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Covariance Structure Analysis of Self-Efficacy and Participation in Chemical Dependence Treatment

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TABLE OF CONTENTS

Chapter Page

LIST OF TABLES

Table Page

Table Page

LIST OF FIGURES

Figure Page

ABSTRACT

This study investigated the effects of participation and self-efficacy on six-month outcome from inpatient Minnesota model chemical dependence treatment. The goal was to determine the extent to which effects on outcome that could be attributed to participation in treatment were mediated by self-efficacy. Covariance structure analyses showed that self-efficacy predicted relapse latency at the six-month follow-up, converging with similar findings from smoking cessation research. A supplementary model including both general participation and a more specific topic group dose measure showed that these factors produced significant but competing effects on self-efficacy, with opposing indirect effects on relapse latency that were completely mediated by self-efficacy. These results support the use of self-efficacy as a common metric for examining treatment effects.

CHAPTER 1

INTRODUCTION

Alcoholism treatment programs based on Minnesota model chemical dependence (MMCD) principles, still the dominant treatment modality in the United States today at the practice level (Kahle & White, 1991), postulate that alcoholism is a disease whose only legitimate treatment goal is abstinence (IOM, 1990). For this reason, MMCD treatment places its primary emphasis on instilling beliefs and skills that will facilitate and maintain abstinence. The goal of MMCD treatment can be seen as an attempt to inculcate both positive and negative outcome expectations regarding alcohol use: life-long abstinence from alcohol is stressed as the only "cure," and attention is directed to the belief that a return to drinking at any level will precipitate full-blown relapse. At the same time, both positive and negative efficacy expectations are also instilled: the individual must be convinced that mastery over drinking impulses can be attained, yet he or she must additionally accept the fact of powerlessness and inability to cope when faced with alcohol (Rollnick & Heather, 1982). In MMCD programs, these changes in expectations are accomplished by breaking down "denial," by providing a new normative reference group consisting of other recovering individuals, and by precipitating a "conversion experience" to a new belief system in which abstinence is paramount (Cook, 1988a).

Apart from a general lack of controlled studies of MMCD outcome (Cook, 1988b; Miller & Hester, 1986), three key problems plague MMCD research. The first of these is

the difficulty of operationalizing core constructs such as surrender of personal control to a "higher power," denial, or the conversion experience believed to be central to treatment success (Cook, 1988a; Marlatt, 1985; Miller, 1985; Miller & Rollnick, 1991; Morgenstern, Frey, McCrady, Labouvie, & Neighbors, 1996; Morgenstern & McCrady, 1992; Room, 1993). This situation has lead to attempts to frame MMCD processes in the language of expectations (cf., Rollnick & Heather, 1982, above), but this carries problems of its own. The expectancy construct has a venerable tradition in psychological research (Bandura, 1986; Bolles, 1975; Fishbein & Ajzen, 1975; MacCorquodale & Meehl, 1954; Maier & Seligman, 1976; Osgood, 1950; Rotter, 1954; 1966; Tolman, 1932), and has been widely used in studies of the etiology, maintenance and treatment of addiction (Stacy, Widaman & Marlatt, 1990). As a result, controversies in this area abound, ranging from the question of alcohol versus abstinence expectations, to debates about additive versus multiplicative combinations in expectancy-value models, or the factor structure of expectancy scales versus cluster models and spreading activation (Brown, 1985; Goldman, 1994; Leigh, 1989; Solomon & Annis, 1989; Young & Oei, 1993). Consequently, there appears to be no widely accepted set of expectancy measures upon which to base a comprehensive analysis of MMCD treatment processes.

This is compounded by a second problem, the question of motivation. The current view in addictions research seems to be that the utility of the confrontational approach employed in MMCD programs is mediated by patient motivation and compliance with treatment recommendations (Brownell, Marlatt, Lichtenstein & Wilson, 1986; IOM,

1990). In fact, motivation is widely believed by those from varying theoretical perspectives to be a key factor in recovery from alcohol-related problems, constituting one of the few consistent predictors of alcoholism treatment outcome (Fawcett, Clark, & Aagesen, 1987; Finney, Moos, & Chan, 1981; Fuller, Branchey, & Brightwell, 1986; Westermeyer & Neider, 1984). As Miller's (1985) cogent review of the subject points out, however, motivation has also been operationalized in various ways, including agreement with therapist, acquiescence to the sick role, expressed desire for help, subjective distress, compliant attitude, and apparent willingness to trust the therapist's judgment. An individual's stated willingness or intention to participate in treatment has been found to be unrelated to actual participation or outcome, and while therapist perceptions have been related to compliance with treatment and outcome, this could well be due to the self-fulfilling nature of therapist expectancies. Miller also notes that the typical trait-based approach to motivation tends to discourage intervention because it attributes motivation to stable internal sources.

A third problem surrounding MMCD research is closely related to the two previous issues. This is the use of "black box" evaluation designs, focusing primarily on the treatment program taken as a whole, or upon differences in comparison with other treatment modalities, rather than upon treatment processes and components (Moos, Finney, & Cronkite, 1990; Morgenstern & McCrady, 1992). By taking steps like assessing the motivational level and degree of participation of patients, process factors like these could be controlled in the examination of post-treatment drinking behavior and we could

determine whether or not this level of specificity enhances the prediction of outcome (Chen & Rossi, 1983). Yet attempts to incorporate process in MMCD evaluations are most often limited to specification of program setting or length of stay, and it is typically assumed that all patients within a program receive the same treatment. Research shows that this is not the case. Treatment experiences differ according to opportunities for choice (Kissen, Platz & Su, 1971), level of alcohol dependence and pattern of use (Simpson & Sells, 1983), pre-treatment history (Billings & Moos, 1984), and pre-existing expectations (Davies, 1981). Patients in the same programs, in varying degrees of withdrawal and recovery, selectively attend to different services (Becker & Jaffe, 1984), function at different levels early and late in treatment (Cernovsky, 1984), and get different amounts of staff attention and program resources (Berman, Meyer & Coates, 1984). As a result, we cannot identify the strength or integrity of treatment provided to the individual patient, much less the reasons for, or the effects of, this type of self-selectivity or differential component delivery (Moos, Finney & Cronkite, 1990). Process analysis is of particular importance in evaluating MMCD programs, since the long-term goal of abstinence, usually measured as the primary outcome criterion in evaluation studies, depends on the short-term goal of inculcating beliefs and converting the patient. Since this intermediate process of conversion is so hard to operationalize and is so closely related to the sticky question of motivation, it has been neglected in the design of MMCD research. As a result, a critical gap exists in this literature.

Calls to focus on these issues have led to significant progress in addressing methodological problems (Longabaugh, 1989), and recent attempts to identify the key components of various schools of treatment are also encouraging (e.g., McLellan, Alterman, Cacciola, Metzger & O'Brien, 1992; Moos, Finney & Cronkite, 1990; Morgenstern & McCrady, 1992). The problem with applying that strategy in this case is that MMCD treatment is intended as a multi-component approach, designed to incorporate promising elements drawn from various sources (IOM, 1990). Survey research confirms that this is the case among practitioners who espouse disease model tenets (McCrady, 1994), and it suggests that the MMCD model is "becoming a more complex treatment approach integrating the therapeutic aspects of other models" (Morgenstern & McCrady, 1992). This eclecticism has received post-hoc justification in light of recent evidence that common stages of change can be identified across addictive behaviors, and that different processes are utilized to best advantage by those at different stages (Prochaska & DiClemente, 1983). Those in contemplation or precontemplation stages, for example, may get the most benefit from a motivational intervention, which aids them in moving to the next stage, whereas those who have already progressed to the action or maintenance phase may be the ones who profit most from behavioral techniques and skills training (Prochaska, DiClemente & Norcross, 1992).

One promising avenue for MMCD research might be to view this type of treatment as an amalgam of motivational interventions (e.g., persuasion and modeling) and skills

training (performance) in the context of self-efficacy enhancement. Bandura introduced self-efficacy

> ...based on the principal assumption that psychological procedures, whatever their form, serve as a means of creating and strengthening expectations of personal efficacy.... By postulating a common mechanism of operation, this analysis provides a conceptual framework within which to study behavioral changes achieved by different modes of treatment (Bandura, 1977, pp. 193 & 195)

As described by Bandura (1977; 1986), self-efficacy is considered a central cognitive mediator of behavior, influencing behavioral choice, and determining the amount of effort expended in performance, and persistence in the face of difficulties. Since its introduction self-efficacy has been successfully applied in domains as diverse as sports performance (Feltz, 1982), vocational choice (Betz & Hackett, 1981), academic performance (Bandura & Schunk, 1981), social skills training (Lee, 1984), and treatment of phobias (Williams & Watson, 1985). In the past two decades, self-efficacy has become one of the most frequently cited terms in the social, clinical and counseling psychology literature (Maddux & Stanley, 1986).

Bandura (1977) identified four sources of self-efficacy information: performance accomplishments, vicarious experience (live or symbolic modeling), verbal persuasion (including interpretation and self-instruction), and visceral experience (e.g., emotional arousal). Performance is thought to be the strongest source of efficacy expectations

because it based on direct evidence of personal mastery, but each of the other sources, albeit weaker, is still a potentially significant contributing factor. Since MMCD treatment incorporates all four sources of efficacy information in its multi-modal package, MMCD treatment effects and key constructs like the intermediate goal of conversion could presumably be assessed in terms of changes in alcohol abstinence self-efficacy. This would do precisely what Bandura intended, putting behavioral treatments for alcohol problems and MMCD treatment in the same metric for study and comparison.

In fact self-efficacy has been widely used as a mediating concept in etiological theories of addictive behavior, and it has a demonstrated track record of success as a predictor of smoking cessation treatment outcome (Baer, Holt & Lichtenstein, 1986; Condiotte & Lichtenstein, 1981; DiClemente, 1981; DiClemente, Prochaska & Gilbertini, 1985; Velicer, DiClemente, Prochaska & Brandenburg, 1985). Unfortunately, only a few studies could be located in which self-efficacy has been examined in relation to alcoholism treatment (e.g., Annis & Davis, 1988; Burling, Reilly, Moltzen & Ziff, 1989; DiClemente, Carbonari, Montgomery & Hughes, 1994; Solomon & Annis, 1990), and in none of these cases was the focus of research MMCD treatment.

To remedy this, and to address some of the issues outlined above, the present study used self-efficacy in conjunction with self-reported levels of treatment participation to examine the extent to which effects on outcome that could be attributed to participation in MMCD treatment were mediated by self-efficacy at discharge.

CHAPTER 2

COMPETING THEORETICAL PARADIGMS

This investigation involves an analysis of MMCD treatment — which is based on a disease model of alcoholism — from the perspective of self-efficacy — a social learning theory principle. The disease model and the social learning approaches have been seen by some as competing theoretical paradigms (cf., McCrady, 1994; Morgenstern & McCrady, 1992). For this reason, a brief overview of the two perspectives will be presented.

The Disease Model

The emphasis in the disease model is on biological parameters, with a focus on the pharmacological effects of alcohol. It is believed that for some individuals a (presumably genetic) predisposition affects alcohol metabolism, such that consumption of alcohol inevitably leads to increased craving and loss-of-control drinking. This is seen as a lifelong condition, with no possibility of cure: the only solution is to maintain complete and total abstinence. This disease is assumed to be latent in the affected individual before the first drink is ever taken, to manifest itself in biological, psychological and social sequellae, and to remain in existence after drinking stops, regardless of the duration of abstinence. The disease model is usually extended to include the concept of "chemical dependence," the belief that addiction-prone individuals are vulnerable to other mood-altering substances as well as alcohol, but the core concept is loss of control. The disease model considers alcoholism to be "chronic, primary, and progressive," meaning that alcohol is the focus of

intervention, rather than merely a symptom of underlying problems, and that if left unchecked, the disease will follow a deteriorating course. From the point of view of this model, any consumption of alcohol after a period of sobriety indicates relapse (Jellinek, 1952; Laundergan, 1982; McCrady, 1994).

Since the cause of the disorder is irremediable, the main focus in treatment programs based on this model (e.g., MMCD treatment) is promulgating this perspective and "converting" the affected individual to the disease model point of view. Cook (1988a) lists four key elements of treatment: (a) attention to the possibility of change, (b) emphasis on the disease concept, (c) a goal of abstinence and improved lifestyle, and (d) participation in a twelve-step program such as Alcoholics Anonymous (AA). These elements encompass growth of a broader awareness, recognition and acceptance of choice and responsibility, and reconstruction of relationships. In this context, the treatment program provides opportunities to focus on change, an atmosphere conducive to and supportive of change, and peer-group counseling, information, and professional guidance.

A Model Based on Social Learning Principles

In recent years, the field has seen the emergence of a new model of the etiology and treatment of alcoholism based on cognitive psychology, social learning theory, and experimental social psychology (Marlatt, 1985). This approach uses an analysis of the expectations, attributions, and intentions believed to constitute the addictive behavior cycle to integrate information about relapse processes across substances.

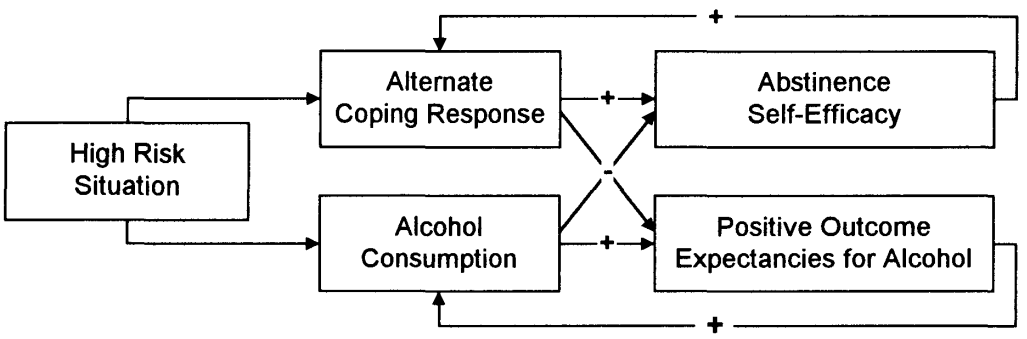
In this analysis, when faced with a difficult or challenging situation, the individual must initiate a coping response. Due to the accumulation of positive outcome expectancies for alcohol — which result from observational learning, as well as direct experience — the coping mechanism selected in response to stress is most likely to be alcohol consumption. Use of alcohol as a coping response is reinforced by its immediately gratifying effects (e.g., drinking calms anxiety), which further strengthens positive outcome expectations for drinking. Over time, initiation of drinking as a coping mechanism becomes a habit, an automatic response to stressful situations. Given this scenario, attempts to change the addictive behavior focus on bringing the habit back to the level of conscious awareness by identifying "high risk" situations (i.e., the stressful circumstances most likely to trigger alcohol use), and developing risk avoidance strategies and alternative coping mechanisms designed to replace alcohol consumption (Marlatt, 1985).

Following Bandura's (1977) analysis of self-efficacy, Marlatt (1985) outlines the events that comprise relapse to alcohol use after a period of abstinence. If the individual initiates an alternative coping response when faced with a high-risk situation, this increases self-efficacy to abstain from drinking, weakens positive outcome expectations for alcohol, and decreases the probability of relapse. However, if no alternative coping response is initiated, or if the coping response is inadequate or unsuccessful, abstinence self-efficacy decreases and the salience of positive outcome expectations for alcohol increases. This is likely to eventuate in a "lapse" or "slip," a retreat into an episode of drinking to cope. To

emphasize the parallel roles of self-efficacy and positive outcome expectancies in this model, the events constituting the relapse process are depicted schematically in Figure 1.

Figure 1

Schematic Depiction of the Relapse Process



Note. Adapted from "A cognitive-behavioral model of the relapse process," by G. A. Marlatt, 1985, in *Relapse prevention: Maintenance strategies in the treatment of addictive behaviors* (p. 38), G. A. Marlatt, & J. R. Gordon, Eds., 1985. NY: Guilford Press. Copyright 1985 by Guilford Press.

Given that a lapse has occurred, a second phase begins that Marlatt (1985) calls the abstinence violation effect. Here, attributions about the cause of the lapse are made. If attributed to internal factors, a negative reaction ensues from comparing immediate behavior (lapse) with ideal behavior (abstinence). The larger the discrepancy (dissonance), the greater are reactions of guilt and self-blame, and the greater the likelihood of either behavioral reactance (retreating to the addictive behavior) or cognitive reactance (redefining the self-image in line with the lapse). Either way, the probability of relapse increases as a result of internal attributions about the cause of the lapse. As Rollnick and

Heather (1982) point out, abstinence-oriented treatment stressing the uncontrollable nature of drinking (i.e., negative alcohol abstinence self-efficacy, or the loss-of-control tenet) may exacerbate this problem, setting the stage for a self-fulfilling prophecy.

Contrasts and Comparisons

One of the main benefits of the disease model is that it removes stigma by absolving the alcoholic from guilt or responsibility for the condition, which allows the person to seek help (Marlatt, 1985). The key facets of treatment are believed to have their effect by enabling the individual to "place trust outside the realm of conscious effort" and focus on the easier step of resisting temptation (Cook, 1988a). Self-help and mutual-aid elements like AA attendance and social reintegration within a community of recovering individuals serve to convert the alcoholic's social network from blame to support, which helps increase responsibility and participation. These elements comprise a well-developed ideology and provide a fixed community of belief, an action program, constructive activity toward shared goals, rewards for sobriety, and the living example of individuals who have remained sober as evidence that recovery is possible.

A central concept in social learning-based treatment, on the other hand, is the view that beliefs about the course of events play a significant role in determining the actual course of events. If any lapse is equivalent to failure, as taught in the disease model, the individual is considered more likely to relinquish efforts to control drinking behavior after a lapse has occurred and yield to what is perceived as "inevitable." By contrast, if a lapse is framed as a chance for new learning to occur, the individual is believed to be less likely

to abandon efforts to control behavior.

As noted by McCrady (1994), contrasts between the two paradigms are apparent. The language is different — the disease model focuses on a presumed disease entity, whereas the social learning approach addresses alcoholism as a learned habit. The disease model is based on a univariate view of the etiology of alcoholism, while in the behavioral model the emphasis is on a variety of internal, environmental, and interpersonal determinants of alcohol-related problems. Disease model proponents see exaggerated beliefs about personal control as part of the problem, as compared to the social learning approach, which tries to enhance mastery and personal control. Abstinence is considered the only appropriate treatment goal from the disease model perspective, whereas the social learning viewpoint focuses on a negotiated, rational choice of goals in which controlled drinking may be considered appropriate for some individuals.

As McCrady (1994) points out, given these different bases (e.g., mastery and self-control versus powerlessness and surrender), an integration at the level of theory seems unlikely, but several commonalities suggest that the two paradigms can be integrated at the level of practice. Both models place a strong emphasis on initial behavior change. Both prescribe courses of action, emphasizing alternate behaviors and activities incompatible with drinking. Both focus on identifying risk situations, and encourage recognition and modification of dysfunctional cognitions. Both stress the role of negative affect in creating high risk situations, and both emphasize the benefits of social networks that reinforce abstinence. Given different theoretical bases but many common elements and processes, a

strategy of matching patients to treatments — coordinating treatment philosophy with the personal characteristics of individuals — may lead to better outcomes overall.

The social psychological relapse prevention approach outlined by Marlatt (1985) and Annis (1986) achieves an admirable and easily-understood integration of self-efficacy and outcome expectancy constructs, and has received a great deal of empirical support, in terms of both its individual components (Baer, Holt, & Lichtenstein, 1986; Condiotte & Lichtenstein, 1981; Curry, Marlatt, & Gordon, 1987; DiClemente, 1986; Harackiewicz, Sansone, Blair, Epstein, & Manderlink, 1987; Mann, Chassin, & Sher, 1987), and in terms of the program as a whole (Annis, 1986; Cooper, Russell, & George, 1988; Velicer, DiClemente, Rossi, & Prochaska, 1990). In contrast, outcome evaluations of treatment programs based on the disease model have yielded equivocal results and been criticized for methodological shortcomings by a number of authors (e.g., Cook, 1988b; Emrick & Hansen, 1983; Holder, Longabaugh, Miller, & Rubonis, 1991; Miller & Hester, 1986; Moos, Finney, & Cronkite, 1990). However, MMCD treatment is intended as a multi-component approach designed to incorporate promising elements drawn from various sources (IOM, 1990), and this includes elements of social learning theory. Recent survey research confirms that such principles are being employed by practitioners who espouse disease model tenets (McCrady, 1994; Morgenstern & McCrady, 1992), suggesting that the two models are neither mutually exclusive nor necessarily incompatible. This supports the use of social learning concepts like self-efficacy as a means of evaluating MMCD treatment.

CHAPTER 3

EMPIRICAL INVESTIGATIONS

Self-efficacy has often been studied in smoking cessation programs, but much less frequently in the context of treatment for alcoholism and eating disorders (DiClemente, 1986; Stephens, Wertz, & Roffman, 1995). In fact, only a small number of studies using self-efficacy in the context of alcoholism treatment outcome could be found. These studies and several of the smoking investigations will be described here.

Self-Efficacy and Smoking Cessation

In one of the earliest reports applying the self-efficacy construct to smoking, DiClemente (1981) compared three different smoking cessation procedures, measuring self-efficacy in 63 individuals one month after quitting and abstinence five months later. None of the demographic or smoking history variables predicted abstinence at follow-up, but self-efficacy did. There were significant correlations between self-efficacy and both weeks of successful abstinence and self-reports of difficulty in maintaining abstinence. Since all subjects were abstinent at the time of the self-efficacy assessment, outcome differences were not attributable to differences in the ability to achieve abstinence. DiClemente concluded that efficacy appeared to be highly positively related to the ability to maintain smoking abstinence, even though the range of the efficacy measure appeared to be restricted. Further, he suggested that his results supported the view that self-efficacy was superior to past behavior as a predictor of subsequent abstinence rates.

Condiotte and Lichtenstein (1981) conducted one of the most widely-cited self-efficacy studies in the addiction field. They collected self-efficacy ratings from 78 participants in two smoking cessation programs both before treatment and at the end of treatment, along with follow-ups at five weeks, eight weeks and 12 weeks after termination. Some subjects were also asked to monitor smoking behavior, mood states, and self-efficacy on a daily basis during the first five weeks after treatment. These investigators found that treatment significantly enhanced self-efficacy in both programs and that self-efficacy continued to increase during the follow-up period. Using multiple regression to predict abstinence status and time to first use, they were able to account for 32% of the variance in smoking status at the initial follow-up, and 48% of the variance in time to first use. Further, they found a high degree of correspondence between the lowest self-efficacy scores and the circumstances surrounding the first relapse to smoking. On the basis of these results, Condiotte and Lichtenstein concluded that there was indeed a strong inverse relationship between abstinence self-efficacy and smoking behavior at follow-up.

A large-scale study of 957 smokers conducted by DiClemente et al. (1985) showed that, as expected, smokers in earlier stages of change (i.e., contemplation or pre-contemplation stages) had lower levels of smoking abstinence self-efficacy, while those who had already quit (i.e., those in the later action or maintenance stages of change) showed higher levels of abstinence self-efficacy. Similar to the results reported by DiClemente (1981), efficacy was found to be superior to past performance as a predictor of smoking behavior in this study. The investigators concluded that self-efficacy reliably

discriminated between smoking status categories, and that it was significantly correlated with specific activities related to cessation and maintenance.

Velicer, DiClemente, Prochaska, and Brandenburg (1985) conducted a smoking cessation study using 116 subjects which demonstrated that scores on their pros of smoking scale (which measured positive outcome expectancies for smoking) were high for those in pre-contemplation, contemplation, and relapse stages of smoking change, but low for those in the action and maintenance stages. Slightly different results were found for the cons of smoking (negative outcome expectancies): those in pre-contemplation and maintenance phases showed low con scores, whereas cons were highest for those in the contemplation and action stages, a finding attributed to the lower salience of smoking cues in general for those in the maintenance phase. More relevant here, self-efficacy was found to be one of the best predictors of smoking outcome at six-month follow-up.

Baer, Holt, and Lichtenstein (1986) conducted a somewhat more methodologically rigorous investigation of self-efficacy with 146 participants in several smoking cessation programs. They attempted to address several questions about self-efficacy measurement, as well as to examine competing models of self-efficacy effects. They found that the self-efficacy measure they used was primarily unidimensional in nature, that there were low to moderate positive correlations with past behavior, and that self-efficacy was unrelated to pre-treatment motivation but significantly positively related to pre-treatment levels of social support. In terms of outcome, their results showed significant positive correlations between self-efficacy and abstinence status, and negative correlations between self-efficacy

and rate of smoking at the one-, two-, three- and six-month follow-ups, although these results were not significant when the sample was restricted to those who had attained abstinence by the end of treatment. They also demonstrated that self-efficacy at earlier follow-up points was highly correlated with self-efficacy at later points. Baer et al. concluded that self-efficacy has incremental predictive validity for smoking rate but not for smoking status, and that a model in which both self-efficacy and smoking status made independent contributions to subsequent smoking status was supported best by their data.

Self-Efficacy and Alcohol Treatment

Burling, Reilly, Moltzen, and Ziff (1989), citing a lack of self-efficacy research in relation to treatment for drug- and alcohol-related problems, set out to examine a series of questions with a group of 419 alcohol and other drug abusers in a therapeutic community treatment setting. Using a modified version of Annis's (1986) Situational Confidence Questionnaire (SCQ), they wanted to determine: (a) if there were baseline differences in self-efficacy between alcoholics and drug users, (b) if self-efficacy increased during treatment, (c) if those with high self-efficacy at discharge were less likely to relapse, (d) if there were self-efficacy differences between abstinent versus relapsed groups at any of several monthly follow-up points, and (e) if those who eventually relapsed had correctly identified the circumstances surrounding the relapse in ratings made during treatment.

Although 419 subjects were admitted to the study, only 56 could be followed after discharge. Results showed that: (a) there were no differences between alcohol and other drug users at baseline, (b) there were significant increases in SCQ scores from baseline to

discharge, (c) abstinent and relapse groups did not show differences in self-efficacy at discharge, (d) those who abstained did show higher follow-up self-efficacy scores than those who had relapsed, and (e) more than half of those who relapsed had correctly identified the circumstances surrounding their relapse. Although Burling et al. questioned the reliability of the self-efficacy ratings given by their patients, their results do not conflict with research in smoking cessation, and it seems equally likely that other methodological factors — such as the low percentage of follow-ups successfully completed over a six-month period — may have created problems in their study.

Solomon and Annis (1990) looked at the relationship between self-efficacy and outcome expectancies for 100 men in residential alcoholism treatment and at a three-month follow-up. They were particularly interested in comparing self-efficacy as measured by the SCQ to the predictive validity of their outcome expectancy scales, which were constructed in a way similar to the pros and cons scales of Velicer et al. (1990). These investigators collected both types of expectancy measures at intake and again at three months after discharge, comparing them to measures of the quantity and frequency of alcohol consumption at follow-up. They found that SCQ scores improved from baseline to follow-up, while the outcome expectancy measures did not. Overall, they found no relationship between their follow-up measures and either outcome expectancy or self-efficacy, but when restricted to those who drank at follow-up, the SCQ was found to account for 16% of the variance in quantity/frequency. Comparing three models of self-efficacy and outcome expectancy effects, they rejected a multiplicative combinatorial

model and a model of parallel influence, concluding that their data best supported a model in which outcome expectancies were positively correlated with self-efficacy but did not provide incremental predictive power.

Examining the stages of change model (Prochaska & DiClemente, 1982) in relation to attendance at Alcoholics Anonymous (AA), Snow, Prochaska, and Rossi (1994) questioned 191 abstinent individuals who had once had drinking problems. They wanted to assess processes of change, compare self-changers to those who attended AA, and examine whether or not levels of AA involvement were associated with distinct patterns of personal characteristics. In addition to other measures, participants completed a self-efficacy scale sent to them by mail. Results showed no differences in self-efficacy levels by categories of AA attendance, nor were differences in self-efficacy found for those at various stages of change, even when AA attendance was used as a covariate. It should be noted, however, that this was an entirely retrospective study in which the sample was restricted to those who had been abstinent for some time.

Finally, DiClemente, Carbonari, Montgomery, and Hughes (1994), in an attempt to validate their newly-created alcohol abstinence self-efficacy scale, compared several self-efficacy factor models using the self-reports of 266 outpatient alcoholics. They found that a four-factor solution fit their instrument best, demonstrating good reliability and discriminant validity, although no measures of predictive validity were reported.

Hypotheses of the Present Study

The paucity of self-efficacy applications in the area of treatment for alcohol and drugs other than nicotine should be apparent from this brief review. This situation is somewhat surprising given the central role this construct is thought to play in models such as the stages of change (Prochaska & DiClemente, 1982) and relapse prevention (Annis, 1986; Marlatt, 1985) approaches. Clearly, additional work in this area is called for, and the present study was designed in part to address this issue.

In the model proposed here, MMCD treatment is seen as an amalgam of motivational interventions, enforced abstinence, and skills training that affects outcome after discharge primarily by enhancing alcohol abstinence self-efficacy. Based on this view of MMCD treatment, it was hypothesized that to the extent that the individual participates in the treatment process, he or she would be exposed to treatment components (e.g., persuasion, models, cognitive elaboration, visceral reactions, skills training) that increase self-efficacy to abstain from alcohol/drug use. Given these two intermediate processes (i.e., participation and self-efficacy change), it was predicted that increased abstinence self-efficacy (confidence) would result in persistence in alternative coping behaviors and enhancement strategies after discharge. In other words, higher abstinence self-efficacy at discharge was expected to forestall a return to alcohol use, which would be manifested as increased relapse latency — i.e., longer time to first use after discharge from treatment.

CHAPTER 4

METHOD

Overview of the Proposed Model

The hypothesized model includes both a measurement model and a path model. The measurement model constitutes a set of confirmatory factor analyses for three of the four data collection panels (i.e., baseline, process, and discharge). The path model consists of two interlocking mediation analyses involving the following questions: (a) Does participation in treatment increase self-efficacy at discharge, controlling for prior levels of risk? and (b) To what extent does self-efficacy at discharge mediate the effect of participation during treatment on outcome after discharge?

The measurement model. Based on previous research (cf., Cooper, Frone, Russell, & Mudar, 1995; Parrella, 1996a, 1996b), risk of use — the first data collection panel, corresponding to baseline — was conceptualized in this study as a second-order factor underlying two types of positive outcome expectancies motivating alcohol use: coping expectancies and enhancement expectancies. Coping is defined here as use of alcohol to escape, avoid, or otherwise regulate negative emotional, interpersonal, and physical states, while enhancement is defined as alcohol use in the service of increasing positive emotional or social experience. Based on an assessment of the frequency of alcohol use in coping and enhancement situations in the six months prior to treatment entry, risk was considered an

index of the degree to which positive outcome expectancies drove pre-treatment alcohol consumption.

Since abstinence-oriented MMCD treatment programs are thought to have their effects by engaging patients in a self-directed change process under the guidance of counselors and peer role models, participation — the second data collection panel, corresponding to treatment process — was conceptualized here as consisting of involvement in the daily activities of the program, paying attention to educational materials such as lectures and films, and contributing during effortful activities and insight-oriented groups. Under the assumption that the active ingredients of MMCD treatment (i.e., elements corresponding to Bandura's (1977) four sources of self-efficacy information) are stochastically distributed among the various activities, materials and experiences encountered during the treatment stay, participation served in this study as a control for patient motivation, compliance, and within-treatment self-selectivity.

Alcohol abstinence self-efficacy, or confidence — the third data collection panel, corresponding to discharge — was, like risk, considered to be a second-order factor underlying abstinence self-efficacy in coping and enhancement situations. Since the referents for these two constructs (risk and confidence) are exhaustive and mutually exclusive behavioral alternatives (i.e., use versus abstinence), risk and confidence were conceptualized as reciprocally related: high risk of use implies low confidence to abstain from use, and vice versa. Just as risk — i.e., expectancies about the value of alcohol use as a method of coping with negative experience or enhancing positive experience — drives

consumption and indexes the likelihood of continued drinking, confidence (self-efficacy to abstain from alcohol use in coping and enhancement situations) is seen as driving abstinence, making it useful as a predictor of the likelihood of continued abstinence. Like participation, which was used to operationalize motivation and the self-selective treatment implementation that presumably results, confidence was used to operationalize a second factor critical to assessment of MMCD effects, the extent to which the "conversion" process believed to mediate treatment outcome occurred. Since risk and confidence were considered to be reciprocally related, these concepts were treated as negatively correlated indices of self-efficacy to abstain from substance use. Furthermore, since the instruments used to collect risk and confidence self-assessments were congeneric measures with parallel scale structures, correlated errors of measurement were anticipated.

Outcome, the fourth and final data collection panel, was defined primarily as the length of time to first use of alcohol or drugs after discharge. Research suggests that about two-thirds of all those who return to alcohol or drug use after treatment do so within the first six months after discharge (cf., Hunt, Barnett, & Branch, 1971). In this study, time to first use (latency) was defined as the number of weeks that elapsed within the six-month follow-up span before the use of alcohol or drugs first occurred. Since latency was measured with a single observed indicator in this study (i.e., self-report of time to first use at follow-up), a sensitivity analysis (cf., Marsh, 1990) was employed, rather than a confirmatory factor analysis, for this data collection panel.

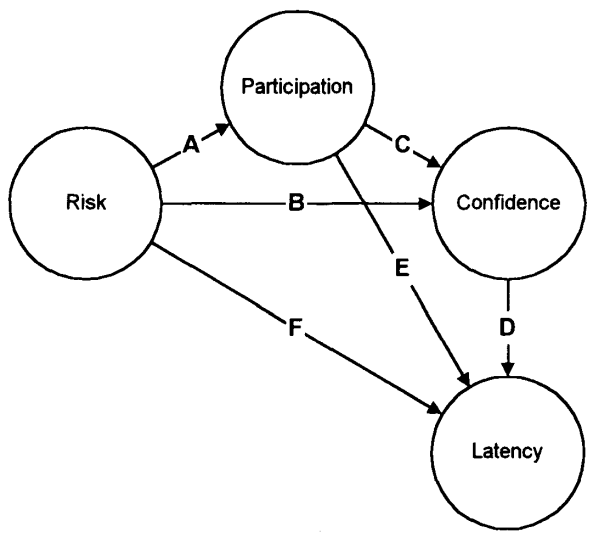
The path model. The pair of interlocking mediational processes hypothesized to transmit MMCD treatment effects are described below. Figure 2 depicts the fully saturated path model on which this chain of transmission was based, with direct paths labeled for reference in the discussion that follows. The first mediational analysis is represented by paths A, B, and C, constituting, respectively: (a) the effect of prior risk on participation in treatment; (b) the effect of risk on confidence, controlling for participation; and (c) the effect of participation in treatment on confidence at discharge, controlling for prior risk. Path A in this model indicates the extent to which positive expectations of use affect participation in treatment. It was hypothesized that this effect would be negative. Path B represents the stability of self-efficacy over time. Since risk and confidence were postulated to be reciprocally related, it was hypothesized that path B would also be negative. Path C, representing the effect of participation in MMCD treatment on self-efficacy at discharge, was predicted to be positive. Taken together, paths A, B, and C addressed the question "Does participation in treatment influence self-efficacy at discharge once the effects of prior risk have been controlled?"

Paths C, D, and E constitute the second set of mediational pathways. As noted above, path C, the direct effect of participation in treatment on self-efficacy at discharge, was predicted to be positive. Path D assesses the degree to which abstinence self-efficacy predicts the latency of subsequent alcohol/drug use. Since this has been demonstrated in prior smoking cessation research, it was predicted that this path would have a positive coefficient. Path E, representing the effect of participation on outcome not mediated by

self-efficacy, was also hypothesized to be positive. Taken together, paths C, D, and E addressed the question "To what extent are effects on outcome that can be attributed to treatment participation mediated by self-efficacy?"

Figure 2

Saturated Latent Variable Path Model



Note. Circles indicate latent variables and arrows represent causal pathways. Letters A - F refer to direct effects discussed in the text. Risk = risk of use; Participation = participation in treatment; Confidence = abstinence self-efficacy; Latency = time to first use.

The final direct path contained in the saturated model, path F, is the residual effect of risk on relapse latency, controlling for participation and self-efficacy. Like other paths emanating from risk, this effect was predicted to be negative. Two total effects, not labeled in Figure 2, were also of interest. These were: (g) the total effect of participation on outcome (i.e., the direct effect in path E, plus the indirect effect represented by the

product of paths C and D); and (h) the total effect of risk on outcome (i.e., the direct effect of path F plus the indirect effects represented by A*C*D, A*E, and B*D).

Subjects

Subjects were 109 men and women who had been admitted as inpatients to a large Midwestern substance abuse hospital for treatment of their alcohol abuse (and, in some cases, for the abuse of other drugs as well as alcohol). Study subjects were all volunteers who participated in a larger study of treatment process issues (see Procedures, below). All volunteers for the larger study were included in the present sample, providing they met the following conditions: (a) they reported use of alcohol in the month prior to treatment, (b) there was no evidence of coexisting eating disorders, and (c) they completed all four of the instruments used in this study (see Measures, below). The first two conditions were employed to reduce the heterogeneity of the sample and ensure significant alcohol involvement on the part of study participants. The third condition was required because the main focus of the present study was an analysis of the covariation of self-efficacy, participation in treatment, and outcome at six months after discharge.

The 109 individuals who met these conditions (71 men, 65%, and 38 women, 35%) ranged in age from 20 to 74 ($M = 39.7$, $SD = 12.4$); twenty-five percent (25%) were under 30, and 27% were over 45. Most (98%) were white, 42% were married, and 25% were living alone at the time of admission. Eighty-five percent (85%) of subjects were high school graduates, and 60% were employed full time. This was the first treatment

episode for 54%, and 29% reported a positive family history of alcohol and/or other drug abuse treatment.

Procedures

All data used in the present study were obtained from an archival data set derived from a two-year study of alcoholism treatment, the Treatment Process Study (TPS; Parrella, Filstead & Ross, 1993), which was funded by the National Institute on Alcohol Abuse and Alcoholism (NIAAA; Grant No. 08455). The principal investigator for the TPS granted the present author access to selected portions of the TPS data set for the purpose of conducting the research described here.

Overview of the TPS. The TPS was designed to operationalize the components and processes of treatment in a standard, abstinence-oriented, inpatient treatment program patterned after the Minnesota model (Cook, 1988a, 1988b). The overall aim of the TPS was to construct a detailed "map" of treatment processes in terms of the activities and services constituting treatment on a day-to-day basis. The main goals of the study were to examine whether or not all patients got the same types and amounts of services, and to explore perceptions, motivation, effort, and mood changes during the program. The TPS included measures of pre-treatment expectations, differential service utilization during treatment, perceived program difficulty, and the impact of individual service components. These data were collected in face-to-face interviews and self-report questionnaires filled out at baseline and discharge, and by means of a self-monitoring workbook (the Treatment

Experience Workbook, TEW) that patients filled out each night during treatment at the end of the day before they went to bed.

Treatment program characteristics and operation. The treatment facility at which the TPS was conducted was a licensed, 104-bed substance abuse hospital admitting about 1800 patients a year. During the TPS, approximately forty percent (40%) of admissions were referred out to other programs or services after a 3-5 day detoxification period. Most of those who remained stayed from 2-4 weeks and received care in an abstinence-oriented, educational MMCD program predicated on the disease model of addiction and the 12-step self-help philosophy of Alcoholics Anonymous. Treatment services offered at this site included focus groups, lectures, films, medical care, psychiatric management, medication, recreational activities, self-help literature, individual counseling, group and family therapy, employer conferences, and self-help group meetings.

TPS procedures. Within 24 hours of admission, all new patients were solicited for participation in the TPS. A short video was shown on the detoxification and assessment units describing the purposes and procedures of the study. The principal investigator for the TPS attended these sessions and answered questions raised by patients. Those who agreed to participate were asked to read and sign a consent form and provide additional information to enable follow-up after discharge. Then, over the course of the next two days, study participants were interviewed and asked to complete several questionnaires in addition to the standard battery of tests obtained for clinical use.

During the initial testing session, study participants were given the first of a series of workbooks (TEW) containing forms for daily self-monitoring. They were instructed in the use of the workbook in face-to-face interviews and asked to go through a "dry run" in which they reconstructed the previous day's activities and experiences. Subjects returned to the research center on the second day to complete the remaining TPS instruments and review the first day of *in situ* use of the workbook. Although invited to return to the research center at any time, especially if they had questions, subjects were scheduled to return once each week to get a new workbook, review the one they turned in, and clarify questions about program activities or completion of workbook sheets. Just before discharge, they came to the research center again to turn in their final workbook and complete retest versions of baseline instruments.

At the scheduled six-month follow-up, those who gave consent for follow-up were called by interviewers in the attempt to locate them and conduct a telephone interview. The study protocol called for 15 phone call attempts per subject, spread over various times of the day and various days of the week. If phone contacts were unsuccessful, two attempts were made to contact subjects through the mail. A span of one month after the scheduled follow-up date was allowed for contact. If phone and mail contact attempts failed, subjects were coded as "lost." If contacts succeeded but subjects refused participation, they were dropped from the follow-up and coded as "refusers." If subjects consented to be interviewed, a 15-to-30 minute structured follow-up interview was conducted over the phone.

Interviews were conducted by trained interviewers using structured interview guides. Questions were framed concerning the six-month period "since discharge." Subjects were assured of their confidentiality, asked to confirm the collateral information provided on the consent form signed during treatment, and permission was requested to contact collaterals. Subjects were then guided through a self-assessment of compliance with discharge recommendations, self-help involvement, contact with treatment agencies, and incidents of alcohol and drug use, including multiple substances and patterns of use. If the subject was part of the group randomly selected for an interrater reliability substudy, he or she was also asked to consent to be re-interviewed.

Additional procedures for the present study. To obtain data for the present study from the TPS archive, administrative records were used to determine which subjects met the three conditions outlined above (viz., alcohol use, no concurrent eating disorders, and presence of the instruments used in this study). In order to protect the confidentiality of TPS participants, all information used to identify individual subjects was removed from the portion of the TPS data base used in this project.

Measures

Demographic and clinical variables. Demographic and clinical information was taken from the TPS administrative record and from the Alcohol and Substance Abuse Questionnaire (ASAQ; Parrella, Filstead & Ross, 1990), a 16-section paper-and-pencil self-report instrument. The ASAQ provides an overview of demographics, the historical development of alcohol and substance use patterns, prior treatment history and family

history of treatment, quantity and frequency measures for alcohol and other drugs, and psychosocial, physiological, and behavioral measures of consequences due to alcohol and other drug use. For this study, only the sample descriptors listed under Subjects, above, were abstracted.

Panel 1: Risk. Utilizing Bandura's (1977, 1986) explication of self-efficacy and Marlatt's (1985) analysis of the relapse process, Annis (1986) proposed a relapse prevention model for treatment of alcoholics based on the idea that treatment will be effective to the extent it increases self-efficacy to abstain from alcohol use in alcohol-related situations in the natural environment. To provide a target for efforts directed at improving self-efficacy, this approach begins with a highly detailed microanalysis of risk situations. The result of this microanalysis constitutes a classic functional analysis (Bootzin, 1975) of drinking behavior, generating an individualized hierarchy of risk areas for use in treatment planning and revealing the strength of alcohol outcome expectancies on the basis of situation-specific assessments of habit strength or risk of use.

The 100-item questionnaire used to conduct this functional microanalysis, the Inventory of Drinking Situations (IDS), was designed to assess situations in which the client drank heavily over the past year. Annis's (1986) development of the IDS drew upon several sources, including discussions with clinicians and alcoholics. As recommended by Bandura (1977) and others (e.g., Ajzen & Fishbein, 1977), IDS risk assessments are made with respect to highly specific situations. The final set of items comprises eight categories, divided into two major classes: intrapersonal states, where drinking occurs in response to

an event that is primarily psychological or physical in nature; and interpersonal states, where a significant influence of another individual is involved. The five subscales classified as intrapersonal are: negative emotional states (NES); negative physical states (NPS); positive emotional states (PES); testing personal control (TPC); and urges/temptations (U/T). The three interpersonal subscales are: interpersonal conflict (IPC), social pressure to drink (SP), and positive social situations (PSS). This classification scheme was derived largely from Marlatt's (1985) content analysis of interviews with chronic male alcoholics about the circumstances surrounding their first relapse after alcoholism treatment.

The variant of this questionnaire used in the present study, the Inventory of Drinking and Drug Situations (IDDS), was a modification of the instrument developed by Annis (1986), used with her permission. The IDDS content was identical to the IDS, except that the instructions and individual items were modified to refer to situations of heavy drinking and/or drug use. The IDDS used in this study was administered as a paper-and-pencil self-report, in which the individual indicated the frequency with which he/she drank or used drugs heavily over the past year, for each of 100 situations, using a four-point rating scale (1 = *never*, 2 = *rarely*, 3 = *often*, and 4 = *almost always*). The eight subscale scores corresponding to the categories listed above were obtained by adding responses within each category. These subscales can be used in raw form, or they can be converted to problem indices, calculated as percentages of maximum scale value. The problem index scores, each ranging from 0 (no risk) to 100 (high risk), were used in the present investigation, and two of the subscales, TPC and U/T, were omitted from the

analysis, since preliminary work suggested a higher degree of factor complexity for these scales than for the remaining six (cf. Parrella, 1996a).

Panel 2: Participation. Participation scores were derived from TEW sheets. Since abstinence-oriented MMCD treatment involves engaging patients in a self-directed change process under the guidance of counselors and peer role models, participation was conceptualized as a multidimensional variable consisting of participating in the daily activities of the treatment program, paying attention to educational materials such as lectures and films, and contributing during insight-oriented group activities. The items comprising the participation index are listed in Table 1. Each item was answered on a six-point scale anchored with the labels 1 = *not at all* and 6 = *very* or *very much*.

Table 1

Participation Items

How much effort did you put into treatment today overall?
How hard did you work today?
Were you interested and paying attention?
Were you motivated?
How much did you participate in today's treatment activities?
Did you contribute when you were in groups?

Although a participation score could, in theory, have been calculated for each day of inpatient treatment, program activities were concentrated on weekdays, so attention was restricted to TEW sheets completed Monday through Friday. Furthermore, since

individual patients within this sample remained in treatment for varying lengths of time, summary participation scores computed across the full length of stay might have unfairly weighted the scores of some subjects over others. Thus, the approach taken here involved computing mean participation item values for all valid non-weekend sheets completed during the first five weekdays in active treatment (i.e., the first five non-weekend days after the period of detoxification). Besides equating subjects in terms of the span over which participation was calculated, this procedure was considered advantageous in that it represents an assessment of participation early in treatment, during a critical phase of program activities. To examine the effect of this computational procedure on study findings, results were compared to those obtained using participation scores computed from all non-weekend sheets across the full length of stay, and length of stay itself (measured in days) was examined as an alternative means of operationalizing participation.

Panel 3: Confidence. Annis's (1986) Situational Confidence Questionnaire (SCQ) was developed as a companion instrument to the IDS, containing 100 parallel items used to assess Bandura's (1977) concept of self-efficacy with regard to the perceived ability to cope with alcohol. Individuals are instructed to imagine themselves in each situation and to rate how confident they are that they would be able to resist the urge to drink heavily in that situation. SCQ responses are given on a six-point "percent confident" rating scale (0, 20, 40, 60 , 80, 100, where 0% and 100% are anchored with the labels 0 = *not at all confident* and 100 = *very confident*). SCQ scales are organized into the same eight areas as the IDS (i.e., NES, NPS, PES, TPC, U/T, IPC, SP, and PSS), but following Bandura

(1977), three types of scores can be computed for each category: (a) self-efficacy level, defined as the number of items with ratings of 60% or higher; (b) self-efficacy strength, defined as the sum of items within the scale; and (c) self-efficacy generality, calculated as the correlation of the strength score across categories.

The SCQ variant employed in the present study was a modification of Annis's (1986) instrument, used with her permission. The SCQ used here was identical to Annis's version in content, except that instructions and individual items were modified to refer to situations of heavy drinking and/or drug use. In addition, because the treatment program was abstinence-oriented, Annis's instructions were also modified to read "resist the urge to drink or use drugs," instead of "resist the urge to drink heavily." In this investigation, self-efficacy strength scores were used, and these were converted to the same 0-100 problem index metric applied to the IDDS. As with the IDDS, two SCQ subscales (TPC and U/T) were excluded from analyses, since preliminary work suggested a higher degree of factor complexity for these scales than for the remaining six (Parrella, 1986b).

Panel 4: Outcome. Two measures of treatment outcome were taken directly from self-reports on the six-month follow-up interviews: relapse status and relapse latency. Both outcome measures were used as a gauge of persistence in efforts to abstain from alcohol and drug use. The status variable indicated whether or not the individual reported any drinking or drug use after discharge, and was coded simply as 0 (abstinence) or 1 (substance use). Relapse latency was defined as the amount of time, in weeks, between discharge and the first use of alcohol and/or other drugs after discharge. To control for

minor variability in the time span over which six-month follow-up data were collected, latency was converted to a 0-100% metric reflecting the amount of the follow-up time span that elapsed before first use, with shorter latency values representing earlier time to first use. All abstinent subjects were assigned a value of 100% for latency, indicating the maximum possible time to first use (i.e., since abstinent individuals did not use alcohol or drugs, post-discharge substance use remained entirely latent). As a check on this coding procedure, all analyses were repeated using raw latency scores (i.e., time to first use in weeks), yielding essentially identical results.

Supplementary measures. One other workbook variable, the mean number of topic groups attended per day during the first week of treatment (NTOPIC), was also included in this study for the purpose of conducting supplementary analyses. Six topic groups were included in this count: (a) seminar, (b) bridge group, (c) compulsive behavior focus group, (d) leisure counseling, (e) relapse focus group, and (f) spiritual growth. For each daily workbook sheet, study participants indicated whether or not they attended each group, and the daily topic score was a simple count of the number of groups attended that day. The value used in this study — NTOPIC, the mean number of daily topic groups in the first week of treatment — was considered an alternative measure of treatment exposure, comprising that set of optional treatment-related discussion groups using more highly personalized and individualized methods, such as role-playing, to focus on recovery-related issues. Like participation, the NTOPIC score was calculated as a mean of the daily topic scores from all valid non-weekend sheets completed during the first five weekdays in

active treatment, but this procedure yielded a single NTOPIC value, rather than a set of six items.

Data Analyses

Preliminaries. All analyses were conducted with the SPSS implementation of LISREL 7.2 (Jöreskog & Sörbom, 1989). Prior to formal analyses, all data were assessed in terms of distributional properties, particularly deviations from normality and excessive kurtosis, which affect the suitability of variables for inclusion in covariance structure analysis. Maximum likelihood estimation was used to derive all factor and path models, and a per-comparison alpha level of .05 was used within each analysis block to assess statistical significance for likelihood ratio (LR) step tests and model chi-square values (see below). Given the relatively small number of subjects, no alpha adjustment was applied to correct for the number of tests performed, since power to detect small effects given the size of the sample was low. All conclusions were also assessed at the .10 alpha level, and differences found are noted. Although covariance matrices were used as input for all analyses, completely standardized parameter estimates are reported here for ease of interpretation.

Assessment criteria. The usual logic of significance testing is reversed in evaluating the overall fit of covariance structure models. That is, a non-significant chi-square value indicates that it is reasonable to accept the hypothesis that the constraints imposed by the model are valid. Since the chi-square is an omnibus test of the hypothesis that the residuals (the differences between the observed and model-implied parameters) do not differ

significantly from zero, non-significance indicates that the proposed model represents a plausible account of the processes that generated the observed data (Bollen, 1989). However, this use of the chi-square statistic has disadvantages that make assessing goodness-of-fit problematic. In particular: (a) it depends on a number of assumptions that are unlikely to be satisfied in practice (Bollen, 1989); and (b) chi-square values increase with increasing sample size, so a model may be rejected in large samples even if it represents the data well, while insufficient power may lead to inflated Type II error in small samples (Tanaka, 1993).

Several investigators have called for a deemphasis on the dichotomous decision strategy epitomized by the classical hypothesis testing approach and a greater emphasis on measures of comparative fit in evaluating covariance structure models (e.g., Bentler & Bonnett, 1980; Breckler, 1990; McDonald & Marsh, 1990; Tanaka, 1993). This strategy uses the fit of a baseline model — usually a "null" baseline in which all indicators are assumed to be uncorrelated — against which to compare the fit of alternative models. That strategy was adopted in the present investigation, but even so, there appears to be no entirely satisfactory and universally-accepted goodness-of-fit statistic (Tanaka, 1993). Accordingly, multiple indices were used in the present investigation as guidelines to evaluate the goodness-of-fit of the models examined. In addition to the chi-square value, degrees of freedom, chi-square goodness-of-fit (GFI) statistic, and root mean square residual (RMR) produced by LISREL, the following additional indices are reported: (a) the ratio of the chi-square value divided by its degrees of freedom, (b) the nonnormed

fit index (NNFI; Tucker & Lewis, 1973; Bentler & Bonnett, 1980), (c) the normed fit index (NFI; Bentler & Bonnett, 1980), (d) the incremental fit index (IFI; Bollen, 1989), and (e) the comparative fit index (CFI; Bentler, 1990).

A cursory overview of fit statistics presented in recent published reports of covariance structure models shows wide variation and no consistent pattern, although Tanaka (1993) provides an interesting approach to their classification. A complete account of comparative fit indices is beyond the scope of this paper, but a brief overview of those used here follows.

1. Since the expected value of a chi-square variate is its degrees of freedom, the chi-square/*df* ratio evaluates how many times larger the chi-square estimate is than its expected value. There is no consensus on what represents "good" fit using this index, however, and proposed values range from 5 on down.

2. The NNFI takes into account sample size effects and it shows good performance as values approach one, but it can be anomalously small, especially in large samples. Furthermore, its sampling variability is substantially larger than some of the other indices, and since it is not normed, it can fall outside the 0-to-1 range, making interpretation difficult.

3. The NFI represents the incremental improvement in fit of the alternative model relative to the baseline in a standard metric ranging from 0 to 1. The NFI does not control for degrees of freedom, however, so apparent improvements in fit can be obtained merely by adding parameters or "overfitting" the data. Also, the mean of the sampling distribution

of the NFI is larger for big than for small samples, so larger samples may give the impression of better fit even if the identical model holds. In addition, the NFI may not reach one even if a model is correct, especially in small samples.

4. The IFI takes degrees of freedom into account and lessens the dependence on sample size, but it is not normed to the 0-to-1 range, and sample size does influence its calculation, such that the IFI is larger for small sample sizes than for big ones.

5. The CFI is normed and has small sampling variability, but it appears to have a small downward bias. For more complete accounts of these benefits and drawbacks, the interested reader is referred to Bentler and Bonnett (1980), Bollen (1989), Breckler (1990), McDonald and Marsh (1990), and Tanaka (1993).

Stepped tests of nested models. Another strategy is applicable for testing nested models. A more restrictive model is nested within a less restrictive model if the two models are identical except for constraints in the more restrictive model setting some of its parameters to a constant or to some function of its free parameters (Bollen, 1989). For example, any version of the saturated path model described above in which a single path or set of paths has been constrained to zero is nested within the fully saturated path model, because such models are identical except for the constraints in the more restricted model setting paths to zero. When models are nested, the likelihood ratio (LR), or chi-square difference test, provides a test of significance of the added constraints. If the LR statistic is significant, this indicates that freeing the constraints that distinguish the more from the less restrictive model produces an improvement in fit, making it unlikely that the more

restrictive model is correct. Thus, in addition to the adjunctive goodness-of-fit indices listed above, the LR statistic was also employed in this study to compare nested models.

Analysis block 1: Confirmatory factor analysis for data collection panels. In a previous investigation (Parrella, 1996a), the first of two studies showed that a correlated Coping and Enhancement factor solution with correlated measurement errors (labeled M4 in that study) provided excellent fit for the six IDDS scales from a group of 453 inpatient alcohol and drug abusers tested at admission to treatment, whereas both a single-factor solution (labeled M1) and a two-uncorrelated factors solution (labeled M2) did not. Although a correlated two-factor solution (M3) provided good fit, the M4 solution — which included correlated errors designed to model potential method effects — produced a statistically significant increment in fit, and was judged to be supported best by the data. In the second study of that investigation, cross-validation of the M4 solution in an independent sample demonstrated invariance of form and factor loadings, but the hypothesis of invariance was rejected for latent variable covariances and observed variable measurement errors. In a related investigation (Parrella, 1996b), invariance of the M4 solution as applied to IDDS data was confirmed in a third sample, and the M4 solution was extended to the SCQ. The IDDS and SCQ factor solutions were then merged in a path analysis, demonstrating a high degree of fit, which improved significantly with the addition of correlated longitudinal errors between parallel IDDS and SCQ scales.

Given the level of fit and stability across independent samples demonstrated for the M4 solution in previous investigations, the first analysis block in the present study applied

the M4 solution directly to the IDDS and SCQ data from subjects in this sample to test measurement hypotheses for these instruments. Goodness-of-fit for the M4 solution was assessed individually for the IDDS and SCQ by comparing each model to its respective null baseline, using the chi-square value and adjunctive fit indices described above. It was predicted that these two confirmatory factor analyses would show excellent fit for the M4 factor solution as applied to IDDS and SCQ data from the present sample.

A similar procedure was applied to the participation items. First, corresponding pairs of items were combined, yielding three indicators representing effort ("How much effort did you put into treatment today overall?" plus "How hard did you work today?"), motivation ("Were you interested and paying attention?" plus "Were you motivated?"), and participation ("How much did you participate in today's treatment activities?" plus "Did you contribute when you were in groups?"). Then a null baseline model was specified containing the three independent indicators and no common factors, against which the goodness-of-fit of the final Participation model was tested. Last, the Participation factor was specified as a single common factor on which all three indicators loaded. In order to provide a measurement scale for the latent variable, the loading of the indicator named participation was constrained to equal one (cf., Bollen, 1989). Since a three-indicator model with two free loadings is just identified, fitting the data perfectly and yielding a chi-square value of zero with no degrees of freedom, in order to conduct goodness-of-fit tests, this model was modified by constraining the effort indicator to equal one, as well (cf., Hayduk, 1987). Goodness-of-fit for this factor solution was assessed in

comparison to the null baseline, using the chi-square value and adjunctive fit indices. It was predicted that the Participation factor derived in this way would fit the data well.

Analysis block 2: Testing paths in the saturated model. In order to assess the significance of the individual paths in the saturated model, the complete path model was examined in this set of analyses. Since the number of subjects in this study was small relative to the number of free parameters estimated in a path model using all variables, the Coping and Enhancement factors of the IDDS and SCQ were considered separately. Thus, two risk-participation-confidence-outcome (RPCO) path models were examined, each of which included three of the indicators from both the IDDS and SCQ: a RPCO model for Coping (using the NES, NPS, and IPC scales) and a RPCO model for Enhancement (using the PES, PSS, and SP scales). Parallel procedures were used to examine both models.

First, a null baseline was specified for each RPCO model in which the 10 indicators (i.e., three IDDS scales, three Participation indicators, three SCQ scales, and the outcome indicator) were constrained to be uncorrelated. Then a fully constrained path model was specified in which factor models derived from the previous analysis block were applied to the data and all six of the predictive paths (i.e., paths A through F, as described above) were constrained to zero. Finally, six additional models were composed, each of which was generated by freeing one of the six paths individually while the remaining five paths were constrained to zero. To assess statistical significance, each of these six step models was compared to the nested fully constrained path model using the LR statistic. Once the path model had been "stepped" in this fashion, a final RPCO model was generated in

which only significant paths were retained. This final trimmed RPCO model was then compared to the null baseline model to assess goodness-of-fit, using both the chi-square statistic and the adjunctive goodness-of-fit indices. To repeat, this sequence was conducted separately for the Coping RPCO and the Enhancement RPCO, and these procedures were conducted twice, once using relapse status as the outcome measure and once using relapse latency as the outcome measure. The Appendix lists covariance matrices for RPCO models involving latency, along with the LISREL commands used to generate the baseline, unconstrained, and trimmed models for Coping and Enhancement.

Analysis block 3: Power and sensitivity. Since the number of subjects in this study was relatively small, analyses were conducted to examine the power of statistical tests to detect predicted effects. In covariance structure models, power can be assessed for individual parameters (such as specific factor loadings, or the predictive paths described above), for groups of parameters, or for the model as a whole. Satorra and Saris (1985), Jöreskog and Sörbom (1989), and Bollen (1989) describe and illustrate procedures traditionally used to estimate power in covariance structure analyses. Since power can only be computed in relation to a specific alternative, these procedures involve specifying parameter values for an alternative model, generating the covariance matrix for this model, analyzing the resulting covariance matrix under the original model, and using the resulting chi-square value to approximate the chi-square noncentrality parameter. This noncentrality parameter is then used in conjunction with tabled values of the noncentral chi-square distribution to estimate the power of a significance test at a given alpha level.

Power assessment using these traditional procedures is highly sensitive to each of the specific parameter values chosen for the alternative model, so testing power for even moderately complex models may involve assessing a large number of combinations of alternative parameter values. Furthermore, these techniques all evaluate the power for a test of exact fit for a specific model, even though a model that provides a close (rather than exact) approximation to real-world relationships is the best that can be realistically expected (cf., Browne & Cudek, 1993). MacCallum, Browne and Sugawara (1996) describe another method of testing power based on the root-mean-square error of approximation (RMSEA), an index that indicates the discrepancy of model fit per degree of freedom. Based on their own work and that of other investigators, these authors provide a set of guidelines for interpreting RMSEA, suggesting that values less than 0.05 represent close fit, values from 0.05 to 0.08 represent fair fit, and values above 0.08 indicate mediocre or poor fit. Using the RMSEA index and these cutoff ranges, MacCallum et al. provide procedures for computing power and determining the minimum number of subjects required to achieve a specified level of power.

Both types of power analyses were conducted in the present investigation. For each of the predictive paths in the two final RPCO models involving latency, power to reject the null hypothesis that the path equaled zero given the alternative value dictated by the size of the parameter in the freely estimated solution was computed using traditional procedures. Since power depends in part on alpha, power was computed using a range of alpha levels to determine whether or not the resulting increase in power that would be

obtained using an alpha larger than .05 would affect study conclusions. For the model as a whole, the RMSEA-based procedure was then applied to assess power under null hypotheses of exact, close, and fair fit.

Outcome indicators in this study were inferred on the basis of responses to a single self-report item, time to first use. In order to examine the effects of varying the level of reliability for this indicator, a sensitivity analysis was conducted for relapse latency. In this analysis, the reliability of the latency indicator was fixed at each of several values (viz., 1.0, .9, .8, .7, .6, and .5) and the two final RPCO models were re-estimated. Following procedures illustrated by Bollen (1989), Hayduk (1987), and Marsh (1990), this was accomplished by fixing the latency error term to x times the variance of the observed latency indicator, where x was equal to one minus the reliability of the indicator.

Analysis block 4: Supplementary analyses. A number of other analyses were conducted to examine the RPCO models. First, models were examined which included participation items computed across the entire length of stay in place of those computed based only on the first week of treatment. Second, length of stay itself (measured in days) was substituted for the Participation factor and the two final RPCO models were re-estimated. Third, the mean number of topic groups attended per day during the first week of treatment (NTOPIC; see Measures, above) was included along with the Participation factor and the final resulting models were re-estimated. These model variants were evaluated in the same fashion described above for analysis blocks 1 and 2.

CHAPTER 5

RESULTS

Descriptive Statistics

Table 2 lists means, standard deviations, and Cronbach's (1951) alpha for the IDDS and SCQ scales for study subjects. IDDS and SCQ scales ranged from 0 to 100, whereas the scale for participation items ranged from 1 to 6. For the six participation items computed over the first week of treatment, alpha was .92 ($M = 4.5$, $SD = 0.72$). When computed across length of stay, alpha for participation was .94 ($M = 4.5$, $SD = 0.66$). Subjects in this sample received, on average, 22.0 days of treatment ($SD = 7.7$), which includes a mean of 2.3 days of detoxification ($SD = 3.0$).

Table 2

Means, Standard Deviations and Alphas for IDDS and SCQ Scales

		IDDS			SCQ		
Scale	Items	*M*	*SD*	Alpha	*M*	*SD*	Alpha
Negative emotional states (NES)	20	50.9	27.8	.97	77.8	19.0	.97
Negative physical states (NPS)	10	36.5	25.9	.90	83.6	18.4	.92
Positive emotional states (PES)	10	49.6	27.8	.93	84.6	19.0	.94
Interpersonal conflict (IPC)	20	39.7	27.2	.97	81.5	18.3	.97
Social pressure (SP)	10	50.1	30.5	.93	79.2	22.7	.95
Positive social situations (PSS)	10	51.3	30.6	.95	82.7	19.2	.94

For the outcome measures, subjects had been out of treatment for a span of 205.3 days on average at the time of the six-month follow-up contact (*SD* = 31.4), this mean being slightly longer than the expected six months. Sixty-nine (69) of those contacted reported no use of alcohol or drugs (63.3%), while 40 (36.7%) reported that some use had occurred since discharge. In absolute terms, those who reported use said that their first use episode occurred an average of 11.3 weeks after discharge (*SD* = 10.7), with a minimum of 0 weeks (used immediately after discharge) and a maximum of 35 weeks. When converted to percentage values, and including those who remained abstinent and were assigned a value of 100 percent latency, subjects in this sample reported an average latency of 77.0 percent of the available follow-up span (*SD* = 36.4). This value is, of course, heavily weighted by those who remained abstinent. The average latency for those who had used was 35.7 percent of the follow-up span (*SD* = 32.3), with a median value of 22.3 percent and a mode of zero. More than half of those who drank or used drugs did so within the first two months after discharge. Table 3 lists means, standard deviations, and zero-order correlations for the sixteen observed indicators used in this study.

Table 3

Means, Standard Deviations and Zero-Order Correlations for Indicators (N = 109)

Indicator	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.
1. rNES	—	.000	.000	.000	.000	.000	.893	.161	.835	.105	.582	.642	.133	.193	.334	.361
2. rNPS	.796	—	.000	.000	.000	.000	.846	.356	.697	.046	.014	.307	.021	.030	.143	.952
3. rPES	.516	.452	—	.000	.000	.000	.253	.024	.206	.459	.898	.152	.263	.098	.070	.452
4. rIPC	.895	.753	.557	—	.000	.000	.983	.125	.657	.121	.526	.356	.060	.184	.268	.773
5. rSP	.525	.414	.695	.597	—	.000	.226	.649	.369	.553	.969	.412	.232	.005	.073	.380
6. rPSS	.583	.436	.735	.631	.916	—	.269	.811	.517	.539	.931	.362	.174	.025	.064	.549
7. EFF	.013	.019	-.111	-.002	.117	.107	—	.000	.000	.585	.573	.072	.728	.856	.460	.369
8. MOT	-.135	-.089	-.216	-.148	-.044	-.023	.731	—	.000	.877	.891	.330	.945	.612	.333	.235
9. PAR	-.020	.038	-.122	-.043	.087	.063	.778	.715	—	.608	.776	.045	.685	.318	.275	.359
10. cNES	-.156	-.192	-.072	-.150	-.058	-.060	.053	.015	.050	—	.000	.000	.000	.000	.000	.088
11. cNPS	-.053	-.235	-.013	-.061	.004	.008	.055	-.013	.028	.824	—	.000	.000	.000	.000	.068
12. cPES	-.045	-.099	-.138	-.089	-.079	-.088	.173	.094	.193	.742	.761	—	.000	.000	.000	.077
13. cIPC	-.145	-.221	-.108	-.181	-.115	-.131	.034	.007	.039	.911	.857	.770	—	.000	.000	.038
14. cSP	-.126	-.208	-.160	-.128	-.270	-.215	.018	.049	.097	.736	.688	.709	.727	—	.000	.143
15. cPSS	-.093	-.141	-.174	-.107	-.172	-.178	.072	.094	.106	.805	.777	.860	.821	.836	—	.016
16. TTFU	-.088	-.006	-.073	-.028	-.085	-.058	.087	.115	.089	.164	.176	.170	.199	.141	.230	—
M	50.9	36.5	49.6	39.7	50.1	51.3	4.5	4.6	4.5	77.8	83.6	84.6	81.5	79.2	82.7	77.0
SD	27.8	25.9	27.8	27.2	30.5	30.6	.8	.8	.8	19.0	18.4	19.0	18.3	22.7	19.2	36.4

Note. A dash ("—") indicates the diagonal; means, standard deviations, and correlations are below the diagonal; *p* values for two-tailed tests of significance of correlations are above the diagonal; small "r" prefix indicates IDDS scales, small "c" prefix indicates SCQ scales. NES = Negative Emotional States; NPS = Negative Physical States; PES = Positive Emotional States; IPC = Interpersonal Conflict; SP = Social Pressure; PSS = Positive Social Situations; EFF = Effort indicator; MOT = Motivation indicator; PAR = Participation indicator; TTFU = Time to First Use as percentage of follow-up span (Latency).

IDDS Factors

Goodness-of-fit indices for IDDS factor models are presented in Table 4. As expected, uncorrelated baseline models fit poorly, as demonstrated by the large and highly

significant chi-square values. Statistics for the M4 solution containing correlated Coping and Enhancement factors and correlated method errors, on the other hand, provided excellent fit, as did the single-factor solutions for Coping and Enhancement considered separately. Figure 3 presents parameter estimates for the IDDS M4 solution.

Table 4

Goodness of Fit Summary for IDDS Factor Factor Models

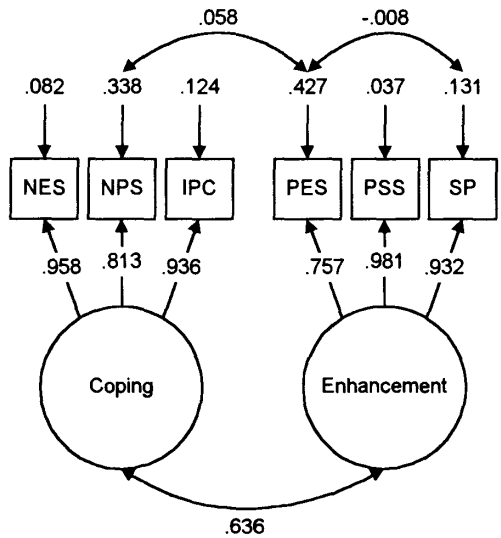
Factor Model	χ^2	*df*	*p*	GFI	RMR	χ^2/df	NNFI	NFI	IFI	CFI
Both Coping and Enhancement (all six IDDS indicators)										
Uncorrelated baseline	631.4	15	.000	.320	445.4	42.1	—	—	—	—
M4, 2 correlated factors	12.6	6	.050	.962	29.2	2.1	.98	.95	.99	.98
Coping Only (NES, NPS, IPC)										
Uncorrelated baseline	285.4	3	.000	.428	421.9	95.1	—	—	—	—
One Coping factor	2.0	1	.161	.988	25.4	2.0	.99	.98	1.0	.99
Enhancement Only (PES, PSS, SP)										
Uncorrelated baseline	281.5	3	.000	.446	494.6	93.8	—	—	—	—
One Enhancement factor	1.1	1	.307	.994	19.9	1.1	1.0	.99	1.0	.99

Note. A dash ("—") indicates not applicable; GFI = LISREL goodness of fit index; RMR = root mean square residual; NNFI = Nonnormed Fit Index; NFI = Normed Fit Index; IFI = Incremental Fit Index; CFI = Comparative Fit Index; NNFI, NFI, IFI and CFI based on comparison to respective uncorrelated baseline.

The total coefficient of determination for observed indicators in the IDDS M4 solution was .998, with squared multiple correlations ranging from .57 to .96. This suggests that the six observed indicators were good measures of the latent Coping and Enhancement variables.

Figure 3

Parameter Estimates for the IDDS M4 Solution



Note. Circles indicate latent variables and boxes represent observed indicators. Anchored singled-headed arrows represent factor loadings, double-headed arrows represent association with no causal direction assumed, and unanchored arrows represent error variance. NES = Negative Emotional States; NPS = Negative Physical States; IPC = Interpersonal Conflict; PES = Positive Emotional States; PSS = Positive Social Situations; SP = Social Pressure. All parameters are completely standardized estimates.

Participation Factors

Goodness-of-fit indices for Participation factor models are presented in Table 5. Again, as expected, the uncorrelated baseline models provided poor fit, whereas the single-factor solutions were well-supported. Figure 4 presents parameter estimates for the Participation solution using indicators from the first week of treatment. Results suggest that the three indicators were excellent indicators of the underlying Participation factor.

Table 5

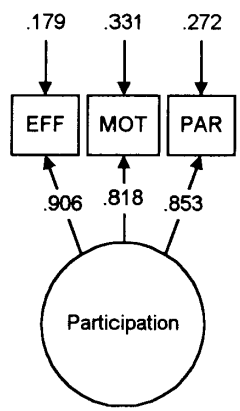
Goodness of Fit Summary for Participation Factor Models

Factor Model	χ^2	*df*	*p*	GFI	RMR	χ^2/df	NNFI	NFI	IFI	CFI
Participation in the first week										
Uncorrelated baseline	196.3	3	.000	.476	.327	65.4	—	—	—	—
One factor	.7	1	.390	.995	.019	.7	1.0	.99	1.0	.99
Participation across length of stay										
Uncorrelated baseline	288.4	3	.000	.421	.290	96.1	—	—	—	—
One factor	.1	1	.795	.998	.004	.1	1.0	1.0	1.0	1.0

Note. A dash ("—") indicates not applicable; GFI = LISREL goodness of fit index; RMR = root mean square residual; NNFI = Nonnormed Fit Index; NFI = Normed Fit Index; IFI = Incremental Fit Index; CFI = Comparative Fit Index; NNFI, NFI, IFI and CFI based on comparison to respective uncorrelated baseline.

Figure 4

Parameter Estimates for the Week One Participation Solution



Note. Circles indicate latent variables and boxes represent observed indicators. Anchored singled-headed arrows represent factor loadings and unanchored arrows represent error variance. EFF = Effort indicator; MOT = Motivation indicator; PAR = Participation indicator. All parameters are completely standardized estimates.

SCQ Factors

Goodness-of-fit measures for the SCQ models are displayed in Table 6. As with the IDDS models, the uncorrelated baselines fit poorly while the hypothesized factor structures fit well. Note, however, that in the original Enhancement-only solution, the error variance for the PSS indicator was slightly negative, a condition known as a Heywood case. Bollen (1989) suggests a number of possible causes and solutions for such problems, one of which is to consider small negative values as essentially equal to zero. Some covariance structure analysis programs, such as EQS (Bentler, 1989), perform this adjustment automatically, preventing Heywood cases by holding ill-behaved parameters at the permissible boundary (e.g., fixing the value at zero). Since LISREL does not perform this correction automatically, the Enhancement only solution was respecified with the error term for PSS fixed at zero. This provided a good fit to the data in this case, although fixing a parameter in this way also yielded an extra degree of freedom for this model. While other solutions are available and should be considered — such as dropping the offending indicator — the size of the error term and the degree of fit for both the M4 solution and the corrected Enhancement-only solution suggested that fixing the PSS error term at zero was the best approach in this case.

As with the IDDS M4 solution, the total coefficient of determination for observed indicators in the SCQ M4 solution was very high (.996). Squared multiple correlations ranged from .70 to .97, suggesting that the six observed indicators were very good measures of the latent Coping and Enhancement variables.

Table 6

Goodness of Fit Summary for SCQ Factor Models

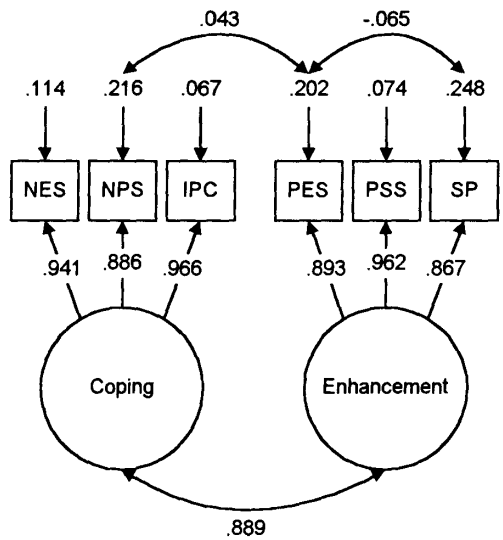
Factor Model	χ^2	*df*	*p*	GFI	RMR	χ^2/df	NNFI	NFI	IFI	CFI
Both Coping and Enhancement (all six SCQ indicators)										
Uncorrelated baseline	760.8	15	.000	.242	251.1	50.7	—	—	—	—
M4, 2 correlated factors	2.9	6	.823	.991	3.0	.5	1.0	.99	1.0	1.0
Coping Only (NES, NPS, IPC)										
Uncorrelated baseline	339.2	3	.000	.401	210.9	113	—	—	—	—
One Coping factor	.1	1	.947	1.0	.6	.1	1.0	1.0	1.0	1.0
Enhancement Only (PES, PSS, SP)										
Uncorrelated baseline	274.6	3	.000	.436	232.4	91.5	—	—	—	—
One Enhancement factor	.2	2	.917	.999	6.1	.1	1.0	1.0	1.0	1.0

Note. A dash ("—") indicates not applicable; GFI = LISREL goodness of fit index; RMR = root mean square residual; NNFI = Nonnormed Fit Index; NFI = Normed Fit Index; IFI = Incremental Fit Index; CFI = Comparative Fit Index; NNFI, NFI, IFI and CFI based on comparison to respective uncorrelated baseline.

Figure 5 presents parameter estimates for the SCQ M4 solution. Note that the correlation between Coping and Enhancement factors was higher for the SCQ M4 solution than for that derived from the IDDS. This indicates that Coping and Enhancement abstinence self-efficacy categories may be less distinct (or, alternatively, more coherent) than the same categories applied to risk of use, replicating the results of previous research (Parrella, 1996b). Overall, then, the confirmatory factor analyses conducted for the first three data collection panels provided strong support for the measurement hypotheses of this study, yielding a good foundation for considering the structural hypotheses represented by the path analyses.

Figure 5

Parameter Estimates for the SCQ M4 Solution



Note. Circles indicate latent variables and boxes represent observed indicators. Anchored singled-headed arrows represent factor loadings, double-headed arrows represent association with no causal direction assumed, and unanchored arrows represent error variance. NES = Negative Emotional States; NPS = Negative Physical States; IPC = Interpersonal Conflict; PES = Positive Emotional States; PSS = Positive Social Situations; SP = Social Pressure. All parameters are completely standardized estimates.

Path Tests and Final Models for Coping

Goodness-of-fit indices for the Coping RPCO models using relapse status as the outcome measure are presented in Table 7, and comparisons among nested models comprising the six step tests for individual paths are presented in Table 8. As predicted, the uncorrelated baseline model provided poor fit, whereas the RPCO model fit the data well. Contrary to predictions, however, the null hypothesis could not be rejected for any of the six paths, as indicated by non-significant LR statistics for each of the stepped paths in

Table 8. Thus, in this case, the final trimmed Coping RPCO using relapse status as the outcome indicator was the same as the fully constrained path model.

Table 7

Goodness of Fit for Coping RPCO Models Using Relapse Status

Path Model	χ^2	*df*	*p*	GFI	RMR	χ^2/df	NNFI	NFI	IFI	CFI
Uncorrelated baseline	874.5	45	.000	.442	159.3	19.4	—	—	—	—
RPCO, all paths constrained	28.9	33	.672	.952	30.1	.9	.97	.96	1.0	.97
RPCO, trimmed	28.9	33	.672	.952	30.1	.9	.97	.96	1.0	.97

Note. A dash ("—") indicates not applicable; GFI = LISREL goodness of fit index; RMR = root mean square residual; NNFI = Nonnormed Fit Index; NFI = Normed Fit Index; IFI = Incremental Fit Index; CFI = Comparative Fit Index; NNFI, NFI, IFI and CFI based on comparison to uncorrelated baseline.

Table 8

Path Steps for Coping RPCO Model Using Relapse Status

Model Comparison	LR	*df*	*p*
Constrained vs. free A	0.15	1	.699
Constrained vs. free B	2.44	1	.118
Constrained vs. free C	0.15	1	.699
Constrained vs. free D	2.32	1	.128
Constrained vs. free E	1.73	1	.188
Constrained vs. free F	0.46	1	.498

Note. LR = likelihood ratio test statistic

Goodness-of-fit measures for the Coping RPCO models using latency as the outcome measure are presented in Table 9, and comparisons among nested models for the six step tests are presented in Table 10. As for relapse status, the uncorrelated baseline model using latency provided poor fit, whereas the RPCO models fit the data well. Contrary to predictions, the null hypothesis could not be rejected for five of the six paths, as indicated by non-significant LR statistics. Freeing path D did provide an improvement in fit using .05 as a cutoff for the LR statistic. Thus, the final trimmed model for the Coping RPCO path analysis using latency as the outcome measure contained only one freely estimated path, path D, predicting relapse latency from confidence at discharge.

Table 9

Goodness of Fit for Coping RPCO Models Using Latency

Path Model	χ^2	*df*	*p*	GFI	RMR	χ^2/df	NNFI	NFI	IFI	CFI
Uncorrelated baseline	875.6	45	.000	.441	162.3	19.5	—	—	—	—
RPCO, all paths constrained	30.0	33	.619	.951	43.3	.9	.97	.95	1.0	.97
RPCO, trimmed	25.6	32	.779	.956	32.8	.8	.97	.96	1.0	.97

Note. A dash ("—") indicates not applicable; GFI = LISREL goodness of fit index; RMR = root mean square residual; NNFI = Nonnormed Fit Index; NFI = Normed Fit Index; IFI = Incremental Fit Index; CFI = Comparative Fit Index; NNFI, NFI, IFI and CFI based on comparison to uncorrelated baseline.

Table 10

Path Steps for Coping RPCO Model Using Latency

Model Comparison	LR	*df*	*p*
Constrained vs. free A	0.15	1	.699
Constrained vs. free B	2.44	1	.118
Constrained vs. free C	0.14	1	.708
Constrained vs. free D	4.33	1	.037
Constrained vs. free E	1.15	1	.284
Constrained vs. free F	0.54	1	.462

Note. LR = likelihood ratio test statistic

Path Tests and Final Models for Enhancement

Goodness-of-fit indices for the Enhancement RPCO models using relapse status as the outcome measure are presented in Table 11, and comparisons among nested models comprising the six step tests for individual paths are presented in Table 12. As with Coping, the uncorrelated baseline model for Enhancement provided poor fit, whereas the RPCO model fit the data much better. Contrary to predictions, the null hypothesis could not be rejected for five of the six individual paths. The significant path in this case was path B, the stability coefficient predicting confidence at discharge from risk at baseline. The final trimmed Enhancement RPCO model using relapse status as the outcome indicator included only path B, yielding a non-significant chi-square value for the trimmed path model.

Table 11

Goodness of Fit for Enhancement RPCO Models Using Relapse Status

Path Model	χ^2	*df*	*p*	GFI	RMR	χ^2/df	NNFI	NFI	IFI	CFI
Uncorrelated baseline	812.4	45	.000	.453	185.8	18.1	—	—	—	—
RPCO, all paths constrained	47.9	33	.045	.918	40.4	1.5	.94	.92	.98	.94
RPCO, trimmed	43.9	32	.078	.924	12.2	1.4	.95	.92	.98	.95

Note. A dash ("—") indicates not applicable; GFI = LISREL goodness of fit index; RMR = root mean square residual; NNFI = Nonnormed Fit Index; NFI = Normed Fit Index; IFI = Incremental Fit Index; CFI = Comparative Fit Index; NNFI, NFI, IFI and CFI based on comparison to uncorrelated baseline.

Table 12

Path Steps for Enhancement RPCO Model Using Relapse Status

Model Comparison	LR	*df*	*p*
Constrained vs. free A	0.50	1	.480
Constrained vs. free B	3.97	1	.046
Constrained vs. free C	0.98	1	.322
Constrained vs. free D	2.03	1	.154
Constrained vs. free E	1.73	1	.188
Constrained vs. free F	0.02	1	.888

Note. LR = likelihood ratio test statistic

Goodness-of-fit for the Enhancement RPCO models using latency as the outcome measure are presented in Table 13, and comparisons among nested models for the six step tests are presented in Table 14. As in previous models, the uncorrelated baseline provided poor fit, whereas the RPCO models fit the data much better. Only two of the six paths

provided an improvement in fit, as indicated by significant LR statistics, when compared to the Enhancement RPCO model in which all paths were constrained to zero. Freeing path B and path D provided significant improvements in model fit. Thus, the final trimmed model for the Enhancement RPCO path analysis using latency as the outcome measure contained two freely estimated paths, path B (predicting confidence from prior risk) and path D (predicting latency from confidence).

Table 13

Goodness of Fit for Enhancement RPCO Models Using Latency

Path Model	χ^2	*df*	*p*	GFI	RMR	χ^2/df	NNFI	NFI	IFI	CFI
Uncorrelated baseline	815.0	45	.000	.451	189.3	18.1	—	—	—	—
RPCO, all paths constrained	50.5	33	.027	.914	54.2	1.5	.94	.92	.98	.94
RPCO, trimmed	40.8	31	.112	.930	16.3	1.3	.95	.93	.99	.95

Note. A dash ("—") indicates not applicable; GFI = LISREL goodness of fit index; RMR = root mean square residual; NNFI = Nonnormed Fit Index; NFI = Normed Fit Index; IFI = Incremental Fit Index; CFI = Comparative Fit Index; NNFI, NFI, IFI and CFI based on comparison to uncorrelated baseline.

Table 14

Path Steps for Enhancement RPCO Model Using Latency

Model Comparison	LR	*df*	*p*
Constrained vs. free A	0.49	1	.484
Constrained vs. free B	3.96	1	.047
Constrained vs. free C	0.97	1	.325
Constrained vs. free D	5.77	1	.016
Constrained vs. free E	1.15	1	.284
Constrained vs. free F	0.42	1	.517

Note. LR = likelihood ratio test statistic

Power

Table 15 displays the results of calculating power using traditional procedures for different alpha levels. The level indicated by the tabled value represents the power of the step tests to reject the null hypothesis that the specified path was equal to zero, given the size of the parameter in the freely estimated solution and the specified alpha level. These values correspond directly to the step comparisons reported above in Tables 10 and 14. As can be seen from an examination of Table 15, power for tests of paths retained in the two final trimmed RPCO models was acceptable, being above an admittedly somewhat arbitrary cutoff level of 0.40. While raising alpha as high as .25 would bring power to acceptable levels for four additional tests, this alpha level would affect the significance of only one path (the B path predicting confidence from risk in the Coping solution).

Table 15

Power to Reject Null Hypothesis for RPCO Models Using Latency

		Alpha				
Path	Beta	.05	.10	.15	.20	.25
Coping RPCO model						
A	-.041	0.07	0.13	0.18	0.23	0.29
B	-.153	0.34	0.46	0.55	0.61	0.66
C	.033	0.06	0.12	0.17	0.22	0.27
D	.192	0.51	0.63	0.71	0.76	0.80
E	.099	0.17	0.27	0.34	0.40	0.46
F	-.039	0.07	0.13	0.18	0.24	0.29
Enhancement RPCO model						
A	.073	0.11	0.19	0.25	0.31	0.37
B	-.200	0.55	0.67	0.74	0.79	0.83
C	.114	0.21	0.31	0.39	0.45	0.51
D	.212	0.60	0.71	0.78	0.82	0.86
E	.089	0.15	0.23	0.30	0.37	0.42
F	-.028	0.06	0.11	0.17	0.22	0.27

Note. Beta = size of completely standardized path coefficient; Tabled values represent power to reject the null hypothesis that the specified path equals zero at the specified alpha level.

Table 16 displays power levels for the overall chi-square test of RPCO models that included latency, calculated using the RMSEA procedure and cutoff values specified by MacCallum et al. (1996). Since power varies depending on degrees of freedom as well as alpha level, power is displayed for RPCO models in which all paths were freely estimated ($df = 27$) as well as for the trimmed models ($df = 32$ for Coping and 31 for Enhancement,

but 32 was used in the power calculations for trimmed models). Using MacCallum et al.'s cutoff values, exact fit was defined here as the power of the chi-square test to detect the alternate hypothesis that RMSEA ≥ 0.05 using a null hypothesis that fit was exact (i.e., RMSEA = 0). Similarly, close fit was defined as power to detect an alternate hypothesis that RMSEA ≥ 0.08 under a null hypothesis that fit was close (RMSEA ≤ 0.05). Fair fit was defined as power to detect the alternate hypothesis that RMSEA ≥ 0.08 when the null hypothesis was fair fit (RMSEA < 0.08).

Table 16

Power at Specified Fit Levels for RPCO Models Using Latency

	Alpha				
Level of fit	.05	.10	.15	.20	.25
RPCO models with paths freely estimated					
Exact fit	0.27	0.39	0.48	0.56	0.62
Close fit	0.35	0.48	0.58	0.65	0.71
Fair fit	0.73	0.82	0.88	0.91	0.93
RPCO models with non-significant paths trimmed					
Exact fit	0.24	0.36	0.45	0.53	0.59
Close fit	0.31	0.44	0.54	0.61	0.67
Fair fit	0.67	0.78	0.84	0.88	0.90

Note. Exact fit = power to detect H_a: RMSEA ≥ 0.05 when H_0:RMSEA = 0; Close fit = power to detect H_a: RMSEA ≥ 0.08 when H_0:RMSEA ≤ 0.05; Fair fit = power to detect H_a: RMSEA ≥ 0.08 when H_0:RMSEA < 0.08; H_0 = null hypothesis; H_a = alternate hypothesis; RMSEA = root-mean-square error of approximation.

As noted previously, the hypothesis of exact fit is probably quite unrealistic for most real-world applications. Overall, power levels for tests of RPCO model fit were considered acceptable. Based on tables presented by MacCallum et al., the minimum number of subjects required to obtain a power of 0.80 under null hypotheses of either close or exact fit would have been approximately 350, or three times the number of subjects actually obtained in the present study.

Sensitivity

Results of the sensitivity analyses for the Coping and Enhancement RPCO models using latency as the outcome measure are presented in Table 17. As expected, none of the paths that did not directly impinge on latency (i.e., paths A, B, and C) were affected by these analyses. Paths D, E, and F — which comprise all the direct effects involving latency — varied in a systematic and logical fashion as the reliability of the latency indicator was modified. Since covariance structure analysis corrects effects for unreliability in the indicators, the result of decreasing levels of reliability was an increase in the size of the standardized path coefficient estimates, as expected. None of the significance levels for these parameters were affected by reliability manipulations, however.

Table 17

Sensitivity of RPCO Path Coefficients to Varying Latency Reliability

	Path					
Latency Reliability	A	B	C	D	E	F
Coping RPCO with freely-estimated paths						
1.0 (original model)	-.041	-.153	.033	.192	.099	-.039
.9	-.041	-.153	.033	.203	.105	-.042
.8	-.041	-.153	.033	.215	.111	-.044
.7	-.041	-.153	.033	.230	.119	-.047
.6	-.041	-.153	.033	.248	.128	-.051
.5	-.041	-.153	.033	.272	.141	-.056
Enhancement RPCO with freely-estimated paths						
1.0 (original model)	.073	-.200	.114	.212	.089	-.028
.9	.073	-.200	.114	.224	.094	-.029
.8	.073	-.200	.114	.237	.100	-.031
.7	.073	-.200	.114	.253	.107	-.033
.6	.073	-.200	.114	.274	.115	-.036
.5	.073	-.200	.114	.300	.126	-.039

Note. Latency reliability was varied by fixing the error term for the latency indicator to (one minus reliability) times the variance of the latency indicator.

Model Variants Involving Length of Stay

Two sets of analyses were conducted to examine the effects of length of stay on study conclusions. First, the Coping and Enhancement RPCO models were re-estimated using participation indicators computed across the length of stay. Second, length of stay itself was included to assess effects attributable to treatment duration. Procedures for examining these supplementary variables included free estimation of paths, as well as a full

series of step analyses parallel to those described previously. Results of these analyses were for all practical purposes identical to the original Coping and Enhancement RPCO models. Neither participation computed across the length of stay nor length of stay in days yielded significant paths beyond those present in the trimmed models described in Tables 7, 9, 11 and 13, so these findings are not presented in further detail here.

Model Variants Involving Topic Groups

The final set of supplementary analyses concerned the effects of a secondary measure of treatment exposure, NTOPIC, the mean number of topic-oriented groups attended per day during the first week of treatment. As described previously, NTOPIC embodied a set of optional discussion groups in which highly personalized methods like role-playing were used to address recovery-related issues. In the supplementary analyses described here, NTOPIC was added to the two RPCO models using latency as the outcome measure, creating two risk/topic group/participation/confidence/outcome (RTPCO) models (one for Coping and one for Enhancement). In addition to the six paths A through F present in the original RPCO models, the RTPCO models had four new paths: A′ (risk to NTOPIC), B′ (NTOPIC to Participation), C′ (NTOPIC to confidence), and E′ (NTOPIC to Latency). Goodness-of-fit for the Coping and Enhancement RTPCO models are presented in Table 18, and the model comparisons constituting path steps are displayed in Table 19.

Table 18

Goodness of Fit for RTPCO Models

Path Model	χ^2	*df*	*p*	GFI	RMR	χ^2/df	NNFI	NFI	IFI	CFI
Coping										
Uncorrelated baseline	908.7	55	.000	.442	148.2	16.5	—	—	—	—
RTPCO, constrained	63.1	43	.025	.911	39.6	1.5	.93	.91	.98	.93
RTPCO, trimmed	34.7	41	.747	.946	29.9	.9	.96	.95	1.0	.96
RTPCO, trimmed at 1.64	31.7	40	.821	.951	13.9	.8	.97	.95	1.0	.96
Enhancement										
Uncorrelated baseline	839.1	55	.000	.459	172.8	15.3	—	—	—	—
RTPCO, constrained	74.6	43	.002	.894	49.5	1.7	.91	.89	.96	.91
RTPCO, trimmed	48.5	40	.169	.925	14.8	1.2	.94	.92	.99	.94
RTPCO, trimmed at 1.64	45.6	39	.218	.929	15.3	1.2	.95	.92	.99	.95

Note. A dash ("—") indicates not applicable; GFI = LISREL goodness of fit index; RMR = root mean square residual; NNFI = Nonnormed Fit Index; NFI = Normed Fit Index; IFI = Incremental Fit Index; CFI = Comparative Fit Index; NNFI, NFI, IFI and CFI based on comparison to respective uncorrelated baseline.

Several things should be noted about the RTPCO solutions. First, results of the step tests for the six original paths (A through F) are almost identical to those derived in the original latency-based RPCO models (compare to Tables 10 and 14). Second, only one of the four new paths achieved significance at the .05 level (i.e., C′, the path predicting discharge self-efficacy from NTOPIC), and this path was significant for both Coping and Enhancement. Third, the trimmed Coping model included two significant paths (path C′ and path D, the latter predicting latency from discharge self-efficacy), while the trimmed Enhancement model included three (path C′, path D, and path B, predicting confidence at

discharge from risk at baseline). Thus, the resulting trimmed RTPCO models were the same as the trimmed RPCO models, with the exception of the addition of path C′. Fourth, the C′ path was negative in both solutions, indicating that more exposure to topic groups served to lower self-efficacy at discharge — a result opposite in direction to the effect on self-efficacy predicted for the Participation factor.

Table 19

Path Steps for RTPCO Models

	Coping			Enhancement		
Model Comparison	LR	*df*	*p*	LR	*df*	*p*
Constrained vs. free A	0.15	1	.699	0.49	1	.484
Constrained vs. free A′	0.00	1	1.00	0.21	1	.647
Constrained vs. free B	2.43	1	.119	3.96	1	.047
Constrained vs. free B′	1.23	1	.267	1.24	1	.265
Constrained vs. free C	0.14	1	.708	0.97	1	.325
Constrained vs. free C′	24.13	1	.000	14.93	1	.000
Constrained vs. free D	4.32	1	.038	5.78	1	.016
Constrained vs. free E	1.15	1	.264	1.15	1	.264
Constrained vs. free E′	0.23	1	.632	0.24	1	.624
Constrained vs. free F	0.54	1	.462	0.43	1	.512
Trimmed vs. Trimmed at 1.64	2.93	1	.087	2.90	1	.089

Note. LR = likelihood ratio test statistic

Finally, an additional modification of the RTPCO models was assessed. Examination of the *t* values for path parameters in the fully saturated models supported the

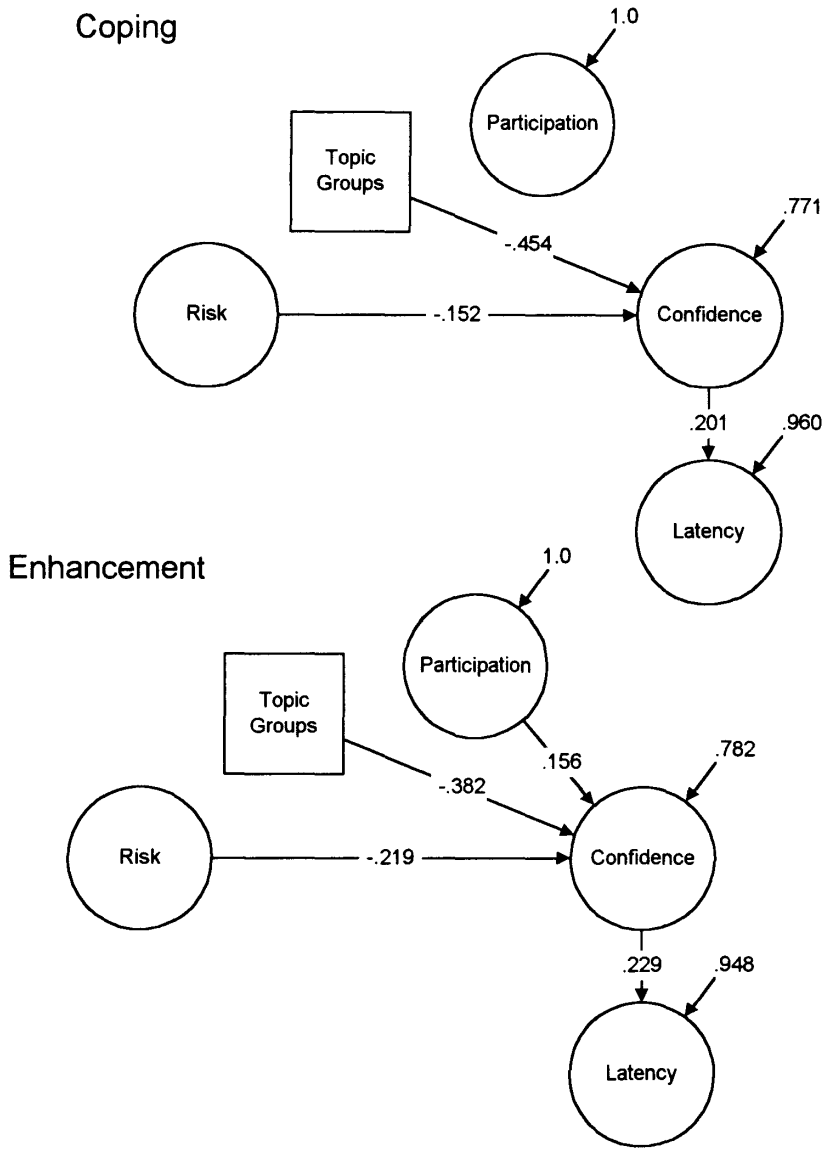
results of the step tests reported in Table 19, with one exception for each model. Typically, a critical *t* of 2 is used to assess parameters for significance: parameters with cutoff *t* values of 2 or higher are generally considered significant at the .05 level, and this means of assessing significance usually converges with the results of the chi-square difference (LR) tests, as found here. In this case, however, inclusion of NTOPIC in the models produced *t* values above 1.64 (i.e., significance at the .10 level) for one additional parameter in each model. For Coping, the 1.64 *t* cutoff also led to significance for the B path (predicting confidence from risk). For Enhancement, the 1.64 cutoff led to inclusion of the C path (predicting confidence from Participation). Note that these result are not reflected by the LR tests of these paths in Table 19, where the steps in which these two additional paths were freed individually did not produce significant results at the .10 level (although the *p* value for the B path in the Coping RTPCO, .119, is close to this level). It should also be noted that the marginal level of significance for these two additional paths was not the case for either of the RPCO models — in other words, results reported in Tables 9 and 10 (for Coping) and Tables 13 and 14 (for Enhancement) were the same whether a *t* cutoff of 2 or 1.64 was used.

Since the RTPCO results based on *t* values and the results based on LR step tests were divergent, and since the number of subjects available in this study led to relatively low power to detect effects, both models were re-estimated and examined using the results based on *t* values of 1.64. Table 18 lists these results as "RTPCO, trimmed at 1.64," and the last test shown in Table 19 provides additional comparisons in which the RTPCO

models with and without the borderline paths are compared. Table 18 shows a very minor improvement in fit for both models when the questionable paths are included. Table 19 demonstrates that, in both cases, inclusion of these paths yields an improvement over the trimmed models significant at an alpha level of .10, which converges with the results of the *t* tests for these parameters. Completely standardized parameter estimates for the resulting RTPCO path models trimmed at 1.64 are illustrated in Figure 6. Except for the paths involving NTOPIC, and the two additional paths that achieved significance at .10 when NTOPIC was included (i.e., path B in the Coping model and path C in the Enhancement model), the parameter estimates reported in Figure 3 are very similar to those produced in the two final trimmed RPCO models (compare to completely standardized betas listed in Table 15).

Figure 6

Final RTPCO Path Models With Parameter Estimates



Note. Circles indicate latent variables, boxes are observed indicators, anchored arrows represent causal pathways, and unanchored arrows represent error variance. Measurement parameters are omitted for ease of presentation. All parameters are completely standardized estimates, significant at $p < .10$.

CHAPTER 6

DISCUSSION

The measurement hypotheses of this study were well supported by confirmatory factor analyses. The M4 factor structure applied to IDDS and SCQ data yielded excellent fit when Coping and Enhancement were considered together, as well as when they were broken into separate models, confirming prior research using these instruments (Parrella, 1996a, 1996b). The items comprising Participation, too, yielded a single well-fitting factor as predicted, and this result held for data based on the first week of treatment as well as for participation data from the entire length of stay. This set of confirmatory factor analyses provided a solid foundation for considering the structural hypotheses represented by the path analyses.

Path hypotheses, however, received only limited support. Although the final RPCO trimmed path models produced good fit, none of the six paths represented in the Coping RPCO model were significant at either the .05 or the .10 level when relapse status was used as the outcome measure. For the Enhancement RPCO with relapse status, which also demonstrated good fit, only the B path representing prediction of confidence at discharge from risk at baseline achieved significance. This lack of evidence of predictive power for both the Coping and Enhancement RPCO models in regard to relapse status was robust across the model variants involving length of stay that were considered: neither length of

stay in days nor participation calculated across length of stay appeared to provide incremental predictive power with respect to relapse status.

When relapse latency was used as the outcome measure, both the Coping and Enhancement RPCO models converged with findings from previous research on smoking cessation treatment. Abstinence self-efficacy at discharge, or confidence, was found to be the only significant predictor of relapse latency in both cases, accounting for about 4% of latency variance in the Coping model and about 5% of the variance in the latency measure in the Enhancement model. In both cases, greater confidence meant longer time to first use. These results, too, were robust across length of stay model variants, and they held for the subgroup of individuals who reported use at follow-up as well as for the sample as a whole (results for those who used mirrored results for the sample as a whole, and so were not presented here). The power of LR step tests to detect effects in the RPCO models described in this study was low for the non-significant paths, but the magnitude of the coefficients for these paths — as represented by estimated parameters in the unconstrained solutions — suggested that such effects would have accounted for only about 1% of the variance in the respective criterion variables, and so would not have been particularly meaningful even if they had attained statistical significance.

Nevertheless, failure to find significant effects of participation in the RPCO models raises questions about the conceptualization of participation used in this study. Although motivation and participation are not the same thing, they are clearly related, so an inability to demonstrate participation effects reinforces Miller's (1985) contention that it can be

difficult to operationalize patient motivation adequately. One possibility is that TPS volunteers who were involved enough in the study to provide data at all four points of data collection (i.e., at baseline, during treatment, at discharge, and at the six-month follow-up) represent a subset of compliant individuals, producing a restriction of range for participation that lowered correlations with other measures. The participation items, however, were normally distributed, lacking either skewness or excessive kurtosis, and this argues against restriction of range due to volunteer status. Another possibility is that the stochastic exposure assumption was faulty — i.e., the assumption that more participation implies more exposure to the sources of self-efficacy information (i.e., visceral experience, vicarious information, verbal persuasion, and performance) identified by Bandura (1977). Correlations of performance items with other workbook measures, however, argue against this interpretation as well. Participation was inversely related to self-reported craving and negative mood in the TPS, and it was positively correlated with the perceived helpfulness of other patients, perceived program impact, and perceived results at discharge. Other correlational analyses demonstrated significant associations between participation and both patient and therapist ratings of patient motivation at baseline (cf., Parrella et al., 1993). Furthermore, a previous investigation using this index showed that both general self-efficacy and positive outcome expectations of treatment at baseline predicted a significant proportion of participation variance (Parrella, 1995).

Of course failure to reject the null hypothesis that participation didn't have significant effects in RPCO models doesn't prove this to be the case. Yet the general

nature of the participation measure used here doesn't take into account potentially significant confounds. Patients may not have felt that program materials made sense or applied to them, or they may have participated from the perspective of critic, selectively seeking attitudinally-congruent information. Nor does the notion of perceived participation control for variation that may be attributable to differential anchors for the referents embodied in the items — for instance, objectively similar levels of effort or contribution may have been perceived and rated differently depending on factors such as extroversion or social anxiety. In other words, the meaning of participation may have varied depending on a number of circumstances. It is not clear, for example, that making several comments on other people's problems during many group sessions necessarily implies more exposure to self-efficacy information than would a single difficult admission over the course of an entire treatment episode. If the question comes down to "Who needs what?" a global index of overall participation may not be specific enough to address the idea that some patients need sensitization or motivation to change while others who are already motivated need training in skills like controlling anger or managing anxiety. A more compelling test of study hypotheses, then, would probably require less global and less subjective measures of exposure to a wider variety of potentially important treatment components.

Having said that, supplementary analyses in which a dose measure of topic groups was introduced produced additional findings that also may help to explain these results. Both the Coping and Enhancement RTPCO models showed statistically significant paths from topic group dose to discharge self-efficacy. The coefficients for these paths were

negative, indicating that more exposure to topic groups tended to produce lower abstinence self-efficacy. Although this result may at first seem counter-intuitive, MMCD treatment is based on a disease model of alcoholism, and the disease model — with its focus on loss-of-control drinking and the unalterable nature of alcoholism — stresses the fragile nature of recovery, as exemplified by slogans such as "one day at a time." This philosophical framework implies that abstinence self-efficacy, or confidence, is anathema to the ongoing process of maintaining abstinence, and a treatment approach emphasizing that viewpoint is consistent with the topic group results found here.

Inclusion of the topic groups measure also led to a borderline level of statistical significance for the path predicting self-efficacy from participation in the Enhancement model, a predicted effect that was opposite in direction to the effect of topic groups. These findings suggest that MMCD treatment may produce competing effects on self-efficacy at discharge, providing support for the contentions of Marlatt (1985) and Rollnick and Heather (1982) that the disease model loss-of-control tenet embodied in MMCD treatment may in fact serve to reduce the likelihood of subsequent abstinence. Simply adding the opposing indirect effects on latency of topic groups and perceived participation in the Enhancement model demonstrates how the net decrease in latency serves to obscure the somewhat smaller but nonetheless positive contribution of patient participation.

These results support the idea that abstinence self-efficacy predicts relapse latency, as Bandura's (1977, 1986) self-efficacy theory would suggest. The absence of direct paths to latency from topic groups and participation also indicates that the competing effects on

latency attributable to these two variables in the Enhancement RTPCO were completely mediated by self-efficacy at discharge. That finding further reinforces the intent behind this study, to assess MMCD treatment using self-efficacy as a common metric to examine treatment effects. Moreover, although a lack of overall effects for MMCD treatment programs has often been attributed to the "washing out" of differential treatment effectiveness for different patients that results from considering only the main effects of treatment, the RTPCO findings reported here point to a somewhat different aspect of this issue: the possibility that a lack of overall effects may be due in part to opposing effects of different components or facets of the treatment experience. This interpretation underscores the value of process analysis, and it emphasizes the importance of attempts to isolate the "active ingredients" of MMCD treatment for detailed study.

Limitations. Several limitations of this study should be acknowledged. Because this was a naturalistic observational study based on correlational analysis of self-report data, rather than a controlled experiment in which variables were manipulated and observed more objectively, it would be inappropriate to draw unequivocal conclusions on the basis of the results reported here. The longitudinal nature of data collection obviates some of the more egregious concerns plaguing causal interpretations of cross-sectional data, making it implausible, for example, that latency caused abstinence self-efficacy, or that participation caused risk. The results reported here do not, however, speak to issues like reciprocal causation, the validity of self-reports, or third-variable interpretations. It may well be, for instance, that risk and participation are reciprocally related across multiple

panels, or that some common unmeasured factor such as social desirability influenced reports of both abstinence self-efficacy and relapse latency. This research was not designed to address such questions, although the sensitivity analysis conducted for relapse latency suggests that inclusion of additional indicators that increase the reliability of the latency measurement would accentuate the effects described here. Additional studies in which potential confounds like these are specified and explored would be required to rule out such threats to validity.

With regard to measurement issues, although most of the indicators used here showed excellent distributional properties, the SCQ scales evidenced some degree of both kurtosis and restriction of range. While models involving these indicators fit the data well and converged with results from other samples, kurtosis may have affected results in some way not immediately apparent. The degree of model fit, and the fact that the effect of abstinence self-efficacy on relapse latency was the only one to reach significance in RPCO models, suggest that this was not a serious problem. It should be noted, however, that the restricted range of SCQ scales probably attenuated correlations, biasing results in a conservative direction. This, along with latency unreliability, less-than-perfect self-report reliability, and the importance of post-discharge factors noted by other investigators (cf., Moos et al., 1990), undoubtedly contributed to the inability of self-efficacy to account for more than a small percentage of the variance in relapse latency.

The use of abstinence status, a dichotomous categorical indicator, as an outcome measure, however, directly violates the normality assumptions of covariance structure

analysis. Maximum likelihood estimation is in general considered relatively robust to violations of these assumptions, and the use of dichotomous indicators in covariance structure models is not unprecedented, being analogous to dummy coding in standard regression analyses (see, for example, Hayduk, 1987, who illustrates this with several models incorporating categorical indicators). Bollen (1989) cites robustness studies suggesting that it is excessive kurtosis, rather than the number of categories, that may create problems in covariance structure models with categorical variables, producing inflated chi-square values and standard errors. Examination of the solutions involving relapse status in this sample shows chi-square values and standard errors comparable in magnitude to those obtained from the continuous latency measure. Nevertheless, the relapse status results reported here should be interpreted cautiously, especially given the absence of significant effects for the predictive paths tested in status-based models.

Another important issue involves sample size and the criteria used to assess statistical significance. In this study, a relatively small number of subjects provided data from all four instruments, resulting in low power to detect small effect sizes. This situation was complicated by the opposing implications of the alpha cutoff for the LR statistic and the chi-square test of overall model fit, which can make choosing a single level of significance problematic. If alpha is lowered to prevent capitalization on chance in the LR tests, models that provide less fit will be accepted using this alpha value as a decision criterion for the chi-square test. If alpha is raised to make the chi-square test more stringent, this relaxes the criterion for LR tests. Furthermore, as noted by Bollen (1989),

when repeated tests are performed on the same data, as when comparing different models, the resulting chi-square test statistics are correlated, and the usual probability levels do not reflect this lack of independence. For these reasons, all results were considered at traditionally-accepted alpha levels of both .05 and .10, and adjunctive fit indices were used, but no correction for the number of tests conducted in this study was performed. Consequently, cross-validation of the findings reported here assumes added importance.

Future directions. A number of directions for future research are indicated. Given the possibility of competing effects for different elements of MMCD treatment, it is important to try to operationalize these elements using more objective measures. For example, structured inventories with behavior-based response options might help to address the subjective nature of the participation index, whereas a voucher system might enable tracking of service component delivery. Multiple sources of participation data (e.g., including participation ratings from therapists as well as patients) could be used to address this issue. A series of measures specifically designed to tap Bandura's (1977) four sources of self-efficacy information seems the next logical step.

Given that a significant contribution of pre-existing outcome expectations to subsequent treatment participation has been demonstrated (cf., Parrella, 1995), outcome expectations and other patient characteristics such as psychiatric comorbidity would be important additions in further tests of the model. The results of this study should be cross-validated on other samples, with larger numbers of subjects, and it would be interesting to see if findings vary by substance type, as previous work (e.g., Filstead, Parrella, & Ebbitt,

1988; Ross, Filstead, Parrella, & Rossi, 1994) has shown both commonalities and differences in the hierarchy of risk situations across substances of primary abuse. Therapist characteristics, too, have been associated with the differential treatment success of clients (e.g., McLellan, Woody, Luborsky, & Goehl, 1988), and since different facilities introduce both unique as well as programmatic variance, a multi-site replication in which both program and therapist characteristics are incorporated would allow specific tests of these factors.

Summary. Although the small sample size raised some questions about the power of statistical tests to detect small effect sizes, the present investigation demonstrated that abstinence self-efficacy at discharge from MMCD treatment predicted subsequent relapse latency, converging with similar findings from research on smoking cessation treatment. Process analyses based on covariance structure modeling revealed significant influences on self-efficacy for both a general measure of treatment participation and for a more specific dose-related measure of treatment exposure, suggesting that different aspects of the treatment program may have exerted competing effects on self-efficacy and, consequently, produced opposing indirect effects on relapse latency. These results support the use of self-efficacy as a common metric for examining addiction treatment based on disease model principles, and they emphasize the importance of process analysis as a mechanism for identifying and assessing the "active ingredients" of MMCD programs.

APPENDIX

Following is the PRELIS code used to generate the covariance matrices used in the RPCO models, along with output comprising the resulting covariance matrices and univariate and multivariate descriptive statistics for the observed indicators. Also included are the LISREL commands used to specify the three RPCO models in Tables 9 and 13 (i.e., the uncorrelated baseline, fully constrained, and final trimmed models). PRELIS and LISREL commands are listed separately for the Coping and Enhancement RPCO models. Note that the order of variables here differs from the order of presentation of the zero-order correlations in Table 3.

Coping

```
subtitle 'RPCO, generate Coping covariance matrix'.
prelis
 /variables= RNES RIPC RNPS PEFFORT PMOTIVN PCONTRB
             CNES CIPC CNPS TTFUPCT (CO)
 /missing=listwise exclude  /type=covariance
 /print=kurtosis  /matrix=out (*).
```

TOTAL SAMPLE SIZE = 109

UNIVARIATE SUMMARY STATISTICS FOR CONTINUOUS VARIABLES

VARIABLE	MEAN	ST. DEV.	SKEWNESS	KURTOSIS	MINIMUM	FREQ.	MAXIMUM	FREQ.
RNES	50.890	27.810	-.150	-.967	.000	6	100.000	2
RIPC	39.661	27.150	.349	-.714	.000	10	100.000	3
RNPS	36.477	25.943	.432	-.707	.000	7	100.000	1
PEFFORT	4.461	.767	-.520	-.006	2.300	1	6.000	1
PMOTIVN	4.641	.755	-.281	-.484	2.700	1	6.000	2
PCONTRB	4.453	.847	-.407	-.307	2.200	1	6.000	2
CNES	77.804	18.991	-.713	-.442	30.000	1	100.000	11
CIPC	81.449	18.321	-1.121	1.124	11.000	1	100.000	21
CNPS	83.613	18.399	-1.172	.782	18.000	1	100.000	30
TTFUPCT	76.987	36.413	-1.253	-.100	.000	7	100.000	70

RELATIVE MULTIVARIATE KURTOSIS = .113714D+01

COVARIANCE MATRIX

	RNES	RIPC	RNPS	PEFFORT	PMOTIVN	PCONTRB
RNES	773.395					
RIPC	675.842	737.115				
RNPS	574.294	530.423	673.030			
PEFFORT	.277	-.044	.374	.588		
PMOTIVN	-2.836	-3.026	-1.746	.423	.569	
PCONTRB	-.474	-.988	.830	.505	.457	.717
CNES	-82.489	-77.064	-94.576	.770	.214	.798
CIPC	-73.820	-90.046	-105.156	.473	.093	.611
CNPS	-27.251	-30.694	-112.196	.770	-.184	.429
TTFUPCT	-89.392	-27.577	-5.464	2.426	3.153	2.735

COVARIANCE MATRIX

	CNES	CIPC	CNPS	TTFUPCT
CNES	360.654			
CIPC	316.989	335.664		
CNPS	287.968	288.900	338.528	
TTFUPCT	113.451	133.036	117.744	1325.932

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TOTAL SAMPLE SIZE =  109

UNIVARIATE SUMMARY STATISTICS FOR CONTINUOUS VARIABLES
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VARIABLE	MEAN	ST. DEV.	SKEWNESS	KURTOSIS	MINIMUM	FREQ.	MAXIMUM	FREQ.
RPSS	51.266	30.551	-.267	-1.143	.000	10	100.000	2
RSP	50.092	30.539	-.204	-1.077	.000	11	100.000	3
RPES	49.587	27.798	-.221	-.846	.000	11	100.000	4
PEFFORT	4.461	.767	-.520	-.006	2.300	1	6.000	1
PMOTIVN	4.641	.755	-.281	-.484	2.700	1	6.000	2
PCONTRB	4.453	.847	-.407	-.307	2.200	1	6.000	2
CPSS	82.667	19.225	-1.066	.110	32.000	1	100.000	28
CSP	79.170	22.666	-1.062	.427	6.000	1	100.000	28
CPES	84.618	18.975	-1.158	.381	25.000	1	100.000	45
TTFUPCT	76.987	36.413	-1.253	-.100	.000	7	100.000	70

```
RELATIVE MULTIVARIATE KURTOSIS =  .111025D+01
```

COVARIANCE MATRIX

	RPSS	RSP	RPES	PEFFORT	PMOTIVN	PCONTRB
RPSS	933.382					
RSP	854.290	932.621				
RPES	624.231	590.038	772.745			
PEFFORT	2.502	2.734	-2.354	.588		
PMOTIVN	-.535	-1.016	-4.540	.423	.569	
PCONTRB	1.624	2.248	-2.874	.505	.457	.717
CPSS	-104.465	-101.148	-93.076	1.054	1.359	1.718
CSP	-148.701	-186.586	-100.500	.305	.841	1.853
CPES	-51.086	-46.014	-72.844	2.519	1.347	3.093
TTFUPCT	-64.561	-94.462	-73.725	2.426	3.153	2.735

COVARIANCE MATRIX

	CPSS	CSP	CPES	TTFUPCT
CPSS	369.614			
CSP	364.167	513.764		
CPES	313.591	304.989	360.063	
TTFUPCT	161.189	116.527	117.601	1325.932

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VITA

Mr. Parrella received his Bachelor of Arts in Psychology from the University of Connecticut in 1979. He received the degree of Master of Arts in Social Psychology from Loyola University in January of 1995, and is currently enrolled in the Ph.D. program in Social Psychology at Loyola University.

Mr. Parrella is employed as a senior consultant with William J. Filstead and Associates, Inc., an Arlington Heights consulting firm specializing in the evaluation of behavioral health care services. He is responsible for designing, implementing, analyzing and reporting contracted studies of behavioral health care clients and treatment providers.

Previously, Mr. Parrella was database manger and analyst at Parkside Lutheran Hospital in Park Ridge. There, he served as co-investigator for National Institute on Alcohol Abuse and Alcoholism (NIAAA) Grant No. AA08455, and as database manager and principle analyst for NIAAA Grant No. AA07409. He was responsible for designing and implementing studies of alcohol and substance abusers, as well as for constructing and managing databases for ongoing research projects, developing and monitoring data collection protocols, and writing research reports and grant proposals.

Mr. Parrella's work has appeared in a number of publications, including the *Journal of Studies on Alcohol, Drugs and Society, Addiction and Recovery, American Journal on Addictions*, and the *Journal of Behavioral Medicine*.

APPROVAL SHEET

The dissertation submitted by David P. Parrella has been read and approved by the following committee:

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The final copies have been examined by the director of the dissertation and the signature which appears below verifies the fact that any necessary changes have been incorporated and that the dissertation is now given final approval by the committee with reference to content and form.

The dissertation is, therefore, accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy.

Nov. 6, 1996
Date

Director's Signature