquisition procedure, and must therefore lie outside the subject's experience with the training stimuli on the dimension of generalization. The independent treatments in experiments meeting these criteria are generally different kinds of discrimination training procedures involving stimuli that are not involved in dimensional acquisition. Honig (1970) calls such training "independent (discrimination) training."

In some experiments, differential reinforcement with such independent stimuli reduces stimulus control on the dimension of generalization in relation to other groups which receive non-differential training or no independent training. In this type of experiment, a negative type of correlation is therefore obtained between stimulus control on the dimension of independent training and on the dimension of generalization - the subjects are said to "select" from the available stimuli used in discrimination training. Such studies support the idea of selective attention. The crucial assumption implicit in any theory of selective attention is that there is some limit to the amount of information processed on a trial, that cues may compete for the subject's attention, and therefore that the amount learned about one cue is critically dependent upon the number and nature of other cues in the situation.

In other experiments, independent discrimination training
leads to an enhancement of stimulus control on the dimension of
generalization, again in relation to other experiments in which
no discrimination training or equal reinforcement is carried out.
In these "enhancement" experiments, the correlation between the
stimulus control developed in training and that observed on the
dimensions of generalization is thought to be enhanced by such
training.

An important distinction which must be made when discussing
selective attention is that between cue selection and cue
utilization. If more than one cue is correlated with reinforce-
ment during training, responding may come under control of the cues
in question to differing degrees. In the experiments on
"blocking" which I will discuss soon, it has been thought that such
an effect has been demonstrated. However, before we can accept
such an interpretation we must be sure that the studies meet two
conditions. First, it must be demonstrated that such selection
may be brought under experimental control. Second, it must be
shown that such a selection occurs during training, and is not due
to the simultaneous presentation of several cues in testing, one
of which may be "dominant", and prevent any other cues from
demonstrating stimulus control. Thus, after cues have been
presented together in training, it is necessary to present them
separately during testing. Otherwise, we may have evidence for
no more than cue utilization during testing, rather than cue
selection during training.
In an experiment mentioned earlier, Reynolds (1961b) trained pigeons on a go/no-go compound discrimination in which the positive stimulus (S+) was a white triangle on a red background, and the negative stimulus (S-) was a white circle on a green background. In order to determine which of the two relevant dimensions was controlling discrimination performance, Reynolds administered a test in which the four elements (white triangle, white circle, red background, green background) were presented separately in extinction. One of the birds restricted its responding almost entirely to the red stimulus, whereas the other bird responded mostly to the white triangle. Reynolds concluded that each pigeon had attended to only one of the two aspects of the compound S+. The other dimension of S+ had apparently exerted no control over responding. However, as Honig (1970) points out, this conclusion is premature. A more sensitive method of testing shows that the "unattended" elements of a compound pattern can exercise more control than appears at first sight.

Farthing and Hearst (1970) reported a study in which they presented compound cues in training and in testing. S+ was a vertical line on a blue background, while a horizontal line on a green background was S-. When the elements were presented separately in testing, colour exercised a great deal of stimulus control, and line orientation exercised little. These results are similar to Reynolds: the animals responded to one positive element and to neither negative element. However, Farthing and Hearst
also presented combinations of positive and negative elements, and these trials demonstrated some control by line orientation. These trials indicate that a lack of responsiveness in testing does not necessarily imply a lack of attention to one element during training. The presence of elements from both training dimensions can be critical in demonstrating stimulus control gained on either dimension.

Experiments on the phenomenon of "blocking" provide evidence for selective attention. This refers to the "blocking" of one cue (used in dimensional acquisition) when it is redundantly introduced after learning on the basis of a different cue (used in independent training) has already taken place. In blocking experiments it must be demonstrated that each element of a stimulus compound can gain stimulus control when both elements are presented initially in training, but stimulus control by one element can be prevented by the preliminary training carried out with the other.

An experiment by vom Saal and Jenkins (1970) is illustrative of the blocking studies. The key-pecking response of pigeons was used in a discriminated trial procedure in two experiments which evaluated the possibility that prior discrimination training on red versus green would block the development of stimulus control by tone versus noise when both stimulus dimensions simultaneously predicted reinforcement. In Phase 1 of Experiment 1, Group D learned a go/no-go discrimination based
on red versus green; Group N was not run: Group R received reinforced trials only; and Group P received partial reinforcement. In Phase 2, all groups learned a go/no-go discrimination based on tone-plus-red versus noise-plus-green. When tested after Phase 2, Group D showed less auditory control than any other group. The differences between Groups D and P were confirmed in Experiment 2. These results provide a convincing demonstration that prior wavelength training can interfere with the learning of an incidental auditory cue.

In blocking experiments, all groups in a given study have had similar experience with the stimuli on the dimension of generalization on which stimulus control is then examined, and varying experience on the independent training dimension, on which stimulus control is concurrently or previously established to varying degrees. The general result reveals a negative relationship between the degree of stimulus control established on the independent dimension and the degree of stimulus control observed on the dimension of generalization. The other major series of experiments in this area involves the opposite relation - i.e., various degrees of stimulus control are achieved through different independent training procedures. Following dimensional acquisition, stimulus control is examined on the dimension of interest by means by a generalization test. Since discrimination training generally results in steeper gradient slopes than non-discriminative procedures, a positive rather than a negative relationship is obtained between such training procedures and the
stimulus control obtained on the dimension of generalization.

An example of an enhancement study using an interdimensional design (i.e., training is carried out so that one stimulus, usually S+, lies on the dimension of generalization, while the other lies on a different dimension, and is orthogonal to the dimension of generalization, that is, differs from all the stimuli on that dimension to the same degree) is an experiment by Switalski, Lyons and Thomas (1966). They first obtained spectral generalization gradients after acquisition with the green stimulus. Half of the subjects were then given multiple-stimulus training with equal reinforcement to this green value and to a white vertical line on a dark background. The rest of the subjects were presented with the same succession of the green stimulus and the white line in a randomized order, but responses to the white line were not reinforced, while the green key was still associated with a VI schedule. Both groups received training for 16 sessions so that both had equivalent experience with the spectral continuum on which stimulus control was ascertained by means of a second generalization test.

Switalski et al. found that equal reinforcement reduced stimulus control on the spectral dimension, since the slope of the second gradient was flatter than that of the first generalization gradient for the animals that received this training procedure. Differential reinforcement had the opposite effect of steepening the spectral gradient. These results confirm the
steepeening effects of interdimensional training observed in earlier work by Jenkins and Harrison (1960) and extended their work by obtaining an opposite effect of non-differential training with the same stimuli.

Another important series of enhancement studies are those involving extradimensional training (i.e., both discriminative stimuli lie on a dimension different from that on which dimensional acquisition is carried out. This technique permits a clear separation between the phases of discrimination training and dimensional acquisition). In a series of studies reported by Honig (1969), pigeons were first trained to peck at a white key, and then were given three kinds of discrimination training involving white and pink. In true discrimination (TD), white and pink were presented in randomized order with only white being reinforced. In pseudo-discrimination (PD), white and pink were presented in the same way, but both were equally reinforced. A third group was trained with the positive colour only (S+ only control), with time-out periods substituted for presentation of pink.

This discrimination phase was followed by dimensional acquisition, in which all birds were presented with three dark vertical lines on a white background for four sessions, during which responding was reinforced on a VI 1-min schedule. The final phase was a generalization test, in which eight orientations of the three dark, parallel lines were presented, together with
the pink and white training stimuli, in random order in extinction. In other words, all groups had similar training and testing with the stimuli lying on the dimension of generalization.

The generalization test results showed that the slopes of the gradients were clearly ordered in the manner that one would expect if the three discrimination procedures had differential effects on an attention state. True discrimination on colour resulted in the strongest stimulus control on the dimension of orientation, while equal reinforcement with two colours produced the weakest stimulus control.

A number of studies have shown that discrimination training in one modality enhances stimulus control in another. Reinhold and Perkins (1955) trained one group of rats to discriminate between a rough and smooth floor in a runway. Half the animals were run in a white runway, and the other half in a black one, and these groups were in turn subdivided so that each kind of floor texture was positive for half the animals. One control group received only positive trials. A second control group received only "negative" trials in the presence of the positive stimuli with reinforcements omitted. In the generalization test, the positive floor texture was maintained, but the colour of the runway was changed from black to white (or vice versa). Reinhold and Perkins found that running times increased to a greater degree for the discrimination group than for the others when the runway colour was changed; in other words, this group showed the
greatest generalization decrement.

In summary, then, the studies described in this brief review indicate that there are a range of attentional effects. In some of the studies, these effects have been selective, and in other enhanced stimulus control with respect to the dimension of generalization was found. Many issues are as yet unresolved, and experiments in the future must specify the conditions under which discrimination training will have a selective effect and those under which it will have an enhancing effect. It is obvious that these studies could contribute much to applied behaviour analysts' efforts to bring behaviour under stimulus control. At this stage, however, no specific applications are suggested. It is merely noted that this area of research should, in the future, have application in a comprehensive behavioural analysis of human problems. For the present, both more basic animal work to clarify important variables and procedures, and parametric data on human performance under similar simple conditions are urgently needed. Honig (1970) suggests that the most promising approach to the general area involves consideration of the degree of overlap, or redundancy, in the presentation of cues from the dimension of generalization and from the independent training dimension.

Selective effects have been shown most clearly in the blocking experiments. In these studies, the cues from the independent blocking dimension were presented simultaneously with the cues in
dimensional acquisition during at least a portion of the total training sequence. In addition, dimensional acquisition involved discrimination training, and the simultaneous presentation of discriminative stimuli from both problems was therefore redundant. In many of the studies involving enhancement of the generalization gradient, cues from the different dimensions were presented separately.

Honig (1970) concludes that, in general, the simultaneous redundant presentation of the two cues in a discrimination situation is a sufficient condition for the production of selective effects. An organism will choose the dimension on which the discrimination is easier, or on which it has received preliminary training before the introduction of the second cue. If, on the other hand, the cues are presented independently, general enhancing effects will be obtained.

The discussion thus far may appear to be unnecessarily discursive, with little solid indication of actual application to the modification of human behaviour. However, it is important that applied behaviour analysts be aware of these studies so that further work may be directed towards answering questions which will allow application of the principles involved. However, lest the reader is worried about the lack of relevance of the stimulus control literature in toto, I shall discuss an area of research in which experimental principles may be applied now - that being data from studies on stimulus fading and the transfer of stimulus control.
Terrace (1963a, b) has described a method of obtaining errorless or nearly errorless transfer of stimulus control across continua from colour to form. His procedure requires that some stimulus already exercises control over a response. Transfer is obtained by pairing the controlling stimuli with stimuli which do not control the behaviour. Both sets of stimuli are initially presented at their maximum values and then those that initially controlled the behaviour are gradually faded out, so that eventually behaviour is left under the control of the introduced stimuli.

One problem with this method is that behaviour may continue under the control of the gradually fading stimulus. In failures of a procedure to transfer stimulus control from brightness to line orientation, Terrace (1966a) noted that it was only in the last few steps of fading that errors occurred, suggesting that behaviour was under control of the dimension being removed rather than coming under the control of the new stimulus. However, in some cases, this problem may be overcome by delaying the onset of the stimuli that originally controlled the response rather than fading them out. One method of achieving this was described by Touchette (1971), in an experiment in which simple form discriminations were acquired errorlessly by three severely retarded boys. Subjects were trained to respond to a red key and not to respond to a white key, and then black figures were superimposed on the lighted keys. Following each correct response, the next trial involved a delay in the onset of the red
stimulus by an additional 0.5 sec (i.e., there was an increasing delay between the presentation of the black figure and the appearance of the red light). Eventually, the subjects responded to the black figure before the key became red, and this was defined as the moment of transfer of stimulus control.

Touchette's procedure has been successfully used by Striefel, Bryan, and Atkins (1974) to transfer stimulus control from motor to verbal stimuli with three profoundly retarded adolescents. In this case, the aim was to teach a series of specific responses to specific verbal instructions. Initially, imitative control of a behaviour (e.g., wave hand, touch knee, shake head, etc.) was established, following which a verbal instruction was presented immediately before the behaviour was modelled. When a correct response was made, the next trial was changed by inserting a delay between the verbal instruction and the modelling of the behaviour. As successful trials continued, the delays were increased. Transfer of stimulus control was considered achieved when subjects responded correctly on five consecutive trials before the behaviour was modelled. The results showed that transfer of stimulus control occurred for all three subjects, in that specific verbal instructions came to control the corresponding motor behaviours. It is important to note that in this study control did not generalize to behaviours that were not trained. However, variations in the verbal instructions did control some appropriate responding. It seems, therefore, that Touchette's delay procedure is a useful technique which could well be utilized in
many training situations, particularly with children and retardates in educational programmes.

In this survey I have tried to indicate only some of the data which are potentially of practical importance to applied behaviour analysis. The stimulus fading experiments are virtually the only ones in the stimulus control field whose data have been directly incorporated into some applied work. It is surprising that perhaps the most researched area, that involving behavioural contrast, has not generated any suggestions for application. (The potential applicability of the contrast data was discussed in Chapter 6). The associated phenomenon of peak shift (Hanson, 1959) has also attracted much experimental interest, but studies with humans indicate that the effect decreases with developmental maturity (Nicholson and Gray, 1972) and that many training procedures eliminate or exclude peak shift. The problem appears to be that the stimulus control area has produced data of considerably less generality than has schedule control research. Many of the data are insufficiently clear-cut, and often do not allow even tentative suggestions as to application to the modification of human behaviour. Thus the descriptive nature of this chapter. I make no apology for this, as I believe that to extrapolate at this point would be unfounded. However, the complex inter-relations between stimuli and behaviour must be more fully investigated and appreciated before behaviour analysis can be applied to complex human behaviours under complex stimulus control. Only with pressure from applied scientists to
investigate relevant parameters will the experimental analysis of stimulus control focus on those areas which are of importance in predicting and controlling human behaviour. At this stage, though, applied behaviour analysts must familiarize themselves with the existing data before they are able to formulate useful questions. Herein may lie the reason for the state of affairs criticized at the beginning of this Chapter.

.....

Note:

It should be noted that there is some excellent work of the nature which is suggested in this thesis and published in places other than the journals and books reviewed here. An excellent example of such work is that by Schilmoeller, K.J. and Etzel, B.C. An experimental analysis of criterion-related and noncriterion-related cues in 'errorless' stimulus control procedures. In B.C. Etzell, J.M. LeBlanc, and D.M. Baer (Eds.), New Developments in Behavioral Research: Theory, Method and Application. Hillsdale, N.J.: Lawrence Erlbaum Associates, 1977. Unfortunately work of this nature does not have wide circulation amongst practicing applied behaviour analysts and therefore the majority of workers in the field are unlikely to be aware of attempts such as this to integrate experimental and clinical data.
An important assumption underlies the type of research from which the data discussed thus far have come. The degree to which this assumption is valid determines, to a large extent, the limits on the applicability of these data to the modification of complex human behaviour. The assumption is that in the experimental analysis of behaviour the outcome of a particular manipulation will not be affected by the specific stimuli and responses we choose to study. This emphasis stems from Skinner's (1935) insistence that we avoid "botanizing" and instead should define behavioural units independently of their environmental context. In this way, Skinner believes we can search for general laws of behaviour which will hold at all levels of the phylogenetic scale. To the extent that the data are specific to particular situations, responses, or organisms used in experimental studies, their generality and, hence, their applicability to other situations, behaviours, or organisms is obviously limited. If we are to proceed, as suggested in the foregoing chapters, with a more detailed and comprehensive application of experimental data to the modification of human behaviour it is essential that we examine the limits of their generality. Because an increasing number of papers suggest that it may not be possible to discover species- or situation-general laws of behaviour, we must examine their import for applied behaviour analysis.
A recent book by Seligman and Hager (1972) has assembled much of the evidence against there being principles of behaviour uncontaminated by species-specific factors. They suggest that Skinner's approach of studying arbitrary events does not have sufficient generality to account for all behavioural phenomena. They suggest, rather, that organisms function on a biologically-determined preparedness of association continuum. Organisms are biologically prepared to make some associations and contraprepared to make others. The associations which are the subject of the typical experimental situation are thus the arbitrary, unprepared associations which occupy the middle ground. Put simply, it is suggested that organisms will do some things naturally or with very little training (those involving prepared associations); other things will be difficult or impossible to train (those involving contraprepared associations); and yet others may be modified according to the contingencies of reinforcement (those involving unprepared associations).

Garcia and Koelling (1966) have shown that pairing a demonstrably perceptible cue with an effective reinforcer does not necessarily result in an association being learned between the two. They suggest that the cue must be "appropriate" (that is, unprepared or prepared, as opposed to contraprepared) rather than just salient. In their experiment, Garcia and Koelling (1966) showed that rats will not associate taste as the CS, with shock as the UCS, and exteroceptive stimuli with stomach illness (contraprepared), but will differentially associate tastes, as CS,
with gastrointestinal illness, as the UCS (prepared), and exteroceptive stimuli, as CS, with shock, as UCS (unprepared). Garcia, Ervin, and Koelling (1966) provided evidence that taste-aversion learning is prepared because of the delay which may be interposed between taste and poison (up to 75-min) without impairing the acquisition of taste-aversion (cf. the extremely short intervals usually required between CS and UCS). Garcia, McGowan, Ervin, and Koelling (1968) and Wilcoxin, Dragoin, and Kral (1971) argue that the taste-aversion phenomenon is an example of a biological mechanism by which natural selection ensures that organisms will select from the sense modality they use to find food, those stimuli they will differentially associate with illness. Wilcoxin et al. suggest, therefore, that while some organisms will associate taste with illness (for example, as in Garcia and Koelling's (1966) study) others will associate stimuli from another modality, such as vision or hearing. On this basis, they predicted that quail, who use vision extensively to identify food, would associate illness with visual rather than gustatory stimuli. Their results supported this prediction.

Some authors (Wilson and Davison, 1969; Revusky, 1973) have pointed to the relevance of these findings for applied behaviour analysis, particularly in the use of aversion therapies. For example, Wilson and Davison (1969) claim that these data contra-indicate the use of shock used to condition aversion to the sight, smell, and taste of alcohol as commonly utilized in the treatment of alcoholics. If, in this context, humans are biologically
similar to rats, we would expect that fear responses resulting from the use of shock may only be conditioned to the sight of alcohol and not its taste and/or smell. As the stimuli involved with vision may very easily be altered by changing the environmental complex (e.g., from a treatment bar to a real life bar of different physical features, types of people, types of drinks), we might expect the effects of such aversion therapy to be only short-term (as, in fact, is often reported). The type of results reported by Garcia and his associates suggest that a chemically-induced aversion to the taste and/or smell of alcohol may have longer-lasting effects. (After all, it is difficult to alter substantially the smell or taste of a preferred drink).

It remains to be experimentally verified whether or not the predictions made on the basis of rat data are true of human behaviour. However, anecdotal clinical evidence would tend to support the predictions (Davison, 1968). Lazarus (1968), for example, reports that aversion therapy with electric shock as the UCS was unsuccessful in the initial treatment of an alcoholic. However, when the UCS was changed to a noxious-smelling substance, alcohol consumption was reported to have fallen rapidly. Lazarus pointed out that shock might be the "appropriate" UCS to use with problems involving visual and/or tactile stimuli and may be inappropriate for problems involving gustatory stimuli.

Revusky (1973), writing about the treatment of alcoholism, discusses some specific data which he considers should determine the conduct of such treatment. He notes, for example, a study by
Domjan and Wilson (1972) in which sickness was induced in rats after saccharin solution has been flushed through their mouths without allowing them to drink it. Control rats which were allowed to ingest the solution prior to sickness were found to have much stronger saccharin aversions than the experimental subjects. The suggestion following from this finding is that alcoholic patients should actually consume an alcoholic drink during treatment. Revusky (1973) lists other similar applied suggestions based on the experimental data.

In an incisive review of Seligman and Hager's (1972) theory of preparedness, Schwartz (1974) has examined the implications for the generality of experimental data of the studies supportive of the theory. He argues that with pigeons as experimental subjects, for example, while there is no argument over key-pecking being a prepared response, it is difficult to discern any effects of this preparedness on the phenomena studied. As he points out, although key-pecking in the pigeon is prepared and bar-pressing in the rat is presumably unprepared, there is little to distinguish these responses as operants under most reinforcement schedules or discrimination procedures. There are instances, however, in which differences are observed. For example, bar-pressing and key-pecking are not similarly affected by differential-reinforcement-of-law-rate (DRL) schedules (Hemmes, 1970; Schwartz and Williams, 1971). What this shows, in essence, is that we cannot treat prepared and unprepared responses as either wholly identical or wholly distinct. While some variables will affect them
similarly, others will not. But it is because the differences are brought to light that we will eventually understand the biological and environmental contributions made to particular responses. Thus, whatever the theoretical status of Seligman and Hager's work, it has done a considerable service in pointing out some of the biological constraints. Revusky's (1973) paper shows that these data may lead to direct treatment suggestions. As Schwartz (1974) concludes:

"The import of the theory of preparedness, and the phenomena on which it is based, is that they limit the generality of laboratory principles. They do, indeed, begin to set the biological boundaries of learning. They force researchers to examine the biological organization of behavior, especially as it constrains the experiential organization of behavior. These developments are wholly salutary. They will ultimately result in rules of induction and inclusion that will make the experimental analysis of behavior more comprehensive, and make attempts at extra-experimental extrapolation more meaningful, even as they are more restrained" (p.196).

In the past we have tended to overlook the interesting differences among schedule performances of different species, in our enthusiasm for the search for general laws of behaviour. Some of these differences have been noted (Staddon, 1973) but only rarely have differences in performance between lower organisms and humans been explicitly studied (e.g., Weiner, 1969). It is such parametric human data which are needed to test the generality of much of the basic operant literature.
There are a number of such phenomena in animal experimentation which are worthy of basic study with humans to determine whether they enter into applied behaviour analytic treatment outcomes. One such phenomenon is behavioural drift. A frequently encountered phenomenon (Keehn and Sabbagh, 1958; Keehn and Webster, 1967; Rachlin, 1969), this refers to declining success exhibited by some animals after attaining high levels of successful avoidance behaviour. Is it possible that some changes in behaviour measured by a clinician are a result of such "drift" rather than of apparently unchanged environmental conditions? Could part of the lack of long-term success with some avoidance procedures in applied behaviour analysis, for example, be a result of drift?

It could be, too, that in some situations we are unable to recover a previously learned behaviour pattern. Morse and Kelleher (1970) use the term metastable to refer to "two different stable patterns of responding maintained under the same schedule parameters, one before and one after an intervening treatment" (p.161). Morse and Kelleher (1966) and Staddon (1965) have detailed evidence of how an original performance cannot in some circumstances be recovered following the removal of a disrupting event. We need to know whether such metastabilities also occur in human behaviour as they could well influence the form of the target behaviour or controlling conditions chosen by a therapist.
In conclusion, there are a number of experiments whose results fall outside the range of simple reinforcement and stimulus control effects. I have not attempted to survey these data, but merely to outline some major areas of concern. In total they point to a need on the part of both experimental and applied behaviour analysts to be aware of the biological boundaries of behaviour. It has been shown (e.g., Revusky, 1973) that such an awareness may well increase the practical utility of applied behaviour analytic techniques.
CHAPTER 10

Conclusions

The plea for a constant interaction between experimental and applied behaviour analysis made here has important implications for a number of areas. Foremost amongst these must be the issue of the training of behavioural psychologists. The data presented have illustrated the lack of knowledge and/or utilization of basic findings from the experimental analysis of behaviour to the modification of human behaviour. In view of the pronounced lack of evidence that applied behaviour analysis is tied explicitly to experimental data, it is ironic that the By-laws of the Association for the Advancement of Behavior Therapy warn against precisely this—presumably referring to people the A.A.B.T. consider not to be legitimate behaviour analysts. The By-laws state:

"There is a danger that clinicians, lacking an adequate background in learning principles and experimental methodology, and unaware of the complex theoretical and technical issues involved, may represent themselves as behavior therapists, while employing techniques and procedures based neither on learning theory nor learning technology. On the other hand, persons trained in experimental methods may attempt the clinical application of laboratory research methods without sufficient regard for the need to acquire other skills essential to good clinical practice" (A.A.B.T. 1969).

This presumably implies that people who have their work published in the A.A.B.T.'s journal (Behavior Therapy) do have an adequate
knowledge of learning principles and experimental methodology. Unfortunately, as I have shown earlier, if judged by the rate of explicit incorporation of experimental data into clinical work, this is simply not the case at present.

Of course, one could argue that even if applied behaviour analysis has only conceptual roots in laboratory findings much good has come of it. Viewing human problems as behaviours with environmental causes has certainly produced a range of treatment techniques allowing us to deal with problems in new areas. An elementary knowledge of laboratory principles has led to the direct transfer of some findings (e.g., applications of shaping). Finally, training in behavioural methodology should increase the likelihood of analysts objectively measuring dependent variables and ensuring that their manipulations are in fact the causes of behaviour change. To the extent that these generalizations are accurate, the belief that applied behaviour analysis is founded specifically on experimental behaviour analysis has, to date, been extremely useful. This belief, remaining unexamined, is now restricting the further growth of applied behaviour analysis. As I have shown repeatedly, there are many areas of experimental study whose data appear to have direct relevance to applied situations. Our reliance on the "tried and true" basic literature is obscuring the contribution these data could make. Most applied behaviour analysts are simply unaware of the data. If we wish to change this situation we must instigate a behavioural analysis of our own behaviour as experimenters and clinicians. We must first
employ our techniques to analyze and modify our own behaviour before turning ourselves loose to modify the behaviours of others.

In attempting to discover the reasons for the non-incorporation of experimental findings into applied practice it may first be useful to delineate the behaviours which distinguish the experimenter from the clinician. Krapfl (1974) has examined both the commonalities and differences between these two groups. Both seek for the controlling conditions of behaviour in the environment. Both select and measure dependent variables with a view to accounting for change by manipulating observable independent variables. Both may be characterized by an interest in objectivity, as defined by Sidman (1960, p.43). In the final analysis, however, it may well be that the distinctions are more important than these shared characteristics.

Sidman (1960) has listed some of the differences between experimental and applied analysts. A major distinction involves the manner in which each must handle variability. The experimenter is often solely interested in investigating sources of variability and is thus often able to explain variable data. The clinician, on the other hand, is usually constrained by practical limits of resources, time, or finance, and must often merely accept variability as a given. He may try to eliminate it, but must usually compromise by making "educated guesses" about ways of handling it. As Krapfl (1974) notes, the experimenter "may continually refine his conditions until they bear little
resemblance to the typical world in order to control variability, but the behavioural engineer cannot work in the abstract condition. His efforts must be carried out, and will be evaluated for effect, in the world to which we are all normally exposed" (p.7).

While both groups may be characterized by an emphasis on measuring behaviour, the particular emphasis given may often differ. The experimenter, being able to choose situations and responses most amenable to recording procedures, will be most closely identified with measurement. The applied person will aim for such precision, but will rarely (if ever) achieve it, sacrificing precision for practical limitations. Further, he cannot always choose behaviours with a view to ease of measurement and he is less likely to be able to institute the same reliability and control procedures.

What these differences amount to, in summary, is that applied behaviour analysis is not as precise, extensive, or "scientific" as the experimental analysis of behaviour because it is limited by the complexity of situations, professional limitations of time, staff, and money, and so forth. The question must be asked, though: Is it enough to just accept these limitations and presume that applying behaviour principles will always be too "complicated" to expect that we can approach the same stringency as found in the experimental field? My answer is an unequivocal negative! A large part of the reason for the shortfall is surely because we have not analyzed our own behaviour in our own terms. As Krapfl(1974)
"There must be a deliberate effort on our part to identify and become sensitive to the controlling elements of the culture which affect us. We must come to recognize all of our interactions with others as reciprocal reinforcing relationships which can be subjected to the same sort of functional analysis as those which we currently investigate" (p.10).

These issues lead us to consideration of the training of applied behaviour analysts. The central issue here is that although the subject matter of applied behaviour analysis is behaviour, many training programmes emphasize only a limited behavioural repertoire, namely, being able to verbalize principles of behaviour. This may be a useful and even vital skill, but it does not necessarily facilitate the actual control of behaviour. The neglect of demonstration of the acquisition of behavioural skills is particularly a feature of short-term courses and workshops. In line with my suggestions that we pay more heed to the experimental literature, I suggest that requiring applied behaviour analysts to obtain laboratory skills with animals may be the most efficacious manner in which to shape a behaviour control repertoire. Wood (1974) expresses the same sentiment in similarly arguing for the acquisition of laboratory skills:

"Certainly, the behaviors one uses to control a rat or a pigeon's behavior are topographically quite different from those that one uses dealing with human behavior in a classroom or
hospital, but they are still a class of behaviors, an operant, maintained by their effectiveness in controlling the ongoing behavior of another organism. This may be important, since function not form, determines the definition of an operant. We want to teach behavior control skills, and strengthening one class of responses, controlling animals, should strengthen others. Certainly many of the components of a successful behavior controller's repertoire have little to do with the particular species with whose behavior he is working' (p.13).

By requiring students to work with animals in an experimental laboratory, the opportunity is given for experiencing the complex contingencies that control behavior and for being shaped by this experience. It is equally vital that applied practitioners remain conversant with laboratory practice. It is necessary that we develop a system in which there are more reinforcers for continuing to demonstrate knowledge of current data and, more importantly, for application of these principles to the modification of human behaviour. The most important use of applied behaviour analysis could well be to conduct a behavioural analysis of the discriminative stimuli, behaviours, and reinforcers necessary to develop a system of integrating experimental and applied endeavours. In my view, this integration should be a deliberate policy of academic training. Those wishing to specialize in applied behaviour analysis should be required to complete experimental work in the laboratory. Conversely, those wishing to commit themselves to the experimental analysis of behaviour, should be required to at least have demonstrated academic, if not practical competence in applied
work. In this way, the applied person will be conversant with the experimental data, and the experimenter will have some conception of the practical problems involved with the application of these data.

The incorporation of extensive experimental data into applied behaviour analysis is important also because of the increasing debate concerning the ethics of behaviour control. There is a new awareness of the need to appreciate the side effects or long-term consequences of intervention in a person's life. I contend that only when we understand the nature of behavioural interactions will any claims that we are able to realistically assess these effects be meaningful. The experimental analysis of behaviour is specifically organized to examine these interactions.

With the mounting concern over ethics has come a specific call for behaviour modifiers (of any orientation) to be accountable for their actions. Once again, I believe that only when we have learned from the experimental analysis of behaviour what are, or could be, the consequences of our behaviours as applied behaviour analysts, will we be in a position to make ourselves accountable. I have indicated a number of areas in which experimental data could beneficially be applied to this end. This process must be actively and extensively pursued if we are to be able to predict and control human behaviour.
In conclusion, the argument presented here is that there are a number of cogent reasons for the routine and extensive inclusion of experimental data into applied behaviour analysis. The first reason is that since this is currently believed by many to be the existing state of affairs, we should either ensure that this is so, or cease making the claim. Data presented in Chapter 1 demonstrate the emptiness of this belief at present. Second, I contend that the power of applied behaviour analysis is being severely limited by the non-reliance on principles elucidated in the animal laboratory. Extensive reference has been made to applications of data in the areas of concurrent, concurrent-chains, and multiple-schedule research, and from research on stimulus control, behavioural interactions, commitment and self-control, Premack theory, conservation theory, and biological constraints on behaviour. The wealth of potential applications suggested by the few studies covered indicates the scope which would be available if an integrated approach to behaviour control were to become commonplace. Finally, I suggested that awareness of the widespread effects of behavioural interventions and the realistic possibility of accountability for behaviour modifiers will only follow when applied behaviour analysis has become widely involved in applying experimental data. Taken together, these arguments should provide powerful impetus for cross-fertilization between the two fields. The task now is to conduct a behavioural analysis of the discriminative stimuli, behaviours, and reinforcers which would ensure that the cross-
fertilization occurs.
REFERENCES


*Association for the Advancement of Behavior Therapy - By-Laws.*  


Ayllon, T., and Haughton, E.  Control of the behavior of schizophrenic patients by food.  *Journal of the Experimental Analysis of Behavior, 1962, 5*, 343-352.


Boakes, R.A. Response decrements produced by extinction and by


Catania, A.C. The psychologies of structure, function, and development. *American Psychologist*, 1973, **28**, 434-443.(a)


Catania, A.C. The concept of the operant in the analysis of behavior. *Behaviorism*, 1973, **1**, 103-116.(c)


Domjan, N., and Wilson, N.E. Contribution of ingestive behaviors to taste-aversion learning in the rat.
Journal of Comparative and Physiological Psychology, 1972, 80, 403-412.


Falk, J.L. The nature and determinants of adjunctive behavior. In R.M. Gilbert and J.D. Keehn (Eds.), Schedule effects: Drugs, drinking, and aggression.

Fantino, E. Choice and rate of reinforcement. Journal of the Experimental Analysis of Behavior, 1969, 12, 723-730. (a)


Farthing, G.W., and Hearst, E. Attention in the pigeon: testing with compounds or elements. Learning and Motivation, 1970, 1, 65-78.


Goetz, E.M., Holmberg, M.C., and Le Blanc, J.M. Differential
reinforcement of other behavior and non-contingent reinforcement as control procedures during the modification of a preschooler's compliance. *Journal of Applied Behavior Analysis*, 1975, 8, 77-82.


Herrnstein, R.J., and Hineline, P.N. Negative reinforcement as shock frequency reduction. *Journal of the Experimental Analysis of Behavior*, 1966, 9, 421-430.


Homme, L.E. Contiguity theory and contingency management.

Homme, L.E. Perspectives in psychology: XXIV. Control of covarants, the operants of the mind. Psychological Record, 1965, 15, 501-511.


Lanyon, R.I. Discussion: behavior modification as a technology. In D. Upper and D.S. Goodenough (Eds.), *Behavior modification in outpatient settings*. Proceedings


Micheal, J. Positive and negative reinforcement, a distinction that is no longer necessary: or a better way to talk about bad things. *Behaviorism, 1975, 2*, 33-44.


Rachlin, H.  Autosshaping of key pecking in pigeons with negative reinforcement.  *Journal of the Experimental Analysis*


Reynolds, G.S. Behavioral contrast. *Journal of the Experimental Analysis of Behavior, 1961, 4*, 57-71.(a)


Reynolds, G.S. Attention in the pigeon. *Journal of the Experimental Analysis of Behavior, 1961, 4*, 203-208.(c)

Reynolds, G.S., and Limpo, A.J. On some causes of behavioral contrast. *Journal of the Experimental Analysis
of Behavior, 1968, 11, 543-547.


Schwartz, B., and Williams, D.R. Discrete-trials spaced responding in the pigeon: the dependence of efficient performance on the availability of a stimulus for collateral pecking. Journal of the


Terrace, H.S. Stimulus control. In W.K. Honig (Ed.), *Operant behavior: Areas of research and application*. 


Van Houten, R., and Sullivan, K. Effects of an audio cueing

Vom Saal, W., and Jenkins, H.M. Blocking the development of stimulus control. *Learning and Motivation*, 1970, 1, 52-64.


