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PARADOXICAL INTENTION: A SOCIAL LEARNING ANALYSIS

AN ABSTRACT OF A DISSERTATION

SUBMITTED TO THE FACULTY

OF

THE GRADUATE SCHOOL OF APPLIED AND PROFESSIONAL PSYCHOLOGY

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RUTGERS

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BY

FREDERICK ROTGERS

IN PARTIAL FULFILLMENT OF THE

REQUIREMENTS FOR THE DEGREE

OF

DOCTOR OF PSYCHOLOGY

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## ABSTRACT

The use of paradoxical approaches to psychotherapy has an extensive history dating to Adler. The practice of paradoxical therapy has been hindered by the lack of a testable theoretical framework. Likewise, little experimental evidence exists for the efficacy of paradoxical techniques. This dissertation examines one paradoxical therapy technique, paradoxical intention (PI), from a social-learning theory perspective. The growing experimental literature on PI is critically reviewed with regard to the methodological adequacy of the studies, and the evidence for the efficacy of PI. A testable theoretical explanation of the action of PI is proposed drawing upon two segments of the experimental-clinical literature: 1) the effects of uncertainty of aversive stimulation on behavior and arousal, and 2) Bandura's self-efficacy theory. Finally, a program of research is proposed to test aspects of the proposed theory as well as to answer other questions about PI. It is concluded that PI is a potentially facilitative adjunct to exposure methods of anxiety and arousal reduction, but that, due to problems of confounding of PI and exposure treatments in the experimental literature, it is unclear whether PI has any specific therapeutic effects beyond facilitation of exposure.

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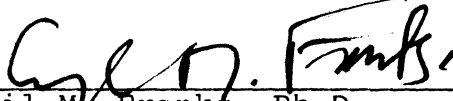
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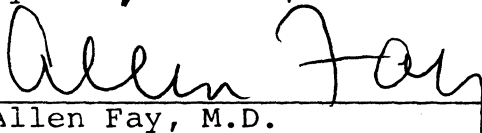
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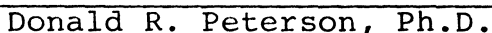


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## TABLE OF CONTENTS

	Page
ACKNOWLEDGMENTS . . . . .	ii
CHAPTER	
I. INTRODUCTION TO PARADOXICAL INTENTION. . . . .	1
II. EXPERIMENTAL STUDIES OF PARADOXICAL INTENTION . . .	8
III. A SOCIAL-LEARNING THEORY OF PARADOXICAL INTENTION. . . . .	46
IV. OVERVIEW AND PROPOSALS FOR FUTURE RESEARCH ON PARADOXICAL INTENTION. . . . .	71
REFERENCE NOTES. . . . .	78
REFERENCES. . . . .	79

## CHAPTER I

### INTRODUCTION TO PARADOXICAL INTENTION

The systematic use of paradoxical techniques to treat a variety of behavioral disorders dates at least to Adler (Mozdzierz, Macchitelli & Lisechi, 1976), but has probably been an informal method of confronting psychological problems for hundreds of years (Fay, 1978). The Oxford American Dictionary (1980) defines "paradox" as "a statement, etc. that seems to contradict itself or to conflict with common sense but which contains a truth..." Within the framework of this definition numerous psychotherapeutic techniques originating in diverse theoretical bases may be classified as paradoxical. Thus any technique that appears to instruct the client(s) to perform behaviors that seem to conflict with a "common sense" approach to a given problem may be considered paradoxical in a strictly technical sense.

Most schools of therapy that use paradoxical techniques go beyond this simple technical definition of paradox. Typically, paradoxical techniques involve the use of what has variously been called "symptom scheduling" (Newton, 1968), "symptom prescription" (Zeig, 1980), "negative practice" (Dunlap, 1930), "therapeutic double bind" (Watzlawick, Beavin & Jackson, 1967), "paradoxical intention" (PI) (Frankl, 1955), "utilization techniques"



(Erickson, 1959), "reframing" (Watzlawick, Weakland & Fisch, 1974), "siding with the resistance" (Sherman, 1968), and "response exaggeration" (Gottlieb & McNamara, 1975). These techniques share the common feature of instructing the client directly or indirectly to actively engage in and/or exaggerate the very behavior he or she has come to therapy to eliminate. Thus an obsessive ruminator might be instructed to rehearse the obsessions even when the felt need to do so is not present (Solyom, Garza-Parez, Ledwidge & Solyom, 1972), or an agoraphobic with panic attacks might be instructed to enter feared situations and attempt to become as anxious and panic stricken as possible (Asher, 1981; Gerz, 1966).

The theoretical rationales for this "non-commonsense" approach to behavioral difficulties are as varied as the terms used to label the procedures. Therapists from schools as diverse as psychoanalysis (Adler, 1914; Nelson, 1968), logotherapy (Frankl, 1955, 1975), communications/systems theory (Haley, 1976, Hoffman, 1971, Watzlawick et al., 1967, Watzlawick et al., 1974) and behavior therapy (Ascher, 1981; Dunlap, 1930; Relinger & Bornstein, 1979) have used paradoxical techniques and proffered theoretical explanations for their apparent effectiveness. These explanations include reversal of the usual patient-therapist relationship, thus forcing the patient to adopt a position opposite to that with which he/she came to therapy (Nelson, 1968); disruption of the communication system surrounding

the symptom , thereby forcing a readjustment of the system that leads to change (Watzlawick, et al., 1974); and interruption of an "exacerbation cycle" (Relinger & Bornstein, 1979) of performance anxiety and attempts to control autonomic arousal (Ascher, 1981, Frankl, 1975). Several extensive, though incomplete, reviews of the theory and application of paradoxical techniques have also appeared recently (Barrack, 1978; Raskin & Klein, 1975; Weekes & L'Abate, 1982).

Paradoxical techniques have had a strong intuitive appeal for clinicians. In spite of this appeal, these techniques are subject to several problems. The first problem, is definitional/terminological. Different writers use different terms to refer to techniques involving a similar core of action. This sort of definitional terminological problem is common in psychotherapy and has prompted some (Frank, 1974; Wachtel, 1979; Goldfried, 1982) to propose that diverse therapies share a common core and can ultimately be merged into a comprehensive system. For reasons to be outlined below, it is my contention that paradoxical techniques can and should be located squarely within a testable theoretical framework derived from social learning theory (Bandura, 1977).

A second problem with paradoxical techniques concerns the scope of their effectiveness and application. Some practioners (eg. Selvini Palazzoli, Boscolo, Cacchin & Prata, 1978; Watzlawick, et al., 1974; Weekes & L'Abate,

1982) view paradox as the central core of psychotherapy. Others (eg., Ascher, 1981; Frankl, 1975) advocate their use in a more limited fashion for problems involving anticipatory anxiety and attempts to control autonomic processes over which voluntary control is usually impossible (examples of such problems include agoraphobia, insomnia, and urinary retention). Still other practioners (eg., Fay, 1978; Lazarus & Fay, 1982) view paradoxical techniques primarily as useful strategies that can be brought to bear to overcome therapeutic impasses encountered in more didactic forms of therapy. Furthermore, therapists from different theoretical backgrounds prefer different levels of focus for their use of paradoxical techniques. Thus, family therapists and behavior therapists might both use a paradoxical intervention to treat the same problem (eg., agoraphobia) but focus respectively on the family system as a whole or on the individual client. The techniques may be topographically similar, but the theoretical reasons for their use are widely disparate from each other.

A final difficulty with paradoxical techniques is the lack of a coherent, empirically testable theoretical framework within which assessment of their efficacy and mechanisms of action might be carried out. In spite of numerous theoretical ideas about how paradoxical techniques work, practioners have been remiss in not generating testable theories. Most recent work, exemplified by that of Weekes and L'Abate (1982) is fraught with vague terminology

that is not amenable to operationalization or objective measurement (Rotgers & Franks, in press). Until recently very little has been reported in the way of empirical study of paradoxical techniques. Most of the literature has consisted of uncontrolled case studies with few attempts at systematic definition, measurement and comparative evaluation of efficacy with established techniques. Even less research has been reported that attempts to investigate mechanisms of action within a coherent, theoretical framework. This lack of empirical support for techniques that appear to be widely used is most puzzling in an era of accountability (Wilson, 1982 b). At least one reason for this is the absence of any clearly stated, testable explanation for the efficacy or mechanisms of paradoxical techniques.

This paper will focus primarily on the second and third problems outlined above. Due to the vastness of the anecdotal literature in this area, and the fact that this literature does little more than provide untested clinical hypotheses to guide a research program in this area, I will focus my attention on those studies that have attempted to examine paradoxical techniques using experimental or quasi-experimental research designs. The majority of these studies concern a particular mode of paradoxical intervention, paradoxical intention (PI) (Frankl, 1955). PI is defined as a prescription that the unwanted behavior be intentionally performed in order to change it.

Three studies will be reviewed (Azrin, Nunn & Frantz, 1980; Gottlieb & Mcnamara, 1975; Levy, 1978) that employed procedures topographically similar to PI (negative practice and response exaggeration) but did not call these procedures PI.

In the first part of the paper I will examine the available experimental literature with a critical eye toward the adequacy of the research and the extent to which it shows that PI is viable and potentially effective. My reading of this literature will suggest that, with certain qualifications and caveats, PI is a viable procedure worthy of further experimental study.

In the second part of the paper, I will attempt to develop a coherent, testable model for the study of PI. I will begin by relating PI to the growing behavior therapy literature on exposure treatments for anxiety-based disorders (see Marks, 1981 and Marshall, Gauthier & Gordon, 1979, for reviews of this area). Exposure treatments and PI share many features, and PI seems to have characteristics that may enhance the basic effects of exposure. These effects and characteristics will be discussed using a model derived from Bandura's (1977) social learning theory coupled with work on the effect of uncertainty of aversive stimulation on behavior (see Averill, 1973; Mineka & Kihlstrom, 1978). In particular, I will focus on how self-efficacy evaluations might correlate with uncertainty and how PI might exert its influence by promoting a



cognitive set which overrides the negative effects of uncertainty and stimulates behavioral changes that, in turn, feedback to enhance self-efficacy.

Finally, I will propose a program of research to assess the adequacy of the model and evaluate the efficacy and mechanisms of action of PI.

## CHAPTER II

### EXPERIMENTAL STUDIES OF PARADOXICAL INTENTION

The experimental-clinical literature on PI is a recent phenomenon, considering the appearance of paradox in the clinical literature as early as the 1920's. The studies to be reviewed have all appeared in the behavior therapy literature since 1975. For two reasons this late appearance of experimental study of PI is puzzling. First, one of the first therapists to report the use of a technique similar to PI was Dunlap, a forerunner of behavior therapy. Dunlap (1928) was one of the first to apply the laws of learning to behavior change, and to suggest that repetition and exaggeration of an unwanted behavior (what he called "negative practice") might lead to its disappearance.

A second part of the puzzle is behavior therapy's long-standing commitment to experimental validation of therapeutic techniques. In fact, this commitment is considered by some (eg., Agras, Kazdin, & Wilson, 1979) to be part of the "central core" of behavior therapy. Given the interest in a PI-like procedure by an early behavior therapist, and behavior therapy's commitment to experimental evaluation of techniques, one would have anticipated a more lively interest in PI:

Perhaps behavior therapy's long-standing neglect of PI can be explained by the psychodynamic basis for the early practice of PI. Certainly Adler and Frankl could not be considered learning theorists, behavior therapists or experimental clinicians. The advocacy of PI by such practitioners may have colored the perception of behavior therapists who, until recently, tended to view the "psychoanalytic establishment" (of which Adler and Frankl could be said to be members) as a foe to be defeated in the laboratory by lucid experimental evaluations of techniques directly derived from learning theory (Agras, et al., 1979). Only recently have behavior therapists begun to examine other therapeutic approaches (cf. Goldfried, 1982; Lazarus, 1971, 1981), thus breaking down the rigid ideological boundaries that have discouraged cross-fertilization. This is not to say that behavior therapists are completely open to cross-fertilization, but they do appear to be more willing to subject techniques from other frameworks to experimental test (see Franks, In Press, for a discussion of a view opposed to cross-fertilization by a prominent behavior theorist). With this background, let us turn to the studies.

In assessing a body of clinical-experimental research several strategies are available. The "Box-score" approach (Glass, 1976) that characterized the outcome review by Luborsky, Singer and Luborsky (1975) evaluates studies

using a dichotomous criterion of statistically significant versus statistically non-significant results. Studies are summed together and evaluated globally with little attention to the adequacy of design (Kazdin & Wilson, 1978).

A second possible strategy is "meta-analysis" (Glass, 1976; Smith & Glass, 1977) in which an "effect size" (defined as "the mean difference between the treated and control subjects divided by the standard deviation of the control group" (Kazdin & Wilson, 1978)), is assessed for each study, and the resulting scores examined to determine which therapeutic procedures produce larger "effect sizes" across studies.

Kazdin and Wilson (1978) have criticized both the box-score and meta-analysis strategies on several grounds centering on the propensity of these approaches to generate sweeping conclusions from methodologically inadequate studies. They propose a "social learning" strategy for research evaluation which focuses on several criteria for a clinical research study. These criteria include: 1) clear specification of techniques and procedures, 2) specification of criteria for terminating therapy, 3) use of appropriate control groups with assessment of credibility of "placebo" controls, 4) use of a variety of measures including specific behavioral measures as well as self-report and other less objective measures, 5) adequate follow-up and use of follow-up data, and 6) cost-effectiveness of treatments in terms of length of therapy, need for therapist involvement, etc.

These criteria supplement those required for adequate experimental design (Campbell & Stanley, 1963). Mahoney (1978) has cited several design factors, summarized from prior writings, that he calls the "10 most common culprits in experimental inadequacy." These are:

"1) selection of a theoretically irrelevant hypothesis or issue; 2) use of a subject sample that is very small or unrepresentative of the population to which generalizations are to be drawn; 3) in the case of between subjects designs, the absence of random assignment to the various experimental conditions; 4) poor specification of the independent variable (s); 5. inadequate standardization, assessment, or description of how the independent variable was implemented; 6. inadequate control for factors other than those of immediate experimental interest; 7. inadequate replication of the cause-effect relationship (either within or between subjects); 8. poor choice specification or assessment of all relevant dependent variables; 9. inadequate data presentation; 10. conclusions or interpretations that are not logically warranted by the experimental procedures."

The experimental literature on PI will be evaluated here within the guidelines proposed by Kazdin and Wilson (1978) and Mahoney (1978).

The grouping of studies for the purpose of review is largely arbitrary, although one endeavors to find a basis in logic. The literature on PI can be grouped in two ways: 1) by the type of disorder treated, and 2) by whether the study was a "process" or "outcome" study. That is, whether the focus of the study was on the parameters of the technique itself or the testing of efficacy relative to other approaches. This review will consider the experimental PI literature on the basis of disorder treated, for two



reasons. First, while the range of disorders treated is diverse, the number is relatively small, thus facilitating easy grouping. Second, the disorders treated are relevant to the theoretical model to be developed below, in that they all involve an attempt to control "uncontrollable" processes. Treatment with PI is an effort to short-circuit this attempt at control.

### PI and Insomnia

Most of the experimental literature on PI (7 of 13 studies) focuses on sleep onset insomnia. Insomnia is a paradigmatic example of the type of disorder to which PI is most frequently applied. It is characterized by both high levels of physiological arousal (Coursey, Buchsbaum, & Frankel, 1975; Monroe, 1967) and a significant cognitive "anxiety" component (Coursey, et al., 1975). Typically, the latter consists of ruminative thoughts that interfere with the natural relaxation process necessary for onset of sleep.

Behavior therapists have traditionally used two strategies to treat sleep onset insomnia: 1) progressive relaxation training, and 2) stimulus control techniques (Bootzin & Nicassio, 1978). PI was first investigated as a possible "ancillary treatment" (Ascher & Efran, 1978) for patients who had been unsuccessfully treated using the two more established techniques.

Two groups of investigators have used both quasi-experimental and experimental designs (Campbell &

Stanley, 1963) to investigate the treatment of sleep onset insomnia with PI; Relinger, Bornstein and their colleagues at the University of Montana, and Ascher and his colleagues at Temple Medical School. Relinger, Bornstein and Mungas (1978) treated sleep onset insomnia in a single female subject who had received no prior treatment for her insomnia, but had used various over-the-counter medications without success. Using a time-series analysis design (Kazdin, 1976), Relinger, et al. (1978) obtained several self-report measures including sleep onset latency, total time sleeping, and number of awakenings during the night. Three weeks of baseline were followed by five daily, half-hour sessions over a one week period. Follow-up assessments were conducted at 1, 3, and 12 months. The subject was instructed to remain awake and record the "exact thoughts and feelings she was experiencing which were keeping her awake (Relinger, et al., 1978)." Further, she was instructed to continue to monitor her thoughts and feelings as long as she had difficulty falling asleep. These recorded thoughts were ostensibly to be used in a "systematic desensitization" hierarchy. This was, in fact, a quasi-desensitization control procedure (Steinmark & Borkovec, 1974) that was used to insure treatment credibility, but was anticipated to have no therapeutic effect. Finally, a control for the demand characteristics of simply receiving treatment was implemented by telling the

subject not to expect any gains until all treatment was complete.

Relinger et al.'s (1978) results indicate significant improvement using PI in all self-report measures except number of awakenings. This measure did show significant change at one-month follow-up. All gains were maintained at three month and one year follow-up assessments.

Relinger and Bornstein (1979) extended the single case study of Relinger et al. (1978) to a larger multiple baseline design using four subjects selected to meet stringent criteria to insure the clinical significance of their insomnia. Self-reports similar to those used by Relinger et al. (1978) constituted the dependent measures, and the PI instructions, rationale, and expectancy control procedures were identical to those used in the Relinger et al. (1978) study.

The major methodological difference from the Relinger et al. (1978) study was the use of the multiple baseline design. Although the study was conducted on a very small sample, the statistical treatment of the data (Revusky's (1967) Rn statistic) required a perfect rank ordering of the treatment effects across subjects for statistical significance to be obtained.

As in the Relinger et al. (1978) study, improvement was found on several measures of insomnia, specifically sleep onset latency (81% decrease) difficulty returning to sleep, and subjective difficulty falling asleep. Due possibly to

the stringent requirement imposed by their statistical analysis, non-significant differences were obtained on several other self-report measures. The changes were maintained at a 3 month follow-up.

Relinger et al. (1978) and Relinger and Bornstein (1979) attribute the effect of PI to the interruption of an "exacerbation cycle" of worries that the subject won't be able to fall asleep. Presumably this "fear of not falling asleep" activates physiological arousal that interferes with sleep-onset. The exact mechanism of PI's effect on this "exacerbation cycle" is not specified. It is unclear why one should need procedures such as PI if mere interruption of the cycle were sufficient to produce change. Why wouldn't other cognitive and behavioral interruption procedures (eg., counting sheep) work as well? Other evidence suggests that interruption procedures do work (Bootzin & Nicassio, 1978). Both relaxation and stimulus control procedures involve some active interruption of pre-sleep activities. Relinger et al.'s (1978) theoretical explanation appears to leave unanswered questions as to the precise effect of PI.

The results from Relinger and Bornstein's laboratory are strongly suggestive that PI is effective for insomnia, but their work does not address questions about differential effectiveness relative to other, established techniques. In addition, the question of whether Relinger et al.'s (1978) procedures are the most effective way of administering PI

is unresolved. Ascher and his colleagues have attempted to answer these questions.

In an uncontrolled multiple case study, Ascher and Efran (1978) presented two different rationales to five desensitization-resistant clients for the use of PI. The first rationale, presented to three subjects, was that the client was to stay awake to gather data for use in further therapy. Expectation was given that the initial desensitization program would eventually work. The second rationale, presented to two subjects, was that the client was to stay awake so more relaxation could be done. This condition confounded relaxation with PI as clients were specifically instructed to continue their relaxation during PI. All subjects received desensitization during the PI phase of the study, further confounding the effects of the two treatments. Aside from the lack of control groups, this confounding of treatments is the major problem with Ascher and Efran's (1978) study, and with a number of other studies of PI.

Ascher and Efran (1978) report consistent improvement in all clients on self-reported sleep onset. However, the methodological inadequacies of this study make its results questionable as support for the differential effectiveness of PI.

Ascher and Turner (1979) corrected many of the methodological problems of Ascher and Efran's (1978) work in reporting the first true experimental investigation of PI.



Using 25 male and female subjects, (10 male, 15 female) who were selected according to criteria similar to those used by Relinger and Bornstein (1979), Ascher and Turner (1979) compared PI with a no-treatment control group and a placebo treatment (pairing of bedtime activities imaginally with neutral scenes) derived from Steinmark and Borkovec (1974). PI consisted of instructing subjects to remain awake as long as possible while lying in bed in a darkened room. These instructions were given at the first of four weekly sessions following a ten day baseline. Subsequent sessions dealt with problems in implementing the technique. Unfortunately, the content of these sessions, as well as the exact rationale given to the subjects for the use of PI, is not reported.

Ascher and Turner's (1979) results indicate significant improvement for the PI group on all measures but a self-report rating of restedness. All data were self-report and no reliability check was done to assess the accuracy of the reports. Unfortunately, no follow-up assessment was reported.

As with the previously reviewed studies, Ascher and Turner (1979) adopt a variation of Frankl's (1960, 1975) exacerbation cycle hypothesis to account for the success of PI. Again, their data shed little light on the validity of this theoretical explanation either of the process of insomnia or of the mechanisms by which PI works.

Turner and Ascher (1979) improved their experimental design by adding an external reliability check of subjects' self-reports. They had spouses/roommates complete a behavioral observation that consisted of an attempt to awaken the subject by whispering his/her name. When it was impossible to awaken the subject, the time was noted. In addition, Turner and Ascher (1979) attempted to control for the possibility of a social desirability response set in subjects' self-reports of sleep onset.

Based on Turner's doctoral dissertation, this study compared PI with the standard behavioral treatments for insomnia; relaxation training and stimulus control. Fifty subjects were treated using the same design as Ascher and Turner (1979); ten days of baseline followed by four weekly sessions of treatment with a one session post-test. The same therapist (Turner) administered all treatments, and, again, the exact rationale for the use of PI was not reported. The results indicate that all three treatments; PI, relaxation and stimulus control, were superior to control groups on all measures. The study failed to demonstrate an advantage for any of the three treatments within the parameters employed.

Turner and Ascher (1979) postulate that all three treatments exert their influence through promoting a perception of self-control. It is difficult to understand how this fits with previous explanations of an exacerbation cycle in which excessive efforts at self-control are viewed

as the crux of the problem in sleep onset insomnia. Once again, while the data of the study are impressive, the theoretical underpinning is unsatisfactory.

Advocates of paradoxical techniques have suggested two formats for their use. The rationale for PI appears important as paradoxical techniques require the client to take actions that appear contrary to common sense. Clients may resist performing tasks that appear to be opposite those which seem likely to be effective, if they are not given a persuasive rationale for doing so. Ascher and Turner (1980) investigated two rationales for PI. The first, rationale (Type A) uses a straightforward, theoretically valid explanation of the reasons for using PI. The second, (Type B) was derived from the work of Watzlawick et al. (1974) and consists of reframing or redefining the meaning of the client's behavior. Thus, the reframing approach appears more manipulative and indirect while the valid explanation approach is more open, honest and direct.

Using a design similar to their previous studies, Ascher and Turner (1980) compared Type A and Type B instructions to placebo and waiting list control groups using 40 subjects. Results indicate a clear superiority for the Type A ("veridical") instructions. In fact, Type B or reframing instructions, were not able to produce any improvement relative to placebo or waiting list control groups.

Ascher and Turner (1980) explain this result by indicating that reframing may not be appropriate with non-resistant clients of the sort they treated. As the reframing instruction couched staying awake in terms of data-gathering aimed at improving the effects of later treatment, Ascher and Turner's (1980) compliant subjects may have taken the therapist seriously and stayed awake intentionally to gather data, thus failing to fall asleep. This is an important finding which indicates that caution must be exercised in using "manipulative" approaches to PI with compliant clients. Had reframing been used exclusively with resistant clients, Ascher and Turner (1980) suggest that it might have fared better. Despite the negative results with a reframing rationale, this study provides further evidence for the efficacy of a straightforward use of PI with insomniacs.

One problem with Ascher and Turner's (1980) study (although their reports include no discussion of this variable) is the use of a single therapist to carry out all interventions. Although previous work on stimulus control and relaxation procedures (eg., Carr-Kaffashan & Woolfolk, 1979; Nicassio & Bootzin, 1974) has failed to find a therapist effect on these procedures, it is clear that the use of a single therapist limits the external validity of Ascher and Turner's (1980) work.

Turner and Ascher (1982) investigated therapist effects on PI using their standard design. They compared Turner's

dissertation data with subsequently gathered data on inexperienced therapists' administration of PI, relaxation, and stimulus control procedures. Their data indicate that the experienced therapist (Turner) was superior to the inexperienced therapists regardless of the technique used. Of interest is the finding that there was a therapist effect for relaxation and stimulus control procedures, contrary to previous reports. In addition, Turner and Ascher (1982) found that inexperienced therapists in their study were unable to produce changes using PI. This would seem a damaging result for PI until one looks at the pre-treatment data which show that the PI group treated by inexperienced therapists was initially much less disturbed than the other groups. This may have produced a "floor effect" such that the potential improvement in this group was limited. However, the data indicate that the experienced therapist's PI group was significantly better at post-test than the clients treated by the inexperienced therapists using PI. This would tend to vitiate the "floor effect" explanation offered by Turner and Ascher (1982). Alternate explanations may be that these clients were not as highly motivated as those who were more disturbed, or that inexperience therapists had, themselves, not been convinced of the efficacy of PI, and that this affected their delivery of the technique. Further, PI was not individualized for each client in this study, possibly diminishing its effectiveness for some of the clients. These findings are puzzling and



indicate that further study of the effects of extraneous variables, such as therapist experience and client motivation, is needed.

There are several broad statements that can be made in summarizing the literature on the use of PI to treat of insomnia. First, the evidence appears clear, in spite of methodological difficulties in the various studies, that PI is an effective treatment for sleep onset insomnia. However, the lack of any conclusive indication of superiority over other established techniques makes it questionable whether PI is the treatment of choice for insomnia. Considering the overlap in the instructions for the three procedures studied, it is unclear whether each is effective on its own or whether they share some common core process. That this may be the case has been suggested by Karlin (Note 1) who has hypothesized that any ritualized procedure that prompts a person to lie down quietly without distractions while focusing on non-anxiety-provoking thoughts may produce relaxation, and consequently sleep onset. It is unclear how this would work with insomniacs who seem to be constantly thinking arousing, worrisome thoughts (see Roth, Kramer, & Lutz, 1976; Storms & Nesbit, 1970), particularly at bed time. It is clear, however, that further investigation of the differential mechanisms, if any, of PI, relaxation and stimulus control appears warranted.

The literature further suggests that PI is most effective, at least with compliant clients, when delivered in a straightforward, honest and veridical manner. There is some indication, that less experienced therapists may fare worse using PI than when using better established procedures.

Several problems emerge from this segment of the PI literature. One is a tendency to confound the various treatments being studied. This is partially the basis for speculation about common mechanisms outlined above, and has been a consistent problem in most of the studies that compared PI to other techniques.

A further problem, possibly crucial to the elucidation of the mechanisms of PI, is the failure to ascertain whether clients actually performed the PI instructions or how they did so. While the Ascher studies attempted continually prompting the use of PI, no assessment was done of the adequacy of application of the technique by the clients. This is essential to ascertain if one wishes to demonstrate unique effects of PI not shared by relaxation and stimulus control.

Finally these studies fail to use a broad range of dependent measures. There is a growing emphasis in the behavior therapy literature (Lang, 1979; Hodgson & Rachman, 1974) on the need to assess behavioral and physiological as well as self-report indicators. The literature on PI and insomnia has neglected both of these areas of measurement

(although Ascher and Turner have reported the use of spouse/roommate behavioral reliability checks, a rudimentary behavioral measure). While these measures are not easily obtainable, and their implementation may appear to interfere with therapy at times, it is puzzling that no study has yet attempted to assess changes in these areas. While one could infer that physiological arousal would decrease upon sleep onset, it would be helpful to know exactly what the process of this change is.

The neglect of physiological measures is unfortunate in light of Borkovec's (1979) finding that insomniacs tend to fall into two groups. One group shows physiological arousal, the other does not. The absence of physiological arousal in the presence of cognitive disturbance in some insomniacs certainly has a bearing on the exacerbation cycle hypothesis. No researcher has yet tapped self-report, behavioral or physiological data which could be said to support this hypothesis. In addition, it is still unclear, how PI attenuates this arousal, whether physiologically or cognitively determined. An interesting question is whether PI is more effective with insomniacs who show arousal in both physiological and cognitive spheres or whether it works better with those who have only cognitive arousal.

Let us now turn to a consideration of three studies of PI in the treatment of other disorders.

PI and Encopresis, Urinary Retention and Agoraphobia

Bornstein, Storm, Retzlaff, Kirby, and Chong (1981) used PI with a nine year old boy who did not have bowel movements on the toilet and soiled himself repeatedly during the day. Bornstein et al. (1981) used the rationale that fear of being unable to have an appropriate bowel movement when seated on the toilet leads to exacerbation of that fear. The subject had been toilet-trained normally, and had no difficulties until age five. PI was attempted after numerous other interventions had failed.

The study followed an ABAB reversal design with baseline observations by the parents interspersed with treatment periods. PI consisted of telling the subject that he must learn to become more comfortable in the bathroom. In order to do this he was instructed to go through all the motions of pulling down his trousers, sitting on the toilet, and acting as if he were going to have a bowel movement, but he was to try not to allow the bowel movement to occur. He was to perform this ritual hourly. The parents were instructed to cease admonishing the boy to have a bowel movement during the period when the ritual was being enacted. Weekly sessions were scheduled with a therapist, but the activities in these sessions are not reported.

After three weeks of treatment, during which the parental reports indicated no soilings at all, the baseline condition was reinstated with the instruction that since therapy had been so successful, the family need no longer

follow the prescription. Soiling immediately returned to its pre-treatment level, whereupon the treatment was reinstated, again leading to a cessation of soiling and a corresponding increase in appropriate bowel movements. The family was instructed that any relapses should prompt re-instatement of the prescription. At a one year follow-up the boy was still continent and having appropriate bowel movements on the toilet.

The ABAB design is limited in its external validity, and does not rule out influence of external variables that might co-vary with the treatment (Mahoney, 1978). In this case part of the treatment consisted of admonishing the subject's parents to stop urging a bowel movement every time the subject sat on the toilet. While consistent with the treatment goal of eliminating anxiety surrounding toileting, this procedure represents a possible confounding of treatments. It is implied in the report that parents' admonitions were punishing to the subject, and it is unclear what the effect of simply leaving him alone in the bathroom without interference might have been. Thus, an alternative explanation for the success of treatment in this case is the removal of an aversive stimulus which inhibited toileting (negative reinforcement). The design of the study does not permit this alternative explanation to be ruled out.

In a similar problem of excessive sphincter control Ascher (1979) reported a multiple case study using a pre-post design (Mahoney, 1978) to evaluate treatment of

urinary retention with PI. Five subjects (three male, two female) were instructed to record their "level of discomfort urinating" for two weeks. Following this baseline period the subjects were given eight weeks of systematic desensitization coupled with in vivo assignments designed to maximize the likelihood of successful urination (eg., increasing fluid intake and going to the bathroom only when urinary urgency was evident, and leaving the bathroom if any anxiety occurred). The subjects in this study had previously reported dissatisfaction with the desensitization phase of treatment. They were given a valid exacerbation theory rationale and then PI instructions to enter a bathroom, engage in all the activities surrounding urination, but not to allow themselves to urinate. They were then to leave the bathroom.

All five subjects' ratings of urinary discomfort declined moderately during desensitization (no statistical analyses were reported, however) with faster improvement following initiation of PI. Subjects terminated when they felt satisfied with their progress, but after no more than six sessions. An informal follow-up at six months indicated that four out of five subjects were still satisfied with their urinary latency and level of comfort in public restrooms.

Several problems are apparent in this study. First, the application of PI immediately following the desensitization procedure, which was clearly producing some

(if only modest) results, confounds the effects of the PI with a trend already apparent in the data (Kazdin, 1981). It is also unclear whether some clients continued to receive desensitization during the PI period. Thus, the problem of confounded treatments appears again in Ascher's work.

A second problem with this study is the weakness of the design itself, and the inadequacy of the dependent measures. The use of self-report data on "difficulty of urinating" gives little indication of subjects' actual behavior. There may be several social desirability effects operating to enhance their ratings, particularly given the fact that a new, supposedly more potent treatment (PI) had been initiated. Ascher did not control for this possibility, or for variations in the subjects' actual behavior. At best this study can be taken as suggestive of efficacy of PI in treating of urinary retention.

Ascher (1981) has extended his study of PI to agoraphobia which appears, in many cases, to be characterized by "anticipatory anxiety" and an exacerbation cycle (Goldstein & Chambless, 1978): Many, though not all, agoraphobia patients (American Psychiatric Association, 1980) report panic attacks that seem to become the focus of cognitively mediated anxiety leading to both avoidance of situations in which panic had been experienced, and attempts to stay close to "safe" places in case a panic attack should occur (Goldstein & Chambless, 1978; Mavissakalian & Barlow, 1981).

Using a multiple baseline design with two groups of five subjects (nine women, one man), Ascher (1981) compared a graded exposure procedure to PI. Following four weeks of baseline that consisted of weekly attempts by the client to complete steps in a behavior avoidance hierarchy, (ten steps increasing in subjective units of discomfort (SUDS) and distance from home up to 100 SUDS) each client was introduced, with weekly intervals and further baseline readings between initiations, to either graded exposure or PI.

Graded exposure consisted of asking the client to attempt up to twice daily to proceed through items on his/her hierarchy in vivo until he/she felt discomfort. At that point the client was to return home. Weekly therapy sessions were held to iron out problems in implementing the procedure, discuss experiences, etc. Clients engaged in graded exposure for six weeks. After six weeks, these clients were then given PI instructions.

PI consisted of instructing the clients, with a valid rationale, to expose themselves to avoidance hierarchy items until they became too anxious to proceed. At that point they were to attempt to focus on the most salient physiological aspect of their anxiety and exacerbate it. They were to remain at that point in the hierarchy, applying PI until the anxiety subsided and they felt comfortable. The client was then to attempt to proceed home, applying PI whenever he/she felt uncomfortable. Each day the client was



to attempt to go further than the previous day. Weekly sessions focused on problems implementing PI and planning the next week's assignments.

Two groups were studied. Group A received graded exposure immediately following baseline with PI after six weeks. Group B received PI immediately after baseline and for the remainder of treatment. The dependent measures consisted of the client's ability to proceed along a behavioral hierarchy to two 100 SUDS targets in a single week. Therapy lasted for varying periods of time due to individual client differences in reaching the behavioral criterion. Other ancillary treatments were continued after the study if needed. All clients were followed-up by self-report assessments at three months.

All but one of the clients benefitted from treatment, but gains from exposure were speeded by post-exposure PI and clients treated initially with PI showed a faster rate of progress toward criterion from the start. The client who showed no gain from graded exposure was able to reach criterion with 14 weeks of PI. For clients treated initially with graded exposure the average total treatment time to criterion was 21 weeks (range = 15-27 weeks). For clients treated with PI from the start the average length of treatment to criterion was 15 weeks (range = 11-21 weeks). This seems to indicate a significant advantage in efficiency for PI.

The PI treatment reported by Ascher (1981) is very similar to in vivo flooding or implosion. In fact, several authors include implosion in their definitions of PI (eg. Hare-Mustin, 1976; Raskin & Klein, 1976). The major difference in Ascher's (1981) study appears to be that the actual procedure was client-directed rather than therapist directed. The next chapter will consider at length the relationship between PI and flooding.

Ascher's (1981) study appears to lend the most support yet for the efficacy of PI. It is a well executed study in which PI was tested against an established, demonstrably effective technique (Emmelkamp & Ultee, 1974) and found to be superior in efficiency with no loss in effectiveness. As with Ascher's previous work, the failure to collect physiological data and assess the reliability of the clients' reports of their progress on the behavioral hierarchy is a methodological weakness. Ascher also fails to provide any assessment of the parameters of his exacerbation model of PI, although he continues to rely on this explanation for the action of PI.

In spite of its flaws, Ascher's (1981) study strongly suggests that PI may be a viable technique to use in a client-guided in vivo exposure program requiring minimal therapist contact. This finding is in contrast to previous work (eg. Mathews, Gelder, & Johnston, 1981) which indicated that flooding was not likely to be effective if delivered in a self-help format.

Several other studies have appeared which have examined PI or similar techniques in other contexts.

Studies of Response Exaggeration, Negative Practice,  
and Non-clinical Analogue Studies

Gottlieb and McNamara (1979) reported a study of response exaggeration in the treatment of hetero-social anxiety in 34 undergraduate male college students. Four exaggeration techniques were examined: 1) exaggerated focusing, in which the subject was to focus his attention on his most salient anxiety response, but make no attempt to alter it, 2) response exaggeration-relevant, in which the subject was to attempt to exaggerate his most salient anxiety response during hetero-social interactions, 3) response exaggeration-irrelevant, in which the subject was to attempt to exaggerate his least salient anxiety response in hetero-social interactions, and 4) exaggeration of consequences, in which the subject was to imagine the worst possible consequence that could result from his anxiety. These treatments were compared to each other and to an attention placebo control in which subjects self-monitored frequency of interactions with females with the expectation that doing this would lead to a reduction in their anxiety.

Subjects self-monitored their hetero-social interactions for one week prior to treatment. Treatment was administered in one session, although this is not clearly specified. Measures were pre- and post-treatment paper and

pencil measures of anxiety, social avoidance, and self-reported behavioral observations. In addition, pre- and post-treatment behavioral observation was conducted of the subject interacting with a female confederate.

The results indicate no difference among the treatment groups and the control group. All subjects indicated an increased willingness to interact with females and obtained improved behavioral ratings on the post-treatment interaction task. Subjects who received response exaggeration treatments reported fewer interactions with women, although the authors suggest this may have resulted from the imposition of a more complex self-report procedure with those subjects.

This study is riddled with problems that make it an inadequate test of response exaggeration as a treatment for hetero-social anxiety. First, treatment was very short and no control was provided for the reactive effects of self-monitoring or social desirability response sets. No reliability check of subjects' self-monitoring was done. The subjects were selected on the basis of questionnaire responses, but had not sought treatment on their own, thus raising the question of the actual severity of their social avoidance. Subjects all received rehearsal following training that consisted of exposure to a female confederate. This exposure, combined with a cognitive coping strategy, may have been sufficient to produce changes in these mildly disturbed subjects. It is also likely that the one week

interval between treatment and post-test was too short to allow enough interactions for treatment differences to emerge, and that the study merely assessed the reactive effects of self-monitoring and social desirability response set. A similar design with longer intervals, better controls and a follow-up assessment might produce very different results. The authors' conclusion that response exaggeration techniques are "not sufficient to produce changes beyond that obtained simply by being in treatment" is not warranted by their data.

Azrin, Nunn and Frantz (1980) compared negative practice (Dunlap, 1928) with a habit reversal or overcorrection procedure in the treatment of nervous tics. Two groups of randomly assigned ticquers were requested to report the number of tic episodes they experienced daily prior to treatment. This constituted the baseline measure. Reliability of self-reports was checked by the therapist in the sessions and by conversations with family members.

The negative practice group practiced their tics in front of a mirror for 30 second periods over an hour duration with brief rests. While practicing they were instructed to say to themselves that "this is what I'm supposed not to do." Practice was done daily following an initial two and one-half hour treatment session in the therapist's office. Habit reversal consisted of teaching the subject to perform, unobtrusively, a competing behavior to the tic. The subjects practiced this in front of a

mirror and in the presence of the therapist. Whenever a tic was imminent or actually occurred, the subject performed the competing response. Daily home practice was prescribed, but the duration was not specified.

Azrin et al.'s (1980) results indicate that habit reversal was clearly more effective than negative practice, decreasing tics 84% the first day, 99% at four and six months. Negative practice produced an average change of less than 40% in the same time span. Thus, while negative practice produced some changes, habit reversal was clearly superior. These results appear to hold even though questions may be raised about the pre-treatment comparability of the two groups and the use of percent-change scores as a dependent measure.

One study has been reported in which a non-clinical analogue format was used to assess the efficacy of PI in reversing an experimentally induced "disorder." Levy (1978) induced "mental blocks" (operationally defined as a response time for a particular instance that was more than twice as long as the subject's average response time per instance) in doing arithmetic problems. Levy (1978) somewhat arbitrarily distinguishes two forms of PI: an active form in which the subject refrains from resisting the unwanted behavior, accepts it and tries to exaggerate it (this is essentially PI as Frankl (1960) defines it), and a passive form in which the subject refrains from resisting, accepts and simply observes the unwanted behavior. It could be argued that

passive PI is equivalent to simple exposure as used by behavior therapists. In addition to comparing the effects of the two PI strategies, Levy (1978) compared a "reframing" presentation of PI to a veridical, straightforward presentation, much in the manner of Ascher & Turner (1980).

The subjects were 81 volunteer normals (37 male, 44 females) who initially worked simple arithmetic problems under an instruction to answer as quickly as possible. The subjects then completed a second similar task under varying instructions. Group A ("reframing") received instructions to expect more "blocks" and to simply observe them as they occurred. Group B (veridical instructions) received a valid set of instructions detailing what PI was and how it might reduce blocks. This group was told "when you encounter a block do not try to resist it, but simply observe it and accept it." Group C simply performed the task twice with no instruction to pay special attention to blocks, while Group D performed the second task under instruction to improve their speed and attend to "non-blocking; the ease with which answers flow."

Active versus passive PI effects were assessed post-task by classification of subjects according to their expectation of whether they would encounter blocks in the second task. Levy (1978) assumed the active subjects actually produced more blocks, while the passive subjects did not. However, Levy (1978) did not assess this, but based his assumption on Rosenthal's (1966) work on

experimental demand characteristics which postulates that subjects "try" to conform to the demands of the experimental situation. This is certainly a questionable assumption without some supporting data that subjects actually behaved this way.

Levy's (1978) results indicate a clear reduction in the number of blocks in subjects who were labelled as active and had received veridical instructions. No other group showed significant reductions in number of blocks.

While Levy's (1978) assumptions about the active-passive PI distinction are questionable, his finding that veridical instructions were more effective in promoting change is consistent with Ascher and Turner's (1980) results. It is unclear, however, whether Levy's (1978) non-veridical group could be regarded as having received PI instructions as they would have been given by a clinical practitioner. Neither the veridical instructions nor the "reframing" instructions explicitly directed the subjects to exaggerate the blocks. Rather Levy (1978) assumed that active subjects exaggerated while passive subjects did not. While Levy (1978) provides some data that suggest that this may have happened, this study cannot be taken as a true test of PI due to the absence of specific instructions to the subjects to exaggerate their unwanted behavior. What is clear is that simple exposure to the unwanted behavior did not produce a reduction in that behavior.



### Overview and Summary

To place the experimental PI literature in perspective it is helpful to examine this body of work in the light of Kazdin and Wilson's (1978) criteria for evaluating outcome research. As they point out, the behavior therapy and psychotherapy outcome literatures have been beset by problems in several areas. The most important of these (as outlined at the beginning of this chapter) are: 1) adequate specification of techniques and procedures to provide for replicability; 2) stating of criteria for terminating therapy; 3) use of appropriate control groups/procedures; 4) use of a variety of outcome measures (eg. assessing the triple response mode: behavioral, self-report and physiological (Lang, 1979)); 5) adequate follow-up and 6) cost-effectiveness. While Kazdin and Wilson (1978) cite other areas of concern (eg. social validation of outcomes) the six areas mentioned relate most directly to the quality of the studies reviewed here. Several other factors, particularly breadth of change, importance of change and the proportion of clients who improve are also important considerations when assessing the value of a therapeutic approach. Let us now turn to a specific discussion of each criterion.

Specification of Techniques and Procedures. The experimental PI literature has been generally adequate with regard to the specification of the PI instructions and the rationale for its use. However, problems have arisen in the

specification of exactly what went on in the therapy sessions aside from the initial PI instructions. Thus, when Ascher (1981) speaks of "questions based on relevant experiences acquired while performing the daily assignments" as being one aspect of the session, it is unclear how the therapist proceeded. Did he verbally reinforce progress? Did he verbally reinforce compliance with PI? We don't know. This lack of data on the "nonspecifics" of the sessions (Lazarus, 1971; Wilson, 1980) is a considerable hindrance to the effective assessment of the quality of these studies and their value as evidence for PI.

In addition to specification of techniques, an important part of experimental procedure is subject selection. For outcome research to have maximum clinical utility it is essential that clinically relevant subject populations be studied. With the exception of the two analogue studies (Gottlieb & McNamara 1979; Levy, 1978) all of the studies of PI reviewed here have used carefully selected subjects who reported clinically significant problems. Unfortunately, the exact nature of the problems was not always clearly delineated. This is most apparent in the studies of insomnia that failed to distinguish between Borkovec's (1979) two types of insomnia. This distinction could turn out to be useful, particularly if PI is more effective with one type than the other.

Subjects in the experimental PI literature have all been clinically impaired and often refractory to other treatments. Many had been treated previously with other techniques and were given PI only when the other methods failed. This raises the question of summative or interactive effects of treatments. This will be discussed below together with the problem of trend effects.

Criteria for Terminating Therapy. Two main criteria for the termination of therapy are: client self-report of satisfaction with progress, and completion of specified number of sessions. One study failed to report length of treatment or termination criteria (Asher & Turner, 1980). In the studies that used client self-report as an indicator of when to terminate (Ascher, 1979, 1981) therapy was terminated at the attainment of the client's therapeutic goal, eg., shorter sleep onset latency, greater ability to travel away from home. While client self-report is not always a reliable indicator of improvement (see Hodgson & Rachman, 1974) it is one important measure that is frequently used. More adequate designs would also have assessed behavioral and physiological indicators of change.

In the studies which gave clients a specific number of sessions (Ascher & Efran, 1978; Ascher & Turner, 1979; Azrin et al., 1980; Bornstein et al., 1981; Gottlieb & McNamara, 1979; Relinger & Bornstein, 1979; Relinger, et al., 1978; Turner & Ascher, 1979, 1982.) all groups received a similar number of therapist contacts thus controlling adequately for

duration of therapy and number of contacts. Overall, the PI literature reflects adequate design in this area.

Use of Appropriate Controls. In the absence of a theoretical formulation on the relationship between PI and exposure, all of the studies reviewed used adequate control procedures. In addition, several (Ascher & Turner, 1979; 1980; Gottlieb & McNamara, 1979; Turner & Ascher, 1979, 1982) made specific efforts to establish credible control/placebo treatments. Unfortunately, the credibility of the control treatments, when used, was assessed only by three experimenters (Ascher & Turner, 1980; Turner & Ascher, 1980, 1982).

The relationship between PI and exposure indicates that an additional control procedure is necessary in PI research. This control would enable the assessment of the relative contributions of PI and simple exposure to treatment outcome. Ascher (1981) controlled to some extent for this apparent overlap of PI and exposure, but not as effectively as he might.

Use of a Variety of Measures. None of the studies reviewed took physiological measures, thus neglecting one crucial component of the anxiety/arousal being treated. All studies collected self-report data, but only a few (Ascher & Turner, 1979; Azrin et al., 1980; Turner & Ascher, 1979, 1982) provided any reliability checks on these data. Four studies collected adequate behavioral observations as part of their data (Ascher, 1981; Azrin, et al., 1980; Gottlieb

& McNamara, 1979; Levy, 1978). Future research would do well, in light of evidence of behavioral, physiological and subjective variations (desynchrony) (Hodgson & Rachman, 1974; Lacey, 1967), to include some measures from each of these categories as outcome indices.

Adequacy of Follow-Up. The PI literature is consistent with most other psychotherapy literature in being extremely variable in the length of reported post-treatment follow-up. Follow-up measurements have ranged from non-existent (Ascher & Efran, 1978; Ascher & Turner, 1979, 1980; Gottlieb & McNamara, 1979; Turner & Ascher, 1979, 1982) to eighteen months (Azrin et al. 1980). The follow-up procedure has ranged from a simple "how are you doing" telephone call (Ascher, 1979; Azrin et al., 1980) to therapist ratings of interview data (Ascher, 1981). Because adequate follow-up is one way of assessing durability of therapeutic changes, it is unfortunate that more attention has not been paid to this factor. However, as a body, the PI literature is more adequate on this score than most of the behavior therapy and psychotherapy outcome literature. Researchers in this area have generally been sensitive to the need for some follow-up, although they have not been very diligent in implementing this procedure.

Cost-effectiveness. While none of the studies reviewed explicitly assessed the cost-effectiveness of PI versus other treatments, several presented data that can be viewed as strong evidence for the cost-effectiveness of PI relative

to other, more established techniques. Particularly relevant to the assessment of cost-effectiveness is Ascher's (1981) study of the treatment of agoraphobia. His finding that PI produced greater changes in less time than graded exposure is encouraging. However, his failure to control for the effect of exposure diminishes the impact of this result. Certainly, the group of studies on insomnia demonstrate significant cost-effectiveness of PI over the use of hypnotic drugs with many sleep-onset insomnia clients. However, PI does not appear more cost-effective overall than relaxation training or stimulus control techniques in treating sleep-onset insomnia. Nevertheless, PI led to significant improvement in an average of five sessions with clients who had previously been resistant to medication and other techniques.

Other issues. Several other issues have been alluded to above. The first is the failure to adequately control for trend effects. Implementing PI immediately following another treatment without a control for trend effects vitiates some of the results reported. In studies that fail to control for trends in the data it is impossible to ascertain whether the effect produced by PI was due to PI alone, if it resulted from an additive effect of treatments, or was due to exploitation of a non-assessed, previously existing trend toward improvement already occurring before PI was introduced (Kazdin, 1978).

A second issue is the tendency to confound PI with

simple exposure or flooding. Ascher's (1981) study is a clear example of this. His results must be interpreted with caution since the PI group received exposure as well as PI. In fact, they received what amounted to flooding in vivo. One could argue that the addition of the cognitive set to exacerbate one's symptoms is a crucial difference between PI and exposure. Though I will argue such a position in Chapter Three, there is, at present, no empirical verification that this is the case.

This leads to the final issue, that of the theoretical underpinnings of PI. Eysenck (1979) has stressed the importance of theory as a guide to research and as an essential facet of behavior therapy. Likewise, Franks (in press) has emphasized that true scientific thinking involves working from a theoretical basis to generate testable hypotheses. The theoretical reasoning in the PI literature has a distinctive post hoc flavor. Nowhere in this literature is there an attempt to specify hypotheses derived from a theoretical framework, specify ways of measuring relevant theoretical constructs, or test a theoretically generated hypothesis about the mechanisms of action of PI. The literature is entrenched in what Franks (in press) calls the "empirical" level of science where one looks for relationships among variables with little attempt to theorize about these relationships or test one's theoretical constructions by attempts at disconfirming theoretically generated hypothesis. While the experimental PI literature

has done an adequate job of indicating that PI is a viable treatment for several behavioral disorders, little attention has been paid to elucidating the mechanisms by which PI might exert its effect. The next chapter will attempt to bridge this gap and begin to move the study of PI to a more scientific level. A model of the mechanisms operating in PI will be proposed a research program to assess the adequacy of this formulation outlined. The model will draw upon several threads that are emerging from the social learning theory literature.



### CHAPTER III

#### A SOCIAL LEARNING THEORY OF PARADOXICAL INTENTION

Views of why paradoxical techniques may work are many and varied, as are the therapeutic uses to which paradox has been put. As mentioned in the Introduction, PI is one of a variety of paradoxical approaches (See Barrack, 1978; Rabkin, 1977; Raskin & Klein, 1976; Weekes & L'Abate, 1982 for reviews of broader applications of paradox in therapy.). Theoretical explanations of the mechanisms of action of PI can be divided into two classes. The first views therapeutic paradox as metacommunication within the relationship between therapist and client. I will label these approaches relationship-based. They include communications theory (Hoffman 1971; Watzlawick, et al., 1967; Watzlawick et al. 1974), dialectical theory (Modzierz et al., 1976; Weekes & L'Abate, 1982), and reactance theory (Brehm, 1966; Brehm, 1976; Wicklund, 1974). While other rationales for using paradox therapeutically have been advanced (cf. Erickson & Rossi, 1975; Nelson, 1968) they overlap with those already cited, or focus almost exclusively on the use of paradox as a means of breaking down therapeutic resistance, rather than as an independent change-producing agent.

The second class of theory of therapeutic paradox, and of PI in particular, can be designated client-based. These views are exemplified by Frankl's "vicious circle" (Frankl, 1975), and Ascher's "performance anxiety" and "exacerbation cycle" hypotheses (Ascher, 1980, 1981). They attribute the effects of PI to specific changes brought about in the client by compliance with PI instructions, in contrast to the relationship-based theories, which focus on the therapeutic relationship as the vehicle of change.\*

The first part of this chapter will review the main aspects of relationship-based and client-based theories with a critical eye toward the testability of the theories. The accessibility of the main theoretical constructs to objective measurement will be discussed, followed by a brief exploration of the relationship between PI and exposure treatments. The final section will propose an alternative to the established theoretical frameworks for PI that not only integrates experimental data with theory, but is amenable to experimental investigation.

### Relationship-based Theories of PI

Communications/Systems Theory. These approaches are most closely associated with the work of Don Jackson and his

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\* (Fay (1978) and Whitaker (1975) point to an additional component of change in paradox--humor. Humor has been used by paradoxical therapists as a means of disrupting negative arousal. The mechanism by which humor accomplishes this has not been clearly explicated. Humor as a factor in paradox will not, therefore, be considered herein.

colleagues at the Mental Research Institute in Palo Alto, California. Based on their observations of communication between family members in both normal families and those with a schizophrenic child, these researchers drew on the mathematical theory of logical types (Whitehead & Russell, 1910) to postulate the theory of the double-bind (Bateson, Jackson, Haley, & Weakland, 1956). While the double-bind theory has received little empirical support in subsequent studies of family interaction (Olsen, 1972), the concept has a great deal of intuitive clinical appeal, and forms one of the keystones of the communications/systems approach to therapy.

Communication/systems theory postulates that all systems of interaction fall into repetitive homeostatic patterns that are stable and resistant to change. Psychological symptoms develop for the purpose of maintaining systemic homeostatic processes. Only by moving outside the system for new solutions can therapeutic changes be produced (Watzlawick et al., 1974). One way to influence systemic homeostasis is to place one or more of the system members in a paradoxical situation that, playing upon the therapeutic relationship or systemic rules, forces the system members to adopt new, more adaptive means of communicating and behaving. This is the essence of the rationale for the "therapeutic double-bind" (Watzlawick et al., 1967), of which symptom prescription (PI) is one example.

The therapeutic double-bind presumably operates by placing the client in the middle of a paradoxical communication. The role of the therapist, as communicated by the context of the therapy situation, is to help the client change problem behaviors. Yet, in giving a paradoxical instruction, the therapist does not instruct the client in eliminating the problem, but tells the client to continue it voluntarily. The therapist does this in the context of a reframing of the problem into an asset. By acting in this manner the therapist is presumably sending the client a contradictory message at two levels of communication. To comply with the context of therapy (the metacommunication that the purpose of therapy is to change problems) the client must disrupt a homeostatic pattern--the symptom. The client will resist this disruption according to the communications/systems model. By contrast, if the client obeys the therapist and continues the symptom voluntarily, then the client's stated goal of change will not be attained. According to this model, the client can only escape this paradoxical situation by altering his/her behavior in some fashion. Once this happens, presumably the therapist has a "foot in the door" to produce further disruptions in the symptomatic homeostatis. Thus, according to this model, the client cannot not change.

The major problems with this approach to PI are its vagueness and non-amenability to measurement. There is

little experimental evidence that therapeutic double-binds (which have explicit parameters outlined by Watzlawick et al., 1967) actually exist or function in the manner postulated by the theory. As with the pathogenic double-bind, there appears to be no support for the therapeutic double-bind theory other than clinical anecdotes. This is due, in part, to the difficulty of operationalizing and measuring the processes that are presumed to operate in a therapeutic double-bind. Perhaps we simply lack the technology to assess communications and their impact, nevertheless we must have reservations about a model that is both untested and apparently untestable.

Dialectical Theory. Dialectics has been invoked in an attempt to explain the workings of therapeutic paradox since the works of Alfred Adler (Modzierz et al. 1976). The most recent exponents of this approach are Weekes and L'Abate (1982). Similar in many ways to the communications/systems approach, dialectical theory also postulates that symptoms are part of a homeostatic system. The dialectical approach differs from the communications/systems approach in focusing on the client's manner of relating to the therapist as a rationale for, and explanation of, the effects of paradox in therapy.

Weekes and L'Abate (1982) view therapy as one example of a dialectical power struggle between opposing forces. These forces are the client, who unconsciously attempts to maintain homeostasis by keeping his symptoms, and the

therapist, who is trying to disrupt homeostasis by ridding the client of the problem behavior.

One of the principles of dialectics is to produce change by accepting what one is trying to alter and reinterpreting it in a positive manner, thus changing a problem into an asset. This is similar to the reframing approach of Watzlawick et al., (1974). By doing this one presumably removes the negative connotation of the symptom, reduces the power struggle between therapist and client, and gives the client a sense of mastery and control (if one is doing something positive, one can change it or not, at will). Presumably this combination of positive reframing, reduction of the resistance to the therapists "influence" and increasing a sense of mastery leads to positive change.

Again, we are faced with a series of abstract theoretical constructs ("dialectics", "power struggle", "reframing") that are difficult, if not impossible, to operationalize and measure experimentally. As with communications/systems theory, there is little if any, empirical support other than clinical anecdotes for the objective existence of any of these patterns of interaction. While the concepts of dialectical theory may be intuitively appealing to some, their utility as explanatory vehicles for PI is questionable.

Reactance Theory. Reactance theory is a social psychological theory derived largely from the work of Brehm (1966). Reactance theory attempts to predict and explain

the behavioral effects of attempts at social influence (Brehm, 1966; Wicklund, 1974). It is a well thought-out theory that is grounded solidly in the experimental social psychology literature.

Reactance theory postulates that people will resist efforts at social influence (show psychological reactance) under conditions in which they believe their freedom of choice and action is threatened. While a reading of the literature (Wicklund, 1974) suggests that individuals vary in the strength of their reactance to perceived threats to their freedom, no reliable method for measuring an individual's propensity for reactance has been devised. Verification of reactance has generally proceeded by creating conditions in which subjects receive various threats to perceived freedom, and then inferring reactance from change or failure to change in response to persuasive communications. (Wicklund, 1974).

Brehm (1976) has applied reactance theory to the clinical context by conceptualizing the therapist-client relationship as one in which the therapist is directing persuasive communications to the client. Various factors in the relationship presumably cause the client to resist direct persuasive communications to change the problem behavior. According to this view, the client sees the therapist's attempts to facilitate change as a restriction of his/her (the client's) freedom of action. By eliminating or changing the symptom, it is assumed that

the client will find himself/herself with a restricted range of possible responses to the situation in which the problem behavior has been the "preferred" response. Therefore, according to reactance theory, the client reacts against direct efforts to change his/her behavior. This is analogous to resistance or noncompliance in the psychoanalytic and behavioral senses (Brehm, 1976).

Paradoxical techniques (including PI) work by taking advantage of the client's reactance to persuasive communications and reverse the pattern. Thus, the therapist makes a direct instruction not to change with the intent that the client, exhibiting reactance to this communication, will do the opposite (Brehm, 1976). Therefore the therapist, in effect, uses the client's reactance "against" the client to produce the desired changes.

The reactance model of PI is heavily dependent on the idea that clients generally resist most efforts by the therapist to change their behavior. However, as Lazarus and Fay (1982) point out, "resistance" is often not a client factor as much as a failure on the therapist's part to adequately plan and carry out effective therapeutic strategies. The universality of "resistance" postulated by psychoanalytic thinkers is certainly open to question.

A second problem with reactance theory is the distinctly post hoc and inferential quality of the measurement of reactance. Reactance is not measured prior



to attempts at influence (Wicklund, 1974), but is inferred post hoc from behavioral evidence of failure to change.

Although people presumably vary in their propensity to display reactance, a predictive measure that can indicate in advance of any attempt to influence a person that he/she is likely to show reactance has yet to be developed.

Nonetheless, reactance theory has been fruitful in predicting behavioral changes across groups of individuals in the social psychology laboratory. Its usefulness as a working concept in clinical settings, where individual prediction is more often needed, is questionable. Thus, while reactance theory is the most adequate of the relationship-based theories of PI, it still leaves much to be desired as a scientific-clinical theory.

Summary of Relationship-Based Theories. Three major theories that center on the therapist-client relationship in explaining the workings of paradox have been reviewed. While these are not the only such theories, they represent the most prominent examples of this genre in terms of empirical support and popularity. All three theories fall short of being satisfactory due to significant problems in operationalizing terms, measuring concepts or both. They all focus on a common thread that will reappear later when I review client-based theories (ie. the issue of freedom, control and mastery). All three theories view symptoms as efforts at controlling some aspect of one's psychological world. Thus, while they are clearly deficient as testable

theories, the relationship-based theories have all pointed to a central issue that will be prominent in the discussion to follow. Let us now turn to client-based theory of PI.

### Client-based Theory of PI

Most of the theoretical thinking about PI has been done by proponents of the relationship-based theories. It is interesting to note that the originator of PI, Frankl, has adopted a client-based theory. This viewpoint has been adopted by later behavioral researchers, notably Ascher (1981), and Relinger and Bornstein (1979). It is surprising that behavior therapists, who have traditionally focused on explanations of behavior that involve situational factors, (Mischel, 1979) have adopted a clearly person-centered view of PI.

The central concept in the client-based theory of PI is the "vicious circle" (Frankl, 1975) or "exacerbation cycle" (Relinger & Bornstein, 1979). This is the notion that many problems are a function of the client's attempt to control some aspect of his/her behavior (falling asleep, onset of panic) that is not amenable to conscious voluntary control. In so doing, the client fails to achieve control, and becomes anxious about the fact that he/she has not been able to "will" the result desired. This failure of control leads to further cognitive and autonomic arousal, which in turn, leads to further efforts at control, etc. It should

be noted that control, or attempts to control, in this case, consist of suppressing autonomic or cognitive arousal by direct conscious effort. Later, I will speak of control in another sense, that of having available an appropriate coping response.

According to client-based theory PI works by interrupting the cycle of attempts at control, subsequent failure, and consequent arousal. By instructing the client to perform (ie. de-control) and exaggerate the unwanted response, the therapist short-circuits attempts at direct control and enables the client to allow automatic arousal reducing processes to take effect.

As with the relationship-based theories, several problems exist in this formulation. First, and probably most important, is the circular reasoning involved in this explanation. The assumption is that if disorders are the result of attempts at control that fail, and if we decontrol the disorder, then the client will be in control again. This formulation obviously includes an assumption about the etiology of the disorders as well as the means by which the client tries to cope with the disorder. While anticipatory anxiety or arousal is present in disorders such as insomnia and agoraphobia, it is not clear that the exacerbation cycle postulated by the client-based theory actually occurs. No empirical evidence is available to verify the existence of an exacerbation cycle that perpetuates these disorders.

It is further unclear how one would measure an exacerbation cycle. It would seem at least hypothetically feasible to do so by simultaneous monitoring of physiological and self-report data. This, however, raises the problem of desynchrony among measures (Hodgson & Rachman, 1974) and an uncertainty about what this might mean for the exacerbation of anxiety and arousal. Moreover stimuli that emanate from internal and external sources, result in different physiological patterns across individuals and situations (Lacey, 1967).

Even if one could adequately measure the exacerbation cycle, it is unclear how one would assess the mechanism by which PI interrupts this cycle. Simply observing that the cycle stops upon implementation of PI instructions says nothing about the mechanisms of action.

Thus, while client-based theory of PI is consistent with part of the evidence on the psychological and physiological correlates of anticipatory anxiety, it suffers from difficulties in operationalization and measurement of its constructs. What is needed is a theory of PI that 1) is based upon measurable constructs, and 2) is amenable to experimental test.

The issue of control is central to the process of PI as explained by client-based theory. It is noteworthy that several divergent theories have all pointed to this issue. Control will be a central concept in my model of PI as well,

but in a different sense from that used by earlier formulations. Before discussing a social learning approach to PI a slight, though important, digression is necessary to discuss the relationship between PI and exposure treatment as used by behavior therapists. This relationship is significant to the model to be proposed.

### PI and Exposure

Marks (1969) was the first to point out the similarity between PI and exposure. Later (Marks, 1972) he specifically included PI in a catalogue of exposure techniques.

Agras (1972) has divided exposure techniques into two groups based upon whether the exposure is carried out in vivo or in imagination. Exposure techniques can also be grouped according to whether exposure to the problem situation is gradual, with an attempt to keep anxiety at a minimum (as in graded exposure or systematic desensitization) or whether high intensity arousal is used (as in flooding in vivo or with anxiety arousing images as in implosive therapy). PI, as generally implemented, seems to be characterized by in vivo flooding as a major part of the technique. That is, the client is directly exposed, as intensely as possible, to the actual problem situation or stimulus. Nonetheless there are important differences between PI and other exposure techniques.

While flooding simply exposes the client to the feared situations, and aims primarily at stimulating maximum anxiety/arousal, PI also provides the client with a "coping" procedure in the form of a conscious attempt to exaggerate the felt anxiety/arousal as much as possible. Thus, arousal is not the aim of PI. Instead, arousal that is already present is offset by "coping responses" that encourage the arousal to peak and then subside.

This difference between PI and in vivo flooding is important theoretically as well as practically. The most frequently cited theoretical explanation for the success of flooding is based upon extinction of the anxiety attending the aversive stimulus through non-reinforced exposure to that stimulus. This implicitly, assumes a conditioning explanation for the generation of the anxiety/arousal problem (cf. Eysenck, 1978), and largely ignores cognitive factors either in the generation or treatment of these problems. Bandura (1977) and Wilson (1982a) have criticized conditioning theories for exactly this neglect of cognitive factors.

Marshall et al. (1979) have provided a conditioning theory of flooding which includes cognition. They note that, at least in phobias, the problem situation/stimulus elicits both fear and negative self-statements. This fact, plus evidence that brief exposure is usually not enough to eliminate phobic responding, forms the basis for defining two parameters that promote effective flooding. These are:

1) maintaining in vivo exposure past peak arousal until a pre-exposure baseline is achieved, and 2) the provision to the client of some sort of coping response that can be used in the exposure situation. It is apparent that these are the main ingredients in PI.

The "cognitive-conditioning" theory of Marshall et al. (1979) fails to account for the mechanism by which the combination of these two factors leads to greater therapeutic effects for flooding done with, as opposed to without, an additional coping response. While extinction or habituation theories (cf. Lader & Mathews, 1968) describe the gross process during exposure (I use the word "describe" rather than "explain" advisedly), they do not clearly elucidate the process by which the client's cognitions are changed, nor do they provide a quantifiable measure by which one can assess the cognitive aspects of this process. The model to be proposed will remedy this situation.

In developing a model of PI, two concepts from the behavioral literature will be used. First, it is proposed that what maintains the symptom in the disorders treated with PI is the client's subjective feeling of uncertainty in the problem situation. Uncertainty is defined, in this model, in terms of the predictability/controllability (P/C) of the problem situation/stimulus event. Thus, the insomniac becomes aroused at bedtime, because he/she is uncertain as to whether he/she will be able to fall asleep. and uncertain what he/she can do if sleep does not occur.

In addition, the person anticipates horrible consequences from not sleeping. Similar reasoning can be applied to agoraphobia with panic attacks, urinary retention, and the other disorders that appear amenable to treatment with PI.

Based upon this formulation of the maintenance of arousal the model draws on a second aspect of the behavioral literature, Bandura's (1977, 1980) concept of self-efficacy, to show how exposure using PI possibly exerts its effect. The self-efficacy literature will also provide a technology for testing this model.

#### A Social Learning Theory of PI

As already mentioned, the disorders for which PI has been shown to be effective share the element of an attempt by the client to exercise control over his/her autonomic functions. These attempts at control all involve avoiding what, for the client, is an aversive event (eg. excessive autonomic arousal) that leads to further aversive consequences. These aversive consequences usually consist either of failure to perform some "natural" bodily function, or an exacerbation of autonomic arousal to very high levels.

A second characteristic of these disorders is that the aversive consequences that the client attempts to avoid are often unpredictable. The insomniac can sometimes fall asleep, the person with urinary retention can sometimes urinate in public bathrooms if they are unoccupied, the



agoraphobic may not experience panic everytime he or she ventures forth.

The primary therapeutic goal of PI is not to teach the client control (in fact, the opposite is taught: that the person cannot control his/her autonomic responses no matter how hard he/she tries), but to provide the client with an easily performed coping response that, when performed in an aversive setting, will lead to a reduction of arousal.

Thus, three factors appear to be central to the disorders typically treated with PI: 1) controllability, perceived or actual, of the aversive event, 2) predictability of the event, and 3) lack of an appropriate coping response when the event occurs.

Based on these three factors I propose that PI operates in the following fashion. The PI instructions, combined with an explicit, veridical rationale for their use, provide the client with a simple, easily implemented cognitive coping response that leads to increased perceived control in the aversive situation. The PI instructions also, by their very nature, reduce the unpredictability of the aversive event and its concomitant arousal since the client is told to produce these himself/herself deliberately.

As the literature to be cited below indicates, these factors (presence of coping response, increased perceived control and predictability) should result in at least a

modest lowering of arousal to the aversive event. This lowered arousal will then permit two things to occur. First, the client, upon observing this lowered arousal when implementing PI, is likely to experience a heightened sense of self-efficacy (Bandura, 1977, 1980). Second, the lowered arousal will probably make it easier for the client to confront the aversive event, thus facilitating the effects of exposure. It should be noted that the arousal referred to is not limited to physiological states, but includes the stimulation of negative self-statements that create a subjective, cognitive arousal (worry).

Once the client learns the PI response he/she can then proceed with exposure. Because a coping response is available, exposure can occur for longer periods until the client's arousal declines to a low level, thus facilitating extinction or habituation (Lader & Mathews, 1968; Marshall, et al., 1979). The cognitive feedback that occurs from the client's positive performance and decreased arousal levels increases the client's feeling of self-efficacy vis a vis the aversive event.

Thus, the implementation of PI serves two therapeutic purposes. It enables the client to carry out the initial stages of exposure more easily, and it provides the client with a coping mechanism with which to respond to the aversive event in the future. In this model then, the specific effect of PI is to enable the client to enter the critical performance-based phases of treatment that have

been shown to be essential for behavior change (Wilson, 1982 a). PI is thus construed as an important, if not essential, ingredient in a larger exposure treatment rather than as the effective and independent therapeutic ingredient.

This model draws upon two areas of the literature that have not been previously tied together. I will now review some of the theoretical thinking and evidence in each of these areas that has led to melding the two into a model of PI.

Literature on Uncertainty (P/C). Numerous excellent reviews of this area of research have appeared in the last ten years dealing with both animal (Seligman, Maier, & Solomon, 1968) and human research (Averill, 1973; Epstein, 1973; Miller, 1979; Miller & Grant, 1980; Mineka & Kihlstrom, 1978; Thompson, 1981). Rather than provide a separate review of specific studies, I will summarize the common conclusions of the various reviewers and cite certain specific studies that relate to my thesis.

The effects of P/C on arousal and anxiety are several. The more predictable an aversive event, the less stressful it is for the organism. Likewise, the more controllable the event (controllability being defined as the availability of a coping response in the face of the aversive event (Miller, 1979; Thompson, 1981)) the less helpless the person feels (Burger & Arkin, 1980), and the lower the arousal that occurs in anticipation of the event (Miller, 1979).

The literature indicates that uncertainty is an extremely aversive state for an organism (Averill, 1973), and factors that enable an organism to achieve greater certainty (ie. more predictability and controllability) will lead to significant reductions in arousal and stress (Epstein, 1973). During conditions of uncertainty the person is confronted with several questions: "what will happen, whether it will happen, when it will happen, and what can be done about it (Lazarus & Averill, 1972);" and "how bad can it get (Miller, 1979)?" Cognitive preoccupation with these questions appears to promote both subjective and physiological arousal (Lazarus & Averill, 1972). Anything that reduces uncertainty appears to also reduce arousal (Epstein, 1973; Mandler, 1972) even if the answers indicate that the aversive event will surely occur. Reduction in uncertainty about the occurrence of an aversive event also seems to reduce the aversiveness of the event itself (Miller, 1979).

Other research has indicated that by lowering arousal through accurate expectations (greater P/C) habituation to aversive events is facilitated and the impact of the aversive event is reduced (Epstein, 1973).

Thus, any procedure that increases P/C will lead to lowered arousal and diminished stress. This is so even if the increase in P/C is only subjective, not actual (Geer, Davison, & Gatchel, 1970). The availability of a coping response is one major factor in making aversive events less

stressful (Thompson, 1981). In addition, there is evidence that self-administration of the aversive event also decreases its impact and stressfulness (Miller, 1979) by making it both more predictable and more controllable.

PI appears to promote several of these palliative factors. It reduces the subjective unpredictability of the aversive event by making it (apparently) self-administered. Second, PI increases controllability by providing a readily implemented coping response. According to the literature on uncertainty, the accomplishment of these psychological tasks should lead to decreased arousal during the anticipatory period as well as to the event itself.

One reservation must be mentioned in applying the uncertainty literature to PI. All of the studies to date have used some sort of external event as the aversive stimulus. In the disorders usually treated with PI the aversive event is an internal one (usually panic, insomnia, anxiety). Can we assume enough of a parallel between the external events used in the literature and internal clinically relevant events to warrant application of this literature to PI? I believe we can, due to the largely cognitive nature of the processes postulated in this literature, and the fact that cognition and external reality often mirror each other closely (Geer et al., 1970). In addition, the attribution of causality in a state of high arousal tends to be external whether or not a clearly definable external event is present (Miller, 1979).

Likewise, reducing arousal seems to shift attributions of causality internally, even though the aversive event may be external. Thus, it appears reasonable to assume enough of a reciprocal interaction between internal and external events to allow for a valid application of the uncertainty literature to internal aversive events.

It has been proposed here that PI reduces arousal by increasing certainty and by providing a coping response. However, this does not cover the entire spectrum, since an integral part of PI is exposure. What is the mechanism by which increasing certainty and reducing arousal enables the client to better carry out exposure? To answer this question we can draw on the concept of self-efficacy (Bandura, 1977).

Self-efficacy and PI. Bandura (1977) has proposed that although treatments with a performance component are most effective in dealing with fears (arousal), the changes produced by such treatments are cognitively mediated. These cognitive mediators consist of two components. Outcome expectations which refer to the client's expectation that performance of a response will lead to a positive outcome, and self-efficacy expectations which refer to the client's expectation of whether he/she can actually perform the response necessary to achieve the expected outcome. If one cannot execute an appropriate response then outcome expectancies become irrelevant. Likewise, if one possesses

no response for the situation at all, outcome expectancies become less important.

According to Bandura (1977), positive self-efficacy expectations (believing one can execute an appropriate coping response) reduce arousal (anticipatory fear) and increase the likelihood that the client will exert a high level of effort in dealing with the problem situation. This effort could be construed, in the therapeutic context, as closely linked with compliance with therapeutic procedures.

According to Bandura (1977, 1980) self-efficacy expectations are derived from several sources: performance accomplishments, level of emotional arousal (lower arousal resulting in more effective coping and higher self-efficacy), verbal persuasion, and situational determinants. As mentioned earlier, performance accomplishments are the most salient sources of efficacy expectations, but the other determinants also have an impact.

Self-efficacy theory provides the final component in the proposed model of PI. It offers a structural framework upon which one can build predictions about uncertainty and the necessary link between PI and exposure. According to this model, PI instructions, delivered veridically, increase self-efficacy and also augment the degree of certainty. The increase in certainty leads to reduced arousal, one factor that contributes to increased self-efficacy. This, in turn, leads the client to be more willing to carry out the

remainder of the procedure, viz., in vivo exposure to the aversive event. As Marshall et al. (1979) and Bandura (1977, 1980) indicate, conditions that facilitate contact and coping with the aversive event will provide performance data upon which the client can base future self-efficacy expectations. Conversely, a client's view of himself/herself as ineffective (low self-efficacy) will lead to both anticipatory arousal and actual arousal in the aversive situation. These, in turn, will feed-back to lowered self-efficacy, expectation of greater pain from the aversive event, and more arousal, unless the person is provided with a coping response. "To the extent one can prevent, terminate, or lessen the severity of aversive events there is little reason to fear them (Bandura, 1977)," or to maintain a state of aroused vigilance in anticipation of them.

A major advantage to the use of self-efficacy theory in explaining PI is the existence of a well-tested technique for assessing self-efficacy expectations for each individual vis a vis particular situations. Bandura has termed this procedure "microanalysis" (Bandura, Adams, & Beyer, 1977; Bandura, Adams, Hardy, & Howells, 1980). Microanalysis enables the monitoring of self-efficacy changes at various points in the therapy process and also allows for assessments of the pattern of correlation among behavioral, subjective, and physiological changes. One could apply a similar technique to the assessment of subjective



uncertainty within the P/C paradigm that has been presented herein. Thus, not only do uncertainty and self-efficacy represent testable and tested concepts applicable to PI, but in addition, a technology already exists for their measurement. The final chapter will discuss some of the remaining issues to be resolved concerning PI and suggest, in outline, several studies to answer these questions within the context of the model proposed.

## CHAPTER IV

### OVERVIEW AND PROPOSALS FOR FUTURE RESEARCH ON PI

I have attempted to explain the mechanisms of PI according to the salient perspective of social learning theory. To summarize briefly: The experimental literature on PI is equivocal. While demonstrating fairly clearly that PI seems to have a therapeutic value with a small range of disorders, this literature provides no direct support for the effectiveness of PI as an independent technique. Due to the failure on the part of investigators to control for the effects of simple exposure in their studies, the most one can say is that PI appears to facilitate the exposure process.

With this limited view of PI in mind, I have proposed a model of how this facilitation effect might work. The model draws on the literatures on the effects of uncertainty of aversive stimulation, and self-efficacy. The gist of the model is that PI facilitates exposure by providing a simple, self-controlled coping response to the aversive events. This has three effects: 1) it reduces uncertainty about the occurrence of the aversive event thus reducing arousal to the prospect of the event, 2) it provides for

self-administration of the aversive event, thereby promoting greater certainty which has been shown to reduce anticipatory arousal, and 3) provides the person with self-efficacy information via verbal persuasion and mild arousal reduction that, combined with a coping response leads to increased self-efficacy expectations, and reduces the potential impact of exposure to the aversive event.

Although the experimental literature on PI is far from adequate, it has produced strikingly consistent results. However, there are many questions that still remain to be explored. Seven questions form the core of the remaining issues surrounding PI. Each question will be discussed briefly with suggestions of studies to investigate each. Like the questions, these proposals are suggestive for future research rather than exhaustive.

1. Relationship between PI and in vivo exposure. A major, perhaps the most important, question left unresolved by the PI literature is the precise relationship between PI and exposure. Does PI exert an effect over and above that of simple exposure, and if so, what is this effect? I have proposed that such an effect does occur, and that it is essentially a facilitation of exposure. It is essential that a study comparing PI plus exposure to simple flooding in vivo be performed if this question is to be answered. While there is a study in the literature (eg. Ascher, 1981) in which PI has been compared to graded exposure, this does

not resolve the question, since PI is much closer procedurally to in vivo flooding than graded exposure.

It would seem most appropriate to use agoraphobic subjects for this and most of the other studies to be proposed. This is so for several reasons. First, agoraphobics can be more easily monitored throughout exposure with less disruption of the therapeutic process than could, for instance, insomniacs. Second, agoraphobia is difficult to treat (Mathews et al., 1981) and provides a significant challenge to PI. Finally, agoraphobia is probably the prototypical anticipatory anxiety disorder and has been thoroughly investigated within a social learning behavior therapy framework (cf. Mathews et al., 1981; Mavissakalian & Barlow, 1981).

2. Effect of PI on uncertainty and self-efficacy. A second question that is directly relevant to the proposed model is whether PI produces the postulated changes in perceived uncertainty and self-efficacy. A repeated measures study that assesses self-efficacy prior to and after PI instructions using Bandura et al.'s (1980) procedure, as well as a similar assessment of uncertainty (ratings of how likely the client considers that the aversive event will occur) is likely to shed light on this question. In assessing uncertainty, both subjective predictability and controllability should be measured. In the latter case, it would be important to word assessment

questions so that controllability (related to outcome) is separated from self-efficacy (related to performance).

Based on the proposed model, one would expect modest increases in self-efficacy ratings, subjective predictability and perceived controllability of the aversive event after the delivery of PI instructions and some practice in the office (ie. prior to actual in vivo exposure to the aversive events). A further control for imaginal exposure during PI, consisting of a group that received no coping instructions (PI), but received instructions to imagine themselves confronting the aversive event, should also be included. Following in vivo exposure one would expect further improvement on all three subjective measures.

3. Does PI reduce arousal to the aversive event prior to actual exposure? One could answer this question in a manner similar to the question of cognitive changes just discussed. Physiological measures of skin conductance level and heart rate would be obtained prior to PI, and immediately after the practice of PI in the office setting. Again, a control for the effects of imaginal exposure alone would be necessary. Lang (1979) has provided a methodology for increasing the external validity of this procedure by including what he calls "response propositions" in the imagery used. If PI produces changes in arousal to images of the aversive event, then one can begin to assert that PI exerts a unique effect.

This study could also be performed in vivo, but at the risk of confounding PI with the effects of exposure.

4. Role of practice in PI. Does practicing in the office affect a client's ability to exercise PI effectively in vivo? If so, how can the office practice of PI be made more efficient? These are difficult questions to answer due to the possible overlap between PI and imaginal exposure. Careful instructional control would be necessary to minimize the client's visualization of the actual aversive situation that is to be the target of PI and in vivo exposure. This type of control appears possible, and such a study would compare the efficacy of PI with and without rehearsal in the office prior to in vivo exposure. Evidence that minimal rehearsal was necessary would enhance PI's viability as a cost-effective technique requiring minimal therapist time.

5. Physiological and cognitive effects of PI. This question might best be answered by a study that continuously monitored physiological changes during in vivo exposure while repeatedly assessing the client's perceptions of self-efficacy and uncertainty. It would be important to have a control group that received in vivo exposure alone without PI instructions. This study would further assess the relative efficacy of exposure with PI versus exposure without PI and would shed light on the pattern of changes in the triple response modes that occur during treatment.

6. Efficacy of PI with other disorders. The anecdotal literature on PI provides some evidence that PI might be

effective with disorders other than those reported in the experimental literature. Among them are obsessive-compulsive disorders (Solyom et al., 1976), compulsive gambling (Victor & Krug, 1967), stuttering (Modzierz et al., 1976), and disorders in which anticipatory arousal appears to play a role (Frankl, 1975). Controlled studies of PI treatment of these disorders are needed if the range of usefulness of PI is to be clearly defined. According to the model presented here, one would predict that PI would be effective with any disorders characterized by high arousal resulting from anticipatory anxiety, absence of a clear, effective coping response, and low self-efficacy expectations.

7. Relationship of PI to other "paradoxical" therapies. PI is but one of a variety of applications of paradox to therapy (cf. Weekes & L'Abate, 1982). It has been subjected to experimental investigation primarily because it is the least complex of the paradoxical techniques. Difficulties of measurement are minimal with PI, and although more complex in the case of process studies they are still manageable. This is due largely to the fact that PI is used as an individual therapy technique.

When one begins to attempt assessments of other paradoxical techniques several problems arise. It is difficult to define operationally the exact technique in the uses of paradox, particularly where treatment involves couples or family groups. As one increases the number of

clients, the problems of measurement seem to increase exponentially. There do appear to be emerging technologies that can be applied to the study of these more complex forms of therapeutic paradox (cf. Jacob and Lessin, 1982), however these are still even more primitive than the methodology used in individual therapy research on PI. Furthermore, the theoretical links that tie PI to other paradoxical techniques are tenuous, simply because the other theories are relationship-based and not subject to adequate scientific study in their present form. Thus, while the relationship of PI to other paradoxical procedures is important, it is probably the least amenable to experimental investigation of the questions about PI.

Concluding remarks. It is clear that, although PI research has moved beyond clinical anecdote to the laboratory, many questions remain unanswered. PI is only beginning to gain both scientific and clinical respectability. As further research is carried out and clinical anecdotes become confirmed or disconfirmed, PI could provide one more key procedure in the behavior therapist's growing armamentarium of prescriptive techniques. More importantly, this research could lead to greater understanding of the mechanisms by which exposure-based treatments operate.



#### REFERENCE NOTES

1. Karlin, R. Personal communication, September, 1981.

## REFERENCES

- Adler, A. Verdrangung und mannlicher protest: ihre rolle und bedeutung fur die neurotische dynamik, 1911. In A. Adler, Heilen und bilden, 1914.
- Agras, W. S. Introduction to chapter six. In W.S. Agras, (Ed.) Behavior modification: principles and clinical applications, Boston: Little, Brown, and Co., 1972.
- Agras, W.S., Kazdin, A.E., & Wilson, G.T. Behavior therapy: towards an applied clinical science, San Francisco: Freeman, 1979.
- American Psychiatric Association, Diagnostic and statistical manual of mental disorders, 3rd. edition, Washington, DC: Author, 1979.
- Ascher, L.M. Paradoxical intention in treatment of urinary retention. Behaviour Research and Therapy, 1979, 17 267-270.
- Ascher, L.M. Paradoxical Intention. In A. Goldstein and E.B. Foa (Eds.), Handbook of behavioral interventions, New York: Wiley, 1980.
- Ascher, L.M. Employing paradoxical intention in the treatment of agoraphobia. Behaviour Research and Therapy, 1981, 19, 533-542.
- Ascher, L.M., & Efran, J.S. Use of paradoxical intention in a behavioral program for sleep onset insomnia. Journal of Consulting and Clinical Psychology, 1978, 46, 547-550.
- Ascher, L.M., & Turner, R.M. Paradoxical intention and insomnia: An experimental investigation. Behaviour Research and Therapy, 1979, 17, 408-411.
- Ascher, L.M., & Turner, R.M. A comparison of two methods for the administration of paradoxical intention. Behaviour Research and Therapy, 1980, 18, 121-126.
- Averill, J.R. Personal control over aversive stimuli and its relationship to stress. Psychological Bulletin, 1973, 80, 286-303.

- Azrin, N.H., Nunn, R.G., & Frantz, S.E. Habit reversal versus negative practice treatment of nervous tics. Behavior Therapy, 1980, 11, 169-178.
- Bandura, A. Social learning theory, Englewood Cliffs, NJ: Prentice-Hall, 1977.
- Bandura, A. The self and mechanisms of agency. In J. Suls (Ed.), Social psychological perspective on the self. Hillsdale, NJ: Erlbaum, 1980.
- Bandura, A., Adams, N.E., & Beyer, J. Cognitive processes mediating behavioral change. Journal of Personality and Social Psychology, 1977, 35, 125-139.
- Bandura, A., Adams, N.E., Hardy, A.B., & Howells, G.N. Tests of the generality of self-efficacy theory. Cognitive Therapy and Research, 1980, 4, 39-66.
- Barrack, R.A. A survey and critique of paradoxical intention and symptom prescription. (Doctoral dissertation Rutgers University, 1979) Dissertation Abstracts International, 1979, 39 (10-B), 5054.
- Bateson, G., Jackson, D.D., Haley, J., & Weakland, J. Toward a theory of schizophrenia, Behavioral Science, 1956, 1, 251-264.
- Bootzin, R.R. & Nicasio, P. Behavioral treatment for insomnia. In M. Hersen, R. Eisler, & P. Miller (Eds.), Progress in behavior modification. New York: Academic Press, 1978.
- Borkovec, T.D. Pseudo (experiential) insomnia and idiopathic (objective) insomnia: Theoretical and therapeutic issues. In H.J. Eysenck & S. Rachman (Eds.), Advances in behaviour research and therapy, London: Pergamon, 1979.
- Bornstein, P.H., Sturm, C.A., Retzlaff, P.D., Kirby, K.L., & Chong, H. Paradoxical instruction in the treatment of encopresis and chronic constipation: An experimental analysis. Journal of Behaviour Therapy and Experimental Psychiatry, 1981, 12, 167-170.
- Brehm, J.W. A theory of psychological reactance. New York: Academic Press, 1966.
- Brehm, S.S. The application of social psychology to clinical practice. Washington, DC: Hemisphere, 1976.
- Burger, J.M., & Arkin, R.M. Prediction, control, and learned helplessness. Journal of Personality and Social Psychology, 1980, 38, 482-491.

- Campbell, D.T., & Stanley, J.C. Experimental and quasi-experimental designs for research. Chicago: Rand-McNally, 1963.
- Carr-Kaffashan, L., & Woolfolk, R.L. Active and placebo effects in the treatment of moderate and severe insomnia. Journal of Consulting and Clinical Psychology, 1979, 45, 153-161.
- Coursey, R.D., Buchsbaum, M., & Frankel, B.L. Personality measures and evoked responses in chronic insomniacs. Journal of Abnormal Psychology, 1975, 84, 239-249.
- Dunlap, K. A revision of the fundamental law of habit formation, Science, 1928, 67, 320-322.
- Dunlap, K. Repetition in the breaking of habits. Science Monthly, 1930, 30, 66-70.
- Emmelkamp, P.M.G., & Ultee, K.A. A comparison of "successive approximation" and "self observation" in the treatment of agoraphobia. Behavior Therapy, 1974, 5, 606-613.
- Epstein, S. The nature of anxiety with emphasis on its relationship to expectancy. In C.D. Spielberger (Ed.) Anxiety: Current trends in theory and research, Vol. 2, 1972 New York: Academic Press, 1972.
- Epstein, S. Expectancy and magnitude of reaction to a noxious UCS. Psychophysiology, 1973, 10, 100-107.
- Erickson, M. Further techniques of hypnosis-utilization techniques. American Journal of Clinical Hypnosis, 1959, 2 3-21.
- Erickson, M. & Rossi, E. Varieties of double bind. American Journal of Clinical Hypnosis, 1975, 17, 143-157.
- Eysenck, H.J. The conditioning model of neurosis. The Behavioral and Brain Sciences, 1979, 2, 155-199.
- Fay, A. Making things better by making them worse. New York: Hawthorn, 1978.
- Fay, A., & Lazarus, A.A. Psychoanalytic resistance and behavioral nonresponsiveness: A dialectical impasse. In P.L. Wachtel (Ed.), Resistance: Psychodynamic and behavioral approaches. New York: Plenum, 1982.
- Frank, J.D. Persuasion and healing. Baltimore: Johns Hopkins Press, 1961.
- Frankl, V.E. The doctor and the soul: From psychotherapy to logotherapy, New York: Knopf, 1955.

- Frankl, V.E. Paradoxical intention: A logotherapeutic technique. American Journal of Psychiatry, 1960, 14, 520-535.
- Frankl, V.E. Paradoxical intention and dereflection. Psychotherapy: Theory, Research, and Practice, 1975, 12, 226-237.
- Franks, C.M. On conceptual and technical integrity in psychoanalysis and behavior therapy, two fundamentally incompatible systems. In H. Arkowitz & S. Messer (Eds.), Psychoanalytic and behavior therapy: Is integration possible? New York: Plenum, In press.
- Geer, J.H., Davison, G.C., & Gatchel, R.I. Reduction of stress in humans through nonveridical perceived control of aversive stimulation. Journal of Personality and Social Psychology, 1970, 16 731-738.
- Gerz, H.O. Experience with the logotherapeutic technique or paradoxical intention in the treatment of phobic and obsessive - compulsive patients. American Journal of Psychiatry, 1966, 123, 548-553.
- Glass, G.V. Primary, secondary, and meta-analysis of research. Educational Researcher, 1976, 5, 3-8.
- Goldfried, M.R. Toward the delineation of therapeutic change principles. American Psychologist, 1980, 35, 991-999.
- Goldfried, M.R. (Ed.), Converging themes in psychotherapy: Trends in psychodynamic, humanistic and behavioral practice. New York: Springer, 1982.
- Goldstein, A., & Chambless, D. A reanalysis of agoraphobia. Behavior Therapy, 1978, 9, 47-59.
- Gottlieb, B.S., & McNamara, J.R. A comparison of response exaggeration techniques. Journal of Clinical Psychology, 1979, 35, 776-778.
- Haley, J. Problem-solving therapy San Francisco: Jossey-Bass, 1976.
- Hare-Mustin, R. Treatment of temper tantrums by paradoxical intervention. Family Process, 1975, 14, 481-486.
- Hodgson, R., & Rachman, S. Desynchrony in measures of fear: II. Behaviour Research and Therapy, 1974, 12, 319-326.
- Hoffman, L. Deviation-amplifying processes in natural groups. In J. Haley (Ed.), Changing families, New York: Grune & Stratton, 1971.

- Jacob, T., & Lessin, S. Inconsistent communication in family interaction. Clinical Psychology Review, 1982, 2, 295-309.
- Kazdin, A.E. Statistical analyses for single-case experimental designs. In M. Hersen & D.H. Barlow (Eds.), Single-case experimental designs: Strategies for studying behavior change. New York: Pergamon Press, 1976.
- Kazdin, A.E. Drawing valid inferences from case studies. Journal of Consulting and Clinical Psychology, 1981, 49, 183-192.
- Kazdin, A.E. & Wilson, G.T. Evaluation of behavior therapy: issues, evidence and research strategies. Cambridge, Ma: Balinger, 1978.
- Lacey, J.I. Somatic response patterning and stress: Some revisions of the activation theory. In M.H. Apley & R. Trumbull (Eds.), Psychological stress. New York: Appleton-Century-Crofts, 1967.
- Lader, M.H. & Mathew, A.M. A physiological model of phobic anxiety and desensitization. Behaviour Research and Therapy, 1968, 6, 411-421.
- Lang, P.J. A bio-informational theory of emotional imagery. Psychophysiology, 1979, 16, 495-512.
- Lazarus, A.A. Behavior therapy and beyond. New York: McGraw-Hill, 1971.
- Lazarus, A.A. The practice of multimodal therapy. New York: McGraw-Hill, 1981.
- Lazarus, R.S. & Averill, J.R. Emotion and cognition: With special reference to anxiety. In C.D. Spielberger (Ed.), Anxiety: Current trends in theory and research, Volume 2. New York, Academic Press, 1972.
- Levy, S.M. Paradoxical intention as applied to blocking in mental work (Doctoral Dissertation, George Washington University, 1978) Dissertation Abstracts International. 1978, 39(1-B), 425. (University Microfilms No. 7810150).
- Luborsky, L., Singer, B., & Luborsky, L. Comparative studies of psychotherapies: Is it true that everyone has won and all must have prizes? Archives of General Psychiatry, 1975, 32, 995-1008.
- Mahoney, M.J. Experimental methods and outcome evaluation. Journal of Consulting and Clinical Psychology, 1978, 46, 660-672.

- Mandler, G. Helplessness: Theory and research in anxiety. In C.D. Spielberger (Ed.) Anxiety: Current trends in theory and research, Vol.2., New York: Academic Press, 1972.
- Marks, I.M. Fears and phobias, New York: Academic Press, 1969.
- Marks, I.M. Flooding (implosion) and allied treatments. In W.S. Agras (Ed.) Behavior modification: Principles and clinical applications, Boston: Little, Brown and Co. 1972.
- Marks, I. Care and cure of neuroses: Theory and practice of behavioral psychotherapy, New York: Wiley, 1981.
- Marshall, W.L., Gauthier, J., & Gordon A. The current status of flooding therapy. In M. Hersen, R.M. Eisler, & P.M. Miller (Eds.), Progress in behavior modification, Volume 7. New York: Academic Press, 1979.
- Mathews, A.M., Gelder, M.G., & Johnston, D.W. Agoraphobia: nature and treatment, New York: Guilford, 1981.
- Mavissakalian, M., & Barlow, D.H. Phobia: An overview. In M. Mavissakalian & D.H. Barlow (Eds.), Phobia: Psychological and pharmacological treatment, New York: Guilford, 1981.
- Miller, S.M. Controllability and human stress: Method, evidence and theory. Behaviour Research and Therapy, 1979, 17, 287-304.
- Miller, S.M., & Grant, R.P. The blunting hypothesis: A theory of predictability and human stress. In P.O. Sjoden, & S. Bates (Eds.), Trends in behavior therapy, New York: Academic, 1980.
- Mineka, S., & Kihlstrom, J.F. Unpredictable and uncontrollable events: A new perspective on experimental neurosis. Journal of Abnormal Psychology, 1978, 87, 256-271.
- Mischel, W. On the interface of cognition and personality: beyond the person-situation debate. American Psychologist, 1979, 34, 740-754.
- Modzierz, G., Macchitelli, F., & Lisechi, J. The paradox in psychotherapy: An adlerian perspective. Journal of Individual Psychology, 1976, 32, 169-184.
- Monroe, L.J. Psychological and physiological differences between good and poor sleepers. Journal of Abnormal Psychology, 1967, 72, 255-264.

- Nelson, M.C. On the therapeutic redirection of energy and affects. In M.C. Nelson, (Ed.), B. Nelson, M.H. Sherman, & H.S. Streat, Roles and paradigms in psychotherapy, New York: Grune & Stratton, 1968.
- Newton, J.R. Considerations for the psychotherapeutic technique of symptom scheduling. Psychotherapy: Theory, Research, and Practice, 1968, 5, 95-103.
- Nicassio, P., & Bootzin, R.R. A comparison of progressive relaxation and autogenic training as treatments for insomnia. Journal of Abnormal Psychology, 1974, 83, 253-260.
- Olsen, D. Empirically unbinding the double bind: Review of research and conceptual formulations. Family Process, 1972, 11, 69-94.
- Rabkin, R. Strategic psychotherapy: Brief and symptomatic treatment. New York: Basic, 1977.
- Raskin, D., & Klein, Z. Losing a symptom through keeping it: A review of paradoxical treatment techniques and rationale. Archives of General Psychiatry, 1976, 33, 548-555.
- Relinger, H., & Bornstein, P.H. Treatment of sleep onset insomnia by paradoxical intention: A multiple baseline design. Behavior Modification, 1979, 3, 203-222.
- Relinger, H., Bornstein, P.H., & Mungas, D.M. Treatment of insomnia by paradoxical intention: A time-series analysis. Behavior Therapy, 1978, 9, 955-959.
- Revusky, S.H. Some statistical treatments compatible with individual organism methodology. Journal of the Experimental Analysis of Behavior, 1967, 10, 319-330.
- Rosenthal, R. Experimental effects in behavioral research, New York: Appleton-Century-Crofts, 1966.
- Rotgers, F., & Franks, C.M. Review of Paradoxical Psychotherapy by G.R. Weekes & L.L'Abate. Child and Family Behavior Therapy, In Press.
- Roth, T., Kramer, M., & Lutz, T., The nature of insomnia: A descriptive summary of a sleep clinic population. Comprehensive Psychiatry, 1976, 17, 117-120.
- Seligman, M.P., Maier, S.F., & Solomon, R.L. Unpredictable and uncontrollable aversive events. In F.R. Brush (Ed.), Aversive conditioning and learning, New York: Academic Press, 1971.



- Selvini-Palazzoli, M., Boscolo, L., Cecchin, G., & Prata, G. Paradox and counterparadox: A new model in the therapy of the family in schizophrenic transaction. New York: Jason Aronson, 1978.
- Smith, M.L., & Glass, G.V. Meta-analysis of psychotherapy outcome studies. American Psychologist, 1977, 32, 752-760.
- Solyom, L., Garza-Parez, J., Ledwidge, B., & Solyom, C. Paradoxical intention in the treatment of obsessive thoughts: A pilot study. Comprehensive Psychiatry, 1972, 13, 291-297.
- Steinmark, S.W., & Borkovec, T.D. Active and placebo treatment effects on moderate insomnia under counterdemand and positive demand instructions. Journal of Abnormal Psychology, 1974, 83, 157-163.
- Storms, M.D., & Nisbett, R.E. Insomnia and the attribution process. Journal of Personality and Social Psychology, 1970, 16, 319-328.
- Thompson, S.C. Will it hurt less if I can control it? A complex answer to a simple question. Psychological Bulletin, 1981, 90, 89-101.
- Turner, R.M., & Ascher, L.M. Controlled comparison of progressive relaxation, stimulus control, and paradoxical intention therapies for insomnia. Journal of Consulting and Clinical Psychology, 1979, 47, 500-508.
- Turner, R.M., & Ascher, L.M. Therapist factor in the treatment of insomnia. Behaviour Research and Therapy, 1982, 20, 33-40.
- Victor, R.G. & Krug, C.M. Paradoxical intention in the treatment of compulsive gambling. American Journal of Psychotherapy, 1967, 21, 808-814.
- Wachtel, P.L. Psychoanalysis and behavior therapy. New York: Basic Books, 1977.
- Watzlawick, P., Beavin, J., & Jackson, D. Pragmatics of human communication. New York: W.W. Norton, 1967.
- Watzlawick, P., Weakland, J., & Fisch, R. Change: Principles of problem formation and problem resolution. New York: W.W. Norton, 1974.
- Weekes, G.R., & L'Abate, L. Paradoxical psychotherapy: theory and practice with individuals, couples and families. New York: Brunner/Mazel, 1982.

- Whitaker, C.A. Psychotherapy of the absurd: With a special emphasis on the psychotherapy of aggression. Family Process, 1975, 14, 1-16.
- Whitehead, A.N. & Russell, B. Principia mathematica, (2nd Ed.), Cambridge: Cambridge University Press, 1910.
- Wicklund, R.A. Freedom and reactance, Hillsdale, NJ: Lawrence Erlbaum Associates, 1974.
- Wilson, G.T. Toward specifying the "nonspecifics" in behavior therapy: A social learning analysis. In M.J. Mahoney (Ed.), Psychotherapy process: Current issues and future directions. New York: Plenum, 1980.
- Wilson, G.T. Fear reduction methods and the treatment of anxiety disorders. In C.M. Franks, G.T. Wilson, P.L. Kendall & K.D. Brownell Annual review of behavior therapy: Theory and practice. New York: Guilford, 1982 (a).
- Wilson, G.T. Psychotherapy process and procedure: The behavioral mandate. Behavior Therapy, 1982(b), 13, 291-312.
- Zeig, J. Symptom prescription techniques, clinical applications using elements of communication. American Journal of Clinical Hypnosis, 1980, 23, 23-33.

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