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Richard L. Amdur

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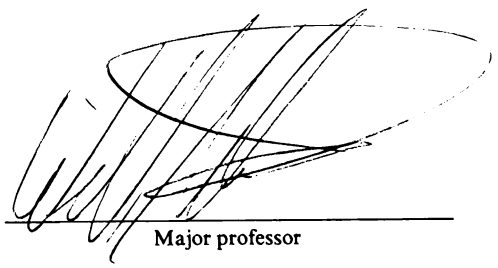

Major professor

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THE PROCESS OF JUVENILE DIVERSION

by

Richard L. Amdur

A DISSERTATION

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in partial fulfillment of the requirements
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ABSTRACT

THE PROCESS OF JUVENILE DIVERSION

by

Richard L. Amdur

Diversion of adjudicated youth out of the juvenile court system and into alternative, community-based programs is an intervention in widespread use throughout the United States. Diversion programs attempt to prevent recidivism, while protecting youth from delinquent labeling and social isolation caused by involvement in the juvenile justice system.

Research evaluating the effectiveness of diversion programs has not provided a clear conclusion about whether these programs are worthwhile. Outcome studies have focused on the size of treatment effects, rather than on the mechanisms by which the interventions work. Knowledge of how interventions work is crucial in designing effective treatments, as well as in accurately assessing treatment effectiveness.

Path analysis was used to investigate the mechanisms of effect for several interventions used in a diversion program for delinquent youth. Subjects were 306 youth referred to the project by the juvenile court. Data were collected at six separate time periods, before, during and after treatment. Measures of self-reported general delinquency, self-reported truancy and drug use, and archival records of police and court contacts were used to measure delinquency. Self-report measures of youth life events and of intervention activities were used to examine the processes by which treatments produced specific changes in the subjects' lives, that would ultimately affect delinquency.

Home and school interventions, thought to reduce delinquency by strengthening the youth's involvement with school and family, did not have the expected effects. This may have been in part because these interventions were most likely to be delivered to the most delinquent, least involved youth. Family conflict both was predicted by and was a predictor of delinquency, making it an important avenue for intervention. School involvement did not affect later delinquency, making it less important as a target of intervention.

Older youth were more delinquent at pre-intervention than younger youth, but less delinquent at follow-up, indicating that maturational factors probably reduced delinquency during the period of data collection. Drug use and truancy followed a different course than delinquency, indicating that these are very different phenomena than delinquency. Implications of the findings for future research were discussed.

ACKNOWLEDGEMENTS

I would like to thank Bill Davidson, the chairman of my committee, who has consistently been patient, trusting and kind. It has always been a pleasure learning from and working with him. He taught me what community psychology was, first by explaining its principles, then by showing me through his own example how they could be carried out. It was important to me, personally and professionally, to be involved with someone who could honestly, and in an uncomplicated way, go out and do what he believed should be done.

Throughout my six years of involvement with the Adolescent Diversion Project, Robin Redner has been encouraging and supportive. I appreciated her willingness to listen to my ideas, even at times when they were vague or unclear, and her ability to be enthusiastic. She worked her way through numerous drafts of comprehensive examination papers and the dissertation, always with helpful comments.

Anne Bogat's comments made the final version more readable. Often a companion on the long drives to and from East Lansing, we spent a great deal of time discussing such relevent topics as the merits of confirmatory versus exploratory factor analysis, the relationship between path analysis and multiple regression, and the importance of clear writing as a way of achieving clear thinking.

Neil Schmitt is responsible for sparking my interest in psychological measurement. His solid grounding in measurement theory and gentle approach in discussing problems made me feel free to put ideas together in ways I wasn't always sure were correct. I knew that (as Bill said), "if an Iowa farm boy thinks it makes sense then it probably does."

The contributions of other colleagues, teachers and friends also must be acknowledged. Mary Ann Reinhart introduced me to path analysis, explaining it so coherently that my first course with John Hunter seemed like a review. She also listened patiently as I tried to figure out what my dissertation topic should be.

It seems that most of the problems I had been thinking about over my years in graduate school were already fully crystallized by John Hunter into a coherent theory that combined measurement, modeling, and data analytic techniques. I'm glad I met him when I did. He taught me as much as I could absorb about confirmatory factor analysis, path analysis, experimental design, meta-analysis and mathematical modeling. He was undaunted by computer printouts that he correctly described as "a blizzard of numbers," and was willing to read through several very densely written comprehensive examination papers and make numerous comments.

I could not have done my part on this Project without all the labor that went into designing the interventions, delivering them, and collecting the data. I'm grateful to all those graduate students who worked on the Project before me, who helped develop, administer and code the measures, train the interviewers and coders, train and supervise the volunteers, and gather data from police and court records.

Finally, my family also deserves thanks for tolerating me for the last seven years. My parents, always generous and supportive, helped make the past few years quite a bit more bearable than they would otherwise have been. My wife, Daniela, moved from Boston to East Lansing to be with me, only to find me unavailable most of the time. I've

cherished the time we've had together. Although I can not claim that my son Spencer's arrival eight months ago has speeded my progress, he has made it easier to leave the green glare of my computer for the warmth of family life and has given me added incentive to be done with school.

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INTRODUCTION

In the two decades since they were first introduced, diversion programs for adolescents identified as delinquents have become an accepted part of the juvenile justice system. Thousands of such programs are now operating in the United States and Europe (Kaufman, 1973; Schwartz, Johnson & Mahoney, 1977; Wright & Dixon, 1977). Many of these programs are supported by state and federal funds (Law Enforcement Assistance Administration, 1976; President's Commission on Law Enforcement and the Administration of Justice, 1967). While diversion programs originally gained popularity during a period of social reform in the 1960's, their continued support will depend, at least in part, on evidence that they are effective in preventing further delinquency.

Studies of the effectiveness of diversion programs have been almost as numerous as the programs themselves. These outcome studies show that while some diversion programs seem to be effective, others clearly are not (e.g. Bohnstedt, 1978; Gensheimer, Mayer, Gottschalk & Davidson, 1985). In this dissertation, the reasons for these discrepant results will be explored. Factors that could account for differential effectiveness in diversion programs will be considered. In addition, factors in the design of evaluation research studies will also be considered as possible causes of conflicting or ambiguous results. Solutions to these research design problems will be proposed. Finally, an experimental diversion program will be described and its effectiveness evaluated. The processes through which diversion treatments might work will be modeled and tested empirically, using path analysis. First, the

history and philosophies guiding the development of the diversion movement will be reviewed.

Juvenile Diversion

Typically, diversion means that adolescents convicted of crimes other than major felonies are referred out of the juvenile justice system, to some form of community-based program. In addition to the benefits of whatever counseling or services the youth receives, there is the added benefit of insulating the youth from the dehumanizing conditions and stigma that accompany court processing (Matza, 1964; Schur, 1969). The type of services provided by diversion programs vary widely (Davidson & Redner, 1983). These include everything from referral to existing social agencies, or "service brokerage" (the minimal approach), to vocational counseling, to focused behavioral interventions. Reasons for the great popularity of diversion will be discussed first. Next, specific diversion interventions that have been used in several programs will be described. This will be followed by a review of studies assessing the effectiveness of diversion.

History. Diversion came about as a reform of a system which itself had been proposed decades earlier as a major reform. The separate system of courts and detention facilities for juveniles was supposed to provide safety and "parent-like concern" for youth who had broken the law, but who were not "hardened criminals" (Krisberg & Austin, 1978). By the early 1970's it was clear that the juvenile court and detention system was not living up to the hopes with which it was founded. Rehabilitation carried out in correctional institutions for juveniles did not appear to be effective (Jesness, 1975; Martinson, 1974). In fact, these

institutions originally designed to reduce crime apparently had the paradoxical effect of contributing to the persistence of criminal behavior among adolescents (Davidson, Koch, Lewis & Wresinski, 1981; Jordon, 1974).

The juvenile court system was also criticized on legal grounds. It was argued that while the system took constitutionally-guaranteed rights away from juveniles, it did not provide the "benevolent concern" that was supposed to be a substitute (Barton, 1976).

The system was also criticized for being discriminatory. The vast majority of adolescents who came before the juvenile court were lower class and minority youth. However, studies of the prevalence of delinquent behavior (using self-report measures) showed that middle and upper class youth engaged in delinquent behavior as often as did lower class youth (Hardt & Peterson-Hardt, 1977; Williams & Gold, 1972). It was proposed that police arrest and processing through the juvenile justice system was not a result of the youth's behavior, but of society's preconceptions and prejudices (Pepinski, 1976).

Social process theories of delinquency. As the call for reform of the juvenile justice system gained momentum as a political issue, theories were being proposed describing social processes which caused adolescents to engage in delinquent behavior. One of the factors influencing the rapid growth of diversion was the belief among social psychologists, sociologists, and government policy-makers that how youth are treated by "the system" could make a difference. Two of the more well-known social process theories of delinquency were social control theory (Hirschi, 1969) and labeling theory (Schur, 1971).

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Control theory, as outlined by Hirschi (1969), proposed that delinquency comes about when the "bond" tying the youth to society breaks down. Hirschi originally proposed a social bond made up of four interrelated elements: attachment to others, commitment to and involvement in conventional activities, and conventional beliefs. Later researchers attempted to show that there were only three elements of the bond, since commitment to and involvement in conventional activities were part of the same cluster of behaviors (Krohn & Massey, 1980). What is more important than the number of distinct elements in the bond is how they affect delinquency.

Hirschi (1969) made an analogy between the elements of the social bond and the structure of the psyche. He said the attachment of the individual to others is "the essence of internalization of norms, conscience or super-ego". And while "attachment to others is the sociological counterpart of the superego or conscience, commitment is the counterpart of the ego or common sense (pp. 16-22)." The point here seems to be that it is not simply external controls and the fear of punishment that prevent delinquency. Instead, what keeps a youth away from delinquent behavior is an internalized sense of right and wrong that is consistent with conventional social mores, a sense of ambition and purpose involving conventional goals, an ability to be realistic about the effect of delinquent behavior on the likelihood of achieving one's goals, and perhaps a sense of empathy derived from one's ability to form attachments, which makes it difficult to engage in aggressive or destructive behavior. Thus Hirschi's theory seemed to be based on internalized controls. This distinguished it from earlier social control

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theories (e.g. Reiss, 1951) which emphasized controls imposed on the youth from external sources.

Labeling theory reversed the causal directions of some of these processes. Actual or presumed delinquency was believed to affect the expectations and attributions of others, which in turn alter the youth's self-perceptions and ultimately affect the process of identity formation (Merton, 1957). According to one version of labeling theory (Hackler, 1970), youth labeled as "delinquent" by others begin to categorize themselves in this way, then act in accordance with the self-categorizations, which leads to further labeling. As this cycle progresses, beliefs and attitudes consistent with the developing delinquent lifestyle are endorsed and accepted, providing further support for continued delinquent activity, and making a change of lifestyle more and more difficult.

Both of these theories of delinquency had implications for treatment nd prevention of delinquency. According to control theory, the youth haracteristics that have a causal impact on delinquency are those ndicative of a weak "bond to society". Traditionally, the three most mportant areas of the youth's life, in which such bonds would tend to anifest themselves, have been thought to be family, school and peer elationships (Hirschi, 1969). To the extent these relationships are trong, the probability of delinquency should be low. This implies that y intervention which strengthens these bonds should reduce delinquency. e interventions to be discussed below did in fact focus on each of ese areas of the youth's life.

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Labeling theory holds that a criminal career starts when a youth is anointed with the "delinquent" label. Parents and teachers who label a child as a "delinquent" will expect the worst from him or her. They may be excessively punitive, may limit their involvement with the child or behave in other ways that exaggerate pre-existing problems (Becker, 1963). While labeling can occur for reasons that have nothing to do with the youth's behavior (for example, racial or ethnic prejudice) it can also occur in response to the youth's actual delinquent behavior. An especially strong source of labeling comes about when the youth is adjudicated in juvenile court (Goldenberg, 1971). Once the label is applied, it provides the fuel for further delinquency. This implies that "treatement" should focus on removing the "delinquent" label. Many of the interventions to be discussed here had this as a major goal. In the next section, some of the procedures that have been used by diversion programs to deter delinquency will be described.

Diversion interventions. In order to understand what actually happens in a diversion program, several components of the diversion approach must be reviewed. These include the organizational setting for the program, who the staff are and what specific interventions are used. Each of these components will be discussed here.

Many of the diversion programs in place today were first developed in the 1960's and early 1970's, at a time when "non-traditional" or alternative" approaches were being taken in dealing with a variety of social problems. The "alternative" approach to community services was based on the idea that hierarchically-organized bureaucratic institutions are by their nature bound to become ineffective and victim-blaming

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(Cowen, 1973; Kelly, Snowden & Munoz, 1977; Rappaport, Davidson, Wilson & Mitchell, 1975; Ryan, 1971; Sarason, 1972). The proliferation of diversion programs came at a time when the treatment philosophies of "community psychology" were sweeping through the fields of mental health (e.g. Kiesler, 1969; Marx, Test & Stein, 1973; Query, 1973; Smith & Hobbs, 1966) and education (e.g. Chesler, 1973; Grauhard, 1972; Sarason, 1971). While in the 1980's the community approach appears to be on the decline in mental health and education, community psychology treatment philosophies continue to find a home in juvenile diversion programs.

A large number of diversion programs are "community-based." This means, at a minimum, that the interventions take place outside of an institutional setting. In order to fully qualify as a community-based program, however, a diversion program must do more than simply provide services outside of an in-patient detention facility. In the language of community psychology, to be community-based meant to be "outside the system." Since "the system" included police and juvenile court staff (i.e., probation officers), fully community-based programs had as little to do with court, police and probation staff as possible. This arrangement was in part based on labeling theory. According to this philosophy, the experience and "expertise" of professional juvenile justice workers serves mainly to aid in the process of labeling youth rather than helping them.

Consistent with the goal of creating as much distance from "the system" as possible, diversion programs are often staffed by nonprofessionals or volunteers (Heller & Monahan, 1977; Levin, 1969; Reiskin, 1971). It was expected that this arrangement would minimize the

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problems of "staff burnout" and bureaucratization, endemic in human services organizations (Amdur, 1982; Rappaport, Chinsky & Cowen, 1974).

Another reason for the wide use of vounteers and non-professionals to staff diversion programs was that professionals tended to avoid certain client groups. It was hoped that less "knowledgable" service providers might be more willing to work with such groups (alcoholics, drug abusers, delinquents) with more of an open mind about the possibility of success (Gruver, 1971).

A further benefit in using volunteer staff was that permanent program staff could concentrate on training and supervision, rather than providing direct client services themselves. This extended their "productivity" (increasing the number of client service hours per staff member). There was also evidence suggesting that volunteers in human service agencies might improve the job satisfaction of human service workers (Weinstein, Gibbs & Middlestadt, 1979).

Theoretically, volunteers and non-professional staff could provide services that would be a good deal more cost effective than those delivered by professionals. Of course this would only be true if non-professionals could provide services that were effective. The evidence (challenged by Nietzel & Fisher, 1981) showed that in a broad range of therapeutic situations, non-professionals were at least as effective as professionals (Durlak, 1979, 1981).

The range of services delivered by diversion programs is quite varied. Dimensions on which programs vary include organizational setting, intensity of services, type of client, training and experience of staff, and treatment model. Some programs are run by probation

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officers at juvenile court (e.g. Bohnstedt, 1978), others by local community volunteers (e.g. Davidson, Seidman, Rappaport, Berck, Rapp, Rhodes & Herring, 1977), and still others by former gang members (Klein, 1971). The services that are provided include job training, remedial education (Shore & Massimo, 1973), parent effectiveness training (Dreikurs, 1971), social services coordination (Gardner, 1973), early detection and prevention (Cowen, Pederson, Babigan, Izzo & Trost, 1973; Rappaport, Seidman & Lamiel, 1977), use of crisis intervention teams (Baron, Feeney & Thornton, 1976), group and/or individual counseling (e.g. Collingwood, Williams & Doud, 1976), and recreation (e.g. Lipsey, Cordray & Berger, 1981). Often, a single diversion program uses interventions from several of these categories concurrently. Several of the more intensive treatment modalities will now be discussed in more detail.

Child advocacy is an intervention based on the idea that delinquency is the use of illegimate means to obtain resources that can not be obtained through legitimate channels (Cloward & Ohlin, 1960). The advocate helps the youth to obtain the same resources legally. Often this means helping the youth get a job, or arranging for job training. It also involves negotiating for the youth in social contexts where the youth feels discriminated against or otherwise unable to meet his or her own needs (e.g. in school, at home, in the legal system, etc.). The intervention may involve advocacy as well as training in self-advocacy.

Behavioral contracting is an intervention based on social learning theory (Bandura, 1977). Family therapy principles (e.g., Satir, 1967) may be employed when contracting is practiced in the family setting

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(e.g., Palmer & Lewis, 1980). The intervention involves establishing specific agreements between the youth and significant others (e.g. family members, teachers, etc.). The agreement specifies what the responsibilities of each party are, and what the consequences of failure to comply will be. It is arrived at by negotiation among all parties involved. The treatment agent is responsible for mediating negotiations and for monitoring compliance with the contract by all parties.

The therapeutic relationship is the intervention most similar to traditional psychotherapy or counseling. It is often based on a Rogerian counseling framework (Gendlin, 1986; Rogers, 1957; Truax & Charkhuff, 1967) in which the volunteer is empathic, non-judgemental and non-interventionist. The relationship intervention assumes that delinquency comes about in part because the youth feels alone, not cared for, powerless or unnoticed. It helps the youth "get in touch with" and verbally express feelings, rather than "acting them out". As interpersonal relationships become easier for the youth to negotiate, problems in many areas of the youth's life (e.g. school, family, peers) should diminish. This in turn should reduce the pressure that impelled the youth toward delinquency.

In addition to theoretically-based interventions, a large number of diversion programs use treatments that in the language of psychotherapy research would be called "non-specific" (generally consisting of recreational activities). In non-specific interventions, the youth has a relationship with someone who is concerned and who wants to help. The treatment agent does not provide specific focused interventions requiring formal training, but spends time with the youth doing whatever comes

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naturally. "Non-specific" interventions might be expected to work via some of the same mechanisms as the relationship approach. To the extent that the youth's delinquent behavior is a result of his or her feeling uncared for and alone, this kind of "attention-placebo" treatment might be helpful.

Research on intervention effectiveness. The effectiveness of diversion programs for young offenders has been evaluated in a number of studies. As might be expected, given the wide variety of programs that exist, the results are not consistent or easy to interpret. A number of studies have shown that youth diverted from further court processing are less likely to recidivate than controls who are not diverted. This was true in diversion programs involving crisis intervention and service brokerage (Baron, Feeney & Thornton, 1976), group and individual counseling immediately after police apprehension (Collingwood, Williams & Doud, 1976), vocational counseling (Quay & Love, 1977), a broad range of services including counseling, recreation, social services, remedial reading and substance abuse treatment (Lipsey, Cordray & Berger, 1981), intensive family therapy or involvement in a community center staffed by minority group volunteers (Palmer & Lewis, 1980), and child advocacy and behavioral contracting delivered by trained college students (Davidson et al., 1977; Seidman, Rappaport & Davidson, 1976). Reports of no reduction in recidivism or self-reported delinquency for diverted youth come from a similar number of studies (Binder, Monahan & Newkirk, in press; Klein & Carter, 1976; Romig, 1978; Venezia, 1972; also see review in Davidson & Redner, 1983).

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Methodological flaws have plagued much of the research on diversion efficacy. Problems that have been pointed out in reviews of diversion outcome studies include inadequate implementation of diversion and inadequate experimental design (Davidson & Redner, 1983; Klein, 1979; Romig, 1978). In fact, these reviewers have concluded that program evaluations have been so seriously flawed that they have not adequately tested the effectiveness of diversion. It seems that while diversion is not a panacea, it can be helpful in reducing recidivism, under certain conditions. What those conditions are remains unclear.

In research fields in which the literature is complex, ambiguous, and of uneven quality, a more quantitative approach to the review process can sometimes help to clarify the apparent confusion. Meta-analysis is a set of quantitative techniques for producing a better estimate of real effects by sorting out the various forms of error (Hunter, Schmidt & Jackson, 1982; Glass & Kliegl, 1983). In meta-analysis, the treatment outcome effect size can be estimated by cumulating results across studies. This set of techniques has been used recently in studying the effects of various forms of psychotherapy (Smith & Glass, 1977; Smith, Glass & Miller, 1980; Shapiro & Shapiro, 1982). A similar approach has been applied to the diversion outcome literature, in an attempt to determine whether diversion is effective, and which variables determine its effectiveness.

The conclusion reached in one recent meta-analysis of diversion outcome studies (Gensheimer et al., 1985) is that evaluative studies of diversion programs vary widely in the quality of methods used and in their conclusions about effectiveness. Thus the 95 percent confidence

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interval around the estimated effect size was fairly wide. It included zero (meaning that the possibility of diversion having no effect could not be ruled out). It also included negative effect sizes (meaning treated groups may have had worse outcomes than controls) and positive effect sizes generally below one standard deviation (meaning diversion may be somewhat effective). This wide variability in results occured even after the variance in effect sizes was corrected for sampling error. This meant that some moderator variable, which differed across studies, was influencing effect size.

The strongest moderator variable (correlated .76 with effect size) was the extent to which the researcher was involved in designing the treatment program. Researchers who designed their treatment programs tended to have more effective programs. This may mean that the result of the evaluation is more dependent on the quality of the research design than on the quality of the treatment program. It could also mean that diversion programs that are established with research in mind are simply more effective than other programs.

Attributes of the program and of the clients also moderated effect size. Diversion programs had more positive effects with youth who were younger (r between age and effect size = -.35) and when treatment agents spent more time working with youth (r = .69 between hours of intervention and effect size). The interpretation of these findings will be discussed in more detail below.

Evaluation of the Diversion Outcome Studies

One important question that is rarely asked in evaluation studies is: if the intervention was not effective, why wasn't it? An apparent

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lack of effect might occur because of a) measurement problems, b) experimental design problems, c) characteristics of the sample (like floor effects, restriction of range), or d) ineffective treatment. The last of these conclusions can not be reached until the first three possibilities have been eliminated. Each of these possibilities will be discussed separately.

Unreliability or measurement error can be a problem for both the dependent (i.e., outcome) variable and the independent (i.e., treatment) variable. In the presence of measurement error in both of these variables, a correlation between the treatment and the outcome variable may be reduced by measurment error to a non-significant "trend" in the results. For example, if both measures had reliabilities of .50, the real relationship between treatment and delinquency would be, on average, twice as large as their observed correlation. In this case, correcting for attenuation in both measures would be appropriate. This is almost never done by outcome researchers, however. If reliabilities are calculated at all, it is to establish that measures can be trusted. Once it is decided they can be, reliability is disregarded by the typical diversion outcome researcher. This means that in most outcome studies, effect size is attenuated by measurement error and the actual effect of the treatment is larger than the observed effect.

A second possible reason for no effects is poor or inadequate experimental design. A common problem is a poorly defined independent variable. Researchers seem to forget that being placed in the diversion condition is not supposed to reduce delinquency. Instead, this is supposed to lead to some specific intervention that the control group

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does not get, which is supposed to reduce delinquency. Yet diversion outcome research rarely includes any measure of this specific intervention. Placement in the experimental or control condition is assumed to be an adequate measure of the strength of this intervention. The problem is that without directly testing this assumption, we can not know whether a lack of treatment effect is produced by an inadequately implemented treatment for subjects in the experimental group. If it is, then we should conclude that the treatment was not delivered, rather than that it was ineffective.

Other assumptions implicit in the usual experimental outcome study are equally important and equally neglected. Researchers accustomed to using the one-way ANOVA approach to evaluation often do not consider why "process" measures, which tap the specific treatment processes assumed to reduce delinquency, are important (Davidson & Redner, 1983). The theoretical models underlying diversion interventions can be quite complex. These ideas can be represented by causal models in which interventions affect mediating processes which affect outcomes. For example, family therapy reduces family conflict, which reduces delinquency. Even if we have an adequate measure of whether or not the intervention was carried out (i.e., a better measure than the treatment group to which subjects were assigned), there is still a major problem if we neglect to measure the mediating process at which the intervention is aimed. If our implicit causal model includes three variables (i.e., intervention, mediator, outcome) but we only have two of these (i.e., intervention and outcome) in the experiment, null results or lack of treatment effects is a result that is uninterpretable. It may have

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occurred because the intervention did not affect the mediator (in which case the treatment was ineffective) or because the mediator did not affect the outcome (in which case the theory of delinquency was wrong), or both. Which of these explanations is correct will determine whether the next treatment that is tried should focus on the same mediatior, or a different one. It seems wasteful of expensive resources to conduct research that does not allow such fundamental questions to be answered.

Omitting process measures not only makes it difficult to understand or interpret experimental results, but it can also lead experimenters to draw the wrong conclusions about whether or not the intervention is effective at all. If we call the intervention, mediator and outcome variables A, B and C respectively, the implicit causal model being tested is A causes B causes C. The problem is that both the A-B and B-C relationships might be non-zero and moderately large, while the A-C relationship is non-significant. This is especially true for studies using small sample sizes. This means that simple independent group experimental designs, in which "process" is not measured, may not be able to provide an accurate assessment of the effectiveness of diversion and are completely inadequate for understanding why the treatment worked or didn't work.

Finally, characteristics of the sample might influence effect size because of treatment-by-subject interaction (e.g., younger youth improve more than older youth). If this were the case, studies evaluating younger clients would show greater effectiveness than studies evaluating the same interventions used on older clients. This would also lead to misleading conclusions about effectiveness based on studies using samples

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that were heterogenous with respect to age. Rather than conclude that treatment is slightly effective, in this situation it would be more appropriate to say that it is very effective for younger youth, but ineffective (or detrimental) for older youth.

Another possibility is that age is correlated with some other variable that determines effect size. One possibility, raised by Gensheimer et al. (1985) is that older youth engage in less delinquent behavior, so that treatment will appear less effective with them due to a floor effect. The estimation of effect sizes in the presence of moderator variables is one part of the problem. An equally important issue is to discover what the basis is for the effects of the moderator. In this case, why does age correlate with effect size?

Measurement error, inadequate research design and moderator effects would all tend to reduce observed effect sizes in diversion outcome studies. It was hoped that meta-analysis could help to sort out the real effects from the error. In the case of juvenile diversion, it may be that the field is not yet "ready" for meta-analysis (Hunter & Schmidt, 1981). This would be the case, for example, if too few outcome studies existed for a particular treatment method. In this case, the sample size for estimating effect size (in meta-analysis the sample size is determined by the number of research studies) would be too small to allow for adequate narrowing of the confidence interval. Another problem for meta-analysis occurs if treatments being evaluated in individual studies are not specified clearly or measured appropriately. This forces the meta-analyst to adopt the practice of lumping together "apples and oranges", a practice that has been criticized in psychotherapy outcome

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meta-analyses (e.g. Mintz, 1983). Gensheimer et al. (1985) reported similar problems of vaguely specified treatment variables in the diversion literaure. It appears to be a general problem in criminological research (Sechrest, West, Phillips, Redner & Yeaton, 1979). Another problem is that if the reliabilities of instruments used to assess treatment implementation and outcome aren't reported in outcome studies and corrected for in meta-analysis, the effect size estimates produced by meta-analysis will be biased toward zero just as they were in the original studies (Hunter & Schmidt, 1981; Hunter, Schmidt & Jackson, 1982). Typically, in the diversion literature, such information is not provided. For these reasons, the overall effect size estimates produced in meta-analyses of diversion outcome (Gensheimer et al., 1985) should be interpreted with caution.

The problems just discussed could be resolved by improved reporting of experimental conditions (especially better specification of the independent variable), reporting of measure reliabilities, and more research. A further problem with attempts to assess the effectiveness of diversion (as well as psychotherapy and other psychological interventions) is the use of independent groups designs in the original outcome studies. Effect size estimates produced in such experiments are invalidated by treatment-by-subjects interactions (Hunter, 1977b). However, the implications of this fact are rarely acknowledged by the outcome researcher or meta-analyst. Treatment-by-subjects interactions occur when response to treatment depends on a third variable. This would occur, for example, if diversion worked for some youth but not for others. Differences in variance between treatment and control groups on

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the dependent variable indicate that such interactions may be occurring (Hunter, 1977b). Treatment-by-subjects interaction may also occur in the absence of differences in group variances (Hunter, 1985b), making them impossible to detect in many outcome studies using independent group designs (they can be detected in within subjects designs, making these designs superior). We do not place too much importance on the size of main effects in individual studies having significant interactions, and for the same reasons, we should not try to cumulate effect sizes across studies in which it is likely that there are interactions.

In an ideal world, the solution to this problem would be to abandon the independent-group design in favor of the within-subjects design. This is impossible in research on treatment outcome because subjects can not be run through one treatment condition, followed by a second and a third treatment condition. Another solution to this problem involves replacing simple between subjects experimental designs with more complex designs involving both experimental manipulations and measurement of ongoing processes within subjects. Such a method will be illustrated in this dissertation.

The response to the problem of treatment-by-subject interactions within individual outcome studies in recent meta-analyses of psychotherapy outcome has been to ignore it and to simply cumulate main effects as if the interaction did not exist. This occurred in one study (Smith et al., 1980) in spite of evidence that a strong treatment-by-subjects interaction existed (i.e. a four-fold difference in variances between experimental and control groups, across studies).

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A significant moderator effect in a meta-analysis is equivalent to an interaction at the level of individual studies. The finding by Gensheimer et al. (1985) of significant moderators of diversion effect size means that simple diversion effect size estimates are superfluous. It appears that diversion may be effective for youth who are younger, when an appropriate level of intervention intensity is provided, and when researchers take part in the design of the intervention and the outcome study. As Gensheimer et al. pointed out, the appropriate course of action should be to learn more about why diversion is more effective for younger youth, how much intervention is enough, and how diversion effectiveness can be improved.

In this section, various reasons were proposed to explain why no clear conclusions about the effectiveness of diversion can yet be made. The evidence shows that overall, diversion is not very effective (Gensheimer et al., 1985). However, some programs are effective with some youth (e.g. Bohnstedt, 1978; Davidson et al., 1977). Individual outcome studies have had difficulty determining why certain interventions are effective, due to confounding variables, interactions and measurement problems. Meta-analysis has so far not been able to resolve these issues, but has pointed the way to further research. Whether diversion can be made more effective, and how it works, are questions that current research strategies can not answer.

Research Goals

Like most psychological interventions, diversion programs provide evaluation researchers with a number of complex problems. The complexity comes about because interventions often include multiple components

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designed to influence a variety of processes in the client. The complexities of the intervention and of the targeted response pose quite a challenge to the researcher interested in knowing which of the intervention's components are the effective ingredients and how they work. Yet without understanding an intervention's mechanisms of action, it is impossible to make it more effective. It also seems to be the case, at least in the diversion literature, that accurate assessment of the effectiveness of the intervention will depend on a better understanding of the processes by which the treatments work.

The first goal of this dissertation is to illustrate one method for studying the mechanisms of effect of juvenile diversion programs. This method should also be applicable to the study of other complex psychological interventions. While the methodology used in this dissertation may be relevant to a broad range of clinical and community interventions, the substantive problem that is the focus of this study is juvenile diversion. The second goal of the present study is to understand why diversion interventions are effective and how they can be made more effective. These questions will be answered by examining data collected over a seven-year period in an adolescent diversion program.

Diversion interventions are based on theories of why adolescents engage in delinquent behavior. This means that testing the efficacy of an intervention is one way of testing the theory on which the intervention was based (Gottfredson, 1984). To the extent that an intervention is successful, one would expect the theory on which it was based to gain in stature. A third goal of this dissertation is to "test" two of the more well-known theories about the etiology of delinquency.

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The extent to which the propositions of control and labeling theories explain the results of the diversion interventions will be examined. Because these two theories are the basis for many of the interventions carried out by juvenile diversion programs, it is worth asking what evidence there is that either of the theories is correct.

While both of these theories of delinquency seem intuitively plausible, researchers have had difficulty finding ways to "test" these theories empirically. Methodological difficulties have prevented researchers from providing compelling support for any of the theories of delinquency, in spite of numerous attempts to match one theory against another (Amdur, 1985b). Researchers attempting to test delinquency theories often misinterpreted their data or used methods inappropriate for testing causal theories. The result has been a confusing but persistent set of arguments in the literature between proponents of different theories. There has also been ongoing controversy between those researchers who see the different theories of delinquency as mutually exclusive (e.g. Hirschi, 1979) and those who believe the theories should be integrated (Conger, 1976; Elliot, Ageton & Cantor, 1979; Elliot, Huizinga & Ageton, 1985). Meanwhile, the relative merits of the two theories as explanations for delinquency remain unsettled.

The reasons for the failures of earlier studies of delinquency were remarkably consistent (Amdur, 1985b). Problems included poor measurement, use of methods inappropriate for testing causal models, lack of testable hypotheses derived from theory, and use of data from only a single time period. These problems will be addressed and rectified in

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the present study, so that some conclusions about the relative validities of the two theories may be possible.

The validity of control theory can be tested by seeing to what extent strengthening the "social bond" leads to a reduction in delinquency. Labeling theory can only be tested indirectly in the present study. This is because we do not have direct measures of labeling, of parental and teacher expectations, or of youth self-categorizations. The test of labeling theory that will be used here depends on two assumptions: that delinquency (defined as deviant behavior itself, or official contacts with police or courts) results in labeling, and that it is this labeling that accounts for many of the effects of delinquency. Thus, labeling theory will be tested by examining the extent to which the youth's delinquency leads to family problems, poor performance in and attitudes toward school, and other undesirable outcomes.

In their recent meta-analysis of psychotherapy outcome studies, Smith, Glass & Miller (1980) said that "evaluation is different, in important ways, from scientific research." They were pointing out the distinction between studies designed to evaluate the effectiveness or "outcome" of treatments and studies designed to elucidate the processes or mechanisms through which treatments have their effects. The present study has as its goal the elucidation, rather than the evaluation, of the process of juvenile diversion. While the distinction between these two modes of research seems clear, this dissertation will raise the issue of whether, in practice, it is possible to separate the two. A case will be made for the conclusion that adequate "evaluative" research is impossible

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unless based on the firm footing provided by prior elucidation of the processes underlying intervention effects. This is, in essence, the conclusion now being reached by psychotherapy researchers from varying theoretical perspectives (Gendlin, 1986; Strupp, 1986; VandenBos, 1986).

Researchers interested in elucidating the treatment process have searched for methods adequate to the task, largely unsuccessfully. Behaviorally-oriented psychotherapy researchers have joined those who are psychodynamically-oriented (e.g. Strupp, 1963) to bemoan the lack of relevance most research has to clinical work (Barlow, 1981). Solutions that have been proposed for psychotherapy research include the use of single-case designs that could be easily implemented by practicing clinicians (Hayes, 1981; Kazdin, 1981; Kiesler, 1981), improved dependent measures (Nelson, 1981), and assessment of clinical significance rather than statistical significance of outcomes (see review in Jacobson, Follette & Revenstorf, 1984).

Although these proposals might advance the study of psychotherapy if implemented, none would provide researchers with the tools to cope with complex sets of data. The complexities involved in the study of psychological treatments were brilliantly illustrated years ago (Kiesler, 1966). However it is only recently that statistical methods have been developed that could help researchers make sense of data consisting of multiple independent, dependent and intervening variables, all at multiple time periods. The present study will use one such method, a form of path analysis (Hunter & Gerbing, 1982).

Some of the specific questions about diversion that this dissertation will seek to answer include the following: what is the

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effect of the youth's age on the diversion process? Why do younger adolescents appear to gain more from diversion than older adolescents? By what mechanisms do interventions like child advocacy and behavioral contracting affect recidivism? Can we demonstrate an effect on recidivism, for these interventions, that is mediated by changes in such youth life events as school and family involvement? To what extent are we able to isolate youth life events that contribute to ongoing delinquency? To what extent is treatment ineffectiveness a function of interventions not having any impact on the significant youth life events just mentioned? To what extent is it a function of these life events not affecting delinquency? What conclusions would an "evaluative" research design have led to that are contradicted by an "elucidatory" design?

In this chapter, the treatment philosophies guiding the development of diversion interventions were described. Two of the theories of the etiology of delinquency that have guided intervention were then outlined and the types of interventions provided by diversion programs were reviewed. Evaluations of diversion interventions were also reviewed. It was pointed out that the evaluations have produced conflicting results. Problems in the design of the typical outcome study were described. The distinction between "evaluative" and "elucidatory" modes of research was introduced. It was pointed out that most research examining psychological interventions has been "evaluative". This has particularly been true in studies of juvenile diversion. In part this trend may be a result of the lack, until recently, of an "elucidatory" technology that was available to the non-statistician. It was pointed out that a method for doing "elucidatory" research on the intervention process is needed.

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Specific questions regarding the diversion process were introduced. In the next chapter, a method based on path analysis will be described for elucidating the diversion process.

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METHODS

Subjects

This study was conducted as part of a larger project designed to implement and evaluate the effectiveness of an adolescent diversion program. Youth referred from juvenile court were randomly assigned to one of seven treatment conditions, or to a treatment-as-usual control group. Those in experimental treatment conditions were assigned a trained volunteer who worked with the youth for a period of eighteen weeks.

A total of 229 youth participated in experimental treatment conditions. They were referred to court mainly for misdemeanors or minor felonies. Youth ranged in age from nine to seventeen (mean age = 14.21; s.d. = 1.5). Seventy-five percent of subjects were white; 25 percent were Hispanic or Black. Eighty-two percent were male; 18 percent were female.

Treatment Conditions

Seven different experimental treatments were used. These were: Action-Undergrad, Action-Community College, Action-Non-Student, Family, Relationship, Low Intensity, and Action-Court Supervised. Each condition will be described separately, but first the elements they had in common will be described.

The project ran for seven successive calendar years. Each fall, a new set of volunteers was recruited and trained. All of the treatment conditions were not used in any one calendar year. During the first four years of the project, only the Action-Undergrad, Family, Relationship, Low-Intensity, and Action-Court Supervised treatments were used. Within

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each of these four years, volunteers were randomly assigned to treatment conditions. During the last three years of the project, only the Action-Undergrad, Action-Community College and Action-Non-Student treatment conditions were used. Here, random assignment to treatment conditions was precluded by the fact that a subject variable was the only dimension on which the treatments differed. Delinquent youth were always assigned randomly to treatments, or to a treatment-as-usual control group.

Each volunteer assigned to a treatment condition received training in the intervention to be used in that treatment condition. Each volunteer worked with a single youth for a period of eighteen weeks. Volunteers were supervised throughout the period of the intervention. This occurred in groups made up of volunteers from a single treatment condition. Except in two conditions (Action-Court Supervised and Low Intensity), training and supervision of volunteers was done by advanced graduate students in psychology, who were in turn supervised by faculty members in psychology. The treatment conditions will now be described in more detail.

Action-Undergrad (N=101). College student volunteers were recruited by advertisements placed around campus and by course announcements mailed to students' home addresses. (Students received course credit for participation in the project). In order to qualify, students had to attend two preliminary meetings in which the project was explained and measures were filled out. The majority of students who decided to participate were majoring in psychology, social work, or criminal justice.

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Training was carried out using a didactic approach including assigned reading, group discussion, role-playing, and periodic tests. The content of training included theories of delinquency, information about the court system, and systematic study of the actual interventions to be used. Included in the training were theoretical ideas about the interventions as well as practical information about how to implement them.

Two different intervention strategies were used in all the Action conditions (i.e. in Action-Undergrad, Action-Community College, Action-Non-Student and Action-Court Supervised). These were behavioral contracting (Stuart, 1971; Stuart & Tripodi, 1973) and child advocacy (Davidson & Rapp, 1976; Tharp & Wetzel, 1969). Some of the theories guiding each of these interventions were outlined in the previous chapter. Training in both of these intervention strategies included segments on evaluation of the youth's needs, including strengths and problem areas; design of interventions focused on creating changes in important problem areas; monitoring of compliance with the intervention; and training of the youth in the use of the same approach to problem-solving.

Action-Community College (N=35). This condition differed from the previous one in one respect only. Instead of using undergraduates from a four-year University, the volunteers in this condition were community college students. On average, these students were older than the other undergraduates, and were more likely to be attending school part-time, and to have jobs and families. Methods of training and supervision, as

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Action-Non-Student (N=18). Again, the content of the intervention in this condition was identical to that just described, as were the methods of training and supervision. This condition differed from the previous two only in the type of volunteers who were recruited. For this condition, volunteers from the local community were recruited by placing advertisements in local newspapers, as well as electronic media, and by soliciting the help of local church and neighborhood volunteer groups. These volunteers were generally older than other volunteers, and were often married women, with husbands who worked and children who were in school.

Both the Action-Non-Student and Action-Community College conditions were designed to test the effects of the age and experience of volunteers on treatment outcome. In these two conditions, the model of intervention and methods of training were identical to those used in the Action Undergrad condition. Thus differences in the strength of intervention or its impact on delinquency between the Action Undergrad and these other two Action conditions were hypothesized to be a result of the differences in volunteer characteristics that existed between these conditions.

Action-Court Supervised (N=12). College students were recruited for this condition in exactly the same way as for the Action-Undergrad condition. The content of training and the strategies used in the intervention were also identical to those in the other Action conditions. What differed in this condition was who trained and supervised the volunteers. Rather than psychology graduate students, the training and

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supervision was conducted by a juvenile court employee, who was in turn trained and supervised by the faculty members in charge of the project. This condition was originally used to test the effects of having the diversion program run "within" versus "outside" the juvenile justice system.

Family (N=24). College student volunteers were used as treatment agents in this condition. The recruitment process was described above. The training process was identical to that already described. However the content of training differed from that used in the Action approach. In the Family condition, only a single intervention strategy was used: behavioral contracting. In particular, the intervention focused on using the behavioral contracting approach in the family. Contracting outside the family (e.g., in school) was not done in this condition.

Relationship (N=12). College students, recruited as already described, were again used as treatment agents. The training process was the same as that already described. In this condition, the content of training was very different than in the Action conditions or the Family condition. The focus of this intervention was the therapeutic relationship. Volunteers were trained in the skills of Rogerian counseling. Supervision involved monitoring the extent to which each volunteer was able to maintain the qualities of empathy, unconditional positive regard and genuineness.

Low Intensity (N=27). This condition was designed as an attention-placebo control group. For reasons discussed in the previous chapter, in some respects it may have functioned through mechanisms similar to those underlying the Relationship, Action, and Family conditions. The extent

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to which training in a specific intervention modality was necessary or beneficial could be determined by comparing interventions used by this group to those used by volunteers from the Action, Relationship and Family conditions.

In this condition, college student volunteers (recruited as already described) were supervised and trained using different procedures than in all the other treatment conditions. Rather than being trained and supervised in small groups (six to eight volunteers with two supervisors), they met in large groups (nine to fifteen volunteers with one supervisor). Rather than meeting weekly, they met monthly. Rather than being trained in specific intervention techniques, they were encouraged to do what came naturally. This generally involved talking to the youth about problems and/or doing recreational activities together. Training was not done by psychology graduate students, but by supervisors recruited from the State Volunteer Bureau.

Measures

Four different sets of measures were used: official measures of delinquency, self-report measures of delinquency, measures of intervention process, and measures of youth life events. Data for official delinquency were collected by examining police records in all neighboring counties and towns, as well as juvenile court files. Data for all other measures were collected by trained interviewers who met with the youth. (These interviewers were also college students, but not the same ones who participated in the intervetion). Specific data collection and measure development procedures will be discussed for each measure separately.

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Self-report measures of delinquency. The self-report measure used here was similar to those used in earlier studies (Gold, 1970; Williams & Gold, 1972). A trained interviewer met with the youth and asked him or her whether or not he or she had engaged in each of 35 behaviors in the preceeding six weeks, and if so, how often.

The measure originally consisted of 35 items. An earlier study using a portion of the current data failed to distinguish more than a single dimension on this measure (Blakely, Kushler, Parisian & Davidson, 1979). In a more recent study using the complete data set (Amdur, 1985a), cluster analysis did find distinct clusters, although these were strongly intercorrelated (items and clusters are listed in Appendix A). It was found that the seven first-order clusters could be grouped into two second-order clusters which were internally consistent (reliabilities are given in Appendix B) as well as having a clear and meaningful interpretation (second order cluster composition is presented in Appendix B).

In the present study, both of the second-order clusters derived from the self-report delinquency measure were used as variables. The first cluster was a general measure of delinquency. It was composed of first order clusters that measured theft, violent and destructive behavior, and major school problems. The second cluster (which was composed of first order clusters from both the self-report delinquency measure and the youth life event measure that will be described below) contained clusters that measured both skipping school and using drugs and alcohol with friends.

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Archival measures of delinquency. The earlier measurement study (Amdur, 1985a) showed that number of police contacts and number of court contacts clustered together to form a unidimensional "official contact" scale and that this cluster structure held up across four time periods. This cluster was the measure of official delinquency that was used in the present study. It was a measure of the extent to which the youth had contacts with police or courts that were officially recorded.

Youth life event measures. Data collection procedures for this measure differed from those used in collecting self-report delinquency data. A structured interview with the youth was taped and later coded by trained raters. (Mean inter-rater reliability for coding interviews was .59 for three scales established in an earlier phase of the study). Target youth were interviewed, as well as the youth's parents and the volunteer. The agreement between these different sources was high (mean r's across scales were .83 for youth-parent, .80 for youth-peer and .87 for parent-peer sources, using earlier scale definitions). Only the youth data were used in the present study. An earlier measurement study (Amdur, 1985a) found ten clusters of life event items which were positively intercorrelated (Appendix A). These clusters formed two second order clusters (in addition to contributing to the Drop Out cluster as described above) (see Appendix B). One of these, a School Involvement cluster, was composed of first order clusters measuring the youth's performance in and positive attitude toward school. The other measured the youth's perception of his or her parents' knowledge of what the youth did with friends and in school. In addition to these second order clusters, two first order life event clusters not fitting into any

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second order clusters were used in the present study. These measured the youth's involvement with peers and the extent to which there were arguments between the youth and his or her parents.

Intervention scales. Data collection procedures for this measure were identical to those used for the youth life events measure. Taped interviews were scored by trained raters (mean inter-rater reliability was .75 using six earlier-defined scales). Again, data collected from the youth were highly correlated with data collected from other sources (mean youth-parent r = .89; mean youth-peer r = .88, using six earlier-defined scales), and only youth data was analyzed in this study.

Cluster analytic procedures identical to those used to develop self-report delinquency and youth life event scales were used in this dissertation to develop intervention scales. This procedure will be described below, after a discussion of the time periods at which data were collected.

Other measures. In addition to measures derived from the four scales just discussed, variables which coded the youth's age and the treatment condition in which the volunteers were trained and supervised were also used in path models. The treatment condition variables were scored as dichotomous variables. Since assignment of youth to treatment conditions was done randomly, it can be assumed that treatment condition variables must be exogenous to all other variables. This means that if any correlation exists between treatment condition and other variables in a path model, the direction of causality is from treatment condition to the other variables.

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Time periods. For each youth-volunteer pair, data were collected at six different time periods (Table 1). The first data collection was at the start of training. The next three data collection periods followed at intervals of six weeks. This meant that the fourth data collection occurred as the intervention terminated. The fifth and sixth time periods corresponded to one- and two-year follow-up, respectively. The self-report delinquency, youth life events and archival delinquency measures were all collected at all six time periods. Intervention scale data were collected during the intervention only.

Period:	
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Youth Life Events	
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Table 1

Time Periods at Which Data Were Collected for Different Measures

Period:	1	2	3	4	5a	6
	Time					
Measure	Start Training	6 weeks	12 weeks Intervention	18 weeks	1-Year Post	2-Year Post
Youth Life Events	+	+	+	+	+	+
SRD	+	+	+	+	+	+
Archival Outcome	+b	+	+	+	+	+
Intervention		+	+	+		

Note. Unless otherwise stated, time periods covered by each measure are the six weeks prior to the time of measurement. SRD = Self-Report Delinquency scale.

a. For 1- and 2-year follow-up, the time period covered is one year, for all measures.

b. The time period covered for archival outcome measures at time one is the entire year prior to starting the program.

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Life event and intervention items referred to events that had occurred in the prior six weeks. Self-report delinquency items referred to the prior year. The prior six-week period was double-weighted, however, since self-report items were actually composites of two responses asking how often each behavior occurred, in the past year, and in the past six weeks. Archival outcome data were coded to correspond to the six time periods for the other measures. The first time period for archival data included all official contacts in the year prior to starting the intervention. The second through fourth time periods of official data recorded official contacts during intervals of six weeks after starting in the project. This meant that time four included official contacts in the six weeks prior to termination of the project. The fifth time period included all contacts in the first year after termination. The sixth time period included all contacts in the second year after termination.

Cluster Analytic Procedure

Cluster analysis (Hunter, 1977a, 1980; Hunter & Gerbing, 1982) was used to determine how to group items from the intervention measure to produce meaningful and reliable scales. The criteria that items had to satisfy in order to be grouped into a cluster included homogeneity of content, internal consistency and "external parallelism" (see Amdur, 1985a; Hunter, 1980, where these criteria are explained in detail).

Once the first-order cluster structure of the intervention measure was determined, the cluster intercorrelations were examined to see whether the clusters were positively intercorrelated. A positive intercorrelation might indicate the existence of a causal relationship

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between these interventions (e.g., intervention A caused intervention B). It could also occur because both intervention variables are measures of a single higher order dimension (e.g., interventions A and B occur whenever intervention C occurs, since C is composed of A and B). It could also occur for other reasons (e.g. A and B are different interventions that occur in different situations, but both are often used in a particular situation which is fairly common). Which of these different alternatives is correct can not be determined by simply looking at the correlations themselves. Since a major goal of this dissertation was to understand the mechanism by which diversion interventions work, it was important to decide which of these explanations for positively intercorrelated first-order clusters was correct. Also, to the extent that intervention clusters derived from the initial cluster analysis were actually measures of a higher-order construct, it would simplify path models if the single higher-order cluster were used, rather than multiple lower-order clusters.

For these reasons, the first order intervention cluster intercorrelation matrix was examined to determine whether second order clusters existed. This was done using cluster analysis of the entire data set (i.e. correlations betweeen intervention first order clusters, youth life event and self-report and official delinquency second order clusters). A second order measurement model for the intervention scale could then be established.

Once this analysis was completed, all the variables that were to be used in the path analyses were available. Before running path analysis

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it was necessary to correct the observed correlations for two kinds of error that would bias the path coefficients.

Data Correction Procedures

While path analysis could be run using the matrix of observed correlations between variables, the path coefficients that would result would then be incorrect. It is generally true for multiple regression and related methods (ordinary least squares path analysis is based on multiple regression) that measurement error in the independent variables creates a systematic downward bias (i.e. toward zero) for many of the beta weights (or path coefficients). While measurement error in a given predictor will reduce its observed beta below the true value of beta for that predictor, betas for other predictors in the equation might be upwardly biased (i.e. away from zero) (Hunter,1985b; Hunter & Cohen, 1974). These biases can lead to misinterpretation of regression or path models. Correction for attenuation solves this problem. Correction for attenuation (or unreliability) involves dividing the observed correlation by the product of the square roots of the reliabilties of the two measures being correlated (Lord & Novick, 1968). The corrected correlation that results is the correlation that we would have found if we had used measures that were less unreliable (in particular, if both measures had been perfectly reliable). All intercluster correlations used in this study were corrected for attenuation.

In longitudinal data there is another source of bias that needs to be corrected. Correcting for attenuation leads to obvious positive bias in test-retest correlations. The bias is obvious because cross-time correlations are occassionally significantly greater than one.

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Correction for attenuation did not create this bias, but made it more obvious. The upward bias is a result of specific factors that influence a measure at more than one time period. Specific factors tend to depress item validity within time periods. That is, the item's correlation with its own cluster will drop since some of its variance is a result of the causal impact of the specific factor, rather than of the contruct measured by the cluster. This means that specific error reduces cluster reliabilities (when calculated using alpha) at the same time that it increases cluster test-retest correlations.

The impact of specific error on test-retest correlations can be estimated and removed (Hunter, Coggin & Gerbing, 1981). The estimation/correction procedure can only be used on clusters (not on individual items). For a given unidimensional cluster at two time periods, the estimate of specific error in the cross-time correlation is based on the ratio of average diagonal element to average off-diagonal element, in the cross-time inter-item correlation matrix. This is because the diagonal elements of this matrix will increase with true score cross-time correlation (for the cluster) and with specific error (for the item). Off-diagonal elements will only be influenced by true-score cross-time correlation (for the cluster).

The influence of specific error on cluster cross-time correlations increases as the number of items in the cluster decreases. This is because the ratio of diagonal elements to off-diagonal elements in the cross-time inter-item correlation matrix increases as the number of items decreases, and it is the diagonal elements of the matrix that cause the problem. Therefore the problem of over-estimated cross-time correlations

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may be especially serious for clusters of two, three or four items. This is the size of many of the clusters used in the present study. All cross-time correlations for clusters used in the present study were corrected for specific error.

Path Analysis

An attempt was made in this dissertation to avoid a purely exploratory approach to the data analysis. This meant that the processes at work in the interventions and the corresponding path models were specified a priori. This reduced the potential for capitalizing on chance. Because of the large number of variables available, some means had to be used to select the models to be examined, from among the very large number of potential models. Models were selected for testing that helped to elucidate the processes through which major elements of diversion were hypothesized to have their effects. Models that helped to evaluate the relative validities of the two causal theories of delinquency were also tested.

A second point has to do with the order in which models were tested. While the overall goal was to find out how the diversion process works, there are at least two ways of interpreting what is meant by "diversion" or "intervention." The first focuses on specific interventions (i.e., the behaviors volunteers actually engaged in). The second focuses on treatment modalities (i.e., the conditions of training and supervision that guided what the volunteers actually did). Since volunteers were assigned to treatment modalities prior to training, the assumption is that treatment modality determines (i.e. has a causal impact on) intervention behavior, and that the reverse can not be true. Models that

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examined the effects of specific intervention behaviors were tested first. Treatment modality variables were then added to these models. In this way, the mechanisms of effect of diversion, in both senses of that term, could be examined.

In this section, a general model of the diversion process will first be proposed. This will be used as a basis for a discussion of the path analytic technique. Once it is clear how path analysis works, a group of models to be tested will be presented.

A general model illustrating the process by which the intervention was believed to work is shown in Figure 1. The model states that delinquency at follow-up is the result of some youth characteristic which is present when treatment terminates (path d). The youth characteristic in question is believed to be present at pre-treatment, as well, and to persist over time (path c). The intervention, which occurs prior to treatment termination, is believed to have a causal impact on the youth characteristic (path b). Furthermore, since the intervention occurs in response to the volunteer's assessment of the youth's problem areas, it is believed that the presence of the youth characteristic at pre will determine whether or not the intervention is delivered, and in what strength (path a). Thus (if we assume the youth characteristic is scored such that positive indicates the socially-desirable direction) the signs of path coefficients a through d in Figure 1 are believed to be negative, positive, positive, negative, respectively.

The path analytic method. In order to understand what it means to "test" a model like the one in Figure 1, it is necessary to know some of the basic principles of path analysis, and in particular, of the

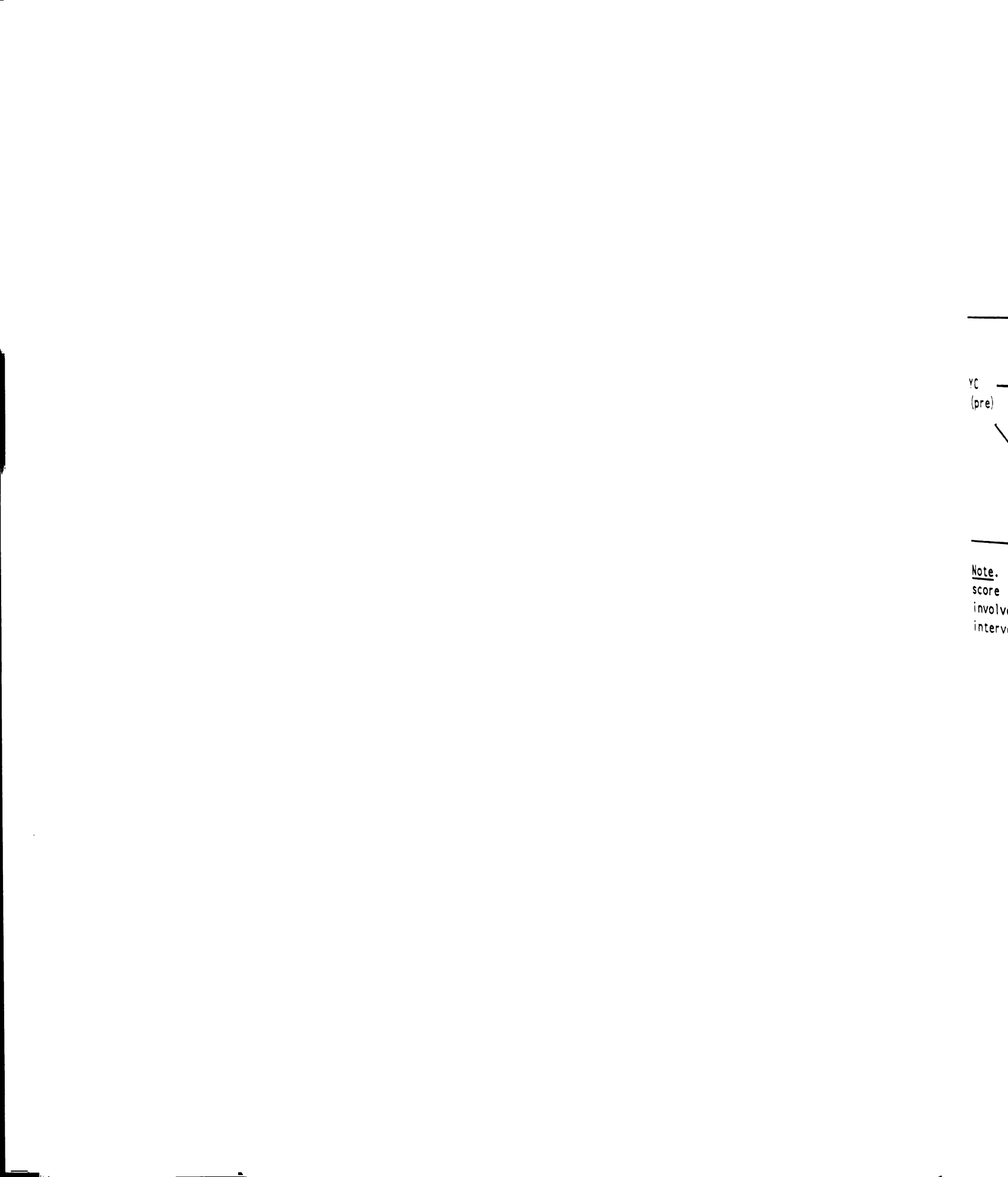
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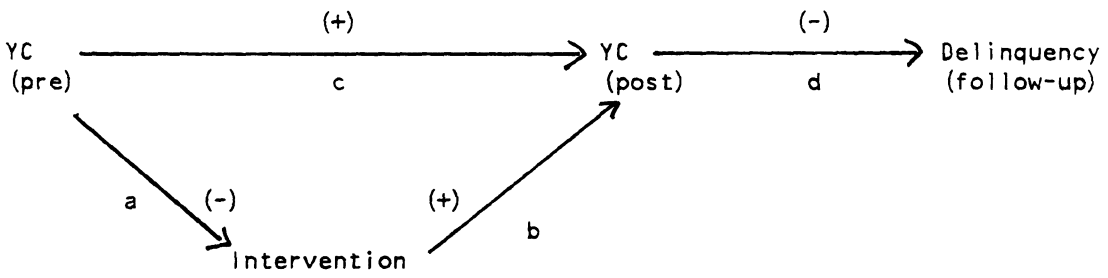
Figure 1

A General Model of Intervention



Note. YC = Youth Characteristic. For the youth characteristic, a positive score indicates the more "socially desirable" direction (e.g. more school involvement). Positive score for intervention indicates stronger intervention. Positive score for delinquency indicates more delinquency.

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"ordinary least-squares" method of path analysis (Hunter & Gerbing, 1982), that was used here. The method begins with the observed correlation matrix for the variables in the model being tested, and with the causal ordering of the variables, specified in the path model. The causal order is the set of statements about which variables have causal impacts on which other variables in the model. Given the simple correlations and causal order, estimated path coefficients are calculated. The path coefficient that is calculated is identical to a beta weight for those cases in which a dependent variable has multiple causal antecedents. (For example in Figure 1, Youth Characteristic-post has two causal antecedents, so both b and c are beta weights). The path coefficient is the simple correlation in those cases where a dependent variable has a single causal antecedent (for example path d in Figure 1). These path coefficients are used to generate a "reproduced" correlation matrix between the variables. The exact procedure for arriving at "reproduced correlations" is outlined in an earlier paper (Amdur, 1985b), and described more fully elsewhere (Hunter, 1977). This "reproduced" matrix contains the set of correlations between the input variables that is consistent with the assumptions about the relationships betweeen variables implicit in the causal order. That is, if the causal order were correct, the "reproduced correlations" would be the same as the observed correlations between variables.

The first question that could be asked about a path model like the one in Figure 1 is whether it in fact describes a set of processes that actually occur. This question, translated into path analytic language, might be phrased: "is the causal order correct?" In order to answer this

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kind of question, the "reproduced correlations" are compared with the actual observed correlations. To the extent that these two matrices are different, the causal order specified in the model is incorrect. To say this another way, to the extent that the causal assumptions made in the path model imply correlations between variables which do not match the actual correlations, the causal assumptions of the model must be false. In path analytic language, another way of asking this question would be: "does the model fit the data?"

The second kind of question that can be asked about a model like the one in Figure 1 is dependent on a positive answer to the first question. If the model does fit the data, a more detailed look at the relative strengths of the path coefficients can begin. This is important because the model can say very different things about the intervention, depending on the relative strengths of the coefficients in the model. For example, possible interpretations of the path model in Figure 1 are given in Table 2, for four hypothetical situations.

In situation 1, the intervention occurs when it should, that is, when the youth characteristic at pre indicates that there is a problem (path a). However the intervention has little impact on the youth characteristic (path b), so that the persistence of the characteristic over time remains high (path c). The youth characterisic does in fact lead to delinquency (path d). Thus the intervention is ineffective in reducing delinquency because it does not have the effect it was expected to have on the intervening process.

In situation 2, the intervention occurs when it is called for (path a) and it has the expected strong impact on the youth characteristic

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Table 2

Possible Interpretation of the Path Model in Figure 1

Hypothetical Situation	Path Coefficients a	b	c	d	Process	"Evaluation-mode" Conclusion
1.	-.40	.10	.70	-.40	Interven. has little impact on an intervening process or on delinquency.	Intervention is ineffective
2.	-.40	.60	.30	-.10	Interven. has strong impact on an intervening process but this is the wrong process.	Intervention is ineffective
3.	-.10	.60	.30	-.40	Interven. reduces delinquency thru its impact on the intervening process. But the interven. rarely occurs when needed.	Intervention is effective
4.	-.40	.60	.30	-.40	Interven. occurs when needed, effectively treats the intervening process and thereby reduces delinquency.	Intervention is effective

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toward which it was targeted (path b). However, this particular youth characteristic is not one that leads to delinquency (path d). Therefore, while the intervention works exactly as expected with respect to the intervening process, it has little impact on delinquency.

In situation 3, the intervention is effective in reducing the persistence over time of the youth characteristic (paths b and c). Since the youth characteristic does lead to delinquency (path d), the intervention, when it occurs, reduces delinquency. The problem is that the intervention rarely occurs when it should (path a). In this case, while in one sense the intervention is effective (i.e., it does reduce delinquency), in another sense it is ineffective. For some reason (e.g. the intervention might be difficult to carry out, the problem might be difficult to assess correctly, etc.) the intervention is not being implemented in situations where it could be effective.

In situation 4, everything works perfectly. The intervention occurs when it is called for (path a), it has a strong impact on the youth characteristic (path b), reducing the persistence of the youth characteristic over time (path c). Since the youth characteristic is an important cause of delinquency, the intervention reduces delinquency.

Table 2 also illustrates the conclusions that would be reached when looking at the same data in an "evaluative" research mode. This might happen, for example, if the researcher examined one-way ANOVA tables with intervention as the independent variable and delinquency at follow-up as the dependent variable. The same result would be achieved by examining the simple correlation between intervention and follow-up delinquency.

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In situation 1, the correlation between intervention and delinquency would be near zero, and the conclusion would be that the treatment had no effect. This would be an accurate conclusion. In situation 2, the correlation between intervention and delinquency would again be near zero, and the conclusion would again be the same: no effect. This would be inaccurate. The intervention actually had exactly the effect it was intended to have. "Strengthening" the intervention would do no good, since it is aimed at the wrong process. In situation 3, the correlation between treatment and delinquency would be fairly high (and negative). The researcher would conclude that the intervention works, and possibly that more volunteers should now be trained to use exactly this intervention. Here, the results of evaluative research would be correct in a sense, but misleading. Something would have to be done to discover the cause of and to solve the problem in implementing the intervention before it would be a very good idea to use it on a larger scale. In situation 4, the correlation between intervention and delinquency would again be large, and the conclusion would again be that the treatment is effective. This conclusion would be accurate. In two of the four possible situations, the "evaluative" approach leads to conclusions that are wrong, or misleading in important ways. Here, the "elucidatory" research mode is not simply a luxury, but a necessity if one is to correctly interpret the data.

The preceeding discussion of alternative interpretations of the meaning of the causal model was based on the assumption that the model did fit the data (i.e. that the causal order specified in the model was correct). What should be concluded if the model does not fit the data?

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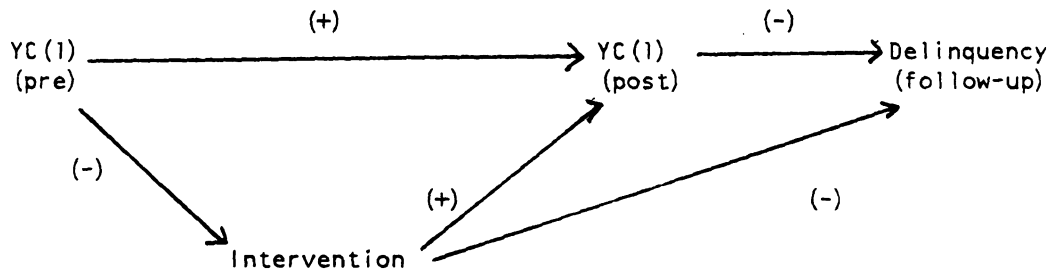
A significant difference between the "reproduced correlation matrix" and the observed correlation matrix indicates that the causal order specified in the model is incorrect. The steps that are taken next will depend on the exact nature of the departures from good fit.

One way in which the model specified in Figure 1 might not fit would be if the reproduced correlation matrix showed a small correlation between intervention and delinquency, while the observed correlation between these two variables was much larger. This would mean that a large portion of the relationship between intervention and delinquency is not mediated by the youth characteristic in the model. Because the model specifies that the intervention has no direct effect on delinquency (i.e. it specifies that all of the intervention's impact on delinquency is mediated through its effect on the youth characteristic), the model is wrong. We could correct the model by simply adding the direct path from intervention to delinquency (Figure 2).

The model in Figure 2 would now fit the data. By adding the extra path, however, we did not improve our understanding of how the intervention works. The added path indicates that the intervention has an impact on delinquency through some mechanism other than the one we first specified. What we need to do is find out what that mechanism is. In path analytic language, this could be seen as a situation in which we have a missing variable (Hunter & Gerbing, 1981). There is some variable other than those specified in the model that mediates the connection between intervention and delinquency. It may be that in addition to the youth characteristic the intervention was aimed at, it also affects another youth characteristic. This hypothesis would lead to a new path

Figure 2

A More Complex Model with One Missing Variable



Note. YC = Youth Characteristic.

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model, which could then be tested (Figure 3). We might realize, however, that it was not just a variable that was missing from the first model, but a whole process similar to the one illustrated in Figure 1. That is, some other process is occurring outside the realm of our original model, in which the intervention is playing a role, and which is leading to delinquency. This could lead to a test of a model like the one in Figure 4.

Exactly what the plan of action should be when a model does not fit will depend on the extent to which meaningful alternative hypotheses can be derived and tested with the available data. Obviously there are quite a variety of ways in which a model can fail, and they can not all be planned for in advance. Because we must be guided by the data to a large extent in choosing alternative models to test, the problem of capitalization on chance must be addressed. This is discussed in a separate section, below.

Models to be tested. This section will provide a detailed summary of the process that was used to generate path models that were tested. Rather than present each model that was tested, a general approach to selecting causal models will be outlined. An attempt will be made to provide the reader with a sense of how models were interpreted and how these interpretations led to the testing of more complex models. In order to do this, the conceptual processes involved in testing path models will be presented in some detail. There is good reason for going into such detail. In the path analytic approach used here (Hunter & Gerbing, 1982), the statistics were fairly simple and straightforward.

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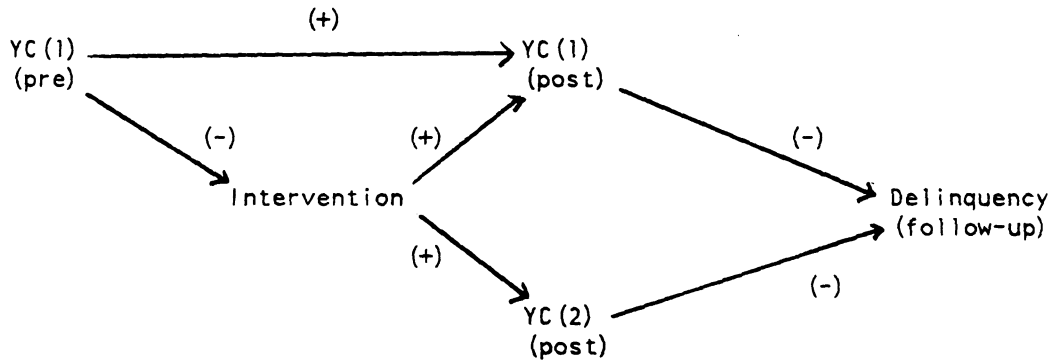
Figure 3

A Model with the Missing Variable Accounted For



Note. YC (1) and YC (2) represent two different youth characteristic variables.

YC(1)
(pre)

YC(2)
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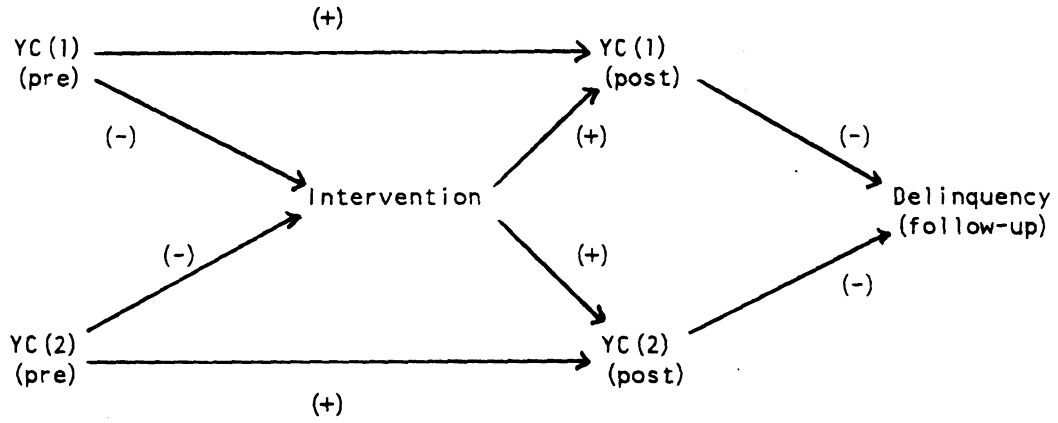
Figure 4

A Model with the Missing Process Accounted For



Note. YC(1) and YC(2) represent two different youth characteristic variables.

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The method of interpreting the findings and modifying path models accordingly determined the validity of the results.

Before introducing the models that will be tested, the use of delinquency measures will be explained. An earlier measurement study (Amdur, 1985a) showed that self-reported drug use, general self-reported delinquency, and official delinquency were in fact three distinct types of delinquency. In the present study, the extent to which these types of delinquency played similar roles in the intervention process was examined by using all three kinds of measure in each of the models that required a measure of delinquency. This was done sequentially. For example, a model like that in Figure 1 would be run three times, once using each of the three delinquency measures. This will be assumed in the presentation of the models that follows.

The first set of models to be examined are identical in form to the models in Figures 1 through 4. In these models, the intervention works by influencing youth characteristics which in turn cause delinquency. Many of the volunteers in this study were trained to respond to certain youth problem areas with specific interventions focused on those areas, as already noted. This means it should be possible to substitute matched pairs of specific youth characteristics and interventions into the general models in Figures 1 through 4. For example, the youth characteristic might be involvement in and enjoyment of school ("investment in school"). The corresponding intervention would focus on improving the youth's school performance, attendance, behavior or attitude. Model 1 would then say that delinquency results from low investment in school (path d), that investment in school is a

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characteristic that persists over time (path c), that school-focused intervention increases the youth's investment in school (path b), and that such interventions are used most often when there is initially a problem with investment in school (path a). Another youth characteristic and intervention that fit conceptually into the same model would be problems in the home domain and interventions designed to improve communication or limit-setting at home.

A model related to the one in Figure 1 is illustrated in Figure 5. Here it is acknowledged that the youth characteristic is not the only process that persists over time. Delinquency persists as well. When the youth characteristic is school or family involvement, the model in Figure 5 (as well as Figures 1 through 4) is consistent with control theory. This is because the model states that delinquency is a result of low levels of involvment.

The labeling perspective would add a direct causal path from delinquency at pre to the youth characteristic at post (path e, Figure 6). This path indicates that delinquent behavior leads to negative labeling by others and by oneself, which subsequently makes it more difficult to have high levels of the positive youth characteristic (e.g. to be involved with family or school). To the extent that the product of paths e and d in Figure 6 is greater than the strength of path f, the central processes in both labeling and control theories are illustrated. This configuration of paths d, e and f (with d x e > f) would mean that delinquency persists over time because of mutual causal influences between delinquency and the youth characteristic. Delinquency reduces

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Figure 5

Control Theory Model that Accounts for the Persistence of Delinquency Over Time

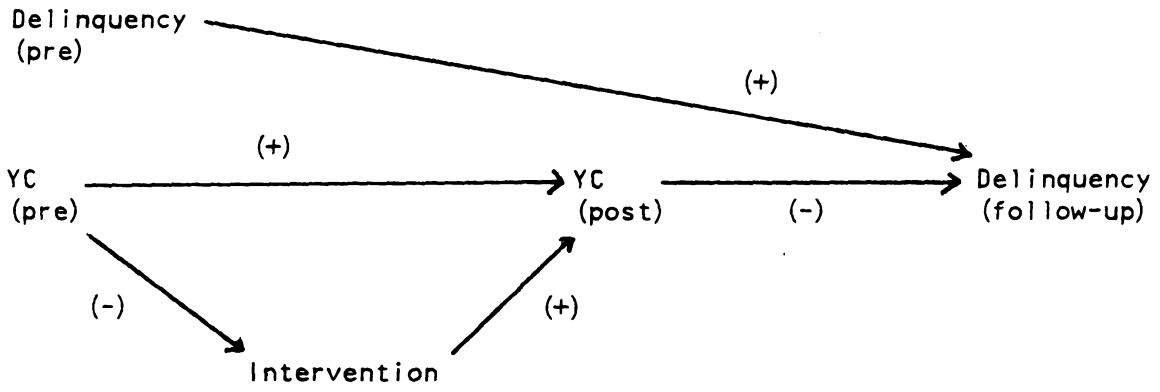
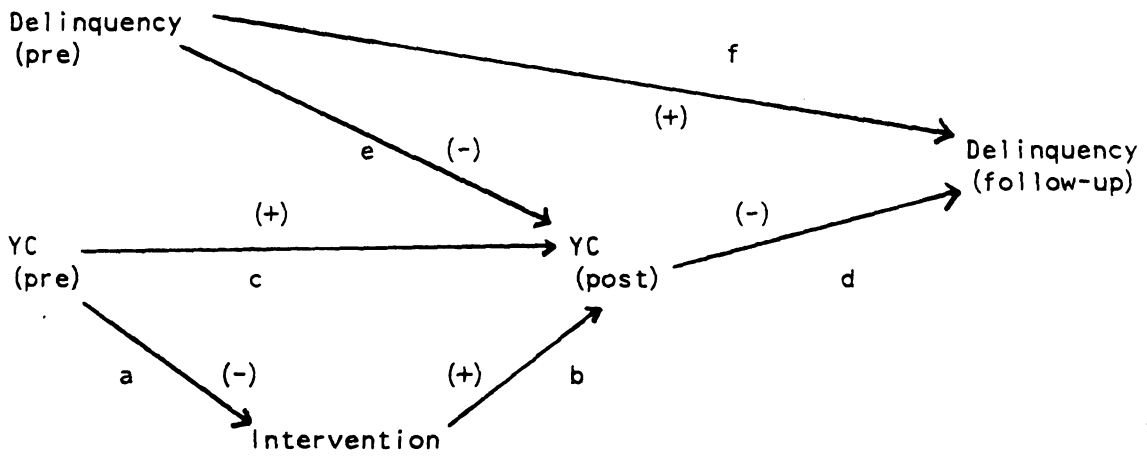


Figure 6

Control and Labeling Theory Model



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subsequent levels of the youth characteristic, and the youth characteristic prevents later delinquency.

The action training condition had two major components: contracting and advocacy. One important question was whether these two components were equally effective (Figure 7). The model in figure 7 can help us sort out the extent to which volunteers trained in the action method are successful because of their use of advocacy (path b) or contracting (path d). It was also expected to tell us something about the extent to which they use each approach (paths a & c).

A major problem with this model becomes more apparent when we acknowledge the persistence of delinquency over time, and add delinquency at pre to the model. We would expect a direct positive relationship between delinquency at pre and at follow-up. But what causal connection should we expect between delinquency at pre and the two interventions? One hypothesis is that the volunteer responds to increased delinquency in the youth by intensifying the intervention (Figure 8). In Figure 8, we see this occurring several times during the course of the intervention. At each time period, the strength of the intervention depends on the level of the youth's delinquency at the previous time period. The important thing to notice here is that the treatments have <u>no effect</u> on delinquency at all. The reason this is important becomes clear in the next example.

It is also possible that the volunteers respond to increased delinquency in their clients by becoming disillusioned and depressed, and therefore reduce the strength of their intervention. This would cause

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Figure 7

Simple Model of Action Training Components

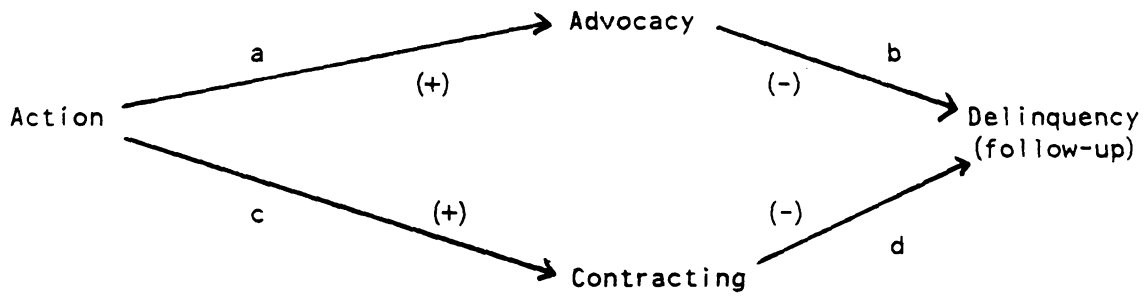
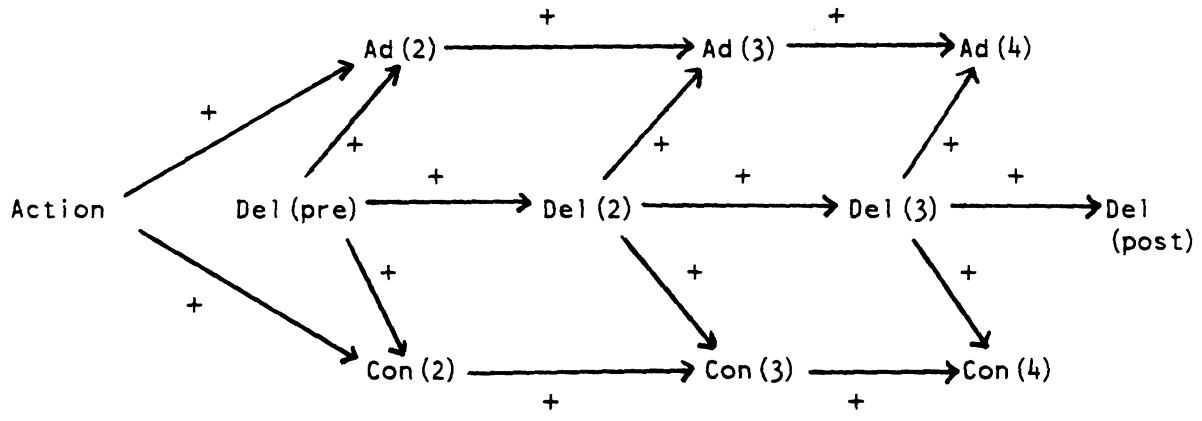


Figure 8

Model of a "Spurious" Treatment Effect



Note. Ad = Advocacy; Con = Contracting; Del = Delinquency.

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all the delinquency-to-intervention paths in model 8 to have negative path coefficients. Now, if model 8 were in fact true, and we tested model 7 and found that it fit, paths a and c could be positive and paths b and d would be negative. In this case we would be wrong to conclude that both interventions reduced delinquency. In fact the non-zero path coefficients for paths b and d would be spurious. Figure 8 gives the true picture. What appears to be a direct causal connection between advocacy and delinquency in Figure 7 is actually a spurious connection resulting from the fact that both advocacy at T4 and delinquency at follow-up share a common causal antecedent: delinquency at T3. Similarly for the apparent contracting-to-delinquency relationship in Figure 7. A more optimistic hypotheses is shown in Figure 9. Here the interventions both exert a "braking effect" on delinquency, slowing it down at each time period. Here the interventions do reduce delinquency. The point is that from testing model 7 alone, we could not tell which of these cases (the one illustrated in Figure 8 or in Figure 9) was the true one. A more detailed examination of models like those in Figures 8 and 9 is required.

The possibility of early recidivism (i.e. rearrest by police or return to juvenile court during the intervention) means that the intervention may not only be a causal antecedent exerting a negative impact on later delinquency. It may also be a causal consequence of recidivism during the intervention period. This effect of early recidivism on intervention would be expected to be particularly important for interventions in which the volunteer acts as an advocate for the youth in the legal system. Rearrest would also be expected to exacerbate

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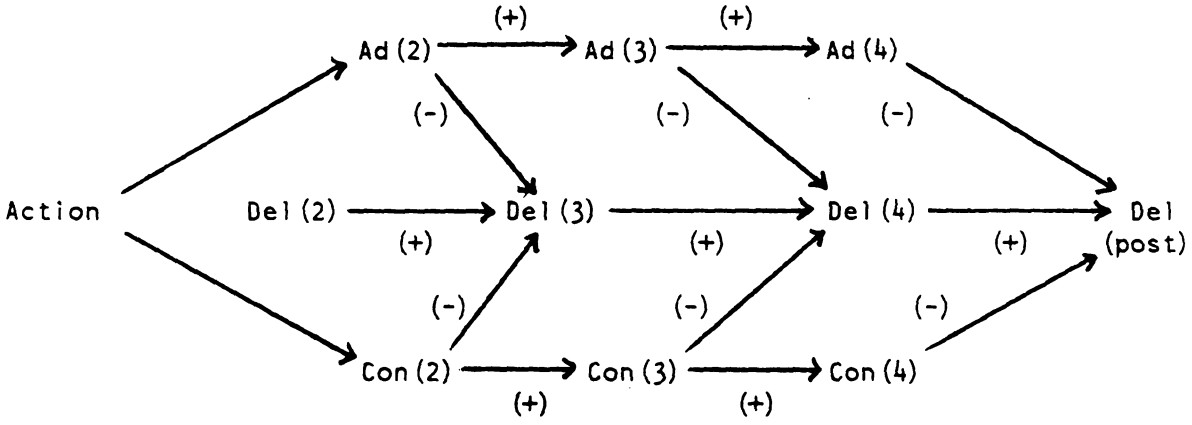
Figure 9

Model in which Interventions Exert a "Breaking Effect" on Delinquency



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problems in other areas of the youth's life (e.g. family, school), that might in turn lead to increased efforts at intervention in those areas. The path model for early recidivism (Figure 10) illustrates this situation. The intervention continues to operate on the youth characteristic and on delinquency, as in earlier models. It causes an improvement in the youth characteristic, which in turn reduces delinquency (paths b and d). However, intervention and delinquency are also related to each other through another path, the spurious one from recidivism (paths e and f). This spurious path means that the intervention will appear to have a positive impact on later delinquency. This would have appeared in model 1 as a direct path from intervention to delinquency, with a positive path coefficient. (It would have been interpreted to mean that intervention increases delinquency). Figure 10 shows why the interpretation of this path as indicating that the intervention increases delinquency would be incorrect. In model 10 we can see that in fact the intervention reduces delinquency.

Model 10 illustrates once again why "evaluative research mode" conclusions can be misleading. The sign of the simple correlation (or ANOVA results) between intervention and delinquency will depend on the relative sizes of the products (e x f) and (b x d). If (e x f) equals (b x d), the observed correlation between intervention and delinquency will be zero and the "evaluative" conclusion will be that the intervention has no effect. This will be the conclusion even if both products, (e x f) and (b x d), are large. In this case, the intervention would actually have a strong effect.

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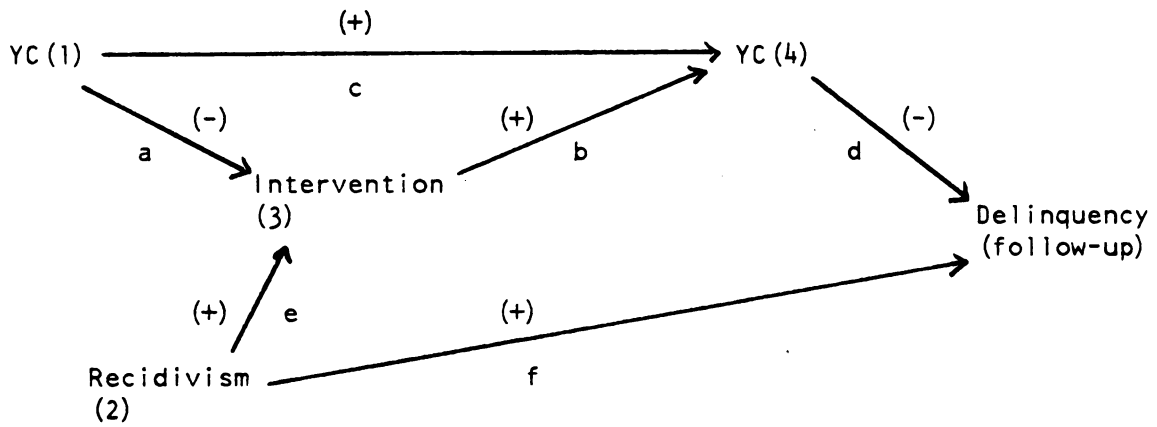
Figure 10

The Effects of Early Recidivism



Note. YC = Youth Characteristic. Positive score for YC indicates the "socially desirable" direction.

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The effects of youth age on the intervention process were tested by adding this variable to models like those just discussed. In these models, youth age could not be causally influenced by any other variables, therefore age had to be an exogenous variable. (This means it could have causal arrows emanating from it, but not pointed toward it). The two hypotheses regarding age effects, discussed earlier, were that 1) older youth are less delinquent and therefore show less improvement in response to treatment (the "maturation hypothesis"), or 2) treatment is simply less effective in reducing recidivism for older youth (the "differential effectiveness hypothesis"). These hypotheses were tested by examining the path model illustrated in Figure 11.

To the extent that hypothesis (1) is correct, age should be negatively correlated with delinquency, both at pre- and post-intervention (paths a and d). This would be consistent with the idea that adolescents "outgrow" delinquency as they reach age 18 or 19. If this were true, it would imply that there is no direct relationship between youth age and strength of intervention (path b). The intervention would continue to be influenced only by the initial level of the youth characteristic, so that the coefficient for path b would be zero.

Hypothesis (1) says nothing about <u>why</u> youth outgrow delinquency. It may be that this occurs as a result of improvement in "mediating processes" such as involvement in school or with family, but this hypothesis says nothing about which mediating processes are the important ones. Thus we would not expect a relationship between age and the other youth characteristic in the model (paths c and e). If we did happen to


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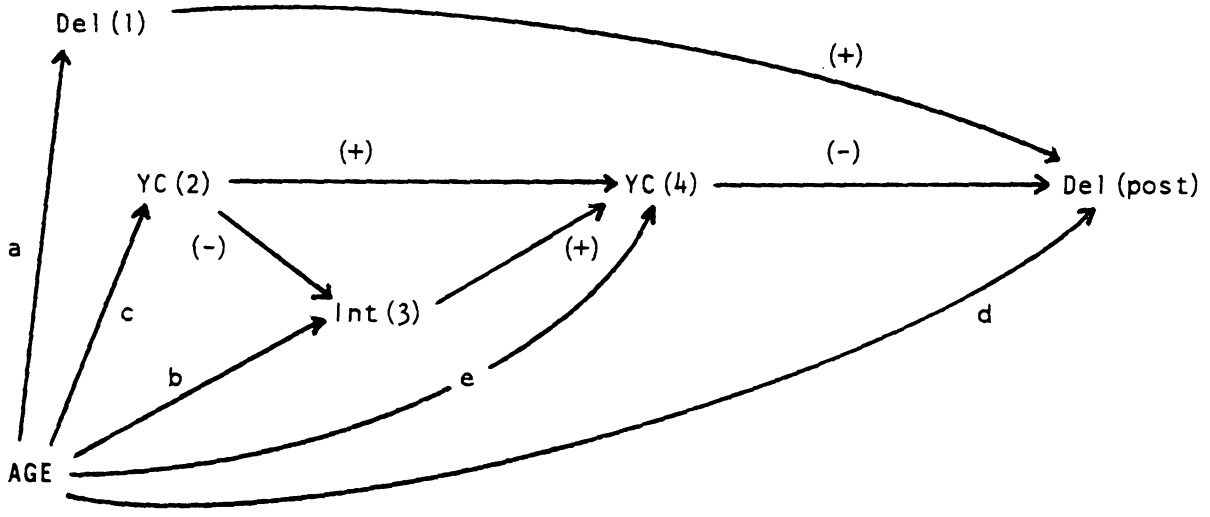
Figure 11

Model of the Effects of Youth Age on the Intervention Process



Path Coefficients

Path	Hypothesis 1 (Maturation)	Hypothesis 2 (Differential Effectiveness)
a	0 or -	0 or +
b	0	-
c	0 or +	-
d	-	+
e	0 or +	-

Note. YC is scored so that positive indicates the "socially desirable direction.

pick important mediating processes, paths c and e would both have positive path coefficients.

Hypothesis (2) would produce a very different path model. Here the assumption is that while age and delinquency are initially uncorrelated (path a), they become positively correlated later (path d) because younger youth have been helped by the intervention while older youth have not. Why the intervention might be differentially effective can be tested to some extent in this model by examining paths b and c. It may be that older youth are less willing to form attachments with treatment agents because they are more "defended" than younger youth or because delinquent role requirements, including mistrust of "authorities", are more engrained. This would be seen in a negative path coefficient for path b. Another possibility is that older youth have more severe family and school problems (since these have presumably had more time to develop and intensify) which are likely to persist over time (paths c and e). Even if the intervention were delivered (i.e. coefficient for path b = 0), a negative coefficient for path e would mean its effect would be reduced.

The procedure used in testing causal models was outlined in this section. It was pointed out that "testing" a causal model involves first deciding whether the causal order of variables in the model is correct. This is accomplished by testing the significance of the differences between two correlation matrices: the matrix of observed correlations and the "reproduced correlation matrix" which is derived from the path model. This determines whether "the model fits the data." Next, the individual path coefficients are examined to interpret the meaning of the model.

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This leads to the testing of related path models. In each case the process begins with an a priori causal model derived from theory.

The significance test for departures from fit. The extent to which sampling error will affect the test of fit of a path model depends on the sample size used in the study, the number of correlations free to vary (which increases with the number of variables in the model and decreases with the number of paths specified in the model), the size of the correlations, and whether or not correlations have been corrected for attenuation. Sampling error increases with smaller samples, more degrees of freedom, and smaller correlations. (The effect of correcting for attenuation on sampling error is explained below). The departures of fit in individual correlations can be tested for significance, as can the fit of the overall model (see Hunter, 1983 for details). If any of the individual correlations show a significant deviation, the model as a whole will also not fit. However the model as a whole may not fit even though no individual correlation shows a significant deviation.

Significance of individual path coefficients. The models tested were initially selected based on theories of delinquency and of intervention. When these models did not fit the data, alterations were sometimes made in the model so that it would fit. This was done when alternative models could be found that fit and that made sense conceptually. Thus a mixed confirmatory and exploratory approach was used. Capitalization on chance was especially likely to occur in the exploratory phase, where the data were to some extent determining which models were tested. In order to guard against the effects of sampling error, the significance of path coefficients was tested.

The usual formulas for computing the significance of beta could not be used here because the path coefficients were based on correlations corrected for attenuation. A procedure exists for calculating the 95% confidence interval around correlations corrected for attenuation (Hunter, 1985a), but not for calculating confidence intervals on beta computed from such correlations. (Remember that path coefficients here are equivalent to betas). Therefore the significance test for path coefficients depended on the assumption that the effect on the confidence interval of correcting for attenuation is similar for betas and correlations.

Correlations corrected for attenuation are generally larger than the observed correlations. At the same time, the confidence interval is wider. (The sampling error increases to the same degree that the correlation does). Still, the lower end of the confidence interval remains fixed when a correlation is corrected for attenuation. This means that a raw correlation will be significant if and only if the corrected correlation is. The situation is more complicated for beta, since the significance of beta is dependent not just on the size of the beta and the N, but on the sizes and positions of other betas in the model. Like correlations, betas that are based on correlations corrected for attenuation are generally larger than betas that are based on raw correlations. Also like correlations, the confidence interval around betas based on corrected r's should be wider than the confidence interval around betas based on raw r's. It thus seems reasonable to assume that betas based on corrected correlations are significant if and only if the beta calculated from uncorrected correlations is significant.

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Path coefficients were tested for significance by submitting the matrix of scale scores to stepwise multiple regression such that the regression model was equivalent to part of a path model. This was done for each segment of each path model (where a segment means each section of a path model that contains a set of predictors that relate to a single criterion variable). Significance levels reported here are those for the raw betas.

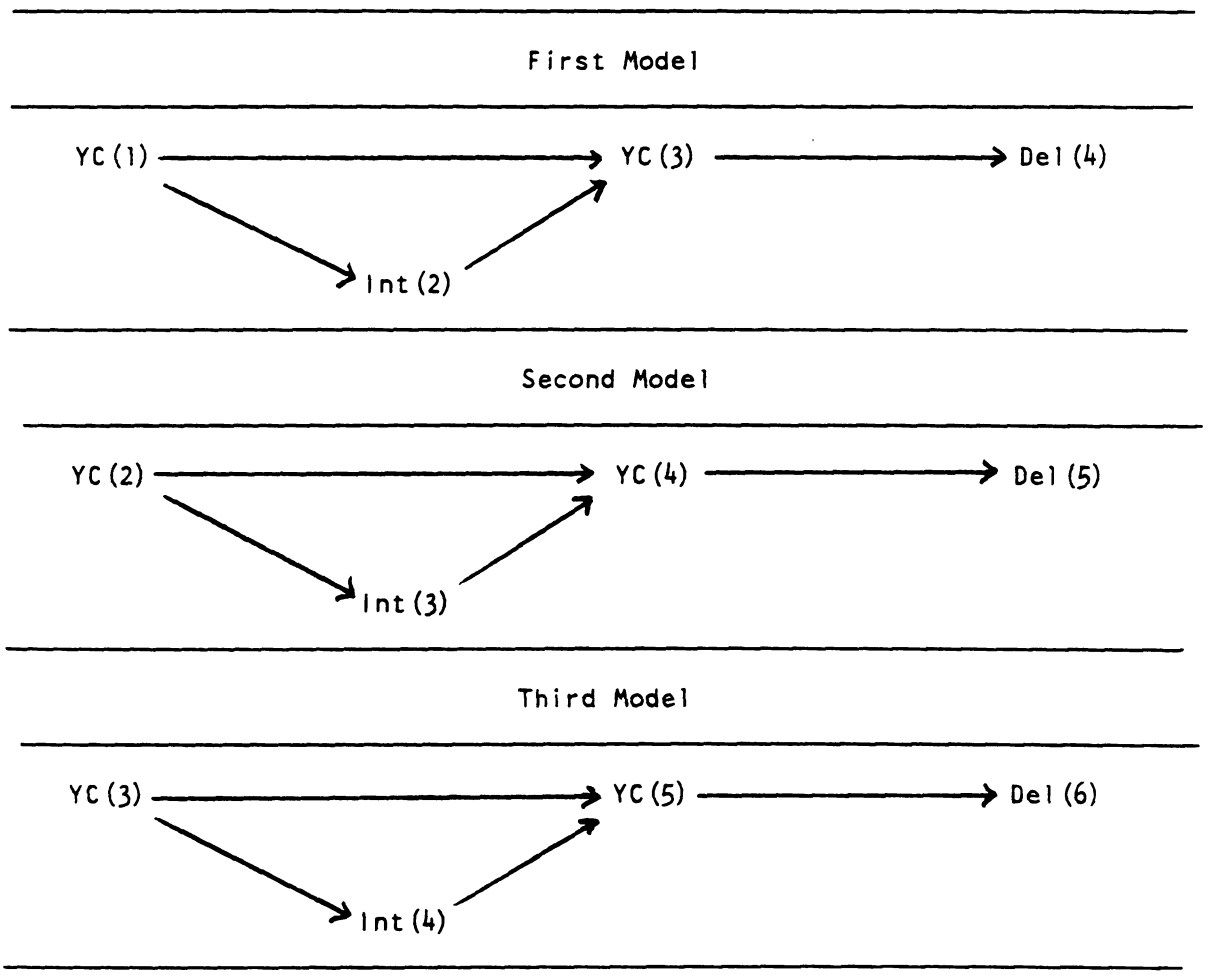
Other Methodological Issues

Handling of multiple time periods of measurement. It should be noted that while six different time periods of data existed for many of the measures used in this dissertation (i.e. for all measures except Intervention scales, for which three time periods exist), many of the models that were tested only required measures to be taken at three, four or five distinct time periods. This raised the question of what to do with the "extra" data, that were not required for a test of a given model. For example, the model in Figure 1 required measures at four time periods. The youth characteristic at pre must be measured prior to the intervention, which must preceed measurement of the the youth characteristic at post, which in turn must preceed the measurement of delinquency at follow-up.

This problem could be solved in one of two ways. The first solution would involve the examination of microprocesses within the intervention. This could be accomplished by testing multiple path models in a sequence (Figure 12) or more complex path models (Figure 13). Both strategies would provide more detailed information than was required to test the original model. For each, a different model would be tested than the one

Figure 12

First Alternative for Handling Multiple Time Periods:
Sequential Path Models



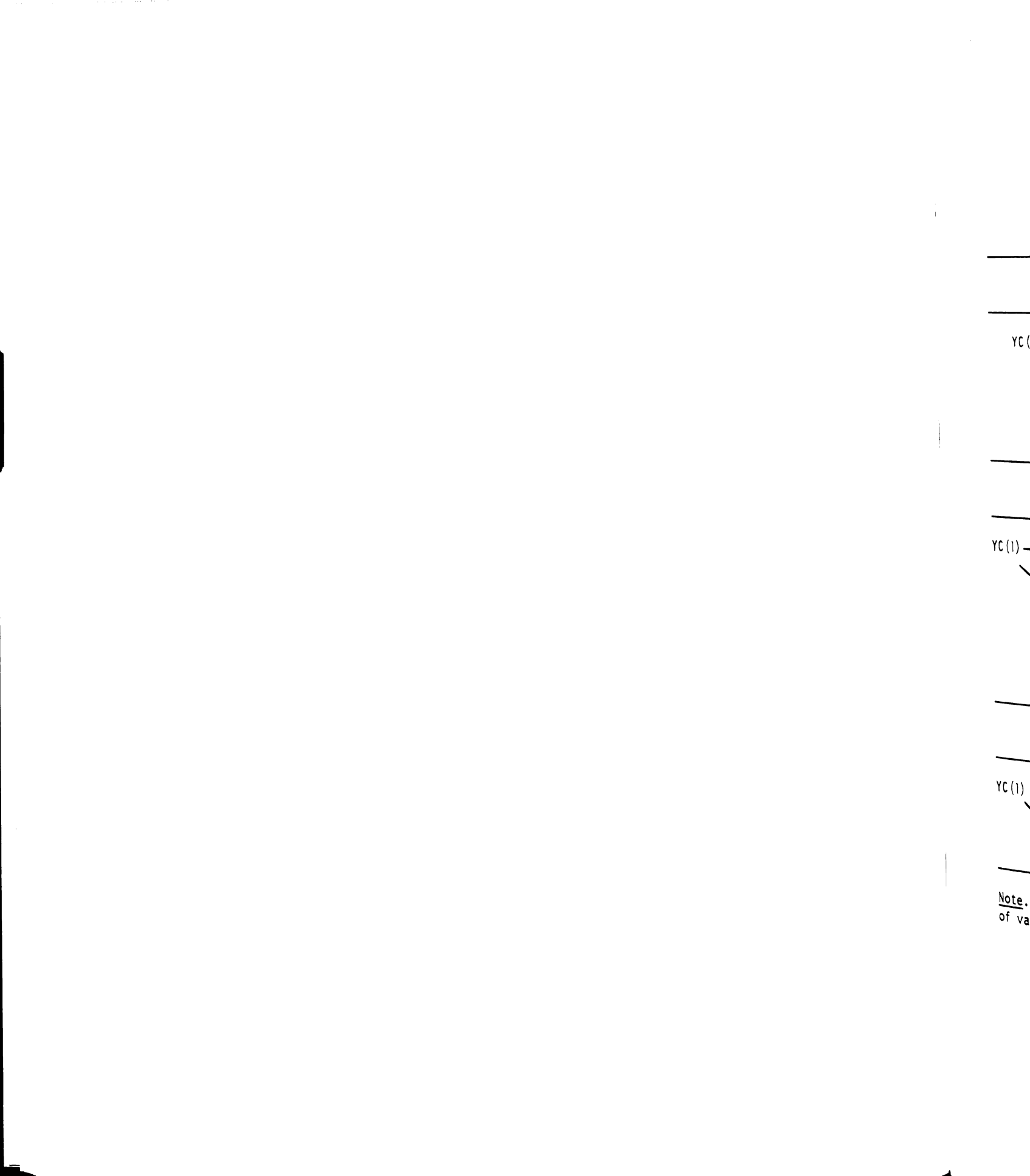

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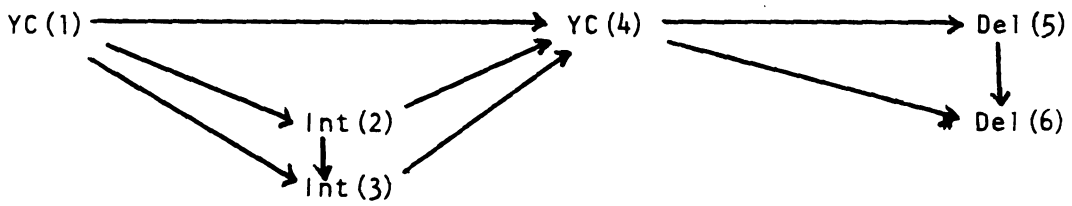
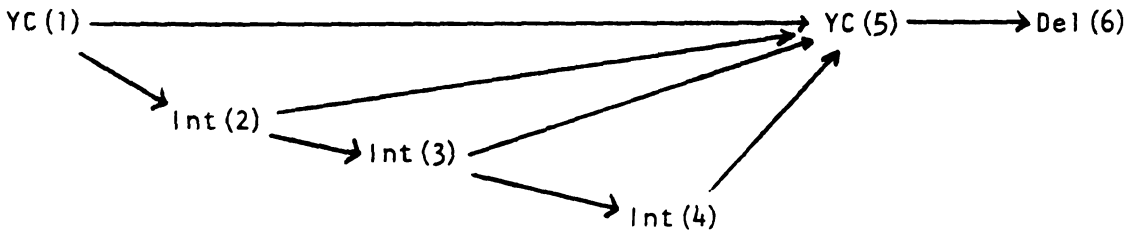
Figure 13

Second Alternative for Handling Multiple Time Periods: More Complex Models

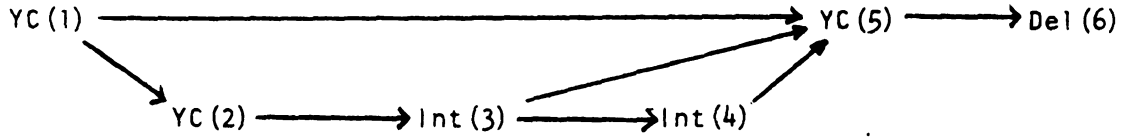
First Model



Second Model



Third Model



Note. There are other possible models not listed here, for this combination of variables.

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originally proposed. Either a more complex model would be tested (e.g Figure 13) or multiple models with different end-points would be tested (e.g. Figure 12). Another approach to this problem assumes that the basic path model is correct, or at least that it is this model that we want to test, and not a more complex model or set of models. Thus "complexifying" the model or changing the endpoints are ruled out as solutions. Instead, a new measurement model is created in which multiple measurements of some variable in the model at different time points are clustered together into a single higher order cluster (see Figure 14). It was this strategy that was used.

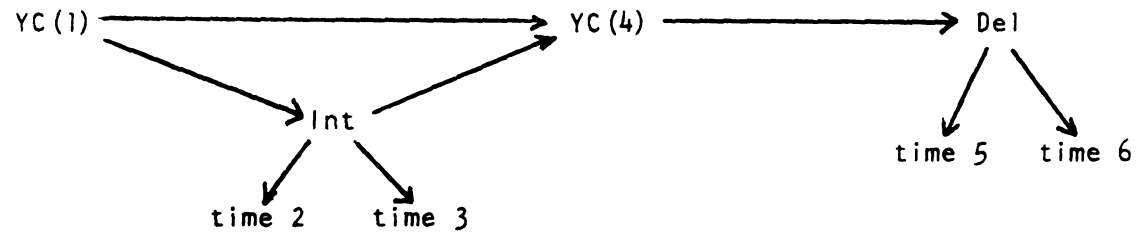
The procedure for clustering variables was the same one described earlier for creating a second order cluster. In order to form such a cluster, variables must be positively correlated with each other, must be more strongly related to each other than to other variables external to the cluster, and must have correlations with external variables that are "parallel" to those of the other variables in the cluster. In examining parallelism, the external variables that were used were the other variables in the model being tested.

This strategy leaves open several possibilities for the particular measures to be cluster within any given model (see Figure 14). While the same basic model is tested in all three examples given in Figure 14, three different clustering schemes are illustrated. In the first model, two of the earlier intervention time periods are clustered, as are the two follow-up delinquency scales. In the second model, all three intervention scales are clustered. In the third model, two youth characteristic scales from the early time periods are clustered, as are

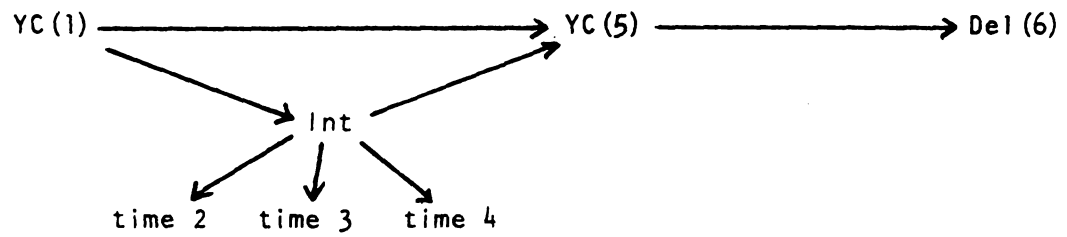
Figure 14

Third Alternative for Handling Multiple Time Periods: Clustering Across Time

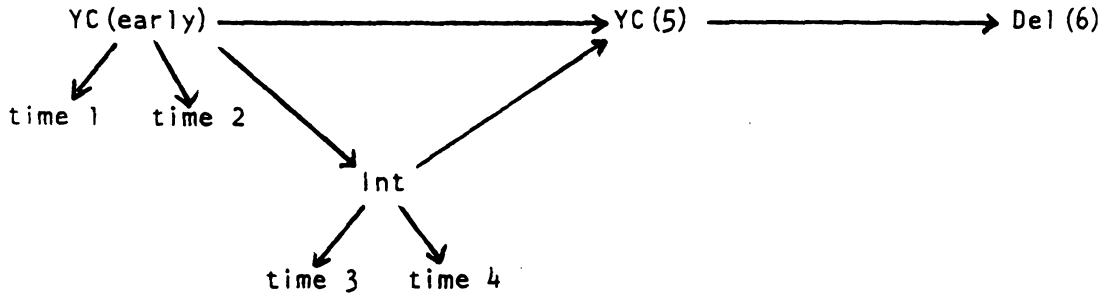
First Model



Second Model



Third Model



Note. Other clusterings are possible as well.

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the two later intervention periods. Other combinations are possible. The second order cluster analysis was used to determine whether the same scale measured at different times should be clustered (that is, whether the scale, measured at different times, actually measured the same construct). This procedure did not tell us how to do the clustering, however. Some convention was required to determine how these different possibilities would be handled.

The first convention that was followed in testing models such as this one was to combine data from times five and six. This means an attempt was first made to cluster all follow-up data together. There were two reasons for doing this. First, loss of subjects begins to occur after the intervention ends, at time four. Combining the data from times five and six allows for a more reliable estimate of the final path coefficient (e.g. path d in Figure 1) because the sample on which the estimate is based is larger than if either time five or time six were used alone. Second, there is generally no substantive reason to believe that scores at times five and six have different meanings. Instead, the best substantive interpretation of these scores is that they are alternate measures of the same construct. This is precisely the situation that calls for creation of a higher order cluster.

A second convention was adopted after the first, if for a given model there was still room for doubt about which variables to cluster (as there would be in Figure 14). When there was a choice between clustering youth characteristic or intervention scales, it was the intervention scales that were clustered. This allowed for a test of whether the intervention scales measured the same construct at each time period. It

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also allowed for a more accurate estimate of the strength of the intervention than would be provided by an intervention score at a single time period. In this way, attention was focused on the interventions more than on the youth characteristics. This was because it was assumed that intervention was more likely to change over time, and because changes in intervention strength are central to the focus of this dissertation. Adoption of these two conventions meant that of the alternative models presented in Figure 14, it is the first model that would be tested.

This procedure for handling the "problem" of having too many data points allowed us to keep the focus on models that were specified a priori. There were times, however, when strategies like those illustrated in Figures 12 or 13 were called for. Testing of sequential models (Figure 12) was used when there was a theoretical reason to believe that the model by which a given intervention worked changed over time and the differences in intervention process across time could be stated in advance. This procedure or the procedure of "complexifying" the model (Figure 13) might also be justified on empirical grounds. This might occur, for example, if the attempt to cluster a set of intervention scales at two time periods showed that the scales did not have parallel correlations with outside variables. This was interpreted to mean that the two scales do not exhibit "causal homogeneity". Such a finding led to generation and testing of models which helped to clarify how the intervention processes changed over time.

Path models as verifiable hypotheses. The techniques that were used in the present study allowed delinquency theories as well as theories of

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intervention to be "tested" empirically by providing a link between the theories of delinquency and of intervention, and the data. Causal processes provide this link. Causal processes are empirically verifiable because they can be translated into structural equations (i.e. path models). Path analysis is a method that can help determine whether path models fit the data. Since causal processes are verbal descriptions of events, their relationships to the various theories of delinquency and of intervention can also be determined. Thus causal processes are the sort of concrete hypotheses, derived from theory and verifiable empirically, that must be examined in order to "test" delinquency theories and theories of intervention (e.g. Popper, 1968).

Causal inferences from correlations. It is often claimed that path analysis and related methods can be used to test causal hypotheses, using correlational data at a single time period (e.g. Liska, 1973; Thompson, Mitchell & Dodder, 1984). Under certain conditions it can be determined that some of the possible causal orders among a set of correlated variables measured at a single time period are more plausible than others. This generally requires an assumption either that one of the variables is causally prior to all the others, or that one variable follows all the others in the causal order (e.g. Liska, 1973). Except when one is dealing with demographic variables like age or sex, which could not be influenced by attitudes or behaviors and thus must be causally prior to such variables, such assumptions can be very difficult to justify (e.g. see comments about Liska, 1973, in Amdur, 1985b. This was one example where assumptions about the causal priority of one

variable in the model may have been false, and may have led to erroneous conclusions).

When we can not justify assumptions about the causal priority of one or more variables in our model, it is often impossible to draw causal conclusions from correlational data at a single time period. In studying the mechanisms of effect of many psychological interventions this is exactly the situation we are in. We might think of the task as isolating the causal process which "connects" the intervention with the outcome. This could be thought of as a situation in which the first variable in a causal chain is the intervention, the last variable is a measure of delinquency, and the question is what are the mediating variables. This is only the simplest form of the problem, however. The problem becomes quite complex if we recognize the fact that in the real world, intervention, mediating process, and delinquent behavior do not always occur in that order. Changes in the intervention might occur in response to early recidivism on the part of the youth, or as a result of changes in the youth's family or school situation, for example. In fact, almost any causal ordering of the intervention, delinquency, and supposedly "mediating" variables is possible. Given this complexity, can anything be said about the mechanisms of effect of the interventions? The answer is no, if we only use data from a single time period. This may seem obvious, but in the field of delinquency research, a body of literature is developing in which causal models are supposedly being tested using single time-period data (see reviews in Amdur, 1985b; Elliot, Huizinga & Ageton, 1985).

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One solution to this problem is to use longitudinal data. The strength of the conclusions that can be reached using such data is based on the assumption that if time 2 follows time 1, an event occurring at time 2 can not cause an event occurring at time 1. This assumption often allows for moderately strong conclusions about causal order among variables to be made, using path analysis of correlational data.

Exactly how much confidence we can have in causal interpretations of longitudinal correlational data depends on the nature of the variables and the completeness of our model. If A is measured at time one and B is measured at time 2, and we find a positive path coefficient between A and B, we would be likely to be correct in inferring that A causes B if A is a variable on which subjects are randomly assigned to levels (i.e. A is the independent variable in an experimental design). In the absence of random assignment, the strength of this inference would rest on the degree of completeness of the path model. This means that even using longitudinal data, in the absence of random assignment correlational data is better able to prove that A does not cause B, than that A does cause B.

Drawing conclusions about the etiology of delinquency. There is one major problem with the use of longitudinal data for testing theories of the etiology of delinquency: when to collect the data. In order for such data to "capture the phenomenon" of the causal processes leading to delinquency, they must be collected during the time that these processes are occurring. If it is expected that a process occurs between the ages of nine and thirteen, and we collect data on fourteen to seventeen year-olds, we will not find out about that process. None of the theories of

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delinquency explicitly states when the crucial time might be. Presumably, this means the processes described by the theories could occur at any time. If it is the case that the processes leading to delinquency are expected to occur all through the youth's childhood and adolescence, the longer the time span over which we collect data, the more likely we are to find evidence of important processes. A research design that used a short time frame and did not find support for a given theory of delinquency could always be criticized for collecting data at the wrong time or for too short a time. Administering a battery of measures to a large number of subjects is difficult even at one or two time periods. Doing this periodically over a span of ten or more years presents enormous practical problems. In the present study, these problems were avoided by asking not about the etiology of delinquency, but about the processes that lead to "recovery" from delinquency.

Using subjects who have had contact with the juvenile courts and collecting data during a period of intervention to test theories of the etiology of delinquency means that the theories are being asked to do more than they were originally designed to do. Presumably, the same theories that attempt to explain how delinquency develops should also be able to explain how it is reduced. The causal processes leading to recovery from delinquency will be different from those involved in the etiology of delinquency, but one would expect that a valid and robust theory of delinquency would explain both kinds of process. This is the price that must be paid in order to be able to use longitudinal data and to focus the data collection at the time when change processes are occurring.

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In this chapter, the structure of the diversion project and the interventions that were used were described. Measurement procedures were presented. Variables to be used in this study, which were developed in a previous measurement study, were listed. Procedures were outlined for developing measures of behavioral interventions. Procedures that correct for two forms of error were outlined, as were procedures for doing path analysis and for determining the significance of path coefficients. The method that was used for developing path models consistent with the research goals, and for testing them, was outlined. Finally, the issue of whether causal inferences can be drawn from correlational data was considered. In the next chapter, the results of cluster analysis of intervention items and of path analysis will be presented.

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RESULTS

Measurement Models

Intervention Scale. The 122 items in the original intervention scale formed 19 unidimensional clusters. Only six of these were relevant for the models being tested in this dissertation, and these will be the only intervention clusters discussed here. The six clusters were labeled: School Performance, School Contract, School Visit, Home Intervene, Contract and Advocacy. (See Appendix C for a list of items within clusters).

"School Performance" was a seven-item cluster examining the extent to which the volunteer focused on improving the youth's school attendance and performance. "School Contract" (three items) asked whether the volunteer set up a contract with school staff and monitored compliance. "School Visit" (four items) asked to what extent the volunteer had visited and spoken with various school officials. "Home Intervene" (13 items) measured the extent to which the volunteer spoke with the youth and his or her parents about problems at home, and focused on several specific problems at home which might have been addressed by the volunteer. "Contract" (five items) asked whether a contract had been established, whether a monitoring system had been set up, and whether the youth received training in contracting from the volunteer. "Advocacy" (six items) examined the extent to which the volunteer had identified relevant targets of change in the youth's environment and had initiated actions to facilitate change.

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Reliabilities for the clusters were quite stable across time (Table 3). The first-order clusters satisfied the three criteria for unidimensional clusters (a rank-one pattern of inter-item correlations within the cluster, parallel correlations with external variables and consistency of item content) at each of the three time periods. Thus the meaning of the intervention measures apparently did not change over time.

The possibility of forming second-order clusters was considered next. Groupings of first order clusters that were consistent in meaning, that satisfied the empirical criteria for unidimensionality, and that clustered in a consistent way at all three time periods could not be found, with one exception. The three school intervention clusters mentioned above did meet the empirical criteria for a forming a second-order cluster. Because one focus of this dissertation was to study the mechanisms of effect of the diversion interventions at a very specific level, it was decided that rather than create a single school intervention second-order cluster, these three scales would be kept separate. This would allow fine distinctions to be made between different kinds of interventions that took place in the school setting. When such distinctions were not required by the model being tested, the three clusters could be grouped together just prior to running path analysis.

Life Events and Self-Report Delinquency at Follow-Up. Before computing cluster scores for the youth life events and self-report delinquency scales at follow-up (times five and six) inter-item correlations were examined to be sure that the measurement model established at times one through four continued to fit the data at

Table 3

Reliabilities of Intervention Clusters

Scale	Number of Items	Time		
		2	3	4
School Performance	7	.87	.83	.84
School Contract	3	.70	.65	.74
School Visit	4	.84	.85	.82
Home Intervene	13	.90	.86	.87
Contracting	5	.94	.96	.96
Advocacy	6	.85	.85	.87

Note. Standardized alpha was used to measure reliability.

follow-up. In the case of both the youth life events and the self-report delinquency scales, it was found that the measurement model establised earlier no longer fit. In the self-report delinquency scale, a clear distinction no longer existed between major and minor theft. While quite a few changes occurred in the youth life events scale measurement model, most were not relevant to the present study. The portion of the measurement structure for youth life events and self-reported delinquency scales at follow-up relevant to this study is presented in Appendix D. While the structure has clearly changed at the level of primary scales, at the second-order factor level, the meaning of the scales is not seriously altered.

Preparation of the Data for Path Analysis

After the measurement model was established for all measures at all time periods, cluster scores were calculated by taking average item scores for each cluster. Subjects with half the items or more missing for a given cluster at a given time period were coded as missing for that cluster at that time period. Pearson product-moment correlations were then calculated for each pair of clusters.

As explained in the previous chapter, it was necessary to correct the correlation matrix for two kinds of error prior to running path analysis. Each correlation was corrected for measurement error by dividing the correlation by the square root of the product of the two reliabilities of the clusters being correlated. Cross-time correlations were corrected for specific error and attenuation simultaneously using the formula mentioned earlier (Hunter & Gerbing, 1981). This was not done for every cross-time correlation in the matrix, because the formula

could only be applied in the case where the measurement model for the cluster remained the same across time. The test-retest correlations that were corrected for attenuation and specific error are presented in Table 4, along with the raw correlations. Cross-time correlations notcorrected for both types of error were corrected for attenuation only, in the same way as other inter-cluster correlations.

Although the measurement model for the Official Delinquency second-order cluster did stay the same across all time periods, cross-time correlations for this cluster were corrected for attenuation but not specific error. This was because the idea of specific error does not apply to a behavioral measure like archival records of police and court contacts. It only applies to measures in which the items might not provide independent samples of the subject's behavior across time periods. This is why an earlier measurement study (Amdur, 1985a) that did attempt to correct these correlations for specific error produced corrected correlations larger than the cross-time correlations corrected for attenuation only. This result would be impossible for a measure in which specific error was present.

Path Models

Initial test-of-fit computations showed that even path models with clear departures from perfect fit did not deviate enough for the test-of-fit statistic to be significant. Thus every path model that was tested fit the data. If the significance test was used as the basis for comparing models (using the current sample size, with average correlations of the magnitude found in this study), all models which came reasonably close to fitting the data would be indistinguishable from each

Table 4

Cluster Test-Retest Correlations
with Corrections for Specific Error

	Times					
Cluster	1-2	1-3	1-4	2-3	2-4	3-4
Youth Life Events/Self-Report Delinquency Scales						
School Involvement						
raw	.63	.58	.56	.66	.62	.73
corrected	.72	.68	.63	.72	.69	.80
Self-Reported Delinquency (SRD)						
raw	.71	.70	.69	.73	.71	.81
corrected	.81	.80	.80	.88	.85	.91
Drop Out						
raw	.81	.72	.66	.78	.74	.78
corrected	.90	.84	.80	.90	.86	.89
Family Conflict						
raw	.57	.58	.49	.67	.58	.66
corrected	.66	.67	.55	.77	.66	.73

(continued)

Table 4, continued

Cluster	Times 2-3	2-4	3-4
Intervention Scales			
School Performance			
raw	.65	.59	.67
corrected	.69	.63	.71
School Contract			
raw	.57	.40	.47
corrected	.82	.50	.50
School Visit			
raw	.49	.38	.44
corrected	.53	.50	.47
Home Intervene			
raw	.65	.52	.68
corrected	.70	.59	.73
Contract			
raw	.56	.31	.57
corrected	.57	.29	.56
Advocacy			
raw	.47	.48	.56
corrected	.52	.54	.62

Note. Raw correlations are observed r's. Correction for specific error used formula given in Hunter, Coggen & Gerbing (1981). This formula produces r's corrected for specific error and for attenuation.

other. Instead, an attempt was made to produce models which fit the data perfectly. The degree of departure from perfect fit was measured by the difference between the input and reproduced correlation matrices. Perfect fit was indicated by complete agreement between these two matrices.

In general, as variables were added to the path models being tested, making them more complex, a more accurate picture emerged about the relationships between variables. Conclusions that might have been reached based on the simpler models often turned out to be inaccurate from the perpective of a more complex model. For this reason, results from the simpler models, that were misleading, will not be presented. Results from the more complex models will be discussed in greater detail, and the ways in which these models improved upon the more simple ones will be described.

Significance tests for beta. The standard error of beta for a given predictor is a function of both the multiple R of all predictors with the criterion, and of the multiple R of the given predictor with all other predictors. This means that whether or not a given beta differs significantly from zero depends not only on the size of the beta, but also on the position of the predictor in the path model. A beta of a given size might be significant at one position in the model, but not significant at another. A further complication in evaluating the significance test is that the N in this study varied for different combinations of variables. Correlations between measures of delinquency or life events generally had N's of approximately 280 to 300. Correlations between intervention scales and other variables generally

had N's of between 190 and 220 (because control subjects were coded as missing for intervention scales, but not for delinquency or life events measures). The N for a given beta was another factor that determined whether or not that beta was significant.

School intervention and involvement. One of the youth characteristics that was believed to have an impact on delinquency was school involvement. The correlations between school involvement at time four and delinquency at follow-up were significant regardless of the measure of delinquency used (Table 5). Correlations with school involvement at time four were -.24 (for SRD), -.34 (for Drop Out) and -.17 (for Official Delinquency). This indicated that youth who were more involved in school at project termination tended to report fewer delinquent acts over the next two years and were less likely to have contact with police or courts. One possible interpretation of this finding is that school involvement leads to reduced delinquency (consistent with control theory). Path coefficients for the direct path between school involvement at time four and follow-up delinquency were not significant, however (Figures 15-17). This indicates that it would be incorrect to conclude from the correlations that school involvement prevented delinquency. Rather than school involvement having a direct effect on delinquency, their negative correlation came about because both shared common causal antecedents.

Using the two self-report delinquency measures (SRD and DO), earlier delinquency had a moderately strong negative effect on school involvement as well as a positive effect on follow-up delinquency (Figures 15 & 17). This accounted for the difference in size between the direct path

Table 5

Correlations for School Involvement, School Intervention, Family Conflict, Home Intervention, Youth Age and Delinquency

	AGE	SV2	SV4	SI3	FC2	FC4	HI3	DO1	DO5	SR1	SR5	OC1	OC5
AGE	100	0	-6	-21	10	4	-12	49	47	-3	-32	13	-19
SV2	0	100	68	-18	-37	-19	-9	-47	-17	-32	-10	-7	-15
SV4	-6	68	100	-24	-38	-33	-10	-38	-34	-34	-24	-12	-17
SI3	-21	-18	-24	100	13	5	32	0	3	27	16	13	21
FC2	10	-37	-38	13	100	66	48	41	27	24	16	8	12
FC4	4	-19	-33	5	66	100	39	25	29	14	34	-6	25
HI3	-12	-9	-10	32	48	39	100	2	10	1	4	1	8
DO1	49	-47	-38	0	41	25	2	100	50	45	-16	12	-2
DO5	47	-17	-34	3	27	29	10	50	100	24	34	0	4
SR1	-3	-32	-34	27	24	14	1	45	24	100	34	28	24
SR5	-32	-10	-24	16	16	34	4	-16	34	34	100	11	41
OC1	13	-7	-12	13	8	-6	1	12	0	28	11	100	27
OC5	-19	-15	-17	21	12	25	8	-2	4	24	41	27	100

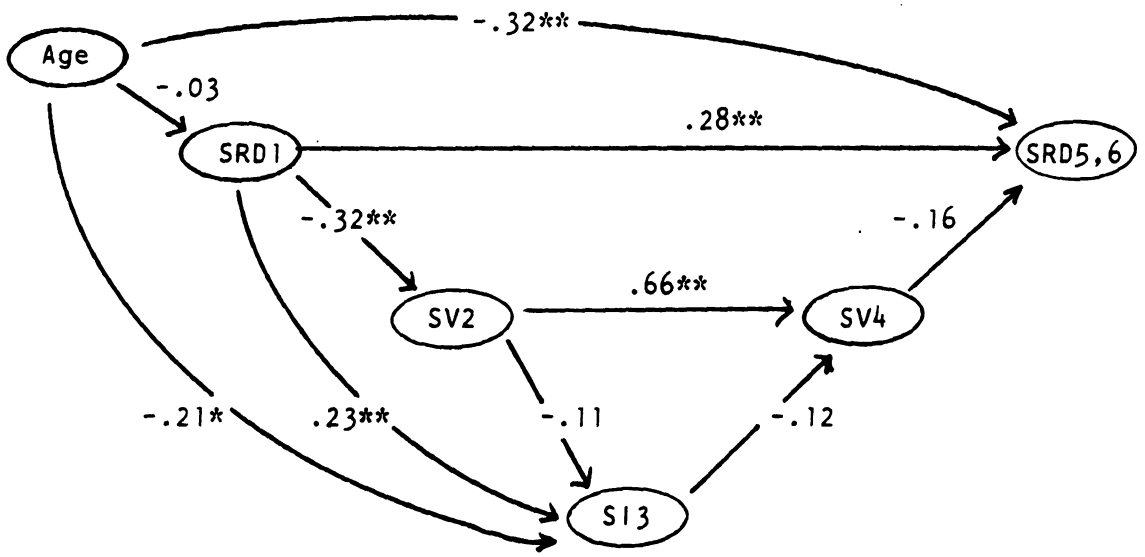
Note. All correlations are corrected for attenuation. Cross-time r's for SV and FC are corrected for specific error as well. For r's that include an intervention variable (SI or HI), $r > .19$ is significant ($p < .05$). For r's that do not include an intervention variable, $r > .15$ is significant ($p < .05$).

Variables:
- SV = School Involvement
- SI = School Intervention
- FC = Family Conflict
- HI = Home Intervention
- DO = Drop Out
- SR = Self-Reported Delinquency
- OC = Official Delinquency

Time periods: 1 = pre-intervention, 2-4 = during intervention, 5 = 1 and 2 years post-intervention.

Figure 15

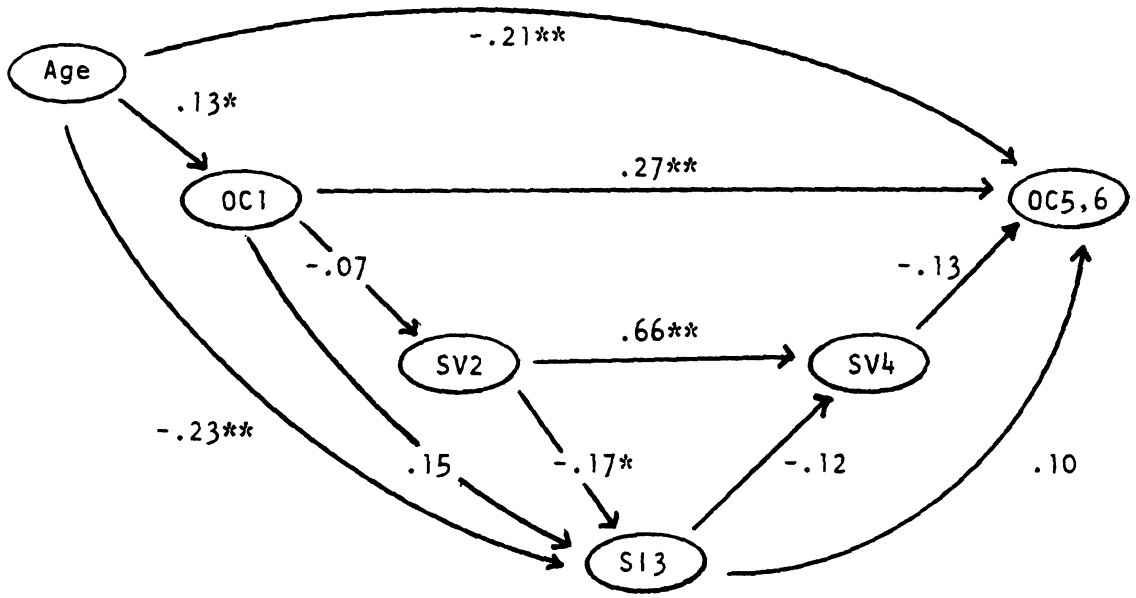
Path Model: School Involvement, Intervention, Self-Reported Delinquency (Pre-Post) and Age



* p < .05
** p < .01

Figure 16

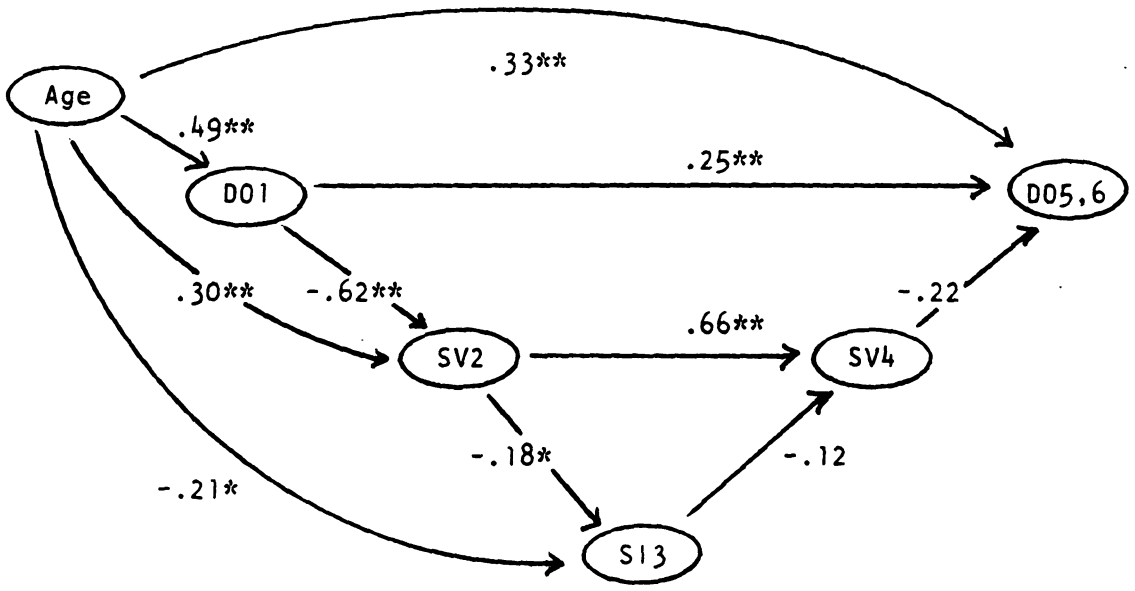
Path Model: School Involvement, Intervention, Official Delinquency (Pre-Post) and Age



* p < .05
** p < .01

Figure 17

Path Model: School Involvement, Intervention, Drop Out (Pre-Post) and Age - Revised Model



* p < .05
** p < .01

coefficient and the correlation. In the case of Official Delinquency, the source of spurious connections was less obvious. Here a number of causal pathways connecting earlier delinquency with both school involvement (at time four) and follow-up delinquency had to be summed to account for the spurious effect. These path analytic results showed that the correlation between school involvement and later delinquency is detemined by a complex set of factors. One of these factors may be a direct effect of school involvement on delinquency, although spurious factors are at least as important. From these results it can not be concluded that school involvement prevented later delinquency, nor is this possibility ruled out.

The general model of diversion interventions discussed earlier proposed that interventions came about in response to problems in the youth's life, and that the interventions led to a reduction in those problems. This was not the case for school involvement and school interventions, however. Although the corrected correlation between school intervention at time three and school involvement at time four was -.24 ($p<.05$), approximately half of that relationship was a spurious effect (Figures 15-17). Both these variables were correlated with prior school involvement (at time two). The direct effect that remained after the spurious part of the relationship was partialed out (beta = -.12) was not significant. A negative correlation between school intervention and school involvement could have indicated that school intervention reduced school involvement. This would have been opposite to the effect proposed by the theory of intervention proposed earlier. The path analytic results indicated that there was not support in these data for the

hypothesis that school intervention had any effect on later school involvement.

While it appeared in two of the models that school involvement did have a direct effect on school intervention (more school intervention occurred for youth with less previous school involvement; see Figures 16 and 17), the model that included SRD at time one changed that picture (Figure 15). Youth who reported more delinquent behavior at time one received more school intervention at time three (beta = .23, $p < .01$). Such youth also tended to be less involved in school at time two (beta = -.32, $p < .01$). This spurious connection accounted for part of the relationship between school involvement at time two and school intervention at time three. The direct relationship between these two variables was not significant (beta = -.11). The reason this path coefficient was significant in the other two models (Figures 16 and 17) was that these were missing the SRD variable. These data provided no evidence that school intervention came about in response to a lack of school involvement.

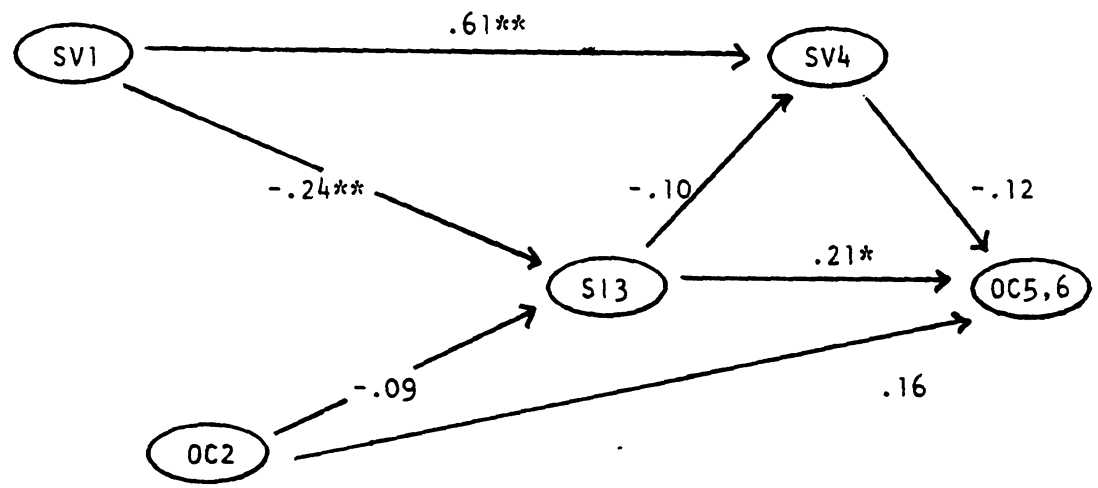
SRD at time one had a direct effect on school intervention at time three (Figure 15). The fact that this was a direct effect meant something different than simply that the two variables were correlated or even that they were correlated and related causally. It was expected that youth who engaged more heavily in delinquent behavior would be less involved in school. It was also expected that school interventions would be more likely to occur for youth who were less involved in school. Delinquency was expected to lead to increased school intervention by this mechanism. In fact, (self-reported) delinquency did lead to increased

school involvement, but not by this mechanism. The models tested here did not examine other mechanisms through which this effect might have occurred.

Preliminary path models (not presented here) suggested that school intervention had a direct positive effect (beta = .16, $p < .05$) on follow-up official delinquency. This would have indicated that youth who received more school intervention were more likely to have contact with police and courts during the two years after the program. The first hypothesis that was tested which might have explained this finding was that it was a spurious effect of previous official delinquency (Figure 18). It was expected that both school intervention and follow-up delinquency were positively related to prior delinquency, and that these relationships with prior delinquency accounted for the relationship between intervention and later delinquency. That is, early recidivism was expected to produce more intensive school interventions and to correlate positively with follow-up recidivism. If this were true, and early recidivism was not included as a variable in the model, school intervention and follow-up recidivism would appear to be positively correlated. Adding early recidivism to the model would be expected to cause a drop in this path coefficient (from school intervention to follow-up recidivism) to near zero. When earlier official delinquency was added to the model it did not cause the direct effect of school intervention on delinquency to drop (beta = .21, $p < .05$). This seemed to indicate that school intervention actually might produce more delinquency at follow-up. This turned out, however, to be in part a spurious effect of youth age: both school intervention and follow-up

Figure 18

Path Model: School Involvement, Intervention and Official Delinquency



* p < .05
** p < .01

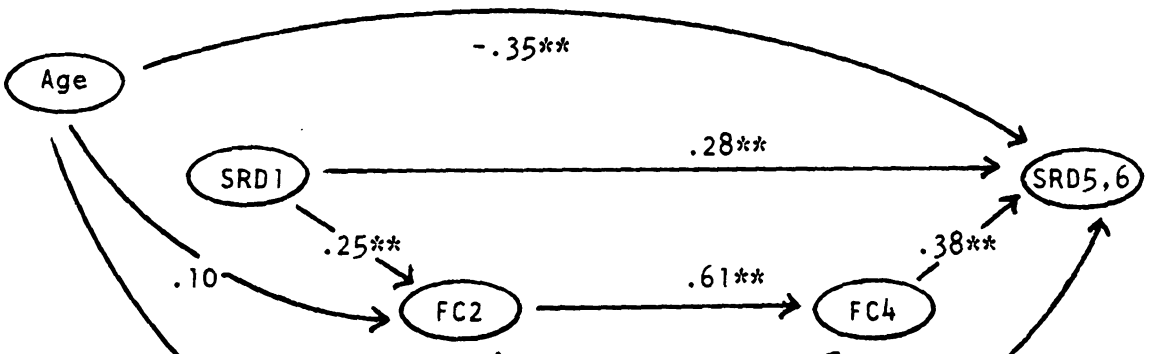
official delinquency were more likely to occur for younger youth (Figure 16). In fact, school intervention did not have direct or indirect effects on follow-up official delinquency.

In spite of their corrected correlation of .00 (Table 5), Drop Out at time one and school intervention at time three were related (Figure 17). Their relationship was mediated by school involvement at time two. Youth who "dropped out" (i.e. reported more drug use and truancy) tended to be less involved in school later (beta = -.62, $p < .01$). Youth who were less involved in school were more likely to receive school intervention later (beta = -.18, $p < .05$). (As already discussed, the mechanism for the relationship between school involvement at time two and school intervention at time three was more complex than the model in Figure 17 showed). Thus Drop Out had an indirect (i.e. mediated) positive effect on later school intervention. This relationship was masked by another variable (age) which spuriously connected Drop Out and school intervention. Older youth were more likely to "drop out" (beta = .49, $p < .01$) and were less likely to receive school intervention (beta = -.21, $p < .05$). This spurious connection was negative and it balanced the mediated effect, which was positive, producing an observed correlation of .00.

Family conflict and home intervention. Family conflict was directly related to later delinquency (Figures 19-21) for all delinquency measures. The betas for this relationship were .38 ($p < .01$) for SRD, .20 ($p < .05$) for Drop Out and .28 ($p < .01$) for Official Delinquency. Youth who reported more arguements and conflicts with parents at time four were more likely to report being involved in delinquent behavior and

Figure 19

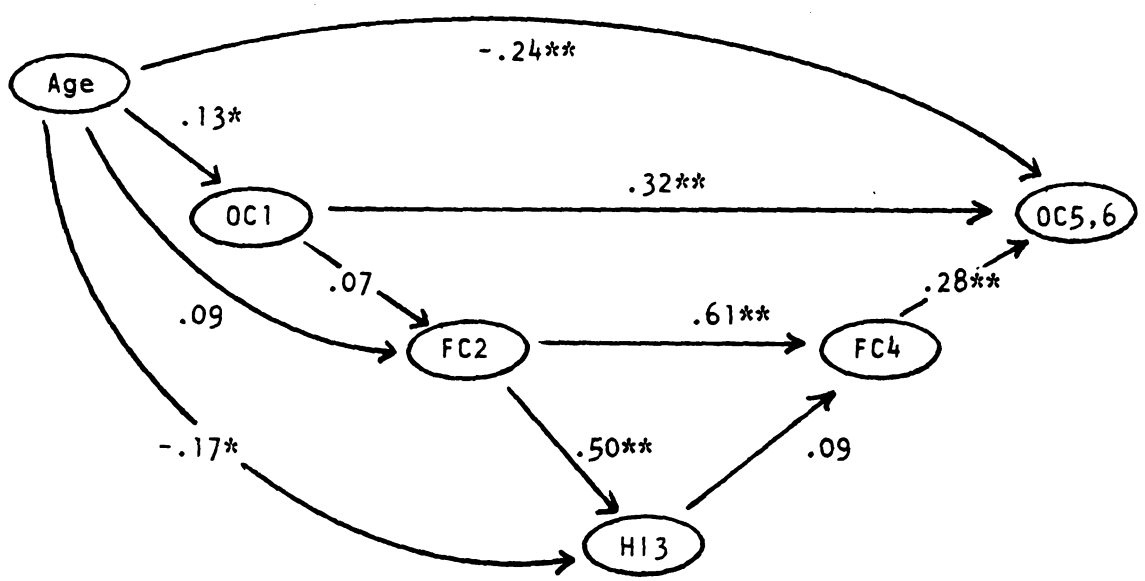
Path Model: Home Problems, Intervention, Self-Reported Delinquency (Pre-Post) and Age



* $p < .05$
** $p < .01$

Figure 20

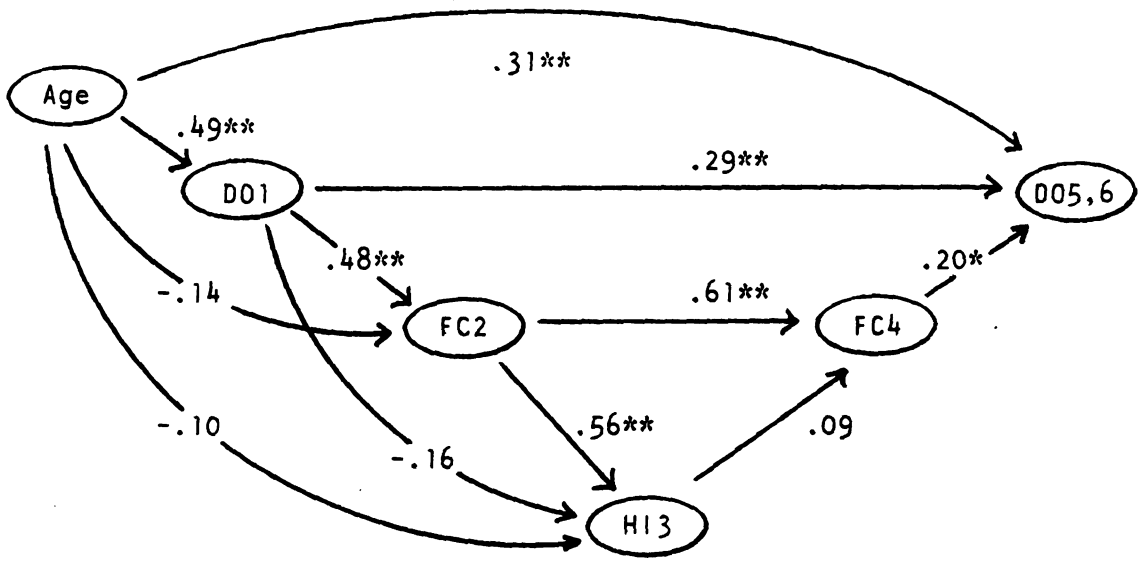
Path Model: Home Problems, Intervention, Official Delinquency (Pre-Post) and Age



* p < .05
** p < .01

Figure 21

Path Model: Home Problems, Intervention, Drop Out (Pre-Post) and Age



* p < .05
** p < .01

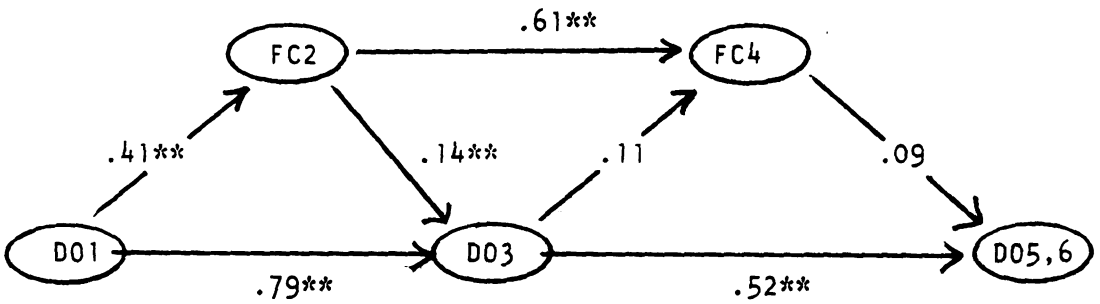
to have contact with police and courts at follow-up. The fact that the betas for this relationship were almost identical to the correlations in all three cases meant that this relationship was a direct one from family conflict to later delinquency.

In addition, there was a relationship in the opposite causal direction for two of the three delinquency measures. Both SRD and Drop Out had direct effects on later family conflict. Betas for the direct path between delinquency at time one and conflict at time two were .25 ($p < .01$) for SRD and .48 ($p < .01$) for Drop Out (Figures 19 and 21). Official Delinquency did not have this effect on family conflict (beta = .07, Figure 20). These findings seemed to indicate that in general, family conflict and delinquency are causally interdependent. The occurence of either one of them makes the other one more likely to occur.

It was decided that this hypothesis should be tested directly by examining path models specifying mutual causal influences between family conflict and delinquency, across time. When these models were examined, results quite different from those expected were obtained. If it was true both that delinquency (measured by SRD and Drop Out) caused family conflict and that family conflict caused delinquency, then one would have expected path coefficients for Figures 22 and 23 to all have been non-zero and positive. This is what the corrected correlations seemed to indicate (Table 5). Significant correlations existed between Drop Out(1) and Family Conflict(2), Family Conflict(2) and Drop Out(3), Drop Out(3) and Family Conflict(4), and between Family Conflict(4) and Drop Out(5,6). A similar set of positive correlations existed for SRD at times one, three and five-six with Family Conflict at times two and four.

Figure 22

Path Model: Relationships Between Home Problems and Drop Out Across Time



Input Correlation Matrix

	DO1	FC2	DO3	FC4	DO5
DO1	100	41	84	25	50
FC2	41	100	47	66	27
DO3	84	47	100	39	55
FC4	25	66	39	100	29
DO5	50	27	55	29	100

Observed Minus Predicted Correlations

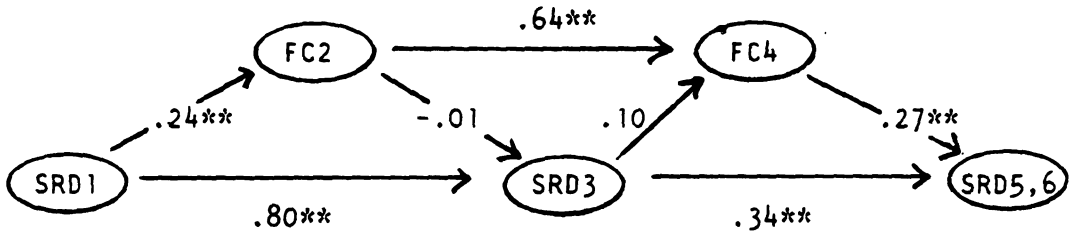
	DO1	FC2	DO3	FC4	DO5
DO1	0	0	0	-9	3
FC2	0	0	0	0	-2
DO3	0	0	0	0	0
FC4	-9	0	0	0	0
DO5	3	-2	0	0	0

Sum of squared deviations = .01

* $p < .05$
** $p < .01$

Figure 23

Path Model: Relationships Between Family Conflict and Self-Reported Delinquency Across Time



Input Correlation Matrix

	SR1	FC2	SR3	FC4	SR5
SR1	100	24	80	14	34
FC2	24	100	19	66	16
SR3	80	19	100	22	40
FC4	14	66	22	100	34
SR5	34	16	40	34	100

Observed Minus Predicted Correlations

	SR1	FC2	SR3	FC4	SR5
SR1	0	0	0	-9	1
FC2	0	0	0	0	-8
SR3	0	0	0	0	0
FC4	-9	0	0	0	0
SR5	1	-8	0	0	0

Sum of squared deviations = .02

* $p < .05$
** $p < .01$

The path models revealed that many of these relationships were spurious. In Figure 22, correlations of .39 (for Drop Out, time three with family conflict, time four) and .29 (for family conflict, time four with Drop Out, times five and six) produced non-significant betas. In Figure 23 as well, only two of the four direct path coefficients connecting family conflict with delinquency were significant. These models seemed to indicate that family conflict and delinquency did not influence each other in a stable way. It may be that in this case, however, "spurious" connections should be interpreted differently than in situations encountered in previously-discussed models. The interpretation of Figures 22 and 23 will be discussed more fully in the next chapter.

Figures 19 to 21 showed that home intervention did not have a significant direct impact on later family conflict (beta = .09). The correlation between home intervention at time three and family conflict at time four was .39 ($p < .01$), but three-fourths of this relationship was spurious. Both home intervention at time three and family conflict at time four were more likely to come about for youth with higher levels of family conflict at time two. This meant that it could not be concluded that home intervention produced more family conflict. Based on these findings, the most likely conclusion was that home intervention had no effect on family conflict.

Figures 19 to 21 indicated that home intervention did not have direct or indirect (mediated) effects on later delinquency regardless of the measure of delinquency that was used. It <u>was</u> related to <u>earlier</u> delinquency indirectly (through family conflict), for two of the

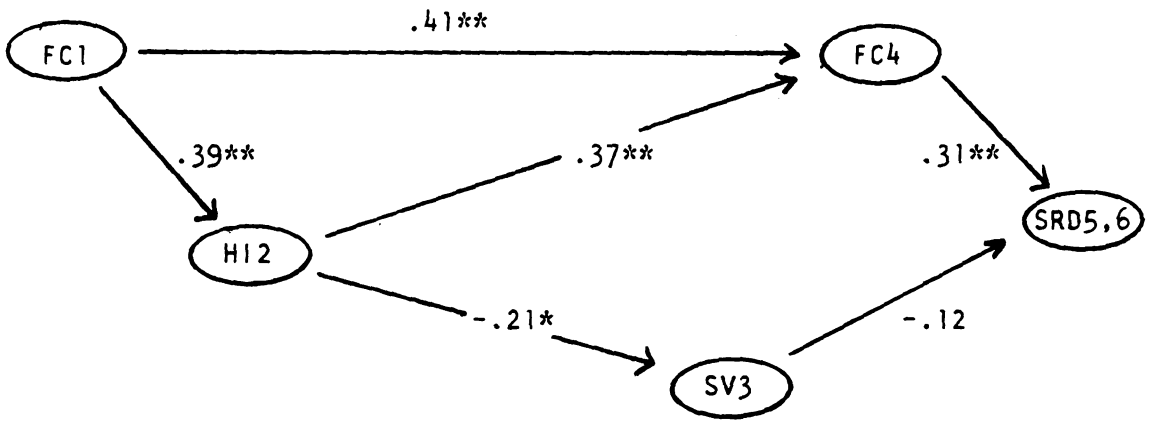
delinquency measures (SRD and Drop Out). This relationships was such that youth reporting more delinquency received more home intervention (because they tended to have more family conflict). That is, it appeared that delinquency (except official) produced family conflict, that family conflict produced more home intervention, that home intervention did not affect later family conflict, and that family conflict produced more delinquency.

Because in some of the preliminary models (not presented here) it appeared that home intervention might have a direct negative effect on later SRD (not mediated by family conflict), an initial attempt was made to explore mechanisms through which such an effect might occur. It was thought that home intervention, in addition to its effect on family conflict, might have impact on school involvement, and through this mediator, reduce delinquency (Figure 24). Instead, it was found that home intervention led to reduced school involvement (beta = -.21, $p < .05$). The net effect of this path from home intervention to follow-up SRD was positive, although since school involvement did not have a significant direct effect on SRD at follow-up (beta = -.12), it was not significant. This model confirmed the idea that interventions could have effects outside the immediate setting in which they were focused, but it did not clarify any alternative mechanisms by which home interventions might have reduced delinquency.

Home intervention and contracting. One important set of questions this dissertation tried to answer had to do with the relationships between different interventions. For each of the three time periods at which they were measured, home intervention and contracting were

Figure 24

Path Model: Family Conflict, Intervention, School Involvement and Self-Reported Delinquency



* $p < .05$
** $p < .01$

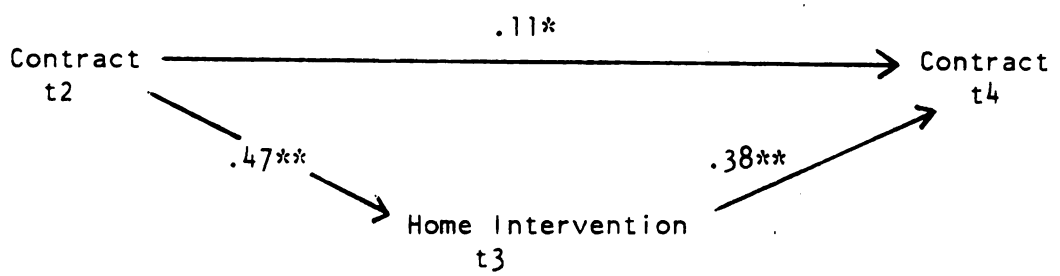
positively correlated. Corrected correlations between these variables were .58 ($p < .01$) at time two, .67 ($p < .01$) at time three and .53 ($p < .01$) at time four. This was to be expected, given the nature of these two interventions. Contracting consisted of specific interventions (e.g. setting up a contract, negotiating its provisions, monitoring compliance, etc.) which were often used in the home setting. Home intervention, as defined here, consisted of a more general set of activities, like talking about problems at home, mediating family disageements, helping the youth or parents to change particular behaviors, etc. Clearly, these two types of intervention would not be expected to occur independently of each other.

The definitions of the two interventions seemed to indicate that if contracting was occurring, so was home intervention. The reverse did not seem to be true: home intervention (the more general set of activities) could occur without contracting. A related question was whether any causal relationships existed betweeen these two types of intervention.

Because these two interventions were each measured at only three time periods, the models that could be used to explore causal relationships between them were fairly simple (Figures 25 & 26). It appeared that causal effects occurred in both directions. The direct effect of contracting at time two on home intervention at time three (beta = .47, $p > .01$) approximately equalled the direct effect of home intervention at time two on contracting at time three (beta = .50, $p < .01$). Direct effects between these two interventions were also significant in both directions at the later time interval (i.e. between times three and four). Here, however, the path from home intervention to

Figure 25

Path Model: Relationships Between Contracting and Home Intervention I.



Input Correlation Matrix

	CN2	HI3	CN4
CN2	100	47	29
HI3	47	100	43
CN4	29	43	100

Observed Minus Predicted Correlations

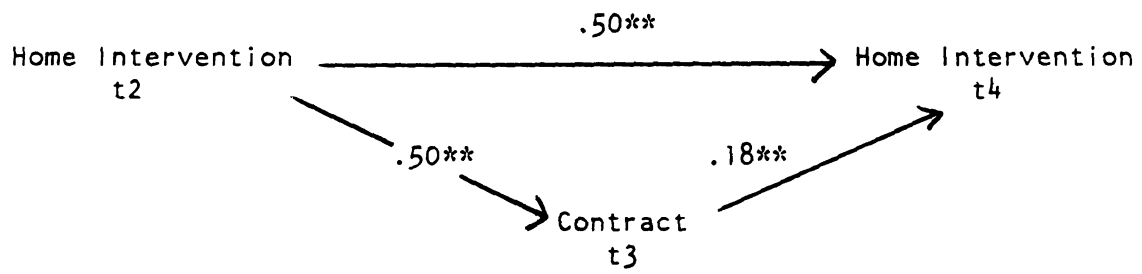
	CN2	HI3	CN4
CN2	0	0	0
HI3	0	0	0
CN4	0	0	0

Sum of squared deviations = .00

* $p < .05$
** $p < .01$

Figure 26

Path Model: Relationships Between Contracting and Home Intervention II.



Input Correlation Matrix

	HI2	CN3	HI4
HI2	100	50	59
CN3	50	100	43
HI4	59	43	100

Observed Minus Predicted Correlations

	HI2	CN3	HI4
HI2	0	0	0
CN3	0	0	0
HI4	0	0	0

Sum of squared deviations = .00

* $p < .05$
** $p < .01$

contracting (beta = .38, $p < .01$) was significantly larger than the path from contracting to home intervention (beta = .18, $p < .01$). It was also true that home intervention persisted over the interval between times two and four ($r(t2,t4) = .59$) to a much greater extent than did contracting ($r(t2,t4) = .29$). The persistence of home intervention from time two to time four occurred whether or not contracting occurred at time three. This could not be said for contracting. Its persistence from time two to time four depended to a large extent on whether more general home interventions occured at time three. To say this another way, home intervention mediated a large part of the relationship between contracting at times two and four, but contracting hardly mediated the relationship between home intervention at times two and four at all. Another way of saying this is that in order for contracting to continue to occur, it was necessary that general home intervention also occur. The persistence of general home intervention was much less dependent on the simultaneous occurrence of contracting.

It appeared that a large portion of the direct effect of contracting on home intervention was spurious. While contracting at time three and home intervention at time four had a corrected correlation of .43 ($p < .01$), the beta for the direct path between them was only .18 ($p < .01$). Most of the correlation between these two variables came about because both resulted from earlier home intervention. These findings only begin to illustrate the complex interplay that probably occurred between different components of the intervention. These relationships were not only complex, but apparently shifted over time.

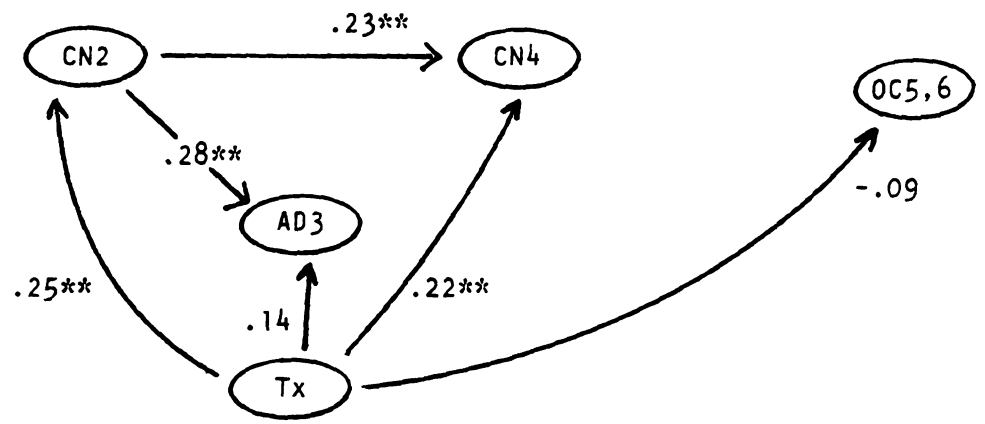
Advocacy, contracting and treatment condition. Like contracting and home intervention, contracting and advocacy were positively correlated within time periods. Their corrected correlations were .50 at time two, .44 at time three and .38 at time four (all $p < .01$). This was expected, since volunteers in the Family and Action treatment conditions were trained to do both types of intervention, while volunteers in the Low Intensity and Relationship conditions were trained to do neither. In other words, the hypothesis was that these interventions were related spuriously, through their relationships with treatment condition.

The path model that was originally tested to examine this hypothesis fit quite poorly (Figure 10). There was a strong relationship between advocacy and contracting not accounted for by the model. This might have been because direct effects existed between these two variables. More complex models were tested in order to examine this possibility (Figures 27 and 28; Table 6). For this series of models, treatment condition was coded as a dichotomous variable with 1 = MSU Action, LCC, Community or Family; 0 = Relationship, Low Intensity or Control, and with Court coded as missing. This meant that in these models, "condition" was a variable measuring whether or not the volunteer's training and supervision supported and encouraged the use of contracting and advocacy interventions.

The initial model in this series showed that condition predicted contracting but it did not predict advocacy. Youth in the Action and Family conditions received more contracting than other youth at time two (beta = .25, $p < .01$) and time four (beta = .22, $p < .01$), but they did not receive more advocacy at time three (beta = .14, ns).

Figure 27

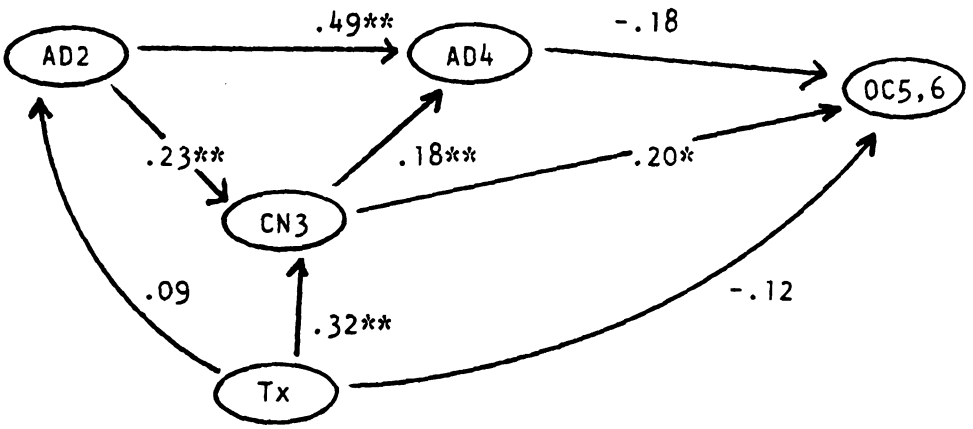
Path Model: Relationships Between Advocacy, Contracting, Official Delinquency and Condition I.



* p < .05
** p < .01

Figure 28

Path Model: Relationships Between Advocacy, Contracting, Official Delinquency and Condition II.



* p < .05
** p < .01

Table 6

Correlation Matrix for Treatment Condition, Interventions and Delinquency

	TX	CN2	CN3	CN4	AD2	AD3	AD4	OC5
TX	100	25	35	28	9	21	19	-9
CN2	25	100	57	29	50	32	22	7
CN3	35	57	100	56	26	44	31	10
CN4	28	29	56	100	12	17	38	3
AD2	9	50	26	12	100	52	54	3
AD3	21	32	44	17	52	100	62	-2
AD4	19	22	31	38	54	62	100	-14
OC5	-9	7	10	3	3	-2	-14	100

Note. All correlations are corrected for attenuation. Cross-time correlations for CN and AD were also corrected for specific error. To be significant, r's including intervention variables must be > .19.

Variables: TX = Treatment Condition (dichotomized)
CN = Contracting
AD = Advocacy
OC = Official Delinquency

Time periods: 1 = pre-intervention, 2-4 = during intervention, 5 = 1 & 2 years post-intervention.

There was a relationship between condition and advocacy at time three that was mediated by contracting at time two. This mediated path between condition and advocacy also appeared in Figure 28, where condition predicted contracting at time three (beta = .32, $p < .01$), which predicted advocacy at time four (beta = .18, $p < .01$). In other words, being in the treatment conditions where volunteers were taught advocacy and contracting meant that a youth was more likely to receive contracting. Youth who received contracting were more likely to receive advocacy later. While significant, these relationships were not large.

The model in Figure 27 showed that the relationship between contracting and advocacy may occur in only one causal direction. While contracting predicted later advocacy, there was no direct relationship between advocacy and later contracting in this model. This conclusion was contradicted by the model tested next (Figure 28). Here advocacy did have a direct effect on later contracting (beta = .23, $p < .01$). This meant that the relationship between advocacy and contracting changed over time. Early in the intervention period, advocacy predicted later contracting. Later in the intervention period it did not.

Figure 28 revealed an interesting connection between contracting and official delinquency. Contracting at time three had a direct positive effect on official delinquency at follow-up (beta = .20, $p < .05$). This relationship was revealed when it was assumed that condition might have a direct effect on outcome that was not mediated by contracting or advocacy. While this direct effect of condition on follow-up delinquency was not significant (beta = -.12), it did produce a slight negative spurious correlation between contracting at time three and official

delinquency at follow-up. This negative effect, combined with the negative mediated path from contracting to official delinquency through advocacy at time four, balanced the direct path coefficient from contracting to official delinquency, producing a corrected correlation of .10 (ns) between contracting at time three and follow-up delinquency. This is why the correlation was not significant while the path coefficient was. Before concluding that contracting caused more delinquency at follow-up, it should be noted that this relationship only existed for contracting at time three. Contracting at time four was unrelated to follow-up delinquency (Figure 27).

Earlier studies of the diversion interventions discussed in this dissertation showed that treatment conditions based on an advocacy/contracting treatment model were more successful in preventing follow-up official delinquency than no treatment or non-specific treatments, and that volunteers trained to do advocacy and contracting did more of it than volunteers not trained in these interventions (Davidson, Redner, Blakely, Mitchell, & Emshoff, 1987; Redner & Blakely, 1986). These findings led to the question of whether treatment conditions using advocacy and contracting produced their impact on official delinquency <u>because</u> of increased advocacy and contracting. The models tested here seemed to indicate that this was not the case. Of these two interventions, only contracting had a direct effect on follow-up official delinquency, and this was a positive effect. Yet condition had a positive direct effect on contracting. When this path through contracting was partialled out, condition still did not have a significant negative effect on delinquency, either directly or indirectly

(e.g. through advocacy). These findings only increased the mystery of how treatment condition might have had impact on delinquency.

Effects of youth age. Two hypotheses being investigated about the relationship between youth age and treatment outcome were a) treatments were more effective for younger youth and were not as effective for older youth, or b) older youth were less delinquent than younger youth, so that there was less room for improvement for older youth, so that the difference between experimentals and controls would not be as large for older youth, making treatments only appear to be more effective with younger youth. Each of these hypotheses could explain the results found in the literature regarding the relationship between the effectiveness of interventions and youth age.

In the present study, the effects of age on delinquency varied, according to the measure of delinquency that was used. Age was not related to SRD at time one (Figure 15), and was negatively related to SRD at follow-up (beta = -.32, $p < .01$). Thus older youth reported less delinquency at follow-up in spite of the fact that older and younger youth reported about equal amounts of delinquent behavior initially. This is opposite to the finding one would expect if younger youth were being helped by the program more than older youth. In that case, age and delinquency would have been uncorrelated initially, then positively correlated (Figure 11).

The relationship between age and official delinquency changed from positive initially to negative at follow-up (Figure 16). Thus older youth had more police and court contacts in the year prior to the start of the program, but fewer in the two years after the program terminated.

The direction of these effects were consistent with the findings for SRD. They may indicate that the program was more effective for older youth (contrary to the findings in the literature).

Age was positively related to drop out at both time one and at follow-up (Figure 17). Older youth were thus more likely to skip school and use drugs at both time periods. This effect was opposite to the one for the other two delinquency measures. Using those measures, older youth appeared to be less delinquent at follow-up than younger youth. Using truancy and drug use as the delinquency measure, older youth were more delinquent than younger youth at follow-up. This suggests that delinquency follows a different course over time than truancy and drug use.

Adding youth age to path models produced other interesting effects. Using drop out as the measure of delinquency, a relationship between age and school involvement emerged, which had been hidden (Figure 17). The corrected correlation between age and school involvement was .00. This masked the fact that age had both direct and indirect effects on school involvement. The direct effect was positive (beta = .30, $p < .01$), indicating that older youth were more involved in school. The indirect effect, mediated by drop out, was negative. This meant that older youth were more likely to drop out (beta = .49, $p < .01$) and that youth who dropped out were less likely to be involved in school (beta = -.62, $p < .01$). The direct and indirect effects exactly balanced each other, and both were moderately strong. Thus it would be wrong to conclude that age and school involvement were unrelated. In fact, older youth who did not

"drop out" were likely to be more involved in school than younger youth, but older youth were also more likely than younger youth to "drop out".

Youth age had effects on other variables in these models as well. Age had a direct negative effect on school intervention (beta = -.21, $p < .05$), independent of its impact on drop out and school involvement (Figure 17). Thus younger youth were more likely to receive school intervention, even after the effects of drop out and school intervention were accounted for. This means that the causal chain from age to drop out to school involvement to school intervention could not explain this effect. The reason for this effect is not known.

There also appeared to be a small direct negative effect of age on home intervention (Figures 19 and 20). Home intervention occurred more often for younger youth (beta = .17, $p < .05$).

DISCUSSION

Measurement Models

The first-order cluster structure for intervention items remained stable over time. This indicated that items asking about specific intervention activities retained their meaning, relative to other intervention items, throughout the period of the intervention. By testing the measurement model using confirmatory factor analysis it could be determined that intervention, youth life events and delinquency variables were all measuring distinct constructs. Had causal modeling been attempted without having first tested the measurement model, it would have been difficult to interpret the causal models, because it would have been possible that predictor and criterion variables were both measures of the same construct. (For a more detailed explanation of this point, see Amdur, 1985b). This was a problem in several earlier studies of delinquency (e.g. Akers et al., 1979; Thompson, Mitchell & Dodder, 1984).

While the composition of the intervention clusters remained the same during the period of the intervention, this did not necessarily mean that the pattern of intercorrelations between the interventions remained the same. It appears that for several interventions, relationships with other interventions and with youth life events changed during the 18-week period of intervention. Because a distinction was made in this study between the causal and the measurement models, these changes could be interpreted as changes in the causal structure for these variables, rather than as changes in the meaning of the variables.

Youth Life Events, Interventions and Delinquency

Effective interventions were expected to have either direct or mediated paths leading to delinquency with a net negative effect. That is, the more intervention that occurred, the less likely later delinquency should be. Using these criteria, neither home nor school interventions appeared to be effective either in improving the youth characteristic on which they focused, or in reducing delinquency through other channels. Both these interventions were apparently delivered with the greatest intensity (or highest probability) to those youth who were the most difficult to help (i.e. those with more self-reported delinquency, more family conflict, less school involvement, and those who were younger). This could account for their lack of success. Another possibility is that the intervention-outcome relationship could be non-linear. That is, the effect of increasing the strength of intervention on the outcome might depend on the level of intervention or of delinquency. The models examined here did not test for this possibility. All that can be concluded is that for the youth in this sample, the levels of home and school interventions delivered were ineffective.

School involvement was not a youth characteristic that affected later delinquency. Thus even if the school intervention had increased school involvement, it presumably would not have had an impact on delinquency. Family conflict was a youth characteristic which did seem to affect later delinquency, regardless of the measure of delinquency that was used. Thus interventions that reduced family conflict presumably would have reduced delinquency. This suggests that home

involvement may be a more central aspect of the "social bond" than school involvement.

Lack of school involvement and increased family conflict were both predicted by earlier delinquency, but only using the self-report measures of delinquency (SRD and Drop Out). This might have been a result of biased reporting (that is, youth who report being 'bad' also report being uninvolved in school and unloved at home). Still, it does indicate that involvement in the legal system (as measured by the Official Delinquency variable) does not seem to account for further problems at home or in school.

Causal Interactions between Interventions

The relationship between general home intervention and more specific behavioral contracting remains unclear. Home interventions occurred consistently over time whether or not contracting intervened. Contracting, however, did not persist as strongly without the occurrence of home interventions as a mediator. In order to clarify what this might mean, it will be necessary to examine the interrelationships between these interventions in the context of their relationships with family conflicts, treatment condition and outcome.

Home intervention clearly came about in response to increased family conflict. It will be interesting to see whether this was also true for contracting. If so, this could account for the postive relationship between contracting at time three and follow-up delinquency. This would simply be a spurious connection resulting from the fact that both variables were positively related to earlier family conflict.

Examining the causal order in the relationships between family conflict, contracting and home intervention could tell us more about the contracting intervention. For example, it might tell us whether contracting evolved slowly, following more general home interventions or whether contracting was a more immediate response, on the part of the volunteer, to the perception of family conflict, which gave way later to more general home interventions. Examination of the causal order of these two interventions with delinquency measures at pre and post could help clarify the mechanisms of effect of both interventions.

This was the strategy that was used in examining the relationships between the advocacy and contracting interventions. It appeared that these interventions "differentiated" from each other over time. At first (i.e. from time two to time three), advocacy seemed to produce more contracting. Later (i.e. from time three to time four), advocacy no longer had a direct effect on contracting. This change in the relationship between the two interventions might have occurred for several reasons. First, the youth would probably become more familiar with these two interventions over time, and thus would be more able to distinguish between them, and report more accurately on which was occurring, toward the end of the intervention period. Second, the two interventions both were designed to change over time. Both interventions started with a phase of evaluation and identification of problems and targets of change. Thus the two interventions might only have differed, from the youth's point of view, toward the end of the intervention period, during the more "action-oriented" phase of intervention. Third, it may have been the case that advocacy techniques were initially used by

volunteers as an entre with the youth's parents or school officials, that could develop into a contracting relationship. Once a relationship developed, problems had been identified, and the volunteer had gained leverage with the significant others through the use of advocacy, contracting techniques might be used.

Eighteen weeks may not have been a long enough period for more than one "cycle" of intervention activities. This might help to explain why the relationship between advocacy and contracting changed later in the intervention period. Toward the end of the intervention period, the volunteer's task was no longer to identify problems and try to solve them, but to help the youth learn these techniques for him or herself. It may be that the advocacy techniques used at time three did not help the youth learn contracting at time four (thus no effect of advocacy on contracting in Figure 26), but that contracting at time three did help the youth learn advocacy at time four (thus the direct effect of contracting on advocacy in Figure 27).

Advocacy, Contracting and Delinquency

Of the two interventions comprising the Action treatment conditions (i.e. advocacy and contracting), advocacy appeared be the more effective one. This cannot be said conclusively however, because even though moderate relationships existed between advocacy and later delinquency, these were not always statistically significant. Comparison of the direct path coefficients from both interventions to follow-up official delinquency (Figure 27) showed that they were significantly different (if we assume the standard errors for these betas to be at most 20 percent larger than for betas computed from raw data), with advocacy more likely

to reduce delinquency, and contracting more likely to increase it. An easy way of improving the models that examined the relationships of these variables to treatment condition and follow-up delinquency would be to include delinquency at pre. In other models (e.g., Figures 15 to 17), inclusion of this variable helped to clarify the picture.

Cross-Domain Effects

The finding that home interventions could affect the school domain indicate that these interventions were probably a good deal more complex in their effects than we were able to discover. Once it is realized that the interventions may influence each other, it is not surprising to find that interventions have effects on a varietey of youth characteristics. It also becomes clear that the amount of data that would be needed in order to observe these interrelationships is immense. In spite of the fact that the present study was based on a data set that was larger (in terms of numbers of subjects, measures and time periods) than most that have been available on delinquency treatments (or any other psychological interventions, for that matter), many of the findings are not conclusive. Instead, they suggest further work that needs to be done. Because it is difficult to implement carefully controlled interventions and to collect data on this large a scale, progress in understanding the mechanisms of treatment effects will be slow.

Family Conflict and Delinquency

Family conflict was a youth characteristic which seemed to affect later delinquency, regardless of the measure of delinquency that was used. This conclusion was brought into question, however, when the mutual causal influences between conflict and delinquency were examined

across time (Figures 22 and 23). These two models made it appear that many of the correlations between delinquency and family conflict at adjacent time periods were spurious. If this were true, it would mean that family conflict and delinquency do not directly influence each other in a stable way.

These models were the result of taking what would have been a recursive causal structure at a single time period (conflict causes delinquency and delinquency causes conflict), and spreading it out over time. The models in Figures 22 and 23 should both be regarded as small pieces of an ongoing, stable process which started before these data were collected, and continued to occur after data collection stopped. This fact has several important implications.

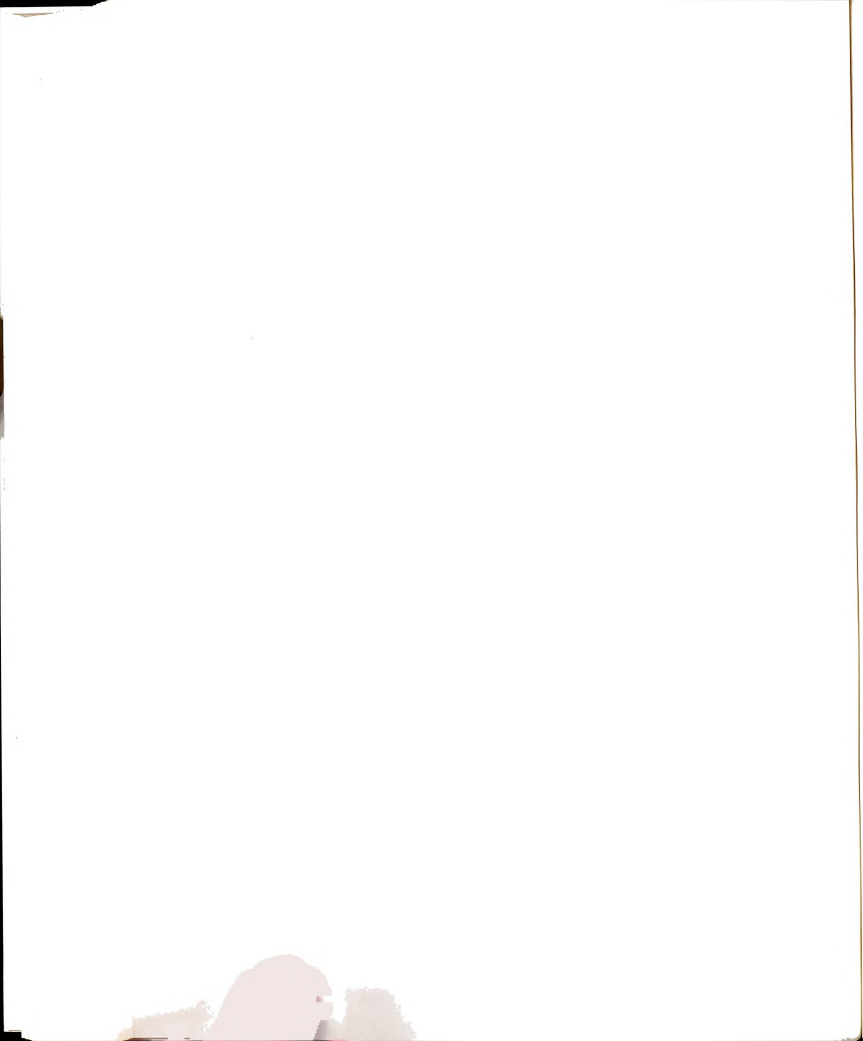
First, the fact that the first path between delinquency and family conflict was significant in both models is not surprising. Had there been an earlier measurement of family conflict taken, this connection (between delinquency at time one and conflict at time two) would probably have appeared to be spurious. Since it was arbitrary which variable was measured first (Family Conflict or Delinquency), not much should be made of the direction of the effect in the first path between delinquency and conflict.

Second, whether or not a path is spurious in these models is determined not only by the last two paths in the chain, but by the whole history of paths that occurred before. To illustrate this point with a concrete example, the corrected correlation between family conflict at time four and drop out at times five and six was .29 ($p < .01$) (Table 5). The path coefficient for the direct path between these two variables

(Figure 22) was .09 (ns). The product of the path coefficients for the two paths immediately preceeding this one in the causal order (.11 and .52) was .06. This left .14 (which was .29 - .09 - .06), or approximately half of the simple r, unaccounted for. The residual for this path was .00, so none of the missing .14 could be attributed to lack of model fit.

In order to find the missing .14, one would have to go back to the beginning of the chain (i.e. start with drop out at time one) and add in all the spurious connections between drop out at time three and conflict at time four. The easy way of doing this is to use the simple r between drop out(3) and conflict(4) (which was .39) to calculate the spurious effect in the final path of this model. This brings the spurious effect for the conflict(4) to drop out(5,6) path to .20 (.39 x .52), solving the mystery (since .20, the spurious effect, and .09, the direct effect, sum to .29, the correlation). This was why, for example, in Figure 23, the path coefficient connecting SRD(3) and conflict(4) was .10 even though the simple r between these variables was .22 and even though the previous path in the chain had a coefficient of only -.01. In general, this is why models like these will tend to have a series of non-significant path coefficients in spite of the fact that all the correlations are significant.

A third consideration in interpreting these models is that even if a perfectly stable system at equilibrium existed for these variables, and even if consistent and moderately large direct effects existed between delinquency and family conflict, sampling error would cause considerable instability in the path coefficients between these two variables.



Sampling error would tend to produce path coefficients for the direct effects between the two variables that sometimes appeared to be significant, sometimes not, in a random pattern. Uncorrected correlations in these models generally had standard errors of .05 to .06. This meant that correlations could fluctuate by .10 to .12 in either direction around the true (uncorrected) correlation and still remain within the 95% confidence interval. Confidence intervals for the corrected correlations would be about 20 percent wider (given average reliabilities of .80). Whether or not a given path coefficient connecting delinquency and conflict was significant depended on the size of the correlation for these two variables, relative to the product of the correlations of the two preceeding variables in the causal order. This product would be expected to vary by at least .20 to .25 simply by virtue of sampling error. Direct effects would then have to be quite large (correlations of approximately .45 or higher) in order to be consistently significant and thus to appear non-spurious. Direct effects that were only moderately large (correlations in the range of .25 to .35) would appear non-significant (i.e. spurious) perhaps 40 to 50 percent of the time, in a random sequence. This is likely to be the explanation for the apparent instability of direct path coefficients between delinquency and family conflict in Figures 22 and 23.

It is also possible that overlap in the time period covered by the self-report scale between times one and three led to the apparent lack of effects of delinquency at T3, when delinquency at T1 was partialed out. In this case, the lack of effect would be an artifact of the measurement procedure.

What all this means is that models 22 and 23 can not be interpreted as disproving the possibility that family conflict and delinquency had direct positive effects on each other that were stable across time.

Different Forms of Delinquency

It is clear from results presented earlier that different forms of delinquency were not "causally homogeneous". The three delinquency measures had different relationships with other variables, whether these other variables were predicting later delinquency or being predicted by earlier delinquency. It was interesting that in spite of these differences, the consistency of all three forms of delinquency over time were the same. Direct paths for each variable from time one to time five were approximately .28.

It has already been pointed out by a number of authors that official delinquency and self-reported delinquency are different phenomena, that occur for different reasons, that have different effects and that require separate measures (Amdur, 1985a; Hindelang, Hirschi & Weiss, 1981). This was confirmed here. Another interesting finding was that drop out (i.e. use of drugs and skipping school) is a form of delinquency that is different from official delinquency and from self-reported general delinquency (i.e. crimes against person or property). This may come as a surprise to authors who believed drug use and general delinquency to be essentially the same (Akers et al., 1979; Elliot, Huizinga & Ageton, 1985).

Youth life events variables which have differential impact on the three types of delinquency remain to be discovered. It will also be interesting to develop causal models which might explain the

relationships between the three forms of delinquency. Preliminary analyses suggest that self-reported and official delinquency may have a relationship in which the causal order is unidirectional (from SRD to officical delinquency), and that youth life events may mediate this relationship. Path analysis of the interrelationships of these variables may help to elucidate the similarities and differences between these different measures of delinquency.

Youth Age

Older youth reported less general delinquency (SRD) at follow-up, but not initially. This contradicted the hypothesis that younger youth were helped by the program more than older youth. This finding could mean that older youth were in fact more likely to be helped by diversion than younger youth. An alternative explanation for this finding might be that older youth "outgrew" delinquency during the two-year period covered by these data. For the older youth in the sample, data were collected from the time they were approximately 16 until they were approximately 19. For the younger youth in the sample, the data collection covered the years from approximately 12 to 15. Thus for the younger group, delinquency was just developing, while for the older group it may have been declining due to "maturation". Another possible explanation for the SRD results might be that older youth were more likely to learn to under-report delinquent behavior by the time follow-up data were collected.

The relationships between age and official delinquency seemed to rule out this last hypothesis, and to strengthen the conclusion that real changes in delinquent behavior were occurring over time, and that these changes were related to the youth's age. Older youth were more likely to

have had police and court contacts in the year prior to the program. They were less likely to have such contacts in the two years after the program.

The current findings cannot conclusively prove or disprove either of the two hypotheses about the relationship between age and effect size in the diversion outcome literature, since they are the results of studying only a single diversion program. They do, however, lend support to one of the explanations, and make the other seem fairly doubtful. When the force of maturation is considered, it seems unlikely that diversion programs will be more effective with younger youth who are just coming into the height of their "delinquent years". A more likely explanation for the findings in the literature (and one that also explains the results of the present study, which would otherwise seem to contradict the literature) is that older youth do not appear to benefit as much from diversion because even without intervention, they would not be very delinquent.

Older youth may have been less delinquent but they were more likely to use drugs and to skip school. The difference between drop out and the other two delinquency measures was quite noticeable here. Age was unrelated to SRD at time one and barely related to official delinquency at time one (beta = .13, $p < .05$). It was strongly related to drop out at time one (beta = .48, $p < .01$)). Age was negatively related to SRD and official delinquency at follow-up with a moderately strong relationship, and was positively related to drop out at follow-up with an equally strong relationship. It is impressive that the direct path from age to drop out at follow-up remained as large as it did after drop out

at time one was partialled out. This means that age has a continuing positive impact on truancy and drug use throughout the teenage years. Unlike the temptation to engage in other delinquent behavior, this does not dissipate for older adolescents (at least not before age 19).

The positive relationship between age and school involvement indicates that it would be wrong to conclude that older youth are less involved in school than younger youth, or that age and school involvement are unrelated. In fact, older youth who stayed in school and did not use drugs were more involved in school (i.e. enjoyed it more, performed better) than were younger youth.

Youth age also had impact on interventions which were independent of its effects on delinquency. Both home and school interventions were more likely to occur for younger youth. This seems to be consistent with other findings which indicated that these interventions were more likely to occur for the most difficult-to-help youth (i.e. those with more family conflict, less school involvement and more delinquency).

These results made it clear that the youth's age cannot be ignored if the mechanisms by which delinquency comes about and is reduced are to be understood. Age had different relationships with each delinquency variable. In addition, the inclusion of youth age in path models often allowed relationships between other variables to emerge which would otherwise have been masked. Because age is unaffected by any other variable, it can be included in path models regardless of the number of time periods of data that are available (i.e. it is always an exogenous variable) with little or no difficulty. These results indicate that age should be included in all models that are tested in the future because

the relationships between delinquency and other variables cannot be understood independently of youth age.

Delinquency Theories

One of the goals of this dissertation was to test the credibility of two theories of the etiology of delinquency. Control theory said that delinquency was caused by a breakdown in the social bonds that tied a youth to cultural values and norms. Labeling theory said that youth who were labeled by others as "delinquents" tended to engage in more delinquent behavior as a result of the label. It also said that delinquency was not just an effect or outcome but a cause of further labeling. Several of the findings of this study had some bearing on these theories.

The fact that family conflict resulted in increased delinquency meant that youth who were more involved with parents (in a conflicted way) were more likely to engage in delinquent behavior. This indicated that it may be an oversimplification to say that lack of involvement alone predicts delinquency (as in control theory). It is not only the existence of social bonds but the quality of social relationships that seem to matter.

It was thought that if labeling theory was correct, official delinquency should result in the labeling of the youth as deviant, which should lead to decreased involvement or increased conflict with others. Yet, official delinquency at time one had no effect either on family conflict or on school involvement.

Thus the results obtained here do not unambiguously support either of these two theories. The results suggested that a more refined version

of control theory is needed which takes into account not just the existence of relationships with others, but the quality of those relationships. Labeling theory seemed to be contradicted by some of the findings here. It should be remembered though, that several variables which labeling theory would postulate as mediating the relationship between official delinquency and later involvement were not measured here, since this study was not designed specifically to test these theories. A path analytic methodology similar to the one used here would probably be useful in any study that did focus on the causes of delinquency.

Evaluative and Elucidatory Methods

In general, as models got more complex, what they said about individual path coefficients became more accurate. In looking through the models tested in this dissertation, it is difficult to find one in which important results would not have been missed or misinterpreted, had a univariate or bivariate approach been taken in the data analysis. In many cases, a bivariate approach would have led to "tentative" conclusions that would have been false (Table 7). It appears that even in applied areas of research, complex methodologies are required. "Elucidatory" methods are necessary not just to understand treatment process, but also to accurately assess treatment effectiveness (Cordray, 1986). In studying the effects of diversion, the situation is one in which a large number of variables produce complex interactions. In such a situation it makes little sense to spend much time considering the "main effects".

Table 7

Differences in Findings of Elucidatory and Evaluative Methodologies

Evaluative Conclusion	Elucidatory Conclusion
1. School involvement prevents delinquency.	1. ---
2. School intervention reduces school involvement.	2. ---
3. Less school involvement leads to more school intervention.	3. ---
4. More delinquent youth are less involved in school and thus receive more school intervention.	4. More delinquent youth receive more school intervention, but not because they are less involved in school.
5. School intervention leads to increased official delinquency.	5. ---
6. Home intervention produces family conflict.	6. ---
7. School involvement is unrelated to youth age.	7. Older youth are more involved in school, but also more likely to be truant and to use drugs. Those who are truant and use drugs are less involved in school.

Note. "---" should be interpreted to mean that a particular relationship is more complex than described using the evaluative methodology.

Future Directions

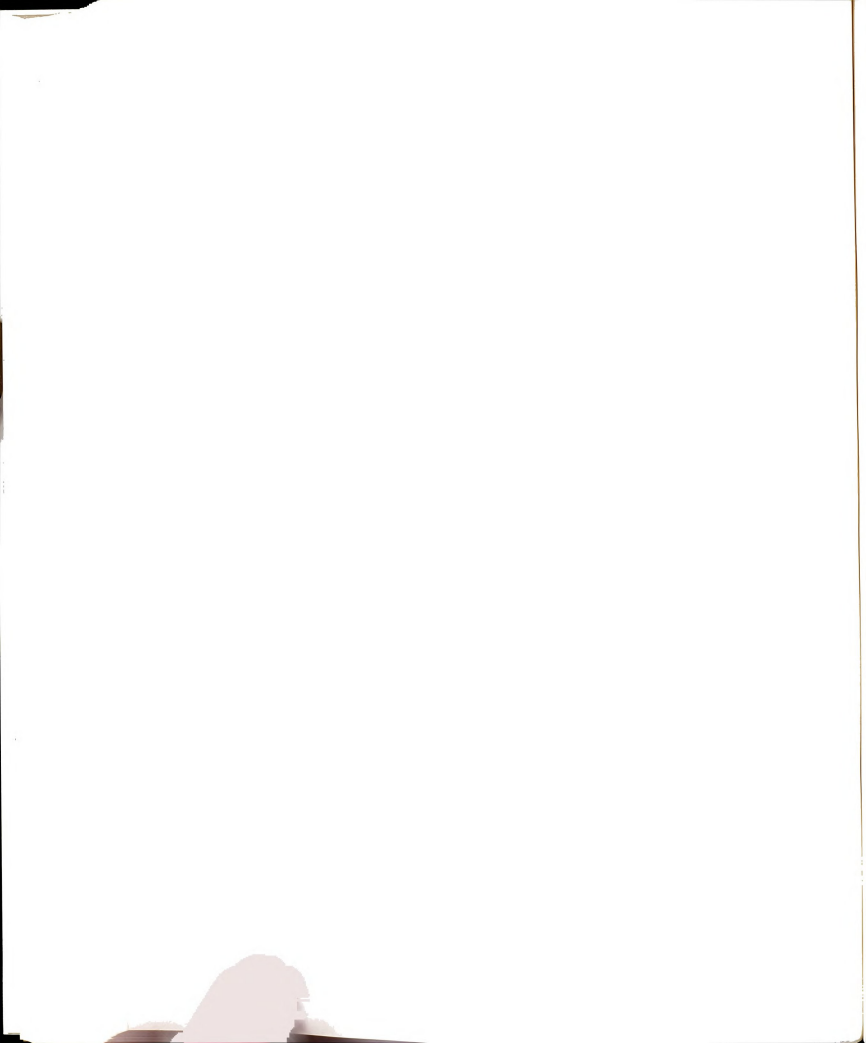
One way to explain the results obtained in this study is to assume that there are three distinct groups of delinquents represented among our subjects. Some delinquent youth are not seriously delinquent and would be likely not to recidivate whatever is done for them ("group one"). Others would be likely to become more and more delinquent in spite of our best efforts ("group three"). A third group has the potential for serious and long-term criminal behavior, but is not beyond helping ("group two"). If this were true, large treatment effects would not be expected, even for the most effective treatment, because only those subjects in group two would benefit from treatment. The size of treatment effects would be determined by the strength of the treatment and by the percentage of subjects who are in group two. This interpretation is supported by the fact that the outcome variable showing the greatest treatment effect in earlier evaluation studies (Redner & Blakely, 1986) was dichotomized official contacts. This meant that effective treatment reduced the number of youth who had official contacts, but not the number of crimes.

It is possible that a moderator effect was occurring. That is, seriousness of the youth's delinquency could have moderated the effect of the intervention on follow-up delinquency, with the intervention reducing delinquency for "less delinquent" youth (as expected under control theory), but increasing it for "more delinquent" youth (as expected by labeling theory). The net effect of the intervention on later delinquency would then be determined by the relative percentage of subjects from each category in the sample. Such moderator effects would

not picked up by our analysis. One way of dealing with them would involve splitting the sample into "more" and "less" delinquent groups, to see whether the causal models are the same for both groups. The problem with this is that the less delinquent group would show very little official delinquency after the single contact at pre. Without any variance on this variable, official delinquency would not be very useful in path models, either as a predictor, a mediator, or an outcome. A better option might be to perform a cluster analysis of subjects in order to split the sample based on profiles of scores, rather than using a single score on official delinquency. This approach is based on the idea that it is not a single variable that determines the effect of the intervention, but a complex pattern of variables. The "moderator" would be considered to be a "type" of subject, rather than a variable.

The next phase of this work will focus on providing an explanation for the effects of experimental treatments. Earlier studies have shown that the more effective treatment conditions utilized child advocacy and behavioral contracting, or an intensive therapeutic relationship intervention model. It will be interesting to elucidate the processes by which these interventions reduced recidivism.

One set of intervention variables that were not examined here were what might be called the "non-specific" factors. These were variables measuring the extent to which the volunteer was involved with, and liked by, the youth and his or her parents. It is possible that these factors were important in producing treatment effects. One piece of evidence that they might have been important is that it was not only the treatment conditions utilizing advocacy and contracting that reduced recidivism in



earlier outcome studies (Davidson, et al., 1987; Redner & Blakely, 1986). The Relationship condition was equally effective. In this condition, interventions like advocacy, contracting, home intervention and school intervention were not taught and were discouraged if they did occur. It is possible that effective interventions were those that fostered empathy in the volunteer, and encouraged involvement with the youth.

APPENDICES

Appendix A

Items and First-Order Clusters for
Self-Report Delinquency and Youth Life Events Scales

Self-Report Delinquency Scale

How often have you:

Minor Theft Cluster

Taken something not belonging to you worth less than $2?
Taken something from a store without paying for it?
Taken things worth less than $50?

B & E Cluster

Gone onto someone's land when he didn't want you there, or without permission?
Gone into a house or building when you were not supposed to be there?
Damaged or messed up something not belonging to you?
Broken into a place and stolen something?

Aggression Cluster

Threatened to hurt someone?
Hurt someone badly enough for him to need bandages or a doctor?
Hit a member of your family (in anger - not horseplay)?
Carried a gun or a knife?
Used or threatened to use a weapon to get something from a person?
Taken something from a person by force?
Beaten up on somebody or fought people physically?

Major Theft Cluster

Taken something not belonging to you worth over $50?
Taken a car without permission of the owner (even if auto returned)?
Bought or gotten something that was stolen by someone else?

Intoxication Cluster

Drunk beer or liquor?
Smoked marijuana?
Sniffed glue or cocaine or taken pills?

(continued)

Appendix A, continued

Skip School Cluster

Skipped class when you were in school?
Skipped a day of school?

Major School Cluster

Been told to bring your parents to school for somnething you did wrong?
Not been allowed to go to school until the superintendent or principal or someone like that told you that you could go again (i.e. being suspended)?
Been sent to the school principal's office for bad behavior in class?

Life Events Items and First Order Clusters

(All questions refer to the last six weeks)

Argue with Parents Cluster

How often do the parent(s) talk to the youth about changing? (never, not very often, sometimes, most of the time, all the time)
How much do the parent(s) and the youth argue about where the youth is going? (never, once a week or less, more than once a week, almost daily, daily)
How much do the parent(s) hassle the youth about the way the youth looks? (never, once a week or less, more than once a week, almost daily, daily)
How much do parent(s) and youth argue about chores? (never, once a week or less, more than once week, almost daily, daily)
How much do the parent(s) and the youth argue about the use of the phone? (never, once a week or less, more than once week, almost daily, daily)
How much do the parents and youth argue about the youth's friends? (never, once a week or less, more than once week, almost daily, daily)
How much do the parent(s) and youth argue about curfew? (never, once a week or less, more than once week, almost daily, daily)
How much do the parents and youth argue in general? (never, once a week or less, more than once week, almost daily, daily)
How necessary is change in the home domain (according to source)? (unimportant, partially important, relevant, central, crucial)
To what extent do the parents hassle the youth about school? (never, once a week or less, more than once week, almost daily, daily)

(continued)

Appendix A-1, continued

Parental Knowledge of Friends Cluster

To what extent do the parents know the specific things the youth does with friends? (not at all, vaguely mention one thing, specifically mention one thing, vaguely mention several things, specifically mention several things)
To what extent do parent(s) know who youth's friends are? (not at all, mentions small proportion of them, mentions half of them, mentions most of them, mentions all of them)
To what extent do the parent(s) know specific things the youth does in free time? (not at all, vaguely mention one thing, specifically mention one thing, vaguely mention several things, specifically mention several things)

Parental Knowledge of School Cluster

To what extent do the parent(s) know what classes the youth takes? (not at all, mention one, mention several, mention most, mention almost all)
How many of the youth's teachers do the parents know of? (none at all, mention one, mention several, mention most, mention almost all)
To what extent do the parent(s) know specific things that the youth does in school? (not at all, vaguely mention one thing, specifically mention one thing, vaguely mention several, specifically mention several)

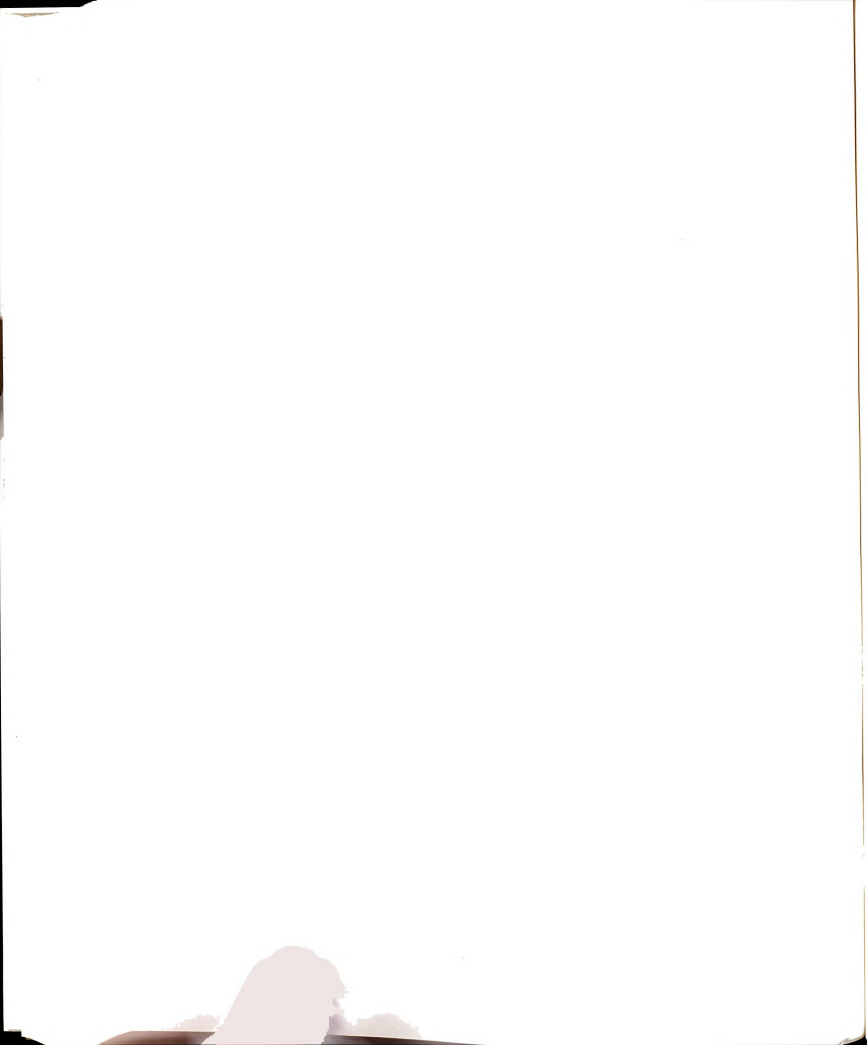
Phys Ed Cluster

To what extent is the youth good at P. E.? (not at all, very little, O.K., pretty good, very good)
To what extent does the youth like P. E.? (not at all, dislikes, says is O.K., likes, mentions as a favorite class)

School Performance Cluster

Is the youth good at particular classes? (no classes, one class, some classes, most classes, all classes)
How many classes does the youth know particular things which are going on? (no classes, one class, some classes, most classes, all classes)
How many academic classes is the youth good at? (no classes, one class, some classes, most classes, all classes)
How many activity classes is the youth good at? (no classes, one class, some classes, most classes, all classes)
To what extent does the youth get passing grades in school? (no classes, one class, some classes, most classes, all classes)

(continued)



Appendix A, continued

School Enjoyment Cluster

How many classes does the youth like? (no classes, one class, some classes, most classes, all classes)
How many classes does the youth dislike? (no classes, one class, some classes, most classes, all classes)
How many teachers does the youth like? (none, mentions one as liked, mentions more than one as liked, mentions one as really liked, mentions more than one as really liked)
How many teachers does the youth dislike? (all, most all, some, one none)

General School Attitude Cluster

To what extent does the youth care about school? (not at all, very little, somewhat, concerned, very concerned)
To what extent is the youth concerned about finishing school? (not at all, very little, somewhat, concerned, very concerned)
In general, what is the youth's attitude toward shcool? (very negative, negative, neutral, positive, very positive)

Skip School Cluster

How often does the youth attend school? (-) (never, once/week or less, more than once/week, almost every day, daily - or the required amount)
How often does the youth skip any classes (on days when youth is in school)? (-) (every day, almost every day, once a week, less than once a week, never)
How often does the youth skip school with friends? (-) (daily, more than once a week, 2-6 times, once, never)

Homework Cluster

How often does the parent(s) help the youth with homework? (never, once a month, once a week, more than once a week, daily)
How often does the youth do homework at home? (never, less than once a week, once a week, more than once a week, almost every day)

(continued)

Appendix A, continued

Time with Friends Cluster

How often does the youth spend time with friends on weekends? (never, one weekend per month or less, a part of almost every weekend, a part of every weekend, most of every weekend)
How often does the youth participate in other spontaneous activities with friends? (never, once a week or less, more than once week, almost every day, daily)
How often does the youth spend time with friends in the afternoons? (never, once a week or less, more than once week, almost every afternoon, every afternoon)
How often does the youth spend time with friends evenings? (never, once, 2-6 times, more than one evening/week, every evening)
How often does the youth spend time at a friend's home? (never, once, 2-6 times, more than once a week, daily)

Party with Friends Cluster

How often does the youth drink with friends? (never, once, 2-6 times, more than once a week, daily)
How often does the youth go to parties with friends? (never, once, twice, 3-4 times, more than 4 times)
How often does the youth smoke dope with friends? (never, not very often, sometimes, most of the time, all the time)

Note. These clusters were derived using confirmatory factor analysis for times 1 through 4, so that they provide the best fit over all four times. The sign "(-)" indicates a negatively scored item. For self-report delinquency items, all items were scored on a four-point scale, with zero=never, 1=once, 2=twice, and 3=three or more times.

Appendix B

Composition and Reliabilities of Second Order Clusters for Self-Report Delinquency and Life Events Measures

Second Order Cluster:	composed of First Order Clusters:
School Involvement (alpha at t1-t4 = .76, .72, .75, .75)	Phys. Ed. (LE) School Performance (LE) School Enjoyment (LE) General School Attitude (LE) Like School Rules (LE) Homework (LE)
Self-Report Delinquency (alpha = .76, .78, .81, .82)	Minor Theft (SRD) B & E (SRD) Aggression (SRD) Major Theft (SRD) Major School Problems (SRD)
Drop Out (alpha = .86, .83, .81, .83)	Skip School (LE) Skip School (SRD) Party with Friends (LE) Intoxication (SRD)
Parental Awareness (alpha = .55, .49, .54, .64)	Parental Knowledge of Friends (LE) Parental Knowledge of School (LE)
Official (alpha = .54, .73, .73, .70)	Court Contacts (archival) Police Contacts (archival)

Note. Second order cluster scores were calculated by taking the mean first order cluster score. Alphas for second order clusters are for times one through four. For first order clusters, SRD = cluster from self-report delinquency measure; LE = cluster from Life Events measure; archival = from archival delinquency measure.

Appendix C

Intervention Scale Items and First Order Clusters

(All questions refer to the last six weeks)

Friend Involvement

How often do the youth's friends spend time with the volunteer and youth? (never, not very often, sometimes, most of the time, all the time).
How often does the volunteer involve the youth's friends in recreation? (never, not very often, sometimes, most of the time, all the time).
How often does the volunteer talk to the youth's friends independently? (never, not very often, sometimes, most of the time, all the time).
How often does the volunteer involve the youth's peers in things? (never, not very often, sometimes, most of the time, all the time).

School Performance

How often does the volunteer monitor the youth's performance in school? (never, less than once a month, once a month, 2-4 times a month, more than 4 times a month).
To what extent is the intervention focused on improving the youth's school performance? (not at all, a very minor part, part of - but not a major focus, one of the major foci, the major focus).
How much do the volunteer and youth talk about school? (never, less than once a month, once a month, 2-4 times a month, more than 4 times a month).
How often does the volunteer talk to parent(s) about school? (never, once, twice, 3-4 times, more than 4 times).
To what extent has the volunteer specified school as a major change area? (not at all, very little, some, considerably, a major focus).
To what extent is the volunteer trying to get the target to go to school more? (not at all, very little, somewhat, considerably, a major focus).
To what extent is the volunteer trying to get the youth to do more homework? (not at all, very little, somewhat, considerably, a major focus).

School Contract

To what extent is the volunteer working on changing the youth's classroom behavior? (not at all, very little, somewhat, considerably, a major focus).
Has the volunteer set up a contract with the youth's school staff? (no, yes).
How often does the volunteer monitor changes the school is supposed to make? (never, less than once a month, once a month, 2-4 times a month, more than 4 times a month).

(continued)

Appendix C, continued

School Visit

How often does the volunteer visit the school? (never, once, twice, 3-4 times, more than 4 times).
How often has the volunteer talked to the school administration? (never, once, twice, 3-4 times, more than 4 times).
How often has the volunteer talked to the school counselors? (never, once, twice, 3-4 times, more than 4 times).
How often has the volunteer talked to the teachers? (never, once, twice, 3-4 times, more than 4 times).

Home Intervene

How often does the volunteer talk with the parent(s) alone about home problems? (never, less than once a month, once a month, 2-4 times a month, more than 4 times a month).
How often does the volunteer talk to the youth about home? (never, not very often, sometimes, most of the time, all the time).
To what extent is the intervention focused on the home? (not at all, a very minor part, part of but not a major focus, one of the major foci, the major focus).
To what extent is the home intervention focused on the youth doing household chores? (not at all, a very minor part, part of but not a major focus, one of the major foci, the major focus).
To what extent is the home intervention focused on improving the youth's attitude? (not at all, a very minor part, part of but not a major focus, one of the major foci, the major focus).
To what extent is the intervention focused on providing the parents information about the youth's comings and goings? (not at all, a very minor part, part of but not a major focus, one of the major foci, the major focus).

(continued)

Appendix C, continued

Home Intervention, continued

How often does the volunteer talk with the parent and youth about home problems? (never, less than once a month, once a month, 2-4 times a month, more than 4 times a month).

How often has the volunteer mediated a family disagreement? (never, once, twice, three times, more than three times).

To what extentdoes the volunteer involve the parents in the planning of the intervention? (never, not very often, sometimes, most of the time, all the time).

To what extent is the home intervention focused on improving the parent's household rules? (not at all, a very minor part, part of but not a major focus, one of the major foci, the major focus).

To what extent is the home intervention focused on having the parents treat the youth more positively? (not at all, a very minor part, part of but not a major focus, one of the major foci, the major focus).

To what extent is the intervention focused on getting the parents and youth to talk more? (not at all, a very minor part, part of but not a major focus, one of the major foci, the major focus).

To what extent has the volunteer specified interpersonal contingencies which need alteration? (mentions nothing, mentions only general things, mentions one specific change, mentions two specific changes, mentions several changes).

Contracting Approach

To what extent has the volunteer specified a contract between the youth and the significant others in his/her life? (mentions nothing, mentions the idea in passing, talks of wanting to do a contract, specified the people, specified the people and behaviors).

To what extent has a contract been used? (mentions nothing, has specific plan, has talked with both parties, has a contract written, has had a contract in operation).

To what extent has the volunteer set up a monitoring system for the contract? (none mentioned, mentions plans to do so, has a specific system ready to implement, has started using one, has started using one and knows of each party's compliance).

To what extent has the volunteer involved the youth and the relevant significant(s) in the contract negotiations? (completely uninvolved - or no contract, minimally involved, somewhat involved, involved in most, involved in everything).

To what extent has the volunteer been instructing the youth and significant others in the methods of contracting? (not at all, very little, somewhat, they have talked about it a fair amount, very actively involves youth in instruction and practice).

(continued)

Appendix C, continued

Advocacy Approach

To what extent has the volunteer specified changes the youth would like made in his/her environment? (mentions nothing, mentions only general theings, mentions one specific change, mentions two specific changes, mentions several changes).

To what extent has the volunteer specified courses of action to facilitate change? (mentions nothing, mentions only vague actions, mentions one specific action, mentions two specific actions, mentions several actions).

To what extent has the volunteer specified individual targets for change? (source mentions nothing, source mentions only categories of individuals, source mentions one specific person, source mentions two specific individuals, source mentions several).

To what extent has the volunteer (with or without the youth) taken specific action to initiate change? (source mentions nothing, source mentions only vague action, source mentions one specific action, source mentions two specific action, source mentions several).

To what extent has the volunteer followed up on change areas? (mentions nothing, mentions only general things, followed up on one specific change, followed up on two specific changes, followed up on several changes).

To what extent did the volunteer involve the youth in the planning and action which has been accomplished? (completely uninvolved, minimally involved, somewhat involved, involved in most, involved in everything).

Appendix D

First Order Scales for Self-Report Delinquency at Follow-Up

Theft Cluster (alphas at t5 & t6 = .81, .74)

Taken something not belonging to you worth less than $2?
Taken something from a store without paying for it?
Taken things worth less than $50?
Taken something not belonging to you worth over $50?
Taken a car without permission of the owner (even if auto returned)?
Bought or gotten something that was stolen by someone else?
Broken into a place and stolen something?

B & E Cluster (alphas at t5 & t6 = .74, .57)

Gone onto someone's land when he didn't want you there, or without permission?
Gone into a house or building when you were not supposed to be there?
Damaged or messed up something not belonging to you?

Aggression Cluster (alphas at t5 & t6 = .79, .69)

Threatened to hurt someone?
Hurt someone badly enough for him to need bandages or a doctor?
Hit a member of your family (in anger - not horseplay)?
Carried a gun or a knife?
Used or threatened to use a weapon to get something from a person?
Taken something from a person by force?
Beaten up on somebody or fought people physically?

Intoxication Cluster (alphas at t5 & t6 = .70, .64)

Drunk beer or liquor?
Smoked marijuana?
Sniffed glue or cocaine or taken pills?

Skip School Cluster (alphas at t5 & t6 = .72, .81)

Skipped class when you were in school?
Skipped a day of school?

(continued)

Appendix D, continued

Major School Cluster (alphas at t5 & t6 = .74, .86)

Been told to bring your parents to school for somnething you did wrong?
Not been allowed to go to school until the superintendent or principal or someone like that told you that you could go again (i.e. being suspended)?
Been sent to the school principal's office for bad behavior in class?

Note. Second order clusters SRD and Drop Out at times 5 and 6 were composed of the following first order scales:

	SRD	Drop Out
(alphas at t5 & t6 =	.75, .69	.68, .54)
	Theft	Intox (SRD)
	B&E	Skip School (SRD)
	Aggression	Party (LD)
	Major School	

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