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A Longitudinal Evaluation of Prevalent Negative Beliefs About Residential Placement for Troubled Adolescents

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To investigate the validity of five prevalent negative beliefs about residential placement, we followed adolescents from a residential program and a comparison group at 3-month intervals for 4 to 8 years. This residential program in the Midwest uses the Teaching-Family Model in which six to eight adolescents live in a family-style environment. The interviews included five scales reflecting youths' views about important aspects of their lives in placement: (1) Delivery of Helpful Treatment, (2) Satisfaction with Supervising Adults, (3) Isolation from Family, (4) Isolation from Friends, and (5) Sense of Personal Control. Hierarchical linear modeling allowed us to estimate group differences while controlling for developmental trends, demographic factors, and prior differences between groups. The two groups were equivalent on all scales before the study. During the following placement, however, the treatment group's ratings were significantly more positive than the comparison group on four of the five scales and approached significance on the fifth. These findings suggest that negative beliefs about life in residential placement for adolescents may not apply to all programs.

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American sentiment toward residential placement for troubled youths is increasingly suspicious, pessimistic, and even hostile (Morganthau et al., 1994; Pecora, Whittaker, Maluccio, & Barth, 1992; Wells, 1991; Wolins, 1974). Yet many families are too dysfunctional to warrant keeping their youths at home and alternatives are scarce (cf., Weisman, 1994). Foster families are simply unavailable for many of the estimated 840,000 children who will require out-of-home placement by 1995 (Select Committee on Children, Youth, and Families, 1990). The most empirically supported criticism of residential placement is its limited positive influence on postplacement problems such as delinquency (Jones, Weinrott, & Howard, 1981; Pecora et al., 1992; Quay, 1986). Yet the most highly touted alternatives to placement (family preservation services and treatment foster care) have not been shown to be superior in this regard. Furthermore, the widespread negative sentiment appears to primarily involve issues pertaining to life within placement. Specifically, lay and professional persons alike appear to believe that, once in placement, life for troubled adolescents inexorably worsens. There is extensive current scientific evidence showing that, before placement, these youths' lives are filled with failure and misery (Eisikovits & Guttman, 1988; Small, Kennedy, & Bender, 1991). But scientific evidence pertaining to life within placement is in short supply.

This knowledge void appears to have been filled by negative beliefs based on older, mostly descriptive literature (e.g., D'Amato, 1969); McEwen, 1978; Polsky, 1965; Schur, 1973; Trieschman & Whittaker, 1972; Trieschman, Whittaker, & Brendtro, 1969), older, precedent-setting court cases (e.g., Donaldson vs. O'Connor, 1974; Morales vs. Turman, 1974), and a combination of theoretical orientation, ideology, media influences, and personal experiences. Among the most prominent of these negative beliefs are those pertaining to the delivery of helpful treatment, relationships with supervising adults, isolation from friends and family, and sense of control.

Delivery of Helpful Treatment. A primary purpose of placement is to provide treatment, yet it is widely believed that youths receive bad treatment, little treatment, or no treatment at all. There are at least three sources of this belief. The first is startling early accounts of life in placement (e.g., Polsky, 1965; Schur, 1973) suggesting treatment of youths was often negligent and/or abusive. The second is the well-documented problem with incongruities between treatment prescribed in residential programs and treatment delivered (Jessness, Allison, McCormick, Wedge, & Young, 1975; Kazdin, 1985; Quay, 1977). In fact, a National Academy of Sciences panel, commissioned to evaluate evidence on the efficacy of rehabilitation programs for offenders, concluded that most evaluation studies were of limited value because little treatment was delivered and the treatment that was delivered often had little resemblance to the treatment prescribed

(Martin, Sechrest, & Redner, 1981; Sechrest, White, & Brown, 1979). Landmark court cases mandating a right to treatment are a third source of belief that treatment has often been lacking in residential placements (e.g., Donaldson vs. O'Connor, 1974; Morales vs. Turman, 1974).

Relationships with Supervising Adults. A common belief is that the relationship between youths in placement and their supervising staff is adversarial, servile, or collusional. The source of this belief is partly in the notion of an inmate counterculture which was portrayed in Goffman's (1961) classic work *Asylums*, predicted in the theoretical work of Sykes (1958), and dates back at least to the work of Clemmer (1940). Some articles have also concluded that most children entering residential programs often already have negative perceptions of authority and that the deprivation of their liberty instigates resistance rather than cooperation (e.g., Empey & Stafford, 1991). These perspectives emphasize how enforcement of rules can undermine the relationship between children and those who provide their daily care (cf. Lundman, 1984; Polsky, 1965).

Isolation from Family and Friends. Other common beliefs are that residential placement produces an inexorable sense of isolation from family and friends (Eisikovits & Guttman, 1988; Empey & Stafford, 1991; Kiesler, 1982). These beliefs have at least two sources. The first is in the logical conclusion that the all-encompassing nature of residential life and the presence of institutional barriers to outside contact can cause a sense of isolation. The second source is from case descriptions and first-hand accounts of life in residential settings that emphasize isolation and disconnection from family and friends (e.g., D'Amato, 1969; McEwen, 1978; Polsky, 1965; Schur, 1973; Trieschman & Whittaker, 1972; Trieschman, Whittaker, & Brendtro, 1969).

Sense of Control. The last common belief of concern here is that residential placement limits the youths' development of a sense of personal control (Empey & Stafford, 1991). When youths are forced to live away from their homes, families, and friends, their sense of control is at risk (Gold & Osgood, 1992; Sykes, 1958). Pessimistic belief pertaining to this risk has expanded over the years due to media-based illustrations of loss of control during and following placement in institutional settings (see for example, the movie *American Me*). The importance of this belief is underscored by research showing that reduced sense of control has a strong relationship to maladjustment within placement (Gold & Osgood, 1992; Martin & Osgood, 1987; Osgood, Gruber, Archer, & Newcomb, 1985).

The impact of residential placement on feelings of control has special clinical and theoretical importance for programs that use external reinforcement systems (e.g., token economies). An influential line of research has suggested that such programs decrease intrinsic motivation to engage in

the prosocial behaviors that the programs intend to promote. The key support for this conclusion is research showing performance decrements following removal from external reinforcement systems (Deci & Ryan, 1985; Lepper & Green, 1978). Although this concern is widely cited in criticisms of behaviorally oriented programs, there have been few tests of the applicability of this research beyond laboratory settings. Only by directly comparing youths in and out of placement can we determine whether external reinforcement actually interferes with their sense of control.

That these five beliefs may be valid for some current programs is not in question here. But the beliefs have a pervasive nature that suggest they are universally valid. In the past 20 years, however, some programs have made changes in their methods and goals with an eye toward improving life in placement. These programs have moved away from the traditional training school format with mostly custodial shift-work staff and moved to a smaller group-home format with a family-type atmosphere and trained staff who live with the youths (e.g., Christian, Hannah, & Glahn, 1984; Fixsen et al., 1978; Lerman, 1975; Lundman, 1984; National Institute of Mental Health, 1971; Small & Alwon, 1988; Wolf et al., 1976). Thus the programs purport to correct past problems (problems that presumably have contributed to the negative beliefs mentioned above—cf. Daly & Dowd, 1992). The purpose of this study is to evaluate the extent to which the beliefs mentioned above are valid for a major family-style program.

METHOD

Research Design

This was a longitudinal quasi-experimental study that included a treatment group of youths who were residents in the program and a comparison group of youths who were referred to the program but did not attend. The complexity of the research design presented some special data analytic problems that we will discuss below, after describing assignment to groups, demographic characteristics of the sample, data collection, and measures.

Residential Program. Perhaps the most prominent and prevalent of the family style residential programs is the Teaching-Family Model (TFM), originally called the Achievement Place Model, which has been described more completely elsewhere (e.g., Phillips, Phillips, Fixsen, & Wolf, 1974; Wolf et al., 1976). Briefly, in the TFM a married couple lives in a large domestic home with six to eight adolescents. Some of the major features

of the program are (a) a token economy-type motivational system wherein youths earn points and exchange them for privileges; (b) a self-government system that allows youths to participate in development of the rules and structure of their daily lives; (c) a focus on teaching social skills from a standardized social skills curriculum; (d) an emphasis on normalization; and (e) a continuous evaluation system, part of which involves the youths evaluating the teaching family couple. At least 22 programs across the country use a certified version of the TFM, and as many as 100 programs use a modified version (Teaching Family Association, 1992).

Treatment and Comparison Groups. From May 1981 through June 1985, all youths who applied for admission to the residential program were considered for possible inclusion in the study. A total of 1,200 applications were processed for eligibility. Inclusion in the study required that (1) the youth be eligible for the program; the eligibility criteria included (a) age range 10 to 17 years, (b) parent or guardian request of placement for at least 1 year, (c) full-scale intelligence quotient of 80 or above, (d) no history of forcing sexual behavior on others (e) no history of psychosis, (f) the youth not be regarded as a habitual felon, (g) the youth not addicted to drugs, and (h) the youth not currently suicidal (793 of the 1,200 met eligibility requirements); (2) the youth and his or her guardian provide informed consent for the study (84 denied consent); and (3) the first interview occur prior to or within 36 hours of the youth becoming a resident (68 cases excluded on this basis). The specific reason for ineligibility was not recorded for an additional 60 cases. Of the 581 participants who were included, 497 (mean age 14.4 years) became residents and comprised the treatment group and 84 (mean age 14.7 years) did not become residents but agreed to participate in the comparison group. The comparison group youths did not become residents for either of two reasons: (1) They were accepted but did not come—typically because placements nearer home were found; (2) admission was precluded because of the naturally occurring wax and wane of vacancies in the program.

During the study, youths were distributed across all homes on campus according to available vacancies (n = 50 homes in 1981, 58 in 1985). Length of stay in the program varied considerably, with a mean of 702 days; at 3 months, 95% (n = 471), at 1 year 73% (n = 355), and at three years 23% (n = 114) were still in the program.

An important consideration was that the comparison group be a treatment-as-usual group and not a "no-treatment group." Each youth was referred to the residential program by a juvenile justice agency, a social service agency, or a treatment provider and virtually all of the comparison group members had received some type of treatment prior to the study. At the 6-month interview, 67% of the comparison group were

living with someone other than their parents, and 54% of the group were in a foster home, group home, or psychiatric setting. Thus, our research design contrasted long-term residential care in a specific program with a variety of "treatments as usual" for troubled adolescents served by social agencies.

Sample Demographics. Table I presents demographic information about the sample. A substantial proportion (30%) were minority group members, with African Americans most prominent (20%). The residential program's first female residents arrived during the later phases of this study, so females comprised only a small portion (8%) of the sample. The mean age of 14.5 years indicates that respondents typically entered the study in midadolescence.

Comparability of Groups. Table I shows the treatment and comparison groups were very similar in terms of race, gender, and age. Further comparison of the groups using the extensive set of measures (e.g., delinquency, time in detention, time in jail, drug and alcohol abuse, aggression, presence of religion) from the initial interview revealed few differences that reached the nominal level of statistical significance, and no dramatic differences of any sort. Additionally, results of data analyses on the five measures used in this study were not different at the initial assessment. Thus, initial indications and subsequent analyses suggest that we located an appropriate comparison group. Still, it was not a randomly assigned control group so we were careful to control for group differences in our statistical analysis.

Table I. Demographic Characteristics of the Treatment and Comparison Groups

Comparison			Treatment		
Charcteristic	*N*	(%)	Characteristic	*N*	(%)
Race			Race		
Caucasian	57	(68)	Caucasian	352	(71)
African American	18	(21)	African American	100	(20)
Hispanic	6	(7)	Hispanic	28	(6)
Other	3	(4)	Other	17	(3)
Gender			Gender		
Male	77	(92)	Male	458	(92)
Female	7	(8)	Female	39	(8)
Interview			Interview		
Original	31	(37)	Original	110	(23)
Modified	53	(63)	Modified	387	(77)

Data Collection

We interviewed the youths when they entered the study (May 1981 to June 1985) and every 3 months thereafter, until the spring of 1987. After that time, respondents who had completed 10 or more interviews were interviewed only once every 6 months. Beginning in July 1988, continuous interviewing ended for any youth who was past the scheduled 13th interview (a criterion that all youths met by late 1988). In 1989 we made a major effort to contact all youths for a final interview, even if we had formerly lost contact with them.

Interviews were conducted by the research staff by telephone. Respondents were promised that all the information they provided would be confidential and that the treatment staff would not have access to the data. Participants were paid for their participation.

As with all longitudinal surveys, there was some sample attrition. Despite determined efforts to track respondents, sample losses were larger for the comparison group, who were spread throughout the country. For the 12-month interview, we obtained data from 83% of the treatment group and 61% of the comparison group. These figures fell to 68% and 44%, respectively, for the 24-month interview and 54% and 36%, respectively, for the 36-month interview. Fortunately, response rates were higher for the final interview (84% for treatment and 70% for comparison groups) due to more intensive searches for respondents and increased incentive payments. Our analysis will capitalize on all of the data available for each subject, and a large majority of subjects in both groups contributed data covering a broad time span. We obtained an average of 11.4 interviews from each respondent.

Measures

Delivery of Helpful Treatment. The measure of helpful treatment was developed specifically for the present study. It consists of nine items and, high scores on it indicate that respondents frequently received counseling and tutoring, performed chores, helped set rules, learned skills, and earned praise, rewards and privileges.

After the first 141 respondents (110 from the treatment group, 31 from the control group) the interview was modified to include additional scales (i.e., Isolation from Family and Isolation from Friends as well as other scales that were not addressed in this study). But, whichever version of the interview a respondent initially received, he or she received that version consistently throughout the study. Additionally, the response scale for the

measure of Delivery of Helpful Treatment was modified—the original version had seven categories, ranging from *never* to *once a day*, and the modified version had five categories, ranging from *never* to *very often*. When response scales differed between the two versions, we transformed scores on the modified version so that their means and standard deviations would match those of the original version. Scale scores on all measures were means across items[6]

We calculated coefficient alpha separately for the original and modified versions of the interview, using the initial interview, the 12-month interview, and the 21-month interview. We also computed test–retest reliabilities for the 3-month intervals after the second, fifth, and ninth interviews. Test–retest reliabilities generally will be lower than internal consistency reliabilities because genuine change is treated as measurement error.

The coefficient alphas of the measure of presence of treatment were satisfactory, ranging from .78 to .81 for the original version and from .78 to .87 for the modified version. Test–retest reliabilities ranged from .50 to .54.

Satisfaction with Supervising Adults. The next measure consisted of four items (fairness, concern, effectiveness, and pleasantness) reflecting the respondents' satisfaction with their relationship with the adults responsible for their care. This measure was taken from Phillips et al. (1974).

For both versions of the interview, respondents answered using a 7-point scale ranging from *completely dissatisfied* to *completely satisfied*. The coefficient alphas of the measure ranged from .79 to .90 for the original interview and from .84 to .90 for the modified interview. Test–retest reliabilities ranged from .35 to .62.

Isolation from Family. Our measure of Isolation from Family first appeared in the National Youth Survey (Elliott, 1980; Elliott, Huizinga, & Ageton, 1985). The five items concern feelings of closeness to one's family versus separation and loneliness. Respondents were free to answer in terms of whomever they thought of as "family." The 5-point response scale for these items ranged from *strongly agree* to *strongly disagree*. This measure appeared only in the modified interview. All respondents answered these questions, whether or not they currently lived with their family of upbringing.

Alpha reliabilities for the measure of *Isolation from Family ranged from .80 to .85. The 3-mont test–retest reliabilities varied from .52 to .60.*

Isolation from Friends. The five-item measure of Isolation from Friends also comes from the National Youth Survey (Elliott, 1980; Elliott et al., 1985). The items closely parallel those for the measure of Isolation

[6]More waves of data were collected for respondents who received the original interview than for those who received the modified interview. To ensure that this difference would not affect the standardization of the two measures, we based the transformation for all waves of data on the means and standard deviations from the initial interview.

from Family, and they use the same 5-point response scale. Again, this measure appeared only in the modified interview.

The alpha reliabilities for our sample were .45 to .65. Although the alpha reliability of this measure was somewhat low, its test–retest reliabilities were comparable to the other measures, ranging from .47 to .70.

Sense of Personal Control. This measure is comprised of four items taken from the Locus of Control scale developed by Nowicki and Strickland (1973). We selected items that assessed whether respondents had a sense of freedom from capricious treatment by authority figures and a meaningful say in daily matters that affected them. The original version of the interview used a 5-point response scale ranging from *disagree* to *agree*, while the response choices for the modified version were simply *disagree* or *agree*.

Alpha reliabilities ranged from .68 to .73 for the original version and from .50 to .74 for the modified version. Test–retest reliabilities ranged from .41 to .80. Table II presents the correlations among the five measures that served as our dependent measures.

Data Analysis

The special strength of this study was the large number of interviews available for each respondent over an extended period. Thus, our examination of group differences before, during, and after treatment was not limited to two or three arbitrary time points. This is especially important for an analysis of long-term residential care, where the length of stay is inevitably highly variable. Realizing the potential for tracing group differences over continuous time requires a complex statistical analysis, we chose hierarchical linear modeling (HLM; Bryk & Raudenbush, 1987, 1992). Our approach to the application of HLM to complex longitudinal evaluation designs was discussed in detail by Osgood and Smith (1995).

Table II. Intercorrelations Between Outcome Measures[a]

Measure	1	2	3	4	5
1. Delivery of Helpful of Treatment	—				
2. Satisfaction with Supervising Adults	−.03[b]	—			
3. Isolation from Family	.00	.00	—		
4. Isolation from Friends	−.03[b]	−.04[b]	.16[c]	—	
5. Sense of Personal Control	.00	.01	.01	.03	—

[a]Number of subjects = 4,395 to 5,650.
[b]$p < .05$.
[c]$p < .01$.

When applied to a longitudinal research design, HLM incorporates both a within-subjects model and a between-subjects model, each defined in terms comparable to a multiple regression model. HLM is hierarchical in that the regression coefficients of the within-subjects model serve as dependent variables for the between-subjects regression model. The within-subjects model uses time and time-related variables (e.g., the dichotomy of pretreatment vs. later occasions) to predict changes in the outcome variable. The between-subjects model uses individual characteristics that do not vary over time to predict the regression coefficients in the within-subjects model. Examples of such individual characteristics include demographic variables or treatment conditions (e.g., treatment vs. comparison). The between-subjects portion of HLM yields regression-type coefficients indicating the "effect" of the predictor variable on a coefficient from the within-subjects model, controlling for the other predictor variables in the model. A significant between-subjects coefficient in HLM is interpreted similar to a significant interaction in ANOVA. For example, if treatment (vs. control) significantly predicts the coefficient for pre-treatment versus later, this indicates that the change from pre-treatment to later scores differs significantly for treatment versus control groups.

HLM was well suited to our study for several reasons. First, we could make use of all the data available for each subject, despite great variability in the timing and number of interviews. This variability arose due to differing start dates, changes over the study in the timing of interviews, and missing data. Thus the data obtained here violated assumptions of other data analytic methods such as repeated-measures analysis of variance and structural equation modeling, both of which require a uniform data structure for all subjects (e.g., Kessler & Greenberg, 1981). Unlike those methods, HLM also allows us to incorporate time as an explicit, continuous variable in the within-subject portion of our analysis. This was especially important for our study because of variability in the duration of treatment and in the dates at which interviews occurred.

The within-subjects focus of HLM also made the most of our quasi-experimental research design. HLM incorporates the growth curve approach to assessing change (Bryk & Raudenbush, 1987, 1992; Rogosa, Brand, & Zimowski, 1982), which provides a more appropriate baseline correction than analysis of covariance and structural equation approaches. The growth curve portion of HLM summarizes smooth time-related trends in the outcome variable, but it is poorly suited for modeling or testing abrupt changes such as those at the beginning and end of residential placement. Therefore we have added dummy predictor variables (see Osgood & Smith, 1995, for technical details), one contrasting the pretest (0) versus all later occasions (1), and another contrasting posttreatment occasions (1)

versus all other occasions (0). This yielded a total of five coefficients in the within-subjects model: a constant, two coefficients for predicting a growth curve with linear and quadratic components, and the two dummy variables.

The between-subjects predictors for four of the five within-subjects coefficients of this model (all except posttreatment vs. other) included white versus nonwhite race, age at initial interview, sex, and variables reflecting the likelihood of the assignment to experimental versus comparison groups and the likelihood of attrition. These last two variables were scores representing the best prediction of the likelihood of group assignment or attrition based on all available measures taken from the pretreatment assessments (called propensity scores by Rosenbaum & Rubin, 1983). Wainer (1989) and Heckman (1989) have supported Rosenbaum and Rubin's contention that controlling for such propensity scores provides an efficient means of adjusting for group differences on a large set of measures.[7]

In addition to these five between-subjects predictors, group (treatment vs. comparison) and length of stay were used to predict the constant and the dummy variable for pretreatment versus later assessments. Length of stay was also used to predict the dummy variable representing posttreatment versus other occasions. The appropriateness of the model used here is documented in a detailed technical analysis of a variety of potential models by Osgood and Smith (1995).

In the model used here, prior individual differences were reflected in the between-subjects coefficients that predicted the constant of the within-subjects model. This was so because the initial interview was coded as zero on all within-subject variables. Thus, a small and statistically nonsignificant coefficient for group would indicate that the treatment and comparison groups were similar at the beginning of the study, after controlling for the demographic factors and propensity scores. Each figure shows the estimated mean pretreatment scores for the treatment and comparison groups. Our HLM model was designed to limit the group difference between treatment and comparison groups to a constant value over time, correcting for pretreatment values. This difference was reflected by the between subjects coefficients for group that predicted the within-subjects variable contrasting pretreatment versus later. In the figures, the growth curves were parallel

[7]An important distinction in HLM is whether within-subject parameters are treated as randomly varying across subjects or as fixed to the specific values generated by the between-subjects model. We treated the polynomial time function as fixed and the other within-subjects components as random. There was insufficient within-subjects information to treat all within-subjects components as random, so we choose those components that were of primary interest for our analysis.

during the treatment period, with a constant difference between the treatment and comparison groups.

Another benefit of this particular HLM model was its capacity to distinguish the treatment period from the posttreatment period for the treatment group. We accomplished this through the additional within-subjects variable that had a value of 1 for interviews conducted after departure from treatment and a value of zero for all other interviews (including all interviews for the comparison group). Coefficients on this variable reflected change from levels while in placement to levels after placement, controlling for overall time trends, which is illustrated at 20 months in the figures (also see Osgood & Smith, 1995).

The only between-subjects predictor included for change at departure was length of stay in the program, because we suspected that length of stay might influence the extent to which reported program benefits were retained later on. We also expected that the effect of length of stay would gradually decline, so we coded this variable as the square root of the number of days in residence. We then subtracted the mean value for the treatment group, and assigned the comparison group a value of zero (i.e., the mean). When coded in this fashion, the constant term for the between-subjects model predicting the posttreatment versus other coefficient equals the overall mean change upon departure (Bryk & Raudenbush, 1992, p. 10). Length of stay was also included as a predictor for initial level and overall change, but not for the polynomial function of time.

RESULTS

Figures 1 to 5 provide a visual summary of our results, and the pertinent numerical results appear in Tables III to VII. The figures contain the time trends for treatment and control groups that correspond to the estimated coefficients from the between-subjects models, which are in turn averages of the within-subjects coefficients (weighted by the precision of the individual subjects' coefficients). The figures show time trends for the average length of stay in the program (20 moths). When a disjunction in the data path occurs between 18 and 21 months (see Figs. 1, 3, and 4), it represents mean changes in the outcome variable upon leaving the program.

Delivery of Helpful Treatment

Figure 1 shows change over time in the extent to which respondents received the kind of treatment espoused by the Teaching-Family model,

Table III. Differences Between Treatment and Comparison Groups on Delivery of Helpful Treatment[a]

	Gamma	Standard Error	t	p
Initial difference	–.185	.169	–1.092	.275
Differential change (treatment vs. comparison group)				
During placement	.962	.171	–5.637	.000
Postplacement	–.637	.188	–3.383	.000
Change at departure (treatment group)	–.302	.069	–4.400	.000

[a]M score = 4.91, SD = 1.444, range = 7.8.

Table IV. Differences Between Treatment and Comparison Groups on Satisfaction with Supervising Adults[a]

	Gamma	Standard Error	t	p
Initial difference	.007	.153	0.048	.962
Differential change (treatment vs. comparison group)				
During placement	–.458	.163	–2.805	.005
Postplacement	–.480	.169	–2.842	.005
Change at departure (treatment group)	.024	.056	0.436	.662

[a]M score = 5.861, SD = 1.133, range = 6.

Table V. Differences Between Treatment and Comparison Groups on Isolation from Family[a]

	Gamma	Standard Error	t	p
Initial difference	–.142	.119	–1.198	.231
Differential change (treatment vs. comparison group)				
During placement	.378	.113	3.342	.001
Postplacement	.275	.127	2.159	.031
Change at departure (experimental group)	.106	.038	2.776	.006

[a]M score = 2.026, SD = 0.768, range = 4.

according to our HLM analysis. The initial difference between the treatment and comparison groups was small and statistically nonsignificant (gamma = –.185, t = –1.09, p = .28), indicating that the groups experienced comparable treatment components in the period immediately before ap-

Table VI. Differences Between Treatment and Comparison Groups on Isolation from Friends[a]

	Gamma	Standard Error	*t*	*p*
Initial difference	–.086	.093	–0.917	.360
Differential change (treatment vs. comparison group)				
During placement	.262	.095	2.747	.006
Postplacement	.214	.102	2.105	.035
Change at departure (treatment group)	.045	.028	1.583	.113

[a]*M* score = 1.958, *SD* = 0.609, range = 4.

Table VII. Differences Between Treatment and Comparison Groups on Sense of Personal Control[a]

	Gamma	Standard Error	*t*	*p*
Initial difference	–.241	.142	–1.691	.090
Differential change (treatment vs. comparison group)				
During placement	–.267	.149	–1.790	.073
Postplacement	–.245	.152	–1.618	.105
Change at departure (treatment group)	–.024	.055	–0.442	.658

[a]*M* score = 4.091, *SD* = 1.205, range = 4.209.

plying to the program.[8] Both groups reported more treatment delivery 3 months later at the next interview. Levels of treatment delivery gradually declined for the remainder of the study, with the comparison group ultimately approaching pretest levels of treatment.

The increase in treatment delivery after the initial interview was considerably greater for the treatment group. The difference between the groups was virtually a full point on the 7-point scale (see Table III, differential change: gamma = –.962, $t = -5.64$, $p < .001$). This coefficient reflects the group difference while the treatment group was in placement, pooling that entire period, and subtracting any pretest difference. Thus, the analysis

[8]In these HLMs, group differences were reflected by the gamma (or unstandardized between-subjects) coefficient for the "effect" of group (i.e., Boys Town vs. comparison) on the coefficients of the within-subject regressions. Because group was coded with a value of zero for the Boys Town sample and a value of 1 for the comparison group, the gamma coefficients indicate the magnitude of mean differences between groups, with positive coefficients corresponding to higher scores for the comparison group. To aid in interpreting these mean group differences, the tables provide the means and standard deviations of the dependent variables (based on pooling all subjects and waves).

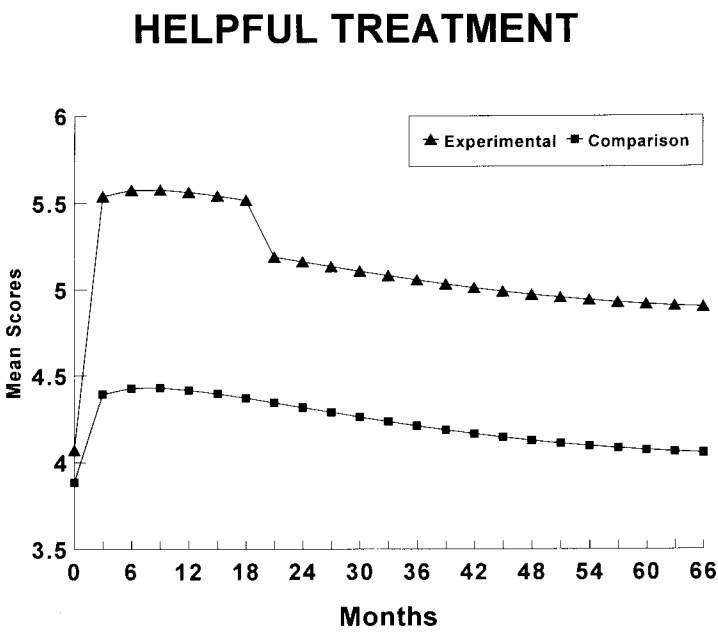

Fig. 1. Mean scale scores over time for Delivery of Helpful Treatment.

reveals that the residential program succeeded in providing considerably higher levels of the intended treatment than would otherwise have been available to these adolescents.

As would be expected, after leaving placement the treatment group reported a decline in the level of treatment delivery (gamma = –.302, t = –4.40, $p < .001$), though their reported levels remained considerably higher than the levels reported by the comparison group (gamma = –.637, t = –3.38, $p < .001$).

Satisfaction with Supervising Adults

In most respects, the pattern of change for the quality of relationships with supervising adults was the same as that obtained for delivery of treatment. Table IV indicates that the initial difference between the treatment and comparison groups was negligible (gamma = .007, t = .05, p = .96). Figure 2 shows that satisfaction with supervising adults increased for both groups, but this change was greater for the treatment group (gamma =

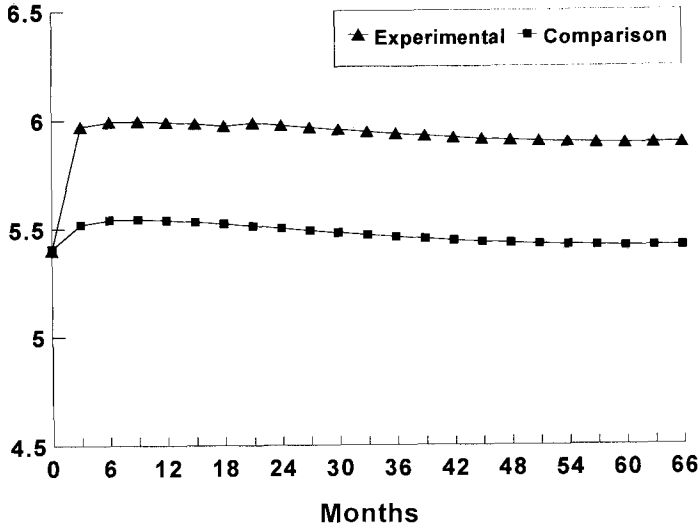

Fig. 2. Mean scale scores for youths over time for Satisfaction with Supervising Adults.

–.458, t = –2.81, p = .005). There was little change in satisfaction when the treatment group left placement (gamma = .024, t = .44, p = .66). Thus, the high level of satisfaction while in placement generalized to adults in subsequent settings, where the treatment group remained more satisfied (gamma = –.480, t = –2.84, p < .005).

Isolation from Family

Table V shows that the treatment and comparison groups had comparable feelings regarding isolation from their families at the time they entered the study (gamma = –.142, t = –1.20, p = .23). Feelings of isolation from family decreased in both groups, but Fig. 3 shows the change was gradual and slight for the comparison group relative to the treatment group. Feelings of isolation from family decreased significantly more for the treatment group (gamma = .378, t = 3.34, p = .001) than for the comparison group.

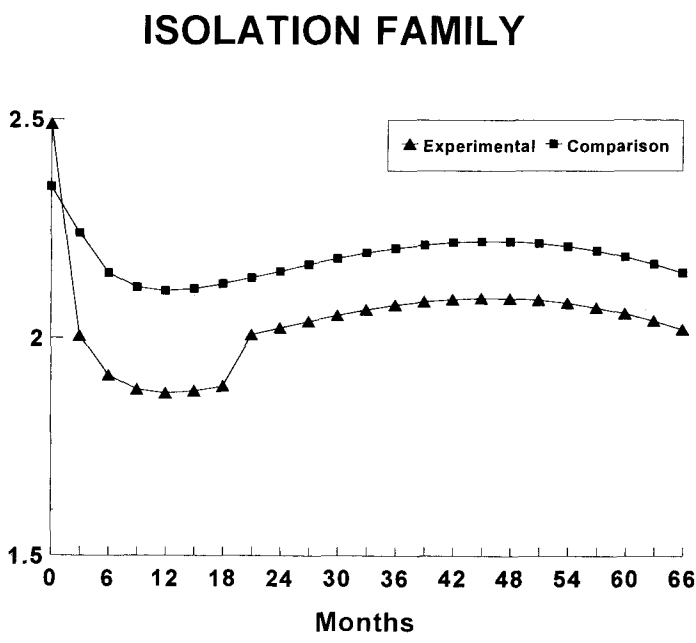

Fig. 3. Mean scale scores over time for Isolation from Family

On the average, the treatment group reported feeling more isolated after placement than they did during placement (gamma = .106, t = 2.78, p = .006). This means that group differences decayed somewhat over time. Even so, there remained a significant postresidential difference between treatment and comparison groups (gamma = .275, t = 2.16, p = .03).

Isolation from Friends

Our findings concerning feelings of isolation from friends appear in Table VI and Fig. 4. Once again, treatment and comparison groups did not differ in pretest feelings of isolation (gamma = –.086, t = –.92, p = .36). Three months later at the next interview, however, the sense of isolation from friends had decreased for the treatment group, while it increased for the comparison group (for differential change, gamma = .262, t = 2.75, p = .006). Thereafter, isolation decreased slightly and gradually for both groups.

The magnitude of the difference between the treatment and comparison groups changed little after leaving the program. The average postplace-

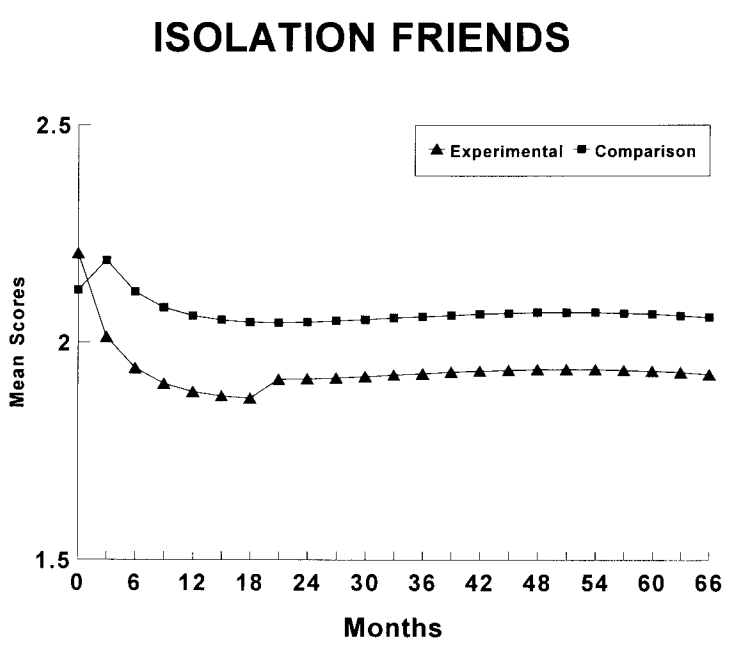

Fig. 4. Mean scale scores over time for Isolation from Friends.

ment change reported by the treatment group was not significantly different from zero (gamma = .045, t = 1.58, p = .11), indicating they continued to feel significantly less isolated from their friends in subsequent settings than did the comparison group (gamma = .214, t = 2.11, p = .04).

Sense of Personal Control. For sense of control, the initial difference between the treatment and comparison groups was somewhat larger than the two previous dependent variables (see Table VII), and it approached statistical significance (gamma = –.241, t = –1.69, p = .09). Figure 5 shows a more gradual increase in sense of control for both groups. Placement tended to bring a greater increase in reported sense of control (than for the comparison group) in their dealings with the adults responsible for their care. This greater improvement was not quite statistically significant (gamma = –.267, t = –1.79, p = .07).

The treatment group reported little change in the sense of control after leaving placement (change at departure: gamma = –.024, t = –.44, p = .66; for difference between the postresidential treatment group and the comparison group: gamma = –.245, t = –1.62, p = .10).

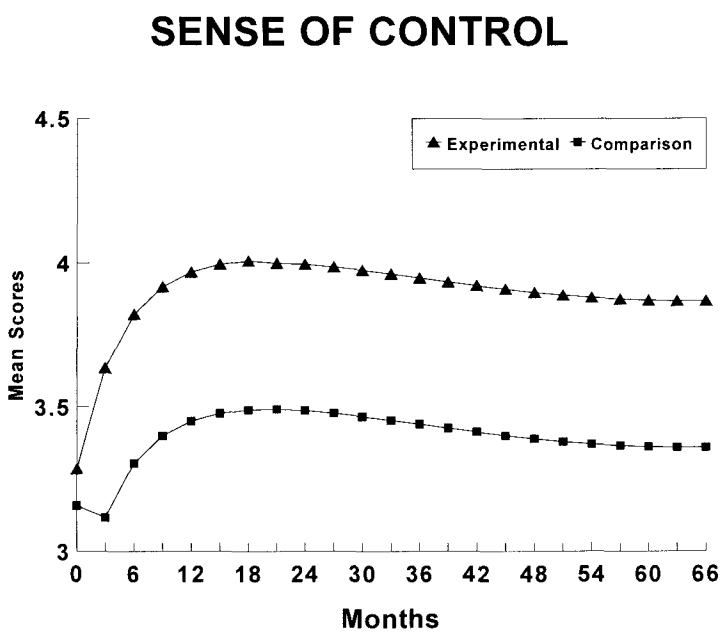

Fig. 5. Mean scale scores over time for Sense of Personal Control.

Length of Stay. There is not sufficient space in the present paper to report results concerning length of stay in the program. Suffice it to say that, for most of these dependent variables, length of stay was related to pretest levels (i.e., youths with more negative prior experience were less likely to complete the program) and to change at departure (shorter stays associated with more negative change, indicating less carryover of positive program effects). But there was no significant relationship between length of stay and change in outcome variables upon entering the program. Thus, our major findings concerning the outcome variables while in the program are not dependent on length of stay.

DISCUSSION

These results are inconsistent with the prevalent negative beliefs about adolescent life within residential placement that we selected for study. For example, contrary to the belief that residential life is devoid of helpful treatment, the study group reported experiencing levels of helpful treatment that were significantly higher than the levels reported by the comparison

group. The treatment scale corresponded closely with components of the Teaching Family Model and thus the treatment group's higher levels seems expected. But bear in mind that, historically, a major criticism of residential programs has been that they so often fail to deliver their *prescribed* treatment (Kazdin, 1985; Martin et al., 1981; Quay, 1977; Sechrest et al., 1979). So we developed the scale to assess the delivery of treatment as prescribed by the TFM. Furthermore, the components of the scale included receipt of counseling, tutoring, and praise, earning privileges, setting rules, doing chores, and learning skills—all of which involve generically helpful experiences that are widely believed to benefit children and are recommended for troubled and untroubled youth by a diversity of parenting and/or youth experts (e.g., Barrish & Barrish, 1989; Becker, 1971; Christophersen, 1988, 1990; Patterson, 1976; Patterson, Reid, Jones, & Conger, 1975; Robin & Foster, 1989). The colloquial wording of the treatment scale is also noteworthy. It used the language of everyday speech (e.g., "How often do the adults you live with show appreciation for the good things you do?") rather than the technical terminology of the TFM (e.g., "negative and positive points," "intensive teaching interactions"). Thus, the items should neither call upon the treatment group to report trivial details of their treatment program nor require the comparison group to identify experiences that are unlikely for the average teenager.

The second major finding in this study is inconsistent with the belief that children in residential placement have adversarial or collusional relationships with their supervising adults. At entry into the study, the treatment group did not differ from the comparison group on their reports of satisfaction with the fairness, concern, effectiveness, and pleasantness of the adults with whom they had lived. But 3 months after admission, the reports from the treatment group became significantly more positive than those from the comparison group. This difference remained highly significant throughout the study.

The third and fourth major findings of this study are inconsistent with the belief that youths in residential programs necessarily feel isolated from family and friends. The treatment and comparison groups did not differ in feelings of isolation from friends and family at the initial interview. Immediately after entering the program, however, the treatment group began feeling significantly less isolated from those they considered family and from their friends than did the comparison group, and these differences lasted throughout the study.

There are several possible explanations for the results pertaining to supervising adults, friends, and family, three of which warrant further discussion. First, the differences may have been due to the orientation of the TFM program. Therein the youths often develop family-like relationships

with their teaching parents and their fellow youths. They may have been thinking of these relationships when they responded to the interviews. Second, healthy contacts with family members are integral to the TFM treatment plan, and the significant results may reflect benefits therefrom. Third, it is even possible that the positive effects of the TFM break up the coercive family patterns which often led to placement, allowing a more positive relationship between child and family to emerge and to continue after departure (this possibility is speculative but it does suggest an important line of new research).

The fifth finding in this study is inconsistent with the belief that children in residential placement gradually lose their sense of control. Over the course of the study, both groups reported an increasing sense of control with a marginally significant greater increase in the treatment group. This finding is important because of the central role of an external reinforcement system (i.e., token economy) in the TFM. Such systems are often criticized for being artificial (e.g., Deci & Ryan; 1985; Lepper & Greene, 1978), but the system included in the TFM must be compared to the actual alternatives in the lives of these troubled adolescents rather than to an abstract ideal. Research by Patterson and his colleagues indicates that the relationships troubled youths have with their parents are often typified by unproductive and unpleasant patterns of negative coercive interactions (Patterson, 1982; Patterson, Reid, & Dishion, 1992). By comparison, youths in the study group may have experienced the system of external rewards in the TFM as clearer, more consistent, and as both enhancing control over their daily lives and increasing their opportunities to noncoercively elicit positive responses from the adults who care for them.

The validity of the findings in this study are enhanced by five features of its design. First, its principal data were from the individuals most capable of assessing life in residential care—the youths themselves. Second, it was longitudinal and involved many interviews over an 8-year period. Most studies evaluating any aspect of residential placement involve assessments that occur at only one or two points in time (e.g., pre and post). But for residential placement, a 24-hour-a-day experience sometimes lasting for years, much valuable information would be missed if assessments were conducted at only pre- and postplacement (cf. Curry, 1991; Whittaker, Overstreet, Grasso, Tripodi, & Boylan, 1988). Third, this study involves one of the largest samples ever included in a study of residential care over such a long period. Fourth, the use of hierarchical linear modeling allowed us to make maximal use of our complex research design to obtain answers to our research questions while controlling for potentially confounding factors. Fifth, this study employed a treatment-as-usual comparison group which is rare

in research on any aspect of residential placement (Curry, 1991; Kazdin, 1985; Whittaker, Overstreet, Grasso, Tripodi, & Boylan, 1988).

There are also a number of study limitations and potential criticisms that must be considered when interpreting our results. The most prominent limitation is that the comparison group was not randomly assigned and is thus subject to selection bias. Concern about this bias is mitigated by the apparent equivalence of treatment and comparison groups, however. Both groups met all the requirements for eligibility into the residential program, were equivalent on all relevant demographic variables, and provided equivalent responses on all scales at the first interview. Nonetheless, true equivalence cannot be established outside of random assignment.

Another limitation involves the variable placements of the comparison group. The study may have been strengthened with a group who received a more uniform treatment experience that could have been more specifically described. But the comparison group does represent typical youths served by the residential program. Additionally, their range of placements during the study reflected the range of placement options (including the youths' own families) available to youths at risk, and thus our description of them as a "treatment-as-usual" group (cf. Friman, Evans, Larzelere, Williams, & Daly, 1993; Hawkins, Almeida, Fabry, & Reitz, 1992).

There was a higher attrition in the comparison group. The high response rate on the final interview for both groups helped reduce this concern, and our analysis strategy minimized the effects of attrition by emphasizing within-subject comparisons. Furthermore, any bias introduced by attrition in the comparison group was likely to have been in the opposite direction of our findings. It is reasonable to assume that it would be more difficult to reach respondents and obtain interviews at those times when youths have poorer adjustment and more negative views of about their life. If that is the case, then the greater attrition in the comparison group would produce a bias toward more positive responses and lessen the contrast between groups.

It is possible that the significantly more positive ratings from the treatment group were influenced by their desire to please or avoid punishment from program staff. It is important to note, however, that respondents were repeatedly assured their answers were confidential and that program staff had no access to the interview data. Additionally, the significant differences between groups remained long after the treatment group left placement.

Perhaps the more positive ratings in the treatment group were due to a favorable bias toward the program rather than its treatment effects. This is a plausible concern but it can be addressed. The scales used do not refer to the program but rather to important features of the youths' daily lives—wherever they happen to be living them. Additionally, previous research

on the TFM demonstrated its positive influence on social skills—especially as they pertain to relationships with adults (e.g., Kifer, Lewis, Green, & Phillips, 1974; Phillips et al., 1974; Wolf et al., 1976). Such skills can have a bidirectional influence—just as youths can be the architects and victims of a coercive process, they can also be the architects and beneficiaries of a noncoercive process (Patterson, 1982; Patterson et al., 1992). Thus it is possible that the TFM program trained youths in the treatment group to have a positive influence on their postplacement social environments, the benefit of which was reflected in their significantly more positive ratings following departure. Lastly, additional data on the two groups indicate that the treatment group had superior outcomes on some more objective measures that also generalized to the postplacement period. Examples include grade point averages, years of school completed, and high school graduation rates (Thompson et al., 1996). These concrete educational effects suggest that differences between the groups were more reflective of behavior change in the treatment group than of positive bias toward the program.

Another limitation involves the dependent measures. Although they have high face and content validity, they have acceptable internal consistency and test–retest reliabilities, and four of the five have been used extensively by other investigators (Elliott, 1980; Elliott et al., 1985; Nowicki & Strickland, 1973; Phillips et al., 1974), they merely involve *ad hoc* scales imbedded in an extensive structured interview with only one informant. Future research would benefit by combining such scales with additional measures such as the educational outcomes mentioned above (e.g., court, employment, mental and physical health records) and by including other informants (e.g., friends, family, teachers, healthcare providers).

A final limitation is that the study focused on only one residential program and model and its results may not be relevant for other programs and models. This is true but not so much a limitation as a starting point for additional research on residential placement options for troubled adolescents. Currently, any residential placement, regardless of program, is believed by many to be a negative event in the lives of youths who are placed in it (Morganthau et al., 1994; Pecora et al., 1992; Wells, 1991; Wolins, 1974). The results obtained here call into question such beliefs, at least as they pertain to the program studied. Nonetheless, the results should not be construed to suggest the converse of these beliefs (i.e., placement is always a positive event). To us the results merely indicate that the benefit (or harm) of residential placement for adolescents is not a settled issue. As the need for out-of-home placements for this nation's troubled children inexorably waxes, we believe it folly for research on residential options to wane.

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