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LATENT TRAJECTORY ANALYSIS OF A HOMELESS MENTAL HEALTH PROGRAM

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Abstract

Intervention analyses which incorporate temporality over a followup period typically note differences in the patterns of "single-curves" for each the experimental and control groups or differences in temporally-based taxonomies between experimentals and controls. But the former fails to allow for the possibility of subgroups of multiple trajectories and the latter collapses time (e.g., average spell durations) and arbitrarily creates cut-points to form its taxonomies. This paper investigates the utility for intervention research of using latent class growth analysis (LCGA). This method incorporates the more complete temporal information used by single-curve approaches to statistically identify the multiple subgroups at the heart of the taxonomic approach. We do this by reanalyzing a critical time intervention study (CTI) of homeless mentally ill men that used both single-curve and taxonomic approaches. By finding, among other things, differences between experimentals and controls in the number, sizes and patterns of latent subgroups than were found in the prior analysis, we suggest the utility of LCGA for assessing service interventions.

Keywords: intervention effects, latent growth class analysis, sequence analysis, homelessness, mental health, critical time intervention

Introduction

Intervention research commonly estimates impact over some post-intervention time period, allowing us to identify temporal differences between intervention and control groups. This research, however, does not typically differentiate temporal patterns among individuals resulting from the intervention. In this paper, we suggest the utility of latent class growth analysis[1, 2] for gauging different effects on individual life trajectories. We do this by re-analyzing data from a randomized critical time intervention (CTI) trial designed to prevent recurrent homelessness among men with mental disorders. The prior analysis,[3] used an 18-month observation period to identify several temporal effects: the intervention group averaged 61 fewer nights homeless; it had half the number of homeless episodes; and its differences from the control group in the probability of extended homelessness grew over time. This paper carries the analysis further by demonstrating how latent class growth analysis can identify different, latent, temporal patterns among individuals resulting from the intervention. By using more of the temporal information in the data and by allowing individuals to vary from each other over time, this analysis portrays a more nuanced dynamic of temporal change than the prior analysis. As a result, it better specifies how subgroups, characterized by different dynamics, were differentially affected by CTI.

Background

Time in homeless intervention research. Intervention research usually focuses on estimating impacts over time without differentiating patterns among individuals. Most commonly, studies estimate differences between test and control/comparison groups in means, proportions and standard deviations at two or more post-intervention time points.[4-11] Point-in-time measures, however, elide temporality, and taking several such measures at a few post-intervention time points (e.g., at 6, 12 and 18 months) may produce too small a sample of the universe of time points program models imply. Further, we do not know if these are the right time points to be sampled. Choices seem to be driven by weak theoretical expectations and strong emphases on culturally accepted time points for a twelve-month calendar. More sophisticated but less common approaches have used time series analysis on a cross-sectionally measured time series on the outcome of interest;[12] random effects models to evaluate change over the post-intervention time period;[13] or, as in the CTI analysis, survival analysis to construct probability curves of post-intervention response.[3] These approaches offer great advances in estimating impact over time but, like point-in-time analyses, they allow only one curve each to be found for test and control groups. They in effect assume variation around the relevant statistic(s) is random. These "single-curve" approaches do not differentiate subgroups of individuals within the test and control groups on the basis of shared responses on the outcome of interest. (Subgroup analyses are possible, of course, but subgroup membership is defined by shared gender, ethnicity, education and so forth, rather than by similarities in the outcome of interest.)

Efforts to differentiate such subgroups have created taxonomies based on individuals sharing similar values on the outcome of interest (alone or combined with other traits) over the entire

post-intervention time period.[3, 14, 15] One approach has been to aggregate or average data for each individual over the post-intervention period and create cut-points to form subgroups. For example, total nights homeless, number of homeless spells and average spell duration are common measures. Cut-points for one or some combination of measures are then subjectively created, generating subgroups. Commonly, a taxonomy of transient, episodic and chronic homeless is developed.[14, 16-18] Aggregating data, however, fails to show intervention impact on ordering and timing of the outcome of interest. It does not distinguish, for example, someone homeless the first three months of follow-up from someone homeless any other three month period. Thus, aggregation does not use available temporal information that helps us better specify impact. Further, cut-points are commonly not derived from either theory or data but created using arbitrary decision rules. This risks creating subgroups that are not very meaningful for assessing the theory and practice of an intervention and may generate significant within-subgroup heterogeneity, making it harder to find effects. Last, because these taxonomies collapse time, they fail to achieve what the "single-curve" studies accomplish: an analysis of the impact on the post-intervention temporal trajectory. These taxonomies succeed, however, where the single-curve approach does not: differentiating subgroups of individuals over follow-up.

CTI study. The CTI analysis assessed impact using both approaches. Survival curves were constructed for experimental and control groups to elucidate differences in homelessness trends over follow-up. This period of 540 days was divided into 18 periods of 30 days (a "month"), and the probability of a major homeless episode (> 30 days) starting in a month for those housed at the beginning of that month was calculated. Cumulative survival probabilities were then calculated, one each for the experimental and control groups. The former declined from a 100% to an 80% chance of retaining housing at the eighteenth month; the latter from 100% to 50%. The study focused on the finding that the difference between the two curves grew over time, in contrast to previous clinical trials of community mental health interventions where effects faded after the intervention period.

The authors also created a taxonomy of homelessness to evaluate experimental/control differences. Four subgroups were arbitrarily defined for the entire sample based on the total number of nights individuals were homeless over follow-up: extended (> 54 nights homeless), intermediate (30-54 nights), transient (1-29 nights) and never (0 nights). The fourth subgroup was not part of the analysis. To test for impact, the distributions of experimentals and controls across subgroups were compared. The major reported finding was that extended homelessness for the experimental group was half that in the control group (21% v 40%); no differences were found for other subgroups.

Taxonomic analysis showed individuals varied in their homelessness; survival analysis showed change over time in homelessness. Taken together, these suggest individuals differed in their temporal patterns of homelessness. By locating one curve for each group, survival analysis implies such differences are random, even as taxonomic analysis suggests they are not. By eliding time, taxonomic analysis leaves open the possibility that individuals did not change over time, even as survival analysis suggests they did. To address whether individuals differed in their patterns of homelessness, we need to simultaneously examine individual change over time and variation among individuals at each time point.

Our approach. We use a technique that does both: latent growth class analysis (LCGA). This method allows us to identify subgroups of individuals who have similar post-intervention trajectories; who share, that is, similarities in timing, duration and sequencing of the outcome of interest over follow-up. It combines reasoning in the two approaches just reviewed. Like

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the taxonomic approach (but in contrast to "single-curve" analyses), LCGA assumes that heterogeneity across individuals in the follow-up period is due to a mixture of distributions in the population representing different underlying subgroups, rather than that heterogeneity is random. Thus, as with the taxonomic approach, LCGA leaves us with latent classes of individuals sharing similar temporal qualities. Like single-curve analyses (but in contrast to taxonomy formation), LCGA uses information on individuals at each measured, post-intervention time point to identify trajectory patterns and uses statistical reasoning rather than subjective judgments to arrive at results. Thus, as with survival, time series and growth curve analyses, LCGA leaves us with statistically-defined trajectories of behavior over the follow-up period. In the end, we have, for both experimental and control groups, subgroups of individual trajectory patterns, thereby producing a more exact understanding of the nature of intervention impact than single-curve or taxonomy approaches.

We carry out our study by re-analyzing data from a well-known evaluation of an intervention designed to prevent recurrent homelessness among mentally ill men.[3] We chose this study because it (a) was well-designed and well-executed, with validated measures and tight program and study procedures; (b) followed up over a relatively long time period; and thus (c) able to employ analyses representing both single-curve and taxonomy approaches to assess impact. Findings from each approach can be usefully compared with results from a latent trajectory approach. In particular, that the initial findings were strong provides a more robust test for the ability of the latent trajectory approach to find further impacts. Additionally, critical time intervention is thought to be a potentially valuable new approach for enhancing continuity of care and reducing risk of homelessness and other adverse outcomes among mentally ill persons following discharge from shelters, hospitals and other institutions. The CTI model has now been applied and evaluated with various populations (e.g., persons being discharged from inpatient psychiatric treatment, homeless mothers with children leaving shelters, homeless veterans treated by specialized outreach teams), and was recently cited as a model program by the President's New Freedom Commission on Mental Health.[19] Demonstrating utility of trajectory analysis in this context, then, would be particularly helpful for furthering continuing research on an important policy initiative.

Methods

Study design. The CTI study was a random assignment study assessing the impact on recurrent homelessness of providing particular services when mentally ill homeless people are transitioning from shelters to housing in the community. The intervention consisted of help strengthening ties to services, family and friends and providing practical and emotional support after leaving the shelter. Each person was assigned a CTI worker to implement a plan transferring care from the shelter to the community and to work with the person in the community for nine months after shelter discharge. After this time, the experimental group received "usual services", as did the control group throughout the 18 month post-shelter period. Usual services involved referrals to mental health and rehabilitation programs of generally high quality, as well as other referrals as needed (e.g., medical care; substance abuse treatment).

The sample was male residents discharged to housing in the community from an on-site, New York City shelter psychiatric program. All subjects had severe mental illness, such as schizophrenia or other psychotic disorders. Ninety-six of the 102 discharged from the program over a two year period participated in the intervention and study. They were randomly assigned to receive either CTI or usual services after post-program housing

placement. After subjects left the shelter, data were collected at 30-day intervals over the 18 month period through structured surveys by trained interviewers blind to experimental or control status of study participants. At initial data collection and at each 30-day assessment, housing data were collected, with high test-retest reliability (kappa = 0.93). The baseline interview collected demographic data (age, education, race/ethnicity), lifetime histories of mental health and alcohol and drug abuse, and information on current psychiatric diagnoses and symptoms. Table 1 shows baseline characteristics of the sample for experimental and control groups. There were no statistically significant differences between groups at $p \leq .05$.

Outcome. Our outcome measure consists of trajectories of homelessness over the observation period, divided into 18 months of 30 days each. A person was considered homeless for an entire 30 day period if he resided in a shelter, on the street, or in any other public place for just one night during that period. The intervention is thus held to a high standard: preventing a single night of homelessness in a given month. Less stringent criteria (≤ 5, ≤ 10 and ≤ 15 nights homeless) produced essentially similar results, albeit with lower statistical power. This threshold approximates that used to construct the CTI taxonomy: at least one night homeless over follow-up placed an individual in one of the three homeless subgroups. It is a somewhat lower threshold than that used in the survival analysis: individuals were counted homeless for a 30-day period if they began that period homeless and were or remained homeless at least 30 days.

Analytic Approach. We first re-analyzed CTI data to replicate descriptive statistics reported in the original article. We then analyzed the data using latent class growth modeling. This approach assumes a population is composed of a mixture of subgroups with distinctive patterns of behavior over time; in this case, patterns in sequence, duration and timing of homelessness. Membership in a subgroup, however, is unobserved; only the mixture of subgroups is observed. Consequently, it is not possible to directly estimate each subgroup's size and pattern of exposure to homelessness over time. Rather, subgroup membership is inferred from the data. In latent class growth modeling, this heterogeneity is captured by an unobserved (latent) categorical variable, and observed outcomes at each time point— homeless status each month—serve as indicators of the latent class variable. Since subgroup membership is unobserved, the proportion of cases in each class is unknown and must be estimated by the model, as must the conditional item probabilities for each class. These models and some applications are described by Muthén[1, 20-23] and by Nagin[2, 24] (for critiques, see Bauer & Curran).[25]

Models were estimated by maximum likelihood using the EM algorithm as implemented by Mplus, version 3.11.[26] We relied on several statistics for evaluating model fit. The Bayesian Information Criterion (BIC)[27] rewards more parsimonious models (fewer latent classes) that more accurately reproduce the data and takes into account the number of parameters used in model estimation. Models having lower BIC values represent improvement over models with larger values. We also use the Lo-Mendell-Rubin likelihood ratio test (LMR LRT),[28] which adjusts the conventional likelihood ratio test for k versus $k+1$ classes for violating regularity conditions to evaluate the proper number of classes.[29] Entropy is a third model fit indicator, showing how well-separated are subgroups produced by the model.[30] It takes a value between 0.0 and 1.0; the closer to 1.0, the more distinct are the subgroups.

We first assessed trajectories for controls,[20] since they represent the counterfactual condition of having received only usual services rather than CTI. Results for experimentals were then modeled. In both cases, we compared models fitting two, three, four, and five trajectory

classes. Consistent with prior analysis of this randomized intervention, we report results not controlling for baseline covariates.

Results

Table 2 reports model fit statistics for controls and experimentals. Panel (a) shows a four class model provides the best fit for the control group. The reduction in BIC is substantial over the three-class model (50.6); the LMR LRT statistic suggests a significant improvement in fit (evaluated at .05 significance level); and the entropy value is excellent (.999). Adding a fifth class does not contribute appreciably to BIC and does not improve LMR LRT or entropy.

Panel (b) of table 2 suggests three classes are sufficient to characterize trajectories present among experimentals. The reduction in BIC over the two class model is large (52.8), the LMR LRT is significant at .05, and entropy is very high (.998). While the LMR LRT also indicates good fit for four and five class solutions, these additional classes do not strongly reduce BIC and result in poorer entropy values. Taken together, these results suggest the experimental group is most parsimoniously characterized by three trajectory classes. Thus, strikingly, only three classes are needed to account for the heterogeneity within the CTI group, while four classes are needed for controls.

For the selected models, figure 1 reports the size and nature of the classes for controls (top) and experimentals (bottom). It shows that three of the classes are similar in nature for both groups, but that class four among controls does not exist in the experimental group. While this trajectory does not contain many men, it is an important subgroup: those with a high probability of becoming homeless quickly after discharge and remaining chronically homeless throughout the observation period.

Further, the largest trajectory class within both groups (class one) is those unlikely to be homeless at any point during the observation period. In particular, a larger portion of experimental (79.1%) than control (60.4%) subjects fall into this class, indicating that, for certain individuals, CTI had an immediate and enduring impact in preventing subsequent homelessness.

Figure 1 also portrays a small class of experimental subjects (class two; 12.5%) that becomes increasingly homeless just after critical time services are ended at month nine. A similar pattern of increasing probability of homelessness exists for more controls (20.8%) but their movement into homelessness occurs earlier and is more linear than the J-shaped pattern found for the experimental group.

Finally, both control and experimental groups contain small subgroups with an inverted U-shaped pattern (class three). An increasing probability of homelessness is followed by a gradual decline reaching almost zero by the end of the observation period. The decline begins somewhat later among experimentals, however, and specifically around the time CTI services end.

Discussion

Some of these results reiterate findings from the prior analysis. This suggests our findings are not simply artifacts of the method. But other findings go beyond the prior analysis, deepening our understanding of CTI by both conflicting with and expanding on prior CTI findings.

Reiterative results. The prior and current analyses both indicate a large majority of men in the study had very low probabilities of becoming homeless over the observation period. That men receiving CTI were overrepresented in this subgroup suggests CTI was more effective than usual services in preventing enduring homelessness.

Also like the prior analysis, the current findings suggest intervention effects may last beyond the nine months of active service provision. The current analysis, however, specifies two different ways that such success may be achieved: the excess of CTI subjects not becoming homeless at all and the absence of chronically homeless among experimentals.

Deepening results. In their survival analysis, Susser et al. found a monotonic decline in the probability of housing retention among both experimentals and controls and an increasing difference between these groups in that probability. By contrast, our results suggest multiple subgroups exist within both experimental and control groups.

Further, these subgroups are different from those suggested by the taxonomy analysis in Susser et al. Specifically, the CTI group does not have a class of persistently homeless and has a class whose increasing homelessness is reversed. Only among controls does homelessness become chronic. These differences suggest the program had stronger effects than initially found: CTI (a) keeps people out of chronic homelessness and (b) dramatically improves housing chances for a subgroup prone to chronic homelessness during the early months of the intervention.

In addition, the subgroup withstanding homelessness only through continuing CTI services (class two) contrasts with the implication of the prior study that CTI effects persisted past the program period for *all* experimentals. Rather, this finding suggests these individuals may need a booster intervention,[31] or greater continuity of service (for example, referral to long-term follow-up by an assertive community treatment team). However, compared to the control group class which struggled against homelessness through usual services but failed earlier and at higher rates (class two), this CTI class showed the effectiveness of CTI for the subgroup of people that seem service dependent.

Limitations

The prior study identified several data limitations, which the reader can consult. Most important for our analysis is that the number of cases is relatively small. This is offset somewhat by the large number of follow-up periods and completeness of follow-up data. That the models fit well and that entropy values are high increases our confidence in the findings. Nevertheless, modest sample size has meant several subgroups contain few individuals. The results should thus be read as more demonstrating the utility of our analytic approach than definitively assessing CTI impact.

Our model evaluation should also be qualified. Reduction in BIC from k-class to k+1-class is the most commonly used index for assessing competing models, but no consensus exists on how large a reduction indicates significant change. Raftery[32] suggests reductions of at least 10 points, and BIC works quite well with small sample size. More research is needed, however, for a stronger consensus regarding BIC in this framework. Following Muthén, we also used LMR LRT to assess competing models, but the validity of testing non-nested models using this method is debated[33] and more research is necessary to better understand its utility in this context.

Implications

The approach demonstrated in this paper is very suggestive for subsequent intervention research. We focus on two implications. One is that we can examine covariate effects on trajectory subgroup formation and on change in trajectory subgroup membership. This will allow researchers to develop and evaluate interventions better tailored to specific trajectory subgroups rather than assuming a universal intervention for a particular population. Alternatively, non-responding subgroups to a universal intervention could receive targeted or booster interventions to improve impacts, and, following TenHave et al.,[34] randomized assignments could be incorporated at this stage to evaluate the impact of these added interventions. A second implication is the need to collect more precise temporal data. Our analysis makes clear the utility of knowing exactly when individuals enter and leave a particular state (such as homelessness) and not simply the number of times and durations of such states. Incorporating such data collection into intervention studies should become fundamental to this research.

Tables and Figures

Table 1. Demographic Characteristics and Diagnoses of Mentally Ill Men in the Experimental and Control Groups

Traits/ Diagnoses	Experimentals (n=48)		Controls (n=48)	
	No.	%	No.	%
Age				
< 35	18	38	20	42
≥ 35	30	62	28	58
Race/ethnicity				
African American	38	79	33	69
Other	10	21	15	31
Education				
< High school	26	54	31	65
≥ High school	22	46	17	35
Lifetime homelessness				
≤ 1 yr	7	15	14	29
> 1 yr	41	85	34	71
Psychiatric hospitalizations				
< 5	28	58	33	69
≥ 5	20	42	15	31
Psychiatric diagnosis*				
Schizophrenia	32	67	33	69
Other	16	33	15	31
Cocaine dependence*				
No	21	44	30	62
Yes	27	56	18	38
Alcohol dependence*				
No	20	42	24	50
Yes	28	58	24	50

Source: Susser et al.[3]
*Lifetime diagnosis

Table 2. Fit Indices for Latent Class
## Growth Analysis Models

(a) Controls

| No. of Classes | Fit Indices | | |
	BIC*	LMR LRT p-value**	Entropy
2	575.487	0.0001	1.000
3	502.893	0.0326	0.999
4	452.260	0.0330	0.999
5	451.790	0.0632	0.995

(b) Experimentals

| No. of Classes | Fit Indices | | |
	BIC*	LMR LRT p-value**	Entropy
2	380.569	0.0665	0.992
3	327.784	0.0003	0.998
4	326.964	0.0444	0.971
5	315.635	0.0170	0.960

 * BIC: Bayesian Information Criteria
 ** LMR LRT: Lo-Mendel-Rubin likelihood ratio test

Figure 1. Class Trajectories for Selected Models
(a) Controls
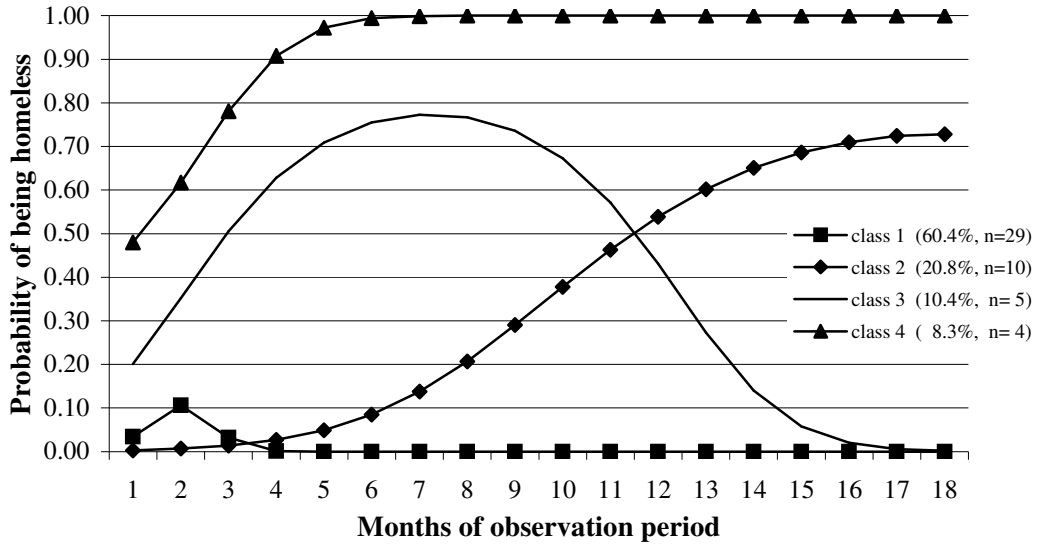
(b) Experimentals
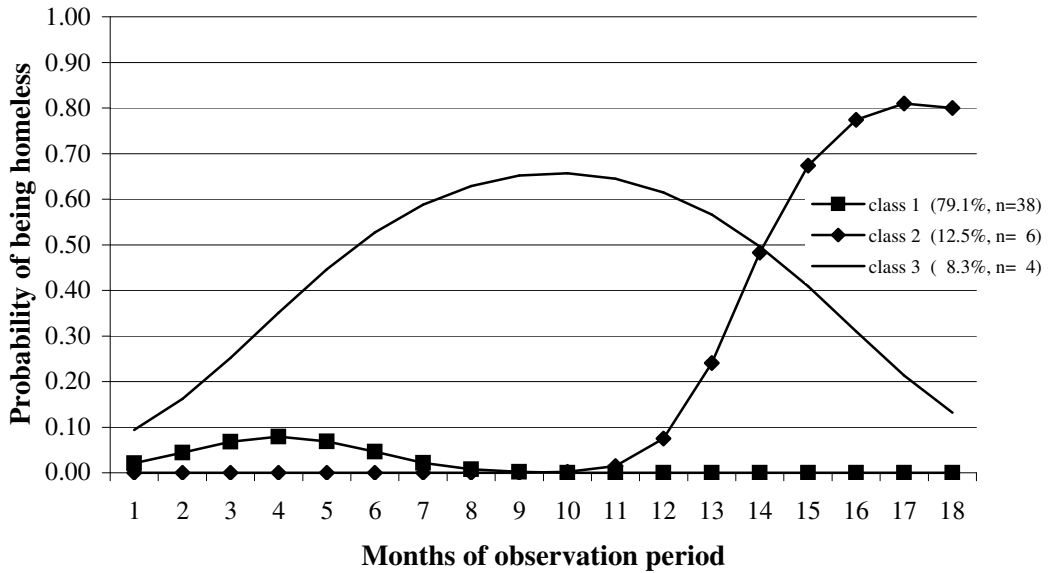
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