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Analytic Methods for Modeling Longitudinal Data from Rolling Therapy Groups with Membership Turnover

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Abstract

Interventions for a variety of emotional and behavioral problems are commonly delivered in the context of treatment groups, with many using rolling admission to sustain membership (i.e., admission, dropout and discharge from group is perpetual and ongoing). We present an overview of the analytic challenges inherent in rolling group data and outline commonly-used (but flawed) analytic and design approaches used to address (or sidestep) these issues. Moreover, we propose latent class pattern mixture modeling (LCPMM) as a statistically and conceptually defensible approach for modeling treatment data from rolling groups. The LCPMM approach is illustrated with rolling group data from a group-based alcoholism pilot treatment trial ($N = 128$). Different inferences were made with regard to treatment efficacy under LCPMM versus the commonly used standard group-clustered latent growth model (LGM); coupled with other preliminary findings in this area, inferences from LGMs may be overly liberal when applied to data from rolling groups. Continued work on data analytic difficulties in groups with membership turnover is critical for furthering the ecological validity of research on behavioral treatments.

Key Words: group therapy; rolling admissions; group membership; missing data; pattern mixtures

Psychosocial treatments for a variety of emotional and behavioral problems are very commonly delivered by providers to patients in the context of treatment groups. Indeed, many types of interventions, ranging from purely process-oriented approaches to highly structured manualized treatments are delivered in the group therapy context (for a review, see Fehr, 2003). For certain conditions, such as alcoholism and substance abuse, delivery of treatment in groups has far surpassed the use of individual-based treatments (e.g., a single patient and a single counselor meeting one-on-one for a therapeutic hour) in community practice (NIAAA/NIDA, 2003).

Both therapeutic and economic factors account for the widespread use of group therapy. From a clinical perspective, many authors have argued that the primary curative mechanisms of group therapy are the interactions and interdependence of group members (e.g., Fehr, 2003; Yalom, 1995). Therapeutically, group therapy (a) provides participants with a safe context for observing and practicing interpersonal skills, (b) exposes participants to immediate feedback from members and leaders about their behavior, and (c) replaces or otherwise augments participants' social networks with individuals (i.e., group leaders and members) who promote change by providing positive psychosocial support. Fiscally, the cost profile of group therapy is superior to traditional individual-based treatment because many more patients can be treated per investment of provider time in a group therapy context (Rounsaville & Carroll, 1997; Weiss, Jaffe, de Menil & Cogley, 2004).

Group Therapy: The Disconnect Between Community Practice and Research

Despite the widespread use of group therapy in community practice, the vast majority of federally-funded research has focused on the development and empirical

evaluation of individual-based treatments (e.g., NIAAA/NIDA, 2003). For example, a recent meta-analytic review of substance abuse treatment outcome studies, covering the last 30 years, found only 24 studies comparing group therapy to other conditions (Weiss et al., 2004). As stated by Weiss and colleagues (2004), "The discrepancy between the widespread use of group therapy in clinical practice and the paucity of research on this topic stems, in part, from the inherent difficulties in conducting meaningful research on group therapy" (p. 348). Many of the "inherent difficulties" which Weiss et al. allude to include (a) difficulties in evaluating and assigning what occurs during the course of a therapy group (i.e., group process); (b) limitations in the control over various elements of treatment delivery; and (c) feasibility issues (e.g., time required to recruit a sufficient number of participants for a cohort).

Our focus in this article is on one specific problem that has arguably been one of the more vexing analytic challenges in the pursuit of ecological validity (i.e., matching treatment research with treatment practice): namely, how is group interdependence modeled *when the treatment group membership itself changes over time* (i.e., turnover) in therapeutic contexts that incorporate an open enrollment for group members. This paradigm is more commonly referred to as "rolling admission" (i.e., treatment group members continually dropping out, terminating and/or joining the group after it initiates). Rolling admission groups are very common in community practice, since this enrollment strategy allows for providers to replenish groups when members drop out prematurely or successfully complete treatment, thereby allowing the group to continue indefinitely (Coviello et al., 2001).

Proper modeling of group interdependence in data generated from clinical trials is critical for two primary reasons, one of which is more conceptual and the other more practical. First, accurate modeling of member interdependence can begin to capture and evaluate, in a data analytic framework, the essence of what many have argued to be the primary curative mechanism of positive therapeutic change. Second (and relatedly), modeling group interdependence allows for an accurate accounting of group-level variance components in an effort to illustrate the ratio of group-level variability to total variability in the outcome (i.e., group-level intraclass correlation) and maintain the nominal Type I error rate (e.g., $p = .05$) for treatment effect detection (Barcikowski, 1981; Hox, 2002).

Turnover in Rolling Groups

Although the analysis of longitudinal data from closed groups presents challenges of its own, rolling groups present a much greater set of conceptual and methodological complexities. For example, closed groups are dynamic to a certain extent because members dropout during the course of the group. However, closed groups end at a specified point in time and then are started anew with new members. Conversely, when rolling groups are considered, there is added complexity to handling group interdependence because of ongoing membership additions, terminations and dropouts. Moreover, the life of these groups can be virtually unending; even when members of a given group have changed entirely over time, the group itself retains, to a greater or lesser extent, the history of the group, which, in turn, is likely to influence its process.

Unfortunately, models for analysis of data derived from groups with rolling membership have not been fully explicated. Because group membership gradually or

abruptly changes over time (e.g., new members are added to the group intermittently while other members drop out or are removed), participants are not, in an analytic sense, consistently nested within a given group because it is not the same "group" over time (at least in terms of member composition).

Common Strategies Used to Analyze Data from Group Therapy Trials

As part of what is fast-becoming conventional practice, nearly all modern analyses of longitudinal data from group-based treatment trials use some variant of the longitudinal growth model (LGM) (e.g., random coefficient models in the mixed modeling framework or structural equation modeling with individually-varying growth parameters as latent variables), used to capture differences in changes over time on outcomes as a function of treatment conditions while accounting for individual- and(/or) group-level clustering of observations (Curran & Hussong, 2003; Fals-Stewart, Birchler, & O'Farrell, 2003). However, there is one primary limitation with most approaches under the generalized linear mixed modeling family (i.e., models that handle non-independence and non-normal outcomes) with respect to changes in group membership over time: an inherent assumption that the composition of the treatment group *itself* does not change[1] (Morgan-Lopez & Fals-Stewart, 2006a). To date, there have not been satisfactory analytic options to handling changes over time in treatment group membership in the analysis of data from rolling groups.

Morgan-Lopez and Fals-Stewart (2006a) identified four common analytic and/or design strategies that investigators have used to deal with difficulties posed by conducting group therapy research: (a) include therapy groups in a study, but ignore the dependencies among members and treat the resulting data from participants as if they

were not nested in a group; (b) include group as a level in the analysis, but ignore the changing group membership resulting from rolling admission; (c) design studies that use closed-enrollment groups of a fixed duration so as to avoid the potentially dramatic changes in membership often observed in rolling admission groups; or (d) avoid the problem completely by designing investigations that simply do not use therapy groups. Although these approaches sidestep certain problems inherent in studies that use rolling groups, they have the potential to either lead to results and conclusions that are faulty (approaches a and b above) or have contributed to a disconnect between how clinical trials are conducted and how treatment is most commonly delivered in community settings (approaches c and d above).

Given the interest in making treatment research more ecologically valid (NIDA, 2003), federal funding agencies have called for more research on group therapy and have specifically highlighted the need to address the analytic complexities that we have described thus far. It appears that there may be two generally viable approaches that have potential for handling incomplete nesting in therapy groups. One such approach that has emerged in behavioral genetics, and may have some potential utility in modeling therapy group turnover, involves weighted random coefficient modeling among individuals nested within groups (e.g., families). The strength of genetic dependencies will vary within a family (e.g., monozygotic twins versus first cousins) and, as such, weights are assigned to individuals within families to capture the differences in the relative proportion of shared and unshared genotypic information (Guo & Wang, 2002; McArdle & Prescott, 2005). In this case, the contribution to non-independence of observations within a family will be weaker among family members that have less common genetic

information. Although similar weighing approaches have been proposed for group therapy research to accommodate group turnover (e.g., Morgan-Lopez & Fals-Stewart, 2006a) these approaches are underdeveloped outside of behavioral genetics; the analog in group therapy research may likely be to weight individuals in the therapy group based on a) the length of time individuals remain in the treatment group and/or b) the level of turnover occurring in the group during the period the individual is a member of the group.

In this article, we examine the utility of latent class pattern mixture modeling (LCPMM; Lin, McCulloch & Rosenheck, 2004; Muthén, Jo & Brown, 2003; Roy, 2003) as a conceptually appealing and statistically defensible alternative to commonly used approaches in the analysis of group therapy trials (e.g., ignoring dependencies among members, failing to model changes in group membership). We describe the theoretical underpinnings of LCPMM and how this emerging framework may be one of the more theoretically attractive approaches presently available to handling session-to-session changes in treatment group membership over time, taking into account differences in treatment group attendance patterns and the point of the calendar year at which the individual enters treatment. We then illustrate differences in results from the analysis of longitudinal data from a group-based alcoholism treatment trial currently using a rolling admission paradigm (Fals-Stewart, Birchler, O'Farrell, Klostermann & Evans, 2005) under LGM for group cluster-correlated data[2] and group cluster-correlated LCPMM.

Method

Participants

Participants were men (N = 128) entering outpatient treatment for an alcohol use disorder. To be eligible, male participants had to (a) be married to a non-substance

abusing female partner for at least one year or cohabiting with a non-substance abusing female partner for at least two years; (b) meet current alcohol dependence criteria according to the *Diagnostic and Statistical Manual of Mental Disorders* (4th ed., *DSM-IV*; American Psychiatric Association, 1994); and (c) have alcohol as their primary drug of abuse based on criteria described in Fals-Stewart (1996). Male participants were excluded if their female partner met *DSM-IV* criteria for a current substance use disorder (except for nicotine). Individuals seeking treatment in this outpatient study were not eligible for admission if they displayed evidence of schizophrenia, delusional (paranoid) disorder, or evidence of other psychotic disorders (on the basis of results of an initial brief screening interview). All eligible participants were given an overview of the project and signed a consent form indicating their understanding of the study and willingness to participate. The project was approved by the Institutional Review Board of RTI International.

Measures

Alcohol use. The Timeline Followback Interview (TLFB; Sobell & Sobell, 1996) uses a calendar and other memory aids to determine alcohol use over a specified period. In this investigation, the TLFB was administered each week during the course of treatment to determine days of alcohol use during the previous seven days (excluding the day of the interview); for analyses in this article, alcohol use was converted to a binary measure (yes/no) of any alcohol use in the past week.

Admission and therapy attendance dates. Log books maintained by the treatment program were used to determine the dates when participants entered the trial and attended sessions. These were cross-validated by examining session notes; there was complete agreement between these sources. This information was used to determine the (a) week of

the trial that the individual began treatment (referred to hereafter as "trial" week) and (b) attendance patterns from weeks 2 through 12 for each individual *regardless of what point during the trial they began treatment* (referred to hereafter as "person" week).

Procedure

Married and cohabiting men entering treatment for an alcohol use disorder (N = 163) were asked, along with their female partners, to participate in an extensive interview to determine eligibility for the study. Of these, 16 (9.8%) male participants declined to participate in the study. Of the remaining couples who agreed to be interviewed (n = 147), 19 (12.9%) met one or more exclusion criteria (e.g., couples with female partners who met abuse or dependence criteria on alcohol or an illicit drug, alcohol not the primary drug of abuse). Thus, 128 couples were included in the final sample. Participants were then randomly assigned to one of two conditions: (a) Getting Along (GA; Fals-Stewart, Nottingham, Skibicki & Birchler, 2004), a group therapy approach designed to address marital and relationship problems, or (b) individual-based treatment (IBT). Participant flow into the trial is illustrated in Figure 1. Restricted randomization (i.e., blocking), using a computer program, was used to balance the groups in terms of size. Condition assignment was concealed from participants until they arrived for their first intervention session.

Description of Treatments

GA. For the 32 sessions conducted as part of this condition, male partners were scheduled to attend twelve 90-minute group therapy sessions that focused on relationship problems and enhancement. In this rolling group, session content focused on (a) identifying relationship factors that might contribute to continued use or relapse; (b)

problem-solving and negotiation strategies designed to help participants address identified relationship problems; (c) teaching effective communication skills, such as 'active listening' and expressing feelings directly; and (d) enhancing relationship satisfaction and increasing positive behavioral exchanges, encouraging participants to acknowledge pleasing behaviors by their intimate partners and engaging in shared recreational activities with their significant others. GA group session content was drawn largely from that of Behavioral Couples Therapy (BCT) for alcoholism (see Fals-Stewart, Birchler, & O'Farrell, 2003) and was adapted and manualized for therapy with male partners-only groups (Fals-Stewart et al., 2004).

The group was conducted weekly during a 68-week period. As with any group with rolling admission, group membership and size varied throughout the trial, with the number of members in the group ranging from three to ten. To describe change in group membership composition from week-to-week, we derived a *Percentage of Group Change Index* (PGCI), which was calculated as 1 – ([number of members who remained the same from the previous week] / [number of members who remained the same from the previous week + the number of members who were present in the group that were not present the previous week + the number of members from the previous week who were missing compared to the previous week]) x 100. The mean (*SD*) PGCI for the GA group was 40.2 (16.5), with a range of 0.0 (i.e., no change in membership from one week to the next) to 80.0

In the remaining twenty 60-minute sessions, male participants attended one-on-one therapy sessions with a counselor. The content of these individual sessions was the same as that used for the IBT condition (see below).

IBT. The 32 sessions conducted as part of this condition were 60-minute one-on-one sessions between a provider and a patient. Individual session content was drawn from the Individual Drug Counseling manual (Mercer & Woody, 1999), which was slightly modified to focus on alcohol dependence; as noted in the manual, such modification is acceptable due to the generic nature of the intervention itself. The treatment is founded on the concept that alcoholism is a spiritual and medical disease, consistent with the philosophy espoused by Alcoholics Anonymous (AA). Participants are encouraged not only to achieve and maintain abstinence from alcohol and other psychoactive substances, but also to attend Alcoholics Anonymous self-help support groups.

Analytic Approach

In the following section, we provide some background on latent class pattern mixture models, a promising analytic approach for handling data from therapy groups with changing membership. We do, however, presume some familiarity with longitudinal growth models. For readers who are not familiar with longitudinal growth models, an excellent, minimally-technical introduction to the basic LGM can be found in Curran and Hussong (2003). For an introduction to LGM for longitudinal data nested within groups, see Muthén (1997); for an example from the substance abuse treatment literature, see Fals-Stewart, O'Farrell and Birchler (2004).

Missing data theory and rolling group structure. In many research contexts, the impact of dropout has been accounted for in standard group cluster-correlated models under the missing-at-random (MAR) assumption (Muthén & Muthén, 1998-2006; Schafer & Graham, 2002). Under MAR, the probability of attendance[3] may depend on data that are observed *but do not depend on data that are missing* (Rubin, 1976; Schafer

& Graham, 2002). If the MAR assumption holds then, as a consequence, the relations of interest (e.g., treatment condition → growth in the outcome) will not differ as a function of the cause of missingness (Hedeker & Mermelstein, 2000).

But in practice, there is a non-trivial likelihood that the MAR assumption will be violated in the context of substance abuse treatment research. For example, Schafer and Graham (2002) suggest that the MAR assumption is most likely to be violated in "clinical studies in which reasons for dropout are closely related to the outcomes being measured" (p.172); dropout may be directly related to the value that *would* have been observed (e.g., failing to attend group due to substance abuse relapse; see also Hedeker & Gibbons, 1997). It may also be unreasonable to expect that the treatment effect will not vary across the cause of missingness. In the context of treatment research under rolling group structure, this would be analogous to assuming that individuals who enter the treatment group late (i.e., roll-ins) or who drop out of treatment early come from the same population as individuals who stay in the treatment group consistently (Morgan-Lopez & Fals-Stewart, 2006a); this also assumes that the treatment effect will be consistent across each of these "sub-groups" (Hedeker & Gibbons, 1997; Hedeker & Mermelstein, 2000). By not making any provision for differences in treatment efficacy among these sub-groups (e.g., completers, early dropouts, late roll-ins, sporadic attendees) standard longitudinal approaches may not properly account for sub-groups for whom the treatment is less effective, particularly those that drop out of treatment early.

Pattern Mixture Approaches. Pattern mixture models (Hedeker & Gibbons, 1997; Muthén, Kaplan & Hollis, 1987; Little, 1993) are an approach used to handle data that are not-missing at-random (i.e., when the probability of missingness *does* depend on data that

are missing). In pattern mixture models, individuals are classified by their patterns of missing data and the parameters of interest are estimated separately across missing data patterns; this has been done either through multiple-group approaches in structural equation modeling (Allison, 1987; Muthén et al., 1987), modeling of missing data pattern by predictor (i.e., treatment) interaction terms in the random coefficient modeling framework (Hedeker & Gibbons, 1997) or more recent multiple imputation (MI) approaches (Demirtas & Schafer, 2003; Schafer, 2003).

In the estimation of pattern mixture models, the interest is usually on a single set of estimates that are averaged across the missing data patterns, with the pattern-specific estimates weighted by sizes of the missing data patterns in the sample. This averaging is done either through equality constraints (in the conventional multiple-group SEM approach; Allison, 1987; Bollen, 1989; Muthén et al., 1987), matrix manipulation of group-specific estimates (in random coefficient models; Hedeker & Gibbons, 1997) or by including the missing data pattern indicators in the imputation model but excluding them from the analysis model in MI (Demirtas & Schafer, 2003; Schafer, 2003).

However, the primary limitation on pattern mixture models under all of the above-mentioned frameworks is that, as the number of missing data patterns become large (and the number of observations within each pattern become sparse), it may become impractical and/or impossible to identify and model all of the parameters within each missing data pattern (Hedeker & Rose, 2000). A second limitation of conventional pattern mixture models is that individuals with the same patterns of missing data are treated as though they have equivalent probabilities of being from the same population, but this has been shown to be untenable in practice (Roy, 2003).

Latent class pattern mixture models (LCPMMs; see Lin, McCulloch & Rosenheck, 2004; Muthén, Jo & Brown, 2003) are both an extension of conventional pattern mixture models and a special case of the general latent variable model which can simultaneously incorporate both continuous and categorical latent variables (Muthén, 2002). LCPMMs extend conventional pattern mixture models by incorporating a finite number of underlying *latent* attendance classes (as opposed to subsetting across *all observed* attendance patterns) (Muthén et al., 2003) which allows for probabilistic membership of being in a particular attendance class (Muthén et al., 2003; Roy, 2003) (See figure and captions under Figure 2). LCPMMs also allow differences in the probability of attendance class membership for individuals with the same observed pattern of missingness given the attendance patterns *and* the outcome trajectories are modeled as indicators of class membership (Roy, 2003).

For practical purposes, the LCPMM has great potential for handling data from rolling groups because, in addition to modeling a finite number of latent attendance classes (e.g., people who attend most every session, dropouts, late-comers, irregular attendees) within and across treatment groups in a trial, LCPMMs also allow for (a) variability in the treatment effect across attendance patterns *even for individuals in the same treatment group* and (b) modeling the point of treatment entry and attendance pattern jointly as functions of latent attendance class membership. The key to the utility of LCPMM for rolling group data is that, during any given point in the life of the trial, the proportions of different types of attendance patterns (and, therefore, different subtypes of patients) can vary at any given slice in time at which the trial is running. Moreover, as

group composition changes (e.g., the proportion of dropouts decreases over time), it can impact differences in treatment efficacy across any given snapshot of the group.

This approach may be critical in increasing the accuracy of inferences made from treatment trial data with rolling groups. Preliminary simulation work has suggested that standard group-clustered LGMs may increase the nominal Type I error rate to .20 or greater when modeling data from rolling groups, while the nominal (i.e., $p = .05$) Type I error rate was maintained under (weighted averaging of parameters across latent attendance groups in) LCPMM analysis (Morgan-Lopez & Fals-Stewart, 2006b). Thus, conventional methods may increase the likelihood that significant treatment effects are detected in a sample when there are no differences in the population in analytic frameworks where turnover is not explicitly modeled (Morgan-Lopez & Fals-Stewart, 2006a, 2006b).

Results

Sample Characteristics

The pretreatment characteristics of participants assigned to the two conditions are presented in Table 1. Random assignment was effective; comparisons of background characteristics of participants assigned to GA or IBT revealed no significant differences (i.e., all *p*s > .25).

Preliminary Identification of Functional Form

The group cluster-correlated finite mixture model within Mplus 4 (Muthén & Muthén, 1998-2006) was used for all analyses under maximum likelihood estimation for non-normal data (i.e., Mplus "MLR" estimation; see Yuan & Bentler, 2000). Prior to fitting the models of interest, the observed alcohol use proportions across the 12 person-

weeks were plotted in order to get a sense of the optimal functional form; assessing the correct latent variable structure a priori also minimizes the likelihood of overextraction of latent classes (Bauer & Curran, 2004). It was concluded that the optimal functional form was piecewise linear with three distinct periods of growth: growth from weeks 1-3, growth from weeks 3-8 and growth from weeks 8-12; the timesteps for each of these three growth parameters (see Table 3) were structured such that the intercept was set at the 12^{th} person week (i.e., estimated level probability of use in the week prior to the participant's last treatment session). These periods of growth directly correspond to the three distinct stages of treatment (initial evaluation, treatment of identified problems, and planning for post-treatment) described recently by Fals-Stewart and Birchler (2006). Graphical analyses were supplemented by a series of unconditional growth models (with probit link functions for categorical outcomes for this and all subsequent analyses) examined under various functional forms. Using the likelihood ratio χ^2, the fit of the above-described unconditional three-piece piecewise linear model ($\chi^2(4064) = 169.917$, $p=1.0$) provided a significant improvement in fit over a conventional linear model ($\Delta\chi^2(2) = 23.26$, $p<.0001$) and a quadratic model ($\Delta\chi^2(1) = 9.153$, $p=.002$) and was used as the functional form of choice for all subsequent analyses.

Identification of Optimal Number of Attendance Classes

Next, a series of three-piece linear probit LCPMMs, with varying numbers of latent attendance classes, was fit to determine the optimal number of classes[4]. As shown in Table 2, global fit statistics (i.e., Bayesian Information Criterion (BIC), Entropy) suggested that a 3-class solution was optimal.

Outcome Analysis 1: Standard LGM

The first set of outcome analyses were conducted under group cluster-correlated piecewise-linear LGM with four growth parameters (i.e., three individually-varying slopes and an intercept at Week 12). These analyses are conducted and presented in order to illustrate results from an analysis that represents the current standard for handling longitudinal treatment data from individuals nested within treatment group(s) (e.g., Fals-Stewart et al., 2004). More importantly, this first outcome analysis serves as the basis for comparison against outcome analyses under the latent class pattern mixture framework. Measures of effect size from both sets of analyses were calculated by converting t-test values to r^2s as recommended by Rosenthal and Rosnow (1991).

Conditional means of the growth parameters. The conditional means of the growth parameters correspond to the growth parameter means for the IBT condition. There were significant decreases in the probability of early alcohol use in the IBT condition (from Weeks 1-3), b = -.590(.101), t = -5.844, p<.0001, r^2 = .210); however the decrease in alcohol use during the intermediate (Weeks 3-8; b = -.064(.044), t = -1.448, p =.15, r^2 = .016) and late periods (Weeks 8-12; b = -.089(.055), t = -1.634, p =.104, r^2 = .019) was non-significant.

Treatment effects. Differences in changes over time in the probability of alcohol use across the Group Treatment versus IBT conditions from Weeks 1-3 were non-significant, b = .114(.101), t = 1.12, p>.25, r^2 = .009. Differences in changes over time in the probability of alcohol use from Weeks 3-8 approached significance, b = .086(.044), t = 1.954, p =.052, r^2 = .028. Plots of the predicted probabilities of use across each treatment condition based on the probit regression parameters showed participants in the Group Treatment condition showed slight increases (from .22 in week 3 to .24 in week 8)

in the probability of alcohol use while participants in the IBT condition showed decreases (from .39 in week 3 to .31 in week 8) during the intermediate period of treatment. Differences in changes over time in the probability of alcohol use from Weeks 8-12 were significant, b = -.167(.055), t = 3.04, p=.002, r^2 = .067. Predicted probability plots suggested that the decreases in alcohol use from weeks 8 to 12 were steeper among Group Treatment participants (.24 to .11) than among IBT participants (.31 to .27). Finally, the difference between IBT participants (.27) and Group Treatment participants (.11) at week 12 termination (intercept) was significant, b = -1.024(.225), t = -4.55, p<.0001, r^2 = .139.

Outcome Analysis 2: Three-Class LCPMM

A second outcome analysis was conducted under a three-class (group-cluster correlated) LCPMM which, in contrast to Analysis 1, takes into account latent attendance sub-groups (based on attendance probabilities from weeks 2-12) and the point of treatment group entry. The probabilities of attendance for each class are shown in Figure 3. Class 1 (Droppers) never exceeded a 42% percent probability of showing up for treatment and by Week 10 were likely to have dropped out of the study entirely. Class 2 (Show-ers) never dropped below a 75% probability of showing up for treatment. Class 3 (Erratics) had a very erratic pattern of attendance, ranging from a high of 77% and a low of 25% between weeks 2-9; however, this group had a very high likelihood of attending treatment (>80%) the last three weeks. The probabilities of class membership in the attendance pattern groups did not differ across the treatment conditions (ps>.071).

Distributions of the week of trial entry. Initial examination of the distributions of the week of trial entry suggested that there were no differences between attendance

groups in when individuals joined the trial. The mean and variance of starting week for Droppers were 24.19 and 237.21. For Show-ers, the mean and variance were 26.33 and 272.91. For the Erratic class, the mean and variance were 24.75 and 304.55. However, graphical analyses (i.e., histograms) and formal univariate tests of normality of the starting week distributions (i.e., Shapiro-Wilks tests) suggested that the means and variances were not sufficient to characterize these distributions (i.e., significant deviation from normality; See Figure 4). While it appears that the proportion of Droppers and Show-ers is fairly uniform across Weeks 1-56 of the trial (no new participants began treatment after Trial Week 56), 50% of the participants that had Erratic attendance patterns entered treatment between Weeks 1-5 of the trial or Weeks 41-45 of the trial. This finding is key because the trial began in late November 2004 (i.e. the week before Thanksgiving 2004), which suggests that (a) the Erratic attendance pattern was most likely to occur among participants who would have been in treatment during the 2004 Winter holiday season (i.e., Thanksgiving through New Year's) or mid-to-late August 2005 (i.e., End-of-Summer through Labor Day weekend) and (b) the proportions of people (and thus the differential treatment effects) from each latent attendance class depended on which part of the calendar year the trial was taking place.

Class-Specific Parameter Estimates. Attendance class-specific growth parameter and differential treatment effect estimates are shown in Table 4. For the Droppers class, the Week 12 drinking probabilities did not deviate significantly from 50% in the IBT condition ($b = .148(.547)$, $t = .27$, $p>.8$). The difference between the group treatment condition and IBT was significant among Droppers ($r^2 = .142$) with the predicted probability of Week 12 drinking among Droppers in the Group condition of 90.9%

(probit/z-score (.148 + 1.185); see Table 4)[5]. Among the Always Attender class, Week 12 alcohol use was significantly lower than 50% (b = -2.616(.422), t = 6.19, p<.001; predicted probability = 0.4%). The Group Treatment condition did not differ significantly from IBT condition in Week 12 alcohol use among Show-ers (p<.25, r^2 = .017). Among the Erratic class, Week 12 alcohol use did not deviate significantly from 50% in the IBT condition (b = .046(.506), t = .09, p>.9). However, among the Erratic class, the probability of Week 12 alcohol use was significantly lower among participants in the group condition than the IBT condition (p=.003, r^2 = .43, Week 12 probability for Erratics in group treatment = 7.6%) ,

*Weighted-averaged conditional means of the growth parameters.*The class-specific estimates were used to calculate[6] a single set of weighted-averaged estimates across the three attendance classes[7] for overall conditional means and treatment effects (Table 5). As shown in Table 5 (under Three-Class LCPMM), there were significant decreases in the probability of alcohol use between Weeks 1-3 (r^2 = .118) and Weeks 8-12 (r^2 = .042) under the IBT condition; however, decreases from Weeks 3-8 were non-significant (r^2 = .014).

Weighted-averaged treatment effects. The weighted-averaged differences in slopes between the Group Treatment and IBT conditions were non-significant for Weeks 1-3 (r^2 = .024) and Weeks 8-12 (r^2 = .001) three-class LCPMM (see Table 5), suggesting that the decreases in the probability of alcohol use in the IBT condition over time were not significantly different in the Group Treatment condition during those periods. While the overall difference in slopes from Weeks 3-8 was statistically significant (with steeper increases in alcohol use from Weeks 3-8 for Group Treatment participants; r^2 = .040), the

overall difference at termination (i.e., Week 12) between the IBT and Group Treatment conditions was non-significant ($r^2 = .002$). These results suggest that under the LGM framework, Group Treatment appears more efficacious than IBT; however, under the LCPMM framework, Group Treatment was not differentially efficacious from IBT.

Discussion

The goals of this article were to (a) highlight the complexities of modeling member interdependence in the presence of continual turnover in group therapy contexts that have rolling admissions, (b) describe methods typically used to address these issues and their limitations, and (c) present a more defensible approach to model data drawn from trials that use rolling groups. We have drawn from recent advances in missing data theory and modeling of unobserved categorical latent variables to make the case that LCPMM is a defensible (and possibly necessary) alternative to standard Longitudinal Growth Modeling approaches for this particular (and fairly common) clinical situation, both of which were compared in this article. LCPMMs handle differences in attendance that are attributable to data that are missing (i.e., non-ignorable missingness) in addition to variability in treatment effects across attendance patterns for individuals within and across treatment groups (Hedeker & Gibbons, 1997; Schafer & Graham, 2002) but does so with a finite set of latent attendance patterns (Lin et al., 2004; Muthén et al, 2003). In this study, we have added a measure of the point of treatment trial entry to aid in the estimation of attendance class membership to allow for the possibility that the "composition" of the treatment group (i.e., differences in the proportion of people from each latent attendance class) varies at different points of the trial.

In comparing results from LGM and LCPMM (albeit in the absence of covariates that would typically be incorporated in treatment effect estimation), we find differences in the assumptions made about attendance patterns and rolling group structure have an effect on differences in the *inferences* made under both frameworks. For example, under standard group cluster-correlated LGM, significant differences in changes over time in the probability of alcohol use are apparent under the standard missing-at-random assumption. The Getting Along group therapy pilot intervention appears more effective than IBT in decreasing alcohol use at end-of-treatment.

However, differences in the effect of GA versus IBT were, for the most part, non-significant under the LCPMM framework; both interventions were not significantly different in reducing alcohol use across the 12-week treatment period. This reduction in the treatment effect under LCPMM is consistent with the assertion that alternative solutions for handling rolling group data (outside of conventional LGMs) may lead to more conservative tests of treatment effects (Morgan-Lopez & Fals-Stewart, 2006a), with preliminary work suggesting that standard LGMs estimated under MAR may be overly liberal in estimating treatment effects with rolling group data (Morgan-Lopez & Fals-Stewart, 2006b).

While clearly the application of pattern mixture models to clinical trial data are not new, there remained an assumption that may not be tenable for *conventional* pattern mixture models: the proportion of individuals in treatment, which are represented under each attendance pattern, is consistent *within any given period of a rolling group trial*. In this study, under the LCPMM framework, we have added a measure of the point of treatment trial entry to a) aid in the estimation of attendance class membership and b) to

allow for the possibility that the "composition" of the treatment group (i.e., differences in the proportion of people from each latent attendance class) varies at different points of the trial.

We found certain patterns of attendance[8] (i.e., erratic) do not occur with consistency throughout the life of the trial as do other latent attendance patterns (i.e., always attenders, dropouts). This erratic group is most interesting, particularly because 50% of the people in this group either entered the group during Weeks 1-5 of the trial or between Weeks 41-45 of the trial. In mapping the trial weeks back to calendar dates, these weeks correspond to the Winter holiday season and the end-of-summer/Labor Day/Start-of-School. As a result, incorporation of the week of trial entry as part of modeling attendance class membership may allow for the estimation (and incorporation into overall treatment effects) of "holiday effects", treatment effects and attendance patterns that may only occur *during certain predictable periods during the calendar year*. It is also interesting to note that, of all three attendance pattern groups, the erratic group had the largest difference in treatment effects in the desired direction (See Table 4), though caution is warranted as this group does have the smallest sample of all the attendance groups.

These class-specific results may also suggest that individuals with different patterns of treatment attendance may benefit from different modes of treatment delivery. For example, individuals who stayed in alcoholism treatment seemed to benefit from either treatment condition, regardless of the mode of delivery (group or individual). Those who seek alcoholism treatment but may not be motivated to remain in treatment seem to have worse outcomes in group settings, though outcomes from individual therapy

are not ideal either. Finally, patients with erratic patterns of attendance have significantly better treatment outcomes when the treatment is delivered in a group setting. This may have implications for matching the mode of delivery to the patients' potential likelihood of engagement and retention in treatment, provided we could reasonably *predict* which type or attendance pattern will unfold for individuals over time within treatment.

Although we have noted the methodological implications of this article, the clinical implications are far more important and critical. In this illustration, we have made different *inferences* concerning (lack of) differences in the efficacy of two approaches to treating alcoholism from a pilot study with a rolling admission structure. This raises at least two questions; the first question is "Which results are we to believe?" In asking this question, we are essentially asking the question "Which set of assumptions of each analytic approach (LGM, LCPMM) are closest to the reality of how rolling treatment groups work?" Experts on missing data have weighed in and suggested that the assumptions of standard LGMs (i.e., missing-at-random) may not be sufficient in the case of treatment outcome studies *in general* (Hedeker & Mermelstein, 2000; Schafer & Graham, 2002) much less trials with rolling group structure. Results from preliminary simulation work on analytic approaches for rolling treatment groups are in line with this thinking (Morgan-Lopez & Fals-Stewart, 2006b).

If it is indeed the case that LCPMMs give a more accurate picture of how rolling treatment groups work and should be modeled, then the second question is "What are the implications of these results for the treatment research community?" Given the ubiquity of group therapy in general, and open enrollment paradigms in particular, it is incumbent upon the research community to apply an analytic model that will lead to defensible

inferences. The failure to do so can have substantial consequences, leading to advocacy of interventions that appear more effective than alternatives when, in fact, they are not.

With federal agencies promoting and more research funds being directed toward group therapy research, more and more clinical trials will appear in the empirical literature that use rolling groups. Of course, this is a welcomed occurrence in that clinical trials will more accurately reflect clinical practice. However, investigators are cautioned to apply analytic models that can adequately capture the changing membership structure of groups so as to avoid important inferential errors. If the LCPMM approach we are advocating is used as defensible alternative to the more standard LGM, it is important to emphasis that it is comparatively conservative. As such, larger samples will likely be needed under LCPMM versus LGM for adequate statistical power, though work on required sample sizes for adequate power in the case of rolling therapy groups is very much in its infancy (e.g., Morgan-Lopez & Fals-Stewart, 2006b). The differences in the methodologies must also take into account the differences in *effect sizes* across approaches, such as the differences we have observed in this study.

We also recognize that many clinical investigators are not versed in LCPMM and may avoid it use despite its validity in this situation. This is not so dissimilar to the introduction of multilevel and standard latent growth models over a decade ago, which were initially resisted due to what was viewed as their complexity, but are now fairly commonplace in the empirical clinical research. Although LCPMM may be complex analytically, it is an approach that is readily accessible to applied researchers[8]. It is also an approach that can be used for different types of treatment trial designs (e.g., group treatment v. no-treatment comparison conditions - provided contact during data collection

can be reasonably considered a "session"); also, additional procedures for weighted averaging of parameters across classes can be done when the treatment condition *is* related to class membership (see the technical appendix; see also Hedeker & Gibbons, 1997, p.74-76).

Categorical versus Continuous Latent Attendance

With recent advances in latent variable modeling, there now exists the possibility of modeling interactions with continuous latent variables (Mushin & Muthén, 1998-2006); there may be interest in modeling treatment effect differences across a continuous latent attendance construct as opposed to a categorical latent construct as we have examined in this study[9]. However, our concern about proposing a continuous form of latent attendance (i.e., a "regular" latent variable for "attend") is two-fold: first, such an approach would force an assumption of a constant linear increase or decrease in the strength of the treatment effect across this continuous latent attendance variable (e.g., as you move up the continuum from "dropout" to "full attender", there is a constant shift in the treatment effect). If this was the case, then we would have expected to see this tendency in our data manifest itself in the rank-ordering of treatment effects from the three classes, with the estimates from the erratic group in the middle of the rank-ordering from largest-to-smallest (e.g., attenders → erratics → droppers) or smallest-to-largest (e.g., droppers → erratics → attenders). Such an approach also runs counter to the way missing data experts have conceptualized missing data patterns under NMAR (i.e., categories), either through observed missing data groupings (Demirtas & Schafer, 2003; Hedeker & Gibbons, 1997) or latent missing data groupings (Lin et al, 2004; Muthén et al., 2003).

Conclusion

Treatment researchers wish to examine the therapeutic effects of rolling groups, but have not had the necessary analytic tools to model the resulting data in a statistically valid fashion. This has placed investigators in the untenable position of either using analytic approaches that may be more likely to lead to incorrect inferences, in spite of "doing the best they can with what they have" in many cases (e.g. Fals-Stewart et al., 1993, 2004), or avoiding the problem altogether by not including rolling therapy groups in their designs. Thus, despite the call from federal agencies and community treatment providers for more research on group therapy, the lack of a solution to analytic challenges of rolling group data has had a stifling effect on this area of research (NIDA, 2003). Although it would be naïve to suggest that analytic barriers are the only ones hindering group therapy research, it has be recognized as a major hurdle (Morgan-Lopez & Fals-Stewart, 2006a; NIDA, 2003; Weiss et al., 2004) and, as such, a concerted effort to address these problems is clearly needed. Ultimately, it is our hope that work on this approach for handling data from substance abuse treatment trials with turnover resulting from open enrollment (along with work on promising approaches from other areas) will spur continued discussion and thought about issues surrounding open enrollment paradigms among treatment researchers and methodologists alike. Such a dialogue will hopefully help move treatment research and treatment-in-practice closer together in terms of ecological validity (NIDA, 2003).

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Footnotes

[1] More specifically, if there *is* change in group membership composition (i.e., due to dropout) the changes have negligible impact because the probability of missingness is fully accounted for by non-missing variables and, as a consequence, a single treatment effect is estimated for the entire population (conditional on missingness), regardless of when or if they dropped out (Hedeker & Mermelstein, 2000). This assumption is likely untenable in practice for clinical trials (Schafer & Graham, 2002) and specifically for rolling group trials (Morgan-Lopez & Fals-Stewart, 2006a).

[2] For the purposes of this article, group cluster-correlated LGM refers to LGM for longitudinal data within groups with the sole purpose of standard error adjustment for non-independence of repeated measures among individuals within groups without an explicit growth model for the group-level. This is in contrast to models where there *is* an explicit model for the group-level (i.e., an explicit "three-level" model).

[3]We use "missingness" and "attendance" synonymously, at the risk of overlooking one clear case where they are not synonymous: namely, the condition where individuals show up for group but do not respond to a particular item (i.e., item-level missingness when they *did* show up). However, w*ithin a given attendance pattern*, item-level missingness is assumed to be missing-at-random, even under models that handle non-ignorable missingness (see Lin, McCulloch & Rosenheck, 2004).

[4]The single-class (group cluster-correlated) LCPMM and standard (group cluster-correlated) LGM under the assumption of data missing-at-random produce equivalent results (Muthén, B.O., personal communication, 17 March 2006). However, single-class LCPMM was used as the standard LGM analog in order to preserve the same number of

variables (i.e., retaining the missing data/attendance indicators that are unnecessary, and thus "ignorable", under standard LGM under ignorability of missingness) in all models for the purpose of comparing Bayesian Information Criterion (BIC) values across models with varying numbers of classes.

[5]Caution is warranted in interpreting growth parameters from missing data classes with very low proportions of data. The parameters that coincide with extremely low proportions of missing data (i.e., Week 12 intercept and Week 8-12 slope for the Droppers class) are based on extrapolation from other data points, under the assumption that the estimated trajectory based on non-missing data reasonably represents the *projected* trend for data points that are missing (Schafer, J.L., personal communication, 25 January 2007).

[6]Weighted averages of the attendance class-specific growth parameters must be calculated external to the analysis, as there are no current alternatives to represent mixtures of parameter estimates *within* the analysis in LCPMMs (Muthén, B.O., personal communication, 21 March 2006).

[7]Overall class proportions are used for weighted averaging since the proportions do not differ significantly across treatment conditions. However this approach can accommodate differences in class proportions across treatment conditions (see Hedeker & Gibbons, 1997, p.73-74; see also the SAS Proc IML program in the technical appendix)

[8]Mplus v4 code for this analysis available upon request.

[9]As suggested by an anonymous reviewer.

Table 1

Pretreatment Characteristics of Men Participating in the Investigation

Characteristics	GA/Group Treatment (N = 64)	IBT (N = 64)
Mean (SD)		
Age	40.44 (10.02)	40.60 (9.95)
Years of education	14.01 (2.00)	13.64 (1.95)
Years married/cohabitating	10.25 (6.68)	9.86 (7.34)
Number of children	1.95 (1.86)	2.02 (1.94)
Annual family income (in thousands $)	39.21 (27.84)	38.63 (29.55)
Years of problematic alcohol use	4.90 (1.84)	4.83 (1.90)
Number (percentage)		
Racial/Ethnic composition		
White	41 (64)	39 (61)
African-American	13 (20)	16 (25)
Hispanic	8 (13)	7 (11)
Other	2 (3)	2 (3)

Note. GA = Getting Along group treatment condition; IBT = individual-based treatment condition: DSM-IV = *Diagnostic and Statistical Manual of Mental Disorders, 4th Edition.*

Table 2

Latent Class Pattern Mixture Model Global Fit Statistics

Classes	BIC	Entropy
One	3942	-
Two	3398	.973
Three	3382	.991
Four	3409	.944

Notes. BIC = Bayesian Information Criterion; smallest BIC value is favored. Entropy = summary measure of the average probability of each individual being in the latent class in which they were ultimately classified. Values of 1.0 indicate "perfect" certainty in latent class membership.

Table 3

Structuring of Three-Piece Linear Model Time Coefficients

Week	Time Coefficients			
	Week 1-3 Slope (β_1)	Week 3-8 Slope (β_2)	Week 8-12 Slope (β_3)	Week 12 Intercept (α)
1	-2	-5	-4	1
2	-1	-5	-4	1
3	0	-5	-4	1
4	0	-4	-4	1
5	0	-3	-4	1
6	0	-2	-4	1
7	0	-1	-4	1
8	0	0	-4	1
9	0	0	-3	1
10	0	0	-2	1
11	0	0	-1	1
12	0	0	0	1

Notes. Time coefficients are structured such that interpretation of the slope for each measure is the estimated change on a given measure across one week for the indicated period.

Table 4

Attendance Class-Specific Probit Regression Coefficients (and Standard Errors)

Effects	Droppers (N = 34)	Show-ers (N = 78)	Erratics (N = 16)
Conditional Growth Parameter			
Means (*SE*)			
Weeks 1-3	.290(.148)	-.732(.137)***	-.394(.298)
Weeks 3-8	.012(.133)	-.194(.060)***	.295(.109)**
Weeks 8-12	-.334(.283)	-.194(.114)	-.456(.135)***
Week 12	.148(.547)	-2.616(.422)***	.046(.506)
Differential Treatment Effects (*SE*)			
Weeks 1-3	-.572(.148)***	-.026(.137)	-.529(.617)
Weeks 3-8	.165(.133)	.302(.060)***	-.534(.111)***
Weeks 8-12	.553(.283)	-.189(.113)	.142(.136)
Week 12	1.185(.549)*	-.485(.422)	-1.473(.509)**

Notes. Conditional Growth Parameter Means correspond to the class-specific growth parameter means (i.e., probit regression intercepts) for the IBT condition. Class-specific differential treatment effects (i.e., probit regression slopes) indicate differences between IBT and Group Treatment (GA), with negative coefficients indicating favorable effects for the *GA* group treatment condition. *p<.05, **p<.01, ***p<.001. For Droppers, n_{IBT} = 15/n_{GA} = 19. For Show-ers, n_{IBT} = 38/n_{GA} = 40. For Erratics, n_{IBT} = 11/n_{GA} = 5.

Table 5

Comparison of Standard LGM and Weighted-Averaged Three-Class LCPMM Results

Effects	Standard LGM	Three-Class LCPMM
Conditional Growth Parameter		
Means (*SE)*		
Weeks 1-3	-.590(.101)***	-.418(.106)***
Weeks 3-8	-.064(.044)	-.078(.054)
Weeks 8-12	-.089(.055)	-.263(.104)*
Week 12	-1.121(.225)***	-1.548(.330)***
Differential Treatment Effects (*SE)*		
Weeks 1-3	.114(.101)	-.233(.120)[tt]
Weeks 3-8	.086(.044)[t]	.160(.057)*
Weeks 8-12	-.167(.055)***	.049(.108)
Week 12	-1.024(.225)***	-.165(.319)

Notes. Parameter estimates under Three-Class LCPMM calculated from the weighted averages of the attendance class-specific parameter estimates shown in Table 4. Standard errors for the Three-Class LCPMM were derived via the multivariate delta method (Bishop, Fienberg & Holland, 1975). Treatment effects (i.e., probit regression slopes) indicate differences between IBT and Group Treatment, with negative coefficients indicating favorable effects for the *GA* group treatment condition. *p<.05, **p<.01, ***p<.001. [t]p=.052, [tt]p=.051.

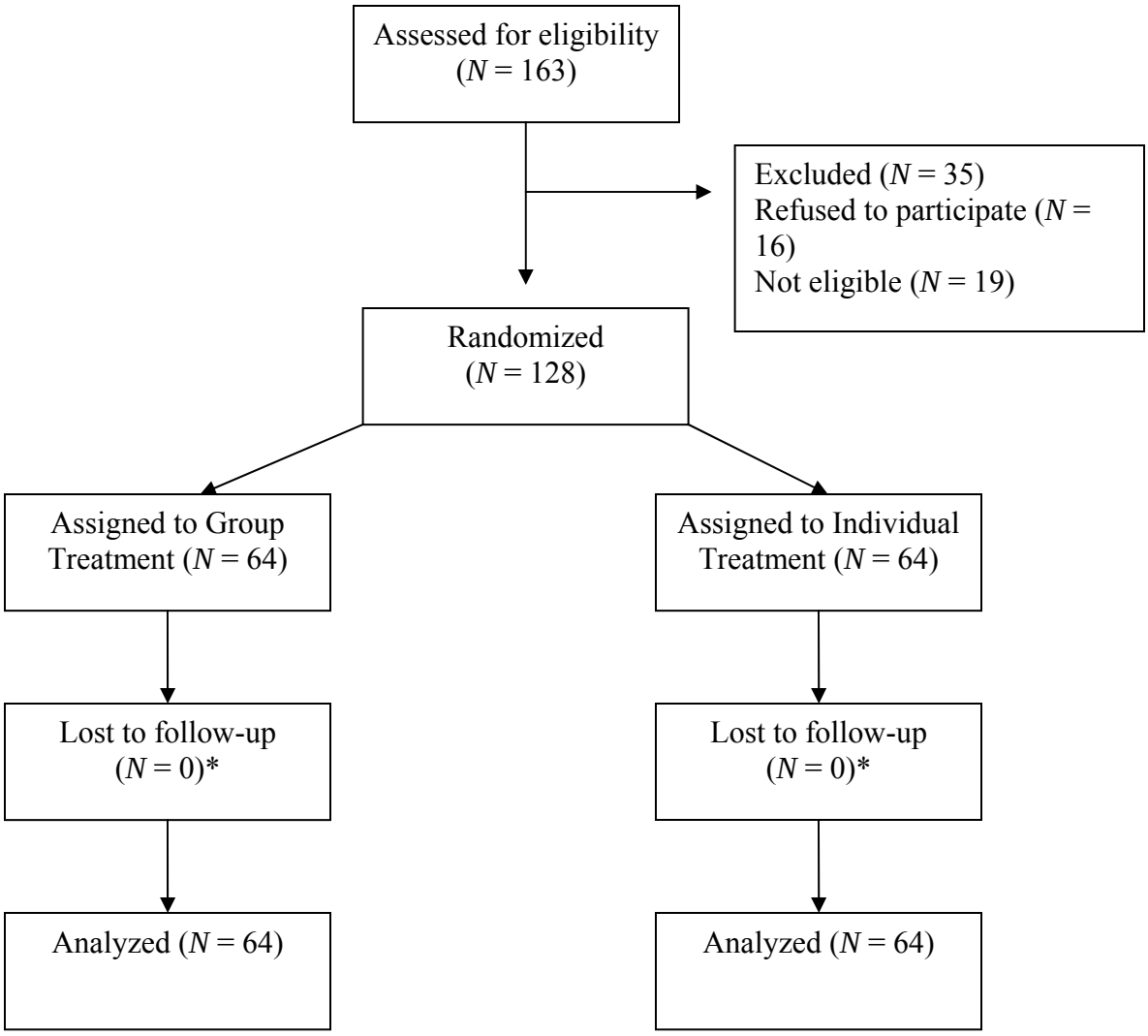
Figure Captions

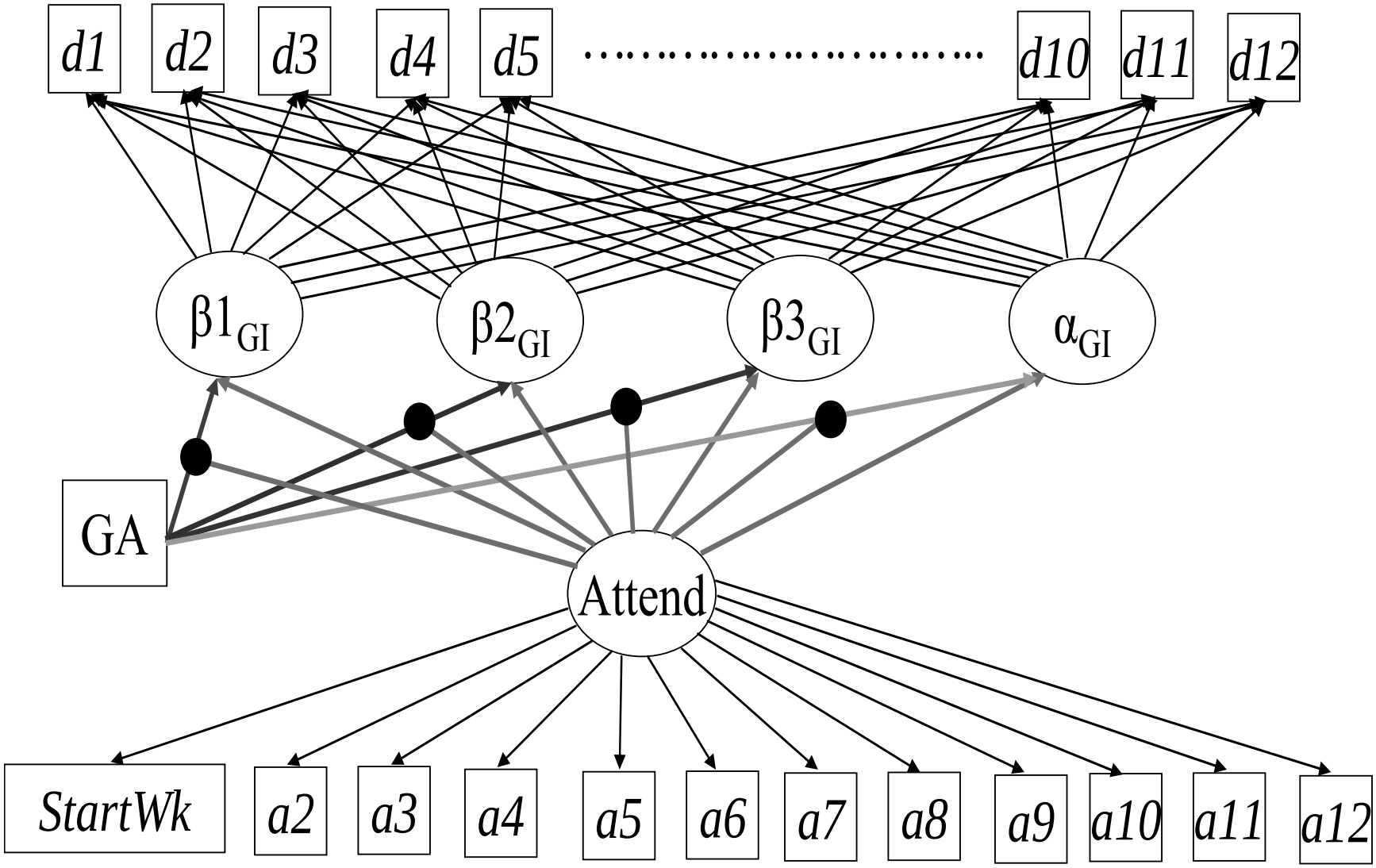
Figure 1.Flow diagram through phases of the trial. Follow-up data not relevant to the current analysis.

Figure 2. Latent Class Pattern Mixture Model. Attend = Latent Attendance Class Variable. d_1-d_{12} = Observed past week drinking from person weeks 1-12. a_2-a_{12} = Binary indicators of treatment group attendance from weeks 2-12. StartWk = The week that the trial was in when individual i joined the trial (range from trial week 1 to trial week 56). GA = Treatment condition (Getting Along = 1; IBT = 0). $\beta 1_{GI}$ = estimated rate of per week change in drinking from weeks 1-3. $\beta 2_{GI}$ = estimated rate of per week change in drinking from weeks 3-8. $\beta 3_{GI}$ = estimated rate of per week change in drinking from weeks 8-12. α_{GI} = estimated rate of drinking at treatment termination (i.e., week 12). Paths from "Attend" to the growth parameters (i.e., $\beta 1_{GI}$, $\beta 2_{GI}$, $\beta 3_{GI}$, α_{GI}) indicate that the conditional means of the growth parameters vary across attendance class. Paths from "Attend" to the GA $\rightarrow$ growth parameter links (as connected by the "dots") indicate that the differential treatment effects vary across attendance class.

Figure 3. Week-to-Week Treatment Attendance Probabilities by Latent Attendance Classes.

Figure 4. Distributions of the Week of Trial Entry by Latent Attendance Classes. The distributions of the proportions for each attendance class sum to 100% *across* the weeks of trial entry. For example, of the participants that were classified in the Erratic class (n = 16), 50% of these 16 participants began treatment between Weeks 1-5 (25%) or Weeks 41-45 (25%) with the remaining bars accounting for the remaining 50% of the Erratic class.


Assessed for eligibility
(N = 163)
Excluded (N = 35)
Refused to participate (N = 16)
Not eligible (N = 19)
Randomized
(N = 128)
Assigned to Group Treatment (N = 64)
Assigned to Individual Treatment (N = 64)
Lost to follow-up
(N = 0)*
Lost to follow-up
(N = 0)*
Analyzed (N = 64)
Analyzed (N = 64)


d1
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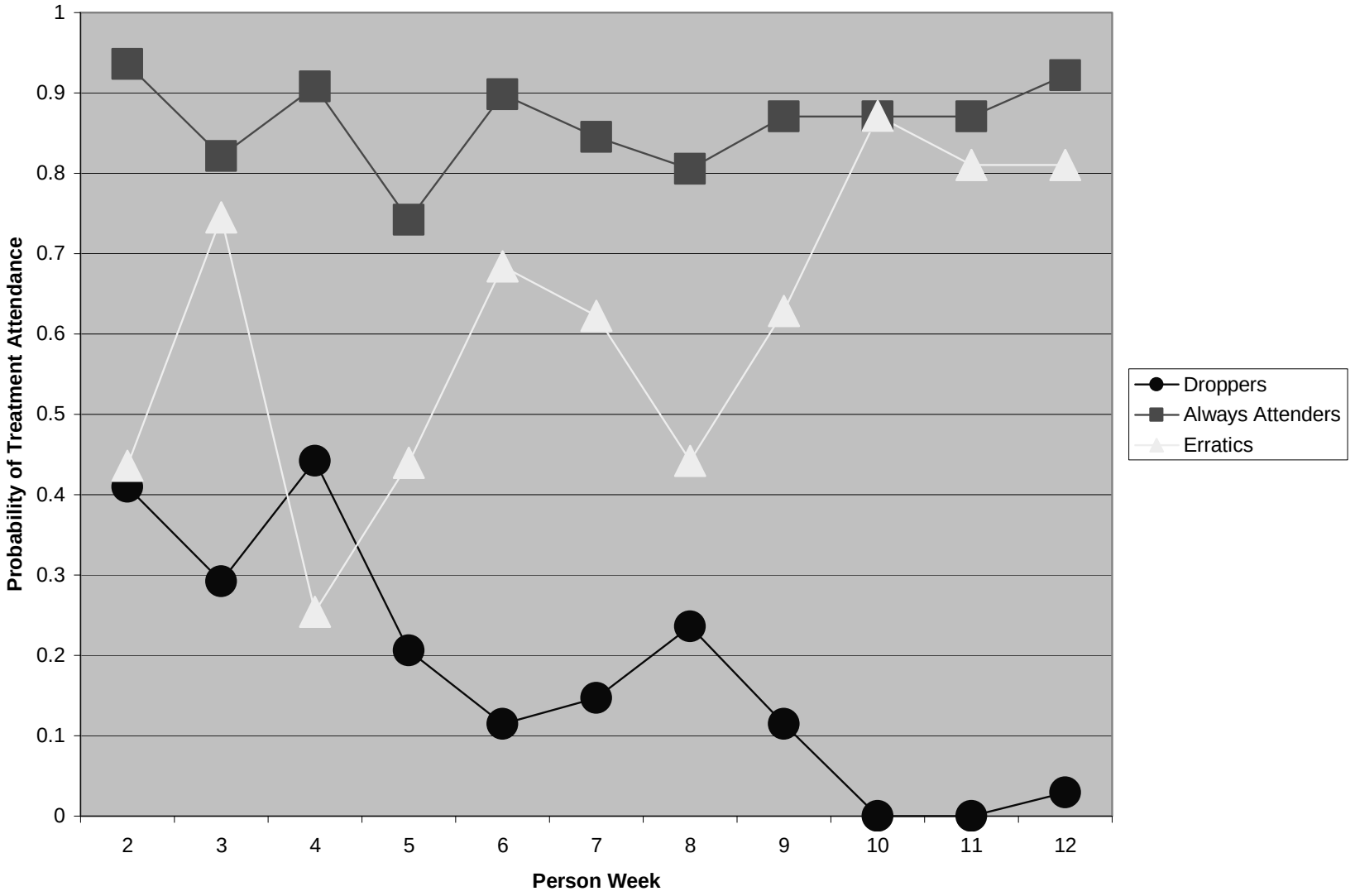
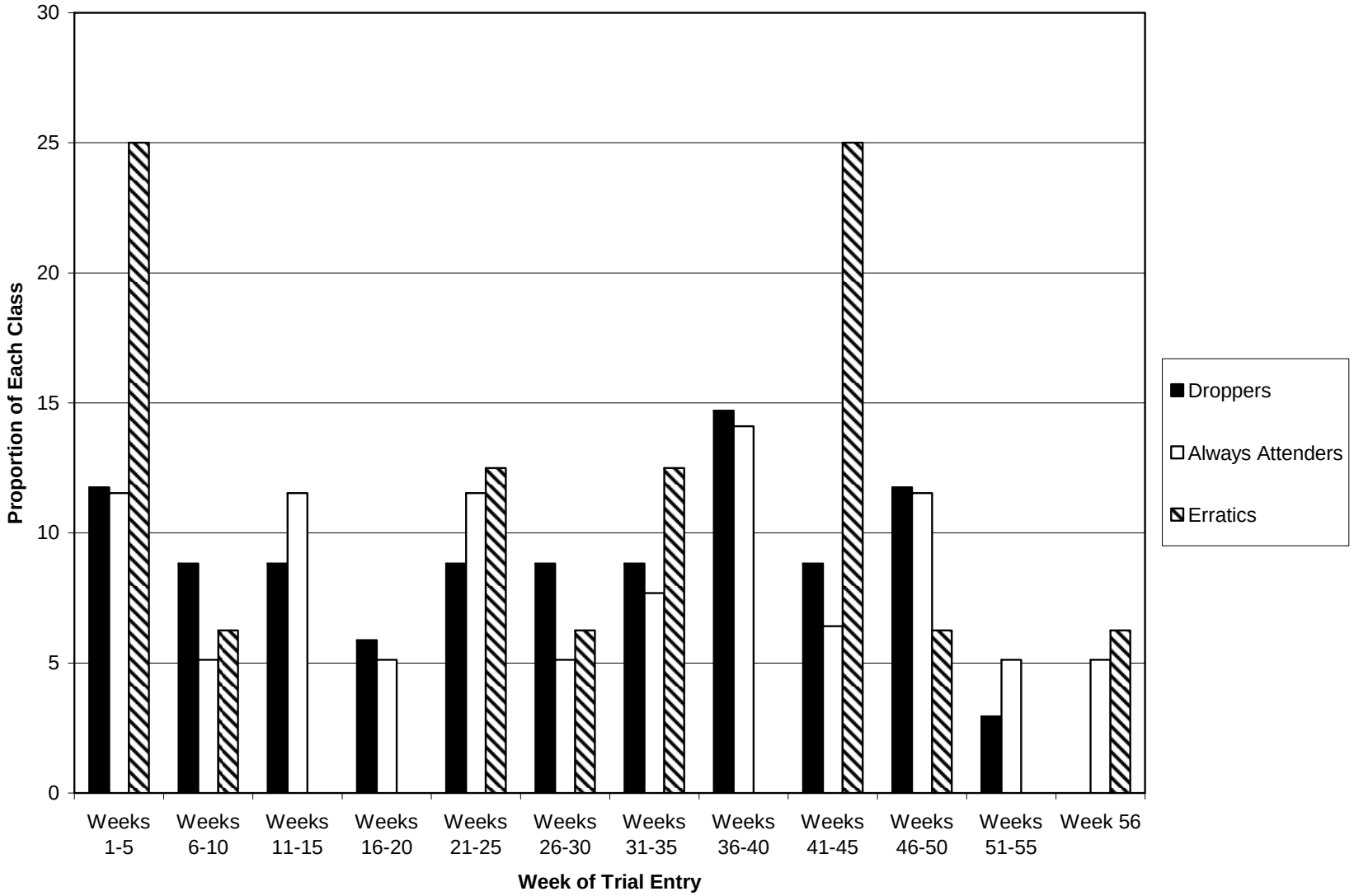
a11
a12


Probability of Treatment Attendance
1
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
2
3
4
5
6
7
8
9
10
11
12
Person Week
Droppers
Always Attenders
Erratics


30
25
20
15
10
5
0
Proportion of Each Class
Weeks 1-5
Weeks 6-10
Weeks 11-15
Weeks 16-20
Weeks 21-25
Weeks 26-30
Weeks 31-35
Weeks 36-40
Weeks 41-45
Weeks 46-50
Weeks 51-55
Week 56
Week of Trial Entry
Droppers
Always Attenders
Erratics

Appendix: Weighted Averaged Standard Errors via the Delta Method

Delta method. The delta method is used to derive the variance of functions (e.g., sums, products, sums of products) of normally distributed random variables (e.g., regression coefficients). The asymptotic distribution of the estimator (i.e., f(**θ'**)) is given by (Bishop et al., 1975, p.493):

$$L[n^{1/2} (f(\theta') - f(\theta))] \rightarrow N \{\theta, ((\partial f/\partial \theta) \Sigma(\theta) (\partial f/\partial \theta)')\} \qquad (1)$$

Where f(θ') is a single function of interest, $\partial \mathbf{f}/\partial \boldsymbol{\theta}$ is an n x q *matrix* of partial derivatives where n = the number of functions and q = the total number of elements that appear at least once within *any* of the n functions (i.e., a Jacobian matrix). Σ(θ') is a q x q covariance matrix among the parameters. The functions of interest in the present case are the weighted averages of parameters (i.e., conditional mean growth parameters, treatment effects) across the three attendance classes; let our functions of interest replace θ':

$$\theta' = \sum_{K=1}^{K} \pi_K \gamma_K \qquad (2)$$

where:

π_K = proportion for attendance class K (where K = 3; Droppers, Always Attenders (aka Show-ers) and Erratics respectively)

γ_K = parameter of interest (i.e., conditional growth parameter means (α, β_1, β_2, β_3), differential treatment effects ($T\alpha$, $T\beta_1$, $T\beta_2$, $T\beta_3$)) within class K

Deriving the variance of the weighted averages of each parameter requires a) the Jacobian matrix of partial derivatives with respect to $f_n(\theta')$ (i.e., $\partial \mathbf{f_n}/\partial \boldsymbol{\theta}'$) and the covariance matrix among the parameters (i.e., $\Sigma(\boldsymbol{\theta}')$), the estimates of which come directly from the Latent Class Pattern Mixture Modeling output.

The partitioned Jacobian matrix is:

$$\begin{bmatrix} W & X & Y & Z \end{bmatrix}$$

Where:

W = $\frac{\partial f_N}{\partial \gamma_D}$ (8 x 8 sub-matrix of partial derivatives (for the 8 functions with respect to the 8 parameters) for the Droppers class)

X= $\frac{\partial f_N}{\partial \gamma_S}$ (8 x 8 sub-matrix of partial derivatives for the Show-ers class)

Y = $\frac{\partial f_N}{\partial \gamma_E}$ (8 x 8 sub-matrix of partial derivatives for the Erratics class)

Z = $\frac{\partial f_N}{\partial \pi_K}$ (8 x 3 sub-matrix of partial derivatives for estimated class proportions)

The covariance matrix among the estimates is pre- and post-multiplied by the matrix of partial derivatives of the functions of interest.

$$\begin{bmatrix} W & X & Y & Z \end{bmatrix} \begin{bmatrix} \sum\gamma_D & \sum\gamma_S\gamma_D & \sum\gamma_E\gamma_D & 0 \\ \sum\gamma_D\gamma_S & \sum\gamma_S & \sum\gamma_E\gamma_S & 0 \\ \sum\gamma_D\gamma_E & \sum\gamma_S\gamma_E & \sum\gamma_E & 0 \\ 0 & 0 & 0 & \sum\pi_K \end{bmatrix} \begin{bmatrix} W \\ X \\ Y \\ Z \end{bmatrix}$$

The square root of the diagonal elements of this new 8 x 8 matrix are the standard errors for the corresponding effect denoted for the function f_N (i.e., the 8 weighted-averaged parameters). (Additional detail and SAS Proc IML code available by contacting the first author or downloading the technical supplement from www.addictionandfamily.org).