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ANTI-POVERTY PROGRAMS AND DELINQUENCY

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The effect of anti-poverty efforts on delinquency is explored in the present paper within the context of the in-school Neighborhood Youth Corps, one of the largest, best known, and most favorably received Federal programs of its kind. A control group of eligible applicants who were not admitted to the program was randomly and specifically established for research purposes, thus eliminating the self-selection factor. An extensive analysis of the police contact histories of the control and NYC enrollees disclosed no evidence that working in the program reduced their encounters with the police.

The provision of jobs, training, or work experience is an important component in many anti-poverty programs, frequently being the base around which the entire program is structured and developed. Within this work-oriented milieu the program participants are offered services in the form of counseling, remedial education, and job supervision. The dialogue supporting such programs has increasingly emphasized their contribution toward reducing delinquency and youth crime by inculcating more positive and socially acceptable attitudes and values in the youths and by constructively occupying leisure time through employment activities, thereby reducing the inclination and opportunity of its recipients to engage in behavior which would make them the objects of law enforcement attention. Examples of serious scientific attempts to explore empirically the hypothesis that such programs reduce delinquency are not only isolated but virtually nonexistent in the literature. Because of this, because the anti-poverty program evaluated in the present study was the in-school Neighborhood Youth Corps—one of the largest and best known federally created and subsidized programs, reaching hun-

dreds of thousands of ghetto youths in their natural milieu and at a time when they are highly susceptible to contacts with the police—and because it was possible to establish a control group of unassailable quality, the findings presented in this paper are particularly relevant.

The in-school Neighborhood Youth Corps (NYC) program provides jobs to students 16 through 21 years of age who come from poor families.¹ During the school term these students are permitted to work as many as fifteen hours a week and up to thirty-two hours a week during the summer, at a standard rate of \$1.25 per hour; a majority of the in-school NYC projects are operated (sponsored) by school boards and educational institutions.² At their employment sites the student

¹ The *out-of-school* Neighborhood Youth Corps program provides "full time" employment to youths who have dropped out of school; one of the out-of-school program's main objectives is to encourage the enrollees to return to school in order to complete their high school education.

² These conditions were in effect at the time the present study was conducted. More recently, there have been efforts to lower the entrance age to 14 years and to provide for a *pay scale* rather than a fixed rate of compensation.

"enrollees" are under the direction of a work supervisor and are also assigned NYC counselors who periodically meet with them to discuss their problems, progress in the program, the role and value of education, and the need to complete high school.

THE GROUPS STUDIED

Exploration of the hypothesis that participation in anti-poverty programs reduces delinquency is explored here by an analysis of the police records of random samples of Negro youths interviewed in the summer of 1966 who, as of the time of sample selection (1) were actively enrolled and working in the in-school Neighborhood Youth Corps program operated by the Public Board of Education in Cincinnati—hereafter referred to as *year-round* enrollees, (2) were about to enter the Cincinnati NYC program for the first time—hereafter referred to as *summer-only* enrollees, and (3) had applied for enrollment and been found eligible but were not accepted into the program in order that they might be used as a control group. The larger study of which the police contact analysis is but a part was a longitudinal survey that involved interviewing NYC and control youths three times over a one-year period, with the first phase of interviewing conducted at the start of the 1966 summer and the last wave in March 1967, a month after the collection of the police contact data. The fact that the police record analysis is based upon those youths who were originally interviewed rather than upon the entire sample of names selected from the NYC records is not a serious limitation because at least four-fifths of the youths in each sample of names selected were in fact interviewed; in addition, virtually all of the non-respondents were un-interviewed because they could not be located within the time available rather than because of refusals or other more substantial factors associated with self-selection.

On June 2, 1966, two-hundred names stratified by sex within school were randomly selected from the active NYC files containing the names of all youths (723) who were enrolled in the program at that point, of which 167 (84%) were interviewed. Because this group had worked in the Neighborhood Youth Corps for at least some part of that school year, they are referred to, for convenience and in order to distinguish them from the second experimental sample to be described below, as *year-round* enrollees. This does not mean that, as of the

time their names were drawn from the active files, they were enrolled in the program for an entire school year, school term or for a total of one year overall. The *summer-only* group consisted of youths who were to enter the NYC for the first time in the 1966 summer program and who were to be terminated at its conclusion, because of a reduction in the number of authorized jobs which would be available in the fall of 1967—this in contrast to the year-round youths who were not summarily terminated from the NYC at a particular point but rather were permitted to remain in the program and were subject to voluntary and involuntary departure for any number of reasons over a long period of time; in fact, fully half of this group were still working in the NYC when the police record data were collected in February 1967. The summer-only enrollees, on the other hand, were exposed to the NYC for a maximum period of 8 to 11 weeks, depending upon their summer assignment; many of them had assignments that lasted 6 to 8 weeks or may have left the project before its completion. While the utilization of two treatment groups, the year-round and summer-only enrollees, with which to compare the controls has certain advantages which will become apparent in the analysis, one which may be mentioned at this point is the variation in the length of program participation which they introduce: the summer-only youths were in the NYC for an average of 8 weeks, while the average length of participation for the year-round enrollees, at the time of the police record determination, was 14 months. Findings based upon the year-round sample may suggest that there is a critical minimal period of program exposure which must obtain before the program's effect on reducing delinquency becomes manifest and that such exposure is not satisfied by a summer's experience in the program.

The summer-only and control groups were established from the 351 names of youths who, as of June 1966, were on the "waiting list" for entrance into the summer program; that is, there were 351 youths in Cincinnati who had applied for admission to the NYC prior to June 1966, had been screened and found eligible, and who had never worked in the program before. These "waiting list" names were arranged alphabetically and randomly assigned to the summer-only or control groups. This procedure led to the creation of an eligible universe of 136 summer-only names (130 of whom were interviewed) and 173 control names

(136 of whom were interviewed). As part of their cooperation and participation in the study, the NYC administrators agreed not to accept any of the controls into the program until all field phases of the investigation were completed, and to terminate all new summer-only (SO) enrollees at the end of the 1966 summer program, this latter group also to be excluded from re-entering until all field phases were completed. It is singularly important to note that the control and summer-only groups were experimentally created specifically for research purposes and were established by a chance termination of *which* "waiting list" youths were to be admitted to the program. This random assignment of youths to the control or treatment (SO) group eliminated bias associated with self-selection, a serious shortcoming in some recent anti-poverty studies which have utilized "no shows"—applicants who do not enroll in programs after being accepted—as a control group.³ Those who *reject* program participation are likely to be considerably different from enrollees, particularly with respect to motivation and associated variables that may be criteria for measuring program effectiveness. No such compromise was required to identify and utilize a control group in the present study in Cincinnati: their exclusion from the Neighborhood Youth Corps was determined by fortuitous circumstances in which the youths themselves played no part and by a methodologically unassailable procedure which could not have resulted in systematic differences between the control and summer-only, or indeed year-round (YR) enrollees.⁴

Because almost all of the youths in the samples were Negroes, cases of white youths were excluded from the study. Thus, the number of Negro youths who were interviewed in each sample and who therefore constitute the groups on which the police record analysis is based is 138 year-round enrollees (82 males, 56 females), 109 summer-only

enrollees (50 males, 59 females), and 119 controls (54 males, 65 females).

SOURCE OF POLICE CONTACT DATA

Identification on all of the youths originally interviewed and accompanying data collection forms were submitted to law enforcement authorities in Cincinnati in order to ascertain whether they had any juvenile or adult record of police contacts, and if so, to record the date and charges entered against the youths for each police contact. After the Master Record File—the central register from which juvenile police contacts were abstracted—had been inspected for this purpose, the forms were sent to the Adult Division of the police department, since at the time of offense record determination many of the "youths" were officially adults, and all *criminal* charges against them were entered on the forms. The Master Record File in the Juvenile Division contained all recorded police contacts which a youth had with the law, regardless of the precinct of origination. Thus, despite the nature of the police contact—of its severity or triviality, and of the location of the offense and the original jurisdiction of police administration over the case—if the police contact was recorded at any level it was routinely processed and entered into the juvenile Master Record File. Furthermore, the Master Record File had been maintained for a sufficiently long period such that it was in operation before any of the youths in the present study were seven years of age, so that it contained all of the ever-recorded delinquencies committed by the youths. Thus, the complete history of recorded juvenile, as well as adult, police contacts was collected. Because a majority of the youths in each sample were 16 years old when they enrolled in or applied to the program, and because approximately nine-tenths of them were between 16 and 18 years of age at the time the police contact data were collected, youths with offense records will frequently be referred to as delinquents, although it is recognized that technically individuals who incur their first police contact after juvenile status are considered criminals.

THE FINDINGS

Delinquency patterns and prevalence are primarily an expression of, or at least largely predictable from, socioeconomic status, sex, race and age; because all of the subjects in the study were

³ PETERSON, AN EVALUATION OF THE CONCEPT OF TRAINEE CAMPS FOR UNEMPLOYED YOUTH.

⁴ Despite the random distribution of waiting list youths into control and summer-only subjects, this procedure did not in any way affect the *total* number of youths who worked in the program, since the 1966 Cincinnati summer program quota could not accommodate all eligible applicants. Thus, while the specific individuals on the waiting list who were admitted to the program were guided and determined by research considerations, the *sum total* of services offered in the 1966 summer program was completely unaffected by the study design.

Negroes of approximately the same age who were living in poverty and because the police record analysis was of course performed separately for males and females, in both experimental groups the distribution and *interaction* of these and other variables associated with Negro poverty were very closely matched with that of the controls. The serious limitation in the summer-only sample in the present analysis is the very brief period of their participation in the Neighborhood Youth Corps, thus constituting a severe restriction on one of the most important "follow-up" police contact exposure periods—the reader will recall that the average duration in the program for the summer-only group was 8 weeks, while that for the year-round enrollees at the time that the delinquency data were collected was approximately 14 months. Obviously, the greater the interval, the more likely that offenses will be committed which result in officially reported police contacts. This variable, however, has been "equated" in the year-round and control groups through natural conditions resulting from the fact that these two groups of youths tended to apply for admission and to enroll in the program over essentially the same time period, and were therefore characterized by the same police contact exposure intervals, e.g., the average length of NYC participation for the year-round youths, at the time the offense data were collected, was 13.6 months while the functionally and analytically equivalent follow-up period for the controls—the interval between their application to the program and the date the offense records were checked—was 12.8 months.

The analysis which follows is based upon the identification of carefully constructed and meaningful time periods in relation to program enrollment and program participation for the year-round and summer-only youths, and the date of application for the controls.⁵ Thus, the police contact profile for the experimental youths has been delineated, inspected, and calculated for the periods *before* they enrolled in the Neighborhood Youth Corps, *while* they were working in the program, and from the point of *enrollment to the date of the offense record check*. The second time period mentioned—*while* the youths were actively enrolled in the program—constitutes perhaps the most relevant temporal base for testing the relationship between NYC participation and delinquency—whereas the last period alluded to com-

bines the *in-program* (*while* the youths were active in the NYC) with the *after program* period (the interval between termination from the NYC and the date of the offense record check) in order to ascertain whether there was any *overall* change in the enrollees' delinquency proneness prior to entering the NYC as contrasted with that which obtained from the time they joined until the offense record data were collected. In the case of the controls, the temporal frame of reference was dichotomized into *before application* (equivalent to the pre-enrollment period of the experimental youths) and *after application*, i.e., from the point of application to the date of the offense record check (approximating the interval between enrollment and the offense record check for both experimental groups and equivalent to the time *while* the YR enrollees were working in the program). The terms "prior" or "previous" will refer to the period before enrollment or application, and the term "subsequent" to the period after enrollment or application to the date of the offense record check. With the preceding comments by way of introduction, the findings may now be presented.

Prior to enrollment/application among the males, a significantly⁶ larger proportion of the controls (63%) than of the experimental samples (38% YR, 34% SO) had committed serious offenses, the latter defined as acts against the person or property violations and hereafter referred to interchangeably as felonies, personal/property offenses, or serious offenses; 37% of the year-round and 39% of the control youths had incurred minor or non-serious police contacts of the misdemeanor variety, while 49% YR, 54% SO and two-thirds of the controls had an offense record of some kind. Among the females in this time period, twice as many controls (19%) as year-round youths (7%), and the same proportion of summer-only youths (19%) had committed serious offenses, twice as many controls (23%) as year-round or summer-only enrollees (10-11%) had incurred minor police contacts, and 37% of the control females had an offense record before applying to the program, compared with only 16% of the year-round and one-fifth of the summer-only females before enrolling, significant differences which place the control females at a "disadvantage" in relation to the year-round females.

Moreover, the average number of felonies,

⁵ The dates on which the year-round youths *applied* to the program were not available.

⁶ The term "significant" refers to statistically significant differences in proportions at at least .05 level of confidence.

misdemeanors, or any offenses committed by all of the controls before application and by all of the experimental youths before enrollment was either virtually identical, or greater among the controls than the enrollees; the same was true concerning the number of offenses of a given typology committed by those who had such charges against them. For example, among all males, the mean number of felony police contacts incurred by the controls before application to the NYC was 1.2, compared with 0.7 for each of the year-round and summer-only groups. Among the male youths who were charged with felonies before enrollment/application to the program, the average number of such charges for this subgroup of control youths was 1.9 compared with 2.0 for the year-round enrollees and 2.2 for the summer-only enrollees. Similarly, the mean number of total police contacts prior to application was 3.3 for the male controls, 3.0 for the male year-round youths, and 3.5 for the male summer-only enrollees. The same trend of results obtained for the females and for the distribution of misdemeanors in the "before" period. Furthermore, because differences in the average number of felony charges, misdemeanor charges, or any police contacts incurred by the total group of control and experimental youths, as well as by those specific subgroups of youths who committed the respective kinds of offenses, generally approached zero and rarely exceeded .3 in all of the tables generated for the delinquency analysis, continued reference to the number of acts or offenses will be avoided, since these differences could hardly be more inconsequential.

Thus, both the control males and females in Cincinnati were noticeably more delinquent in their police contact typology in the pre-application period than were the corresponding experimental youths, and this difference was greatest between the controls and the year-round sample—the experimental group which offered the most critical opportunity for testing the effect of NYC participation on delinquency and youth crime.

While they were in the NYC the proportion of year-round males who incurred police contacts represented a 33% reduction from the proportion who had an offense record prior to joining the program (from 49% to 33%), but the proportion of control males who committed offenses after applying for admission—encompassing a follow-up exposure period which was equivalent to that of the year-round males—was 39% less than the proportion who were offenders prior to trying to join the

NYC (from 67% to 41%). Similarly, the male YRs charged with serious offenses decreased from 38% before enrollment to 17% while in the program—a 55% reduction in those who committed felonies while working in the NYC—whereas the proportion of control males with offenses against the person/property decreased by 68% (from 63% before to 20% after application). Thus, despite the fact that the control males were more serious offenders prior to application than were the year-round males prior to enrollment, the latter were equally as likely to commit serious offenses while they were active in the NYC program as were the control males after application; nor were there any significant differences in the proportion of the year-round versus control males in this same time period who committed misdemeanors or who had any offenses charged to their record. *Accordingly, there is no evidence that NYC participation reduced delinquency among its enrollees while they were working in the program, a somewhat unexpected finding if for no reason other than that the program utilized approximately 1,000 hours of what would otherwise have been leisure time and therefore opportunity for misbehavior.* Nor was program participation related to delinquency prevention after termination from the NYC: the YR males had worked in the NYC fully six times as long as the SO males (11.5 months versus 8.3 weeks), and the exposure period after termination was shorter for the year-round (3.6 months) than for the SO males (5.3 months); yet, the proportion of both groups who committed offenses after leaving the program was the same (23% YR and 20% SO). In the interval between enrollment/application and the offense record check—which was twice as long for the controls (13 months) as for the summer-only enrollees (7 months)—there were no significant differences between the male controls and either of the experimental samples in the proportion of youths who were charged with serious, minor, or any offenses; indeed, with one exception, the proportions were virtually identical: 21% YR, 20% C and 12% SO were charged with felonies, 29% YR, 28% C and 26% SO with misdemeanors, and 39% YR, 41% C and 36% SO with any offenses.

The above analysis is based upon the gross effects of the program in reducing anti-social behavior without identifying specific subgroups of youths with records and determining whether these individuals benefited from program participation as reflected in their police contact profiles in appropriate follow-up periods. The crucial test of the

NYC's effect on youth crime is its ability to insulate those youths who were delinquent prior to enrollment from continuing to commit offenses *while* they are working in the program, as well as from the point of entrance to the program throughout the subsequent exposure period (*i.e.*, up to the point of the offense record check)—thus yielding the program's *net* rather than *gross* effects on delinquency; exclusive focus on the latter approach has serious shortcomings, not noted by the author of a recent study of the effects of the out-of-school Neighborhood Youth Corps program on police contacts, which purported to demonstrate that *that* program reduced delinquency among the female enrollees.⁷

Among the youths who had serious police contacts prior to enrollment/application, the proportion of year-round males who committed serious

⁷ WALTHER & MAGNUSSON, A RETROSPECTIVE STUDY OF THE EFFECTIVENESS OF OUT-OF-SCHOOL NEIGHBORHOOD YOUTH CORPS PROGRAMS IN FOUR URBAN SITES 116-124. In addition, these authors utilize a highly questionable approach in inferring program effects on youth crime, one based upon changes in the *number* of police contacts and changes in the proportion of total police contacts which were serious. For example, Walther and Magnusson report that before application to the program the 115 experimental youths had a total of 294 police contacts compared with only 15 police contacts after application; these figures compare with 250 police contacts incurred by the 115 controls before application and 23 police contacts after application, and apparently take some consolation in the "greater" reduction in the police contacts of the enrollees. Stated somewhat differently, these figures indicate that the experimental youths were charged with an average of 2.6 police contacts before applying to the NYC and 0.1 after application; in the same time periods the average number of police contacts for the controls went from 2.2 to 0.2. Similarly, the authors make much of the "finding" that before application 49% of all police contacts incurred by the enrollees were serious ones (143 out of 294) and that after application only 20% (3 out of 15) were. By contrast, so the authors reason, before application 38% of the total police charges against the controls were serious ones (96 out of 250), which increased to 48% after application (11 out of 23). Thus, Walther and Magnusson infer positive program effect on the basis that the proportion of serious charges in relation to total police contacts decreased from 49% to 20% among the NYC participants and increased from 38% to 48% among the controls. This approach is open to serious criticism on the basis of its logic and meaningfulness alone, not to mention that by using a measure of central tendency, the same figures reveal that the average number of serious offenses committed by the experimental youths before application was 1.2 compared with 0.0 after application, while among the controls the mean reduction in serious police contacts was from 0.8 to 0.1. What these statistics on serious police contacts suggest much more strongly than program effect is that after applying to the out-of-school Neighborhood Youth Corps virtually none of the youths in either group were charged with serious offenses.

offenses while they were working in the NYC was identical to that of the male controls charged with offenses against person/property subsequent to application (one-quarter). Similarly, among those with misdemeanor offenses or any record previously, there were no significant differences in the proportion of year-round versus control males who had minor or any police contacts charged against them *while* in the NYC and *after* application, respectively. Finally, holding constant police contact typology prior to enrollment/application, there were no significant differences subsequently between the control and the experimental males: (1) of those with serious previous offenses, 29% YR, 24% SO and 25% C incurred serious police contacts subsequently, (2) of those with minor offenses previously, 37% YR, 33% SO and 35% C continued to commit such offenses subsequently, and (3) of those with any previous offense record, 48% YR, 48% SO and 50% C were offenders subsequently. As indicated earlier, the difference in the *number* of police contacts which characterized the individual samples was so small that a routine presentation of this information is unwarranted. Illustrative of this point is that among those with felony charges prior to enrollment/application, the average number of serious charges incurred subsequently was 0.3 for the year-round, 0.3 for the controls and 0.4 for the summer-only males; similarly, disregarding previous records, the average number of felonies committed by those charged with such acts in the subsequent period was 1.3 for the year-round, 1.3 for the controls and 1.7 for the summer-only males; this same pattern of differences in the number of police contacts approaching zero obtained for the remaining typological subgroups (number of misdemeanors and number of any police contacts) of youths.

Up to this point the analysis has been concerned with whether NYC participation tended to insulate youths who prior to enrollment had committed serious offenses from continuing to do so once they started to work in the program, those who were minor offenders previously from continuing to commit or engage in misdemeanor behavior, and youths who had known records of previous contacts from incurring subsequent police contacts. Another related aspect of this analysis, however, is whether NYC participation prevented youths who prior to enrollment had no serious charges against them from committing felonies after they entered the program, as well as those who had no previous misdemeanors from incurring

minor police contacts subsequently. On both of these criteria, NYC participation continued to demonstrate no effect on reducing criminality: of the 51 year-round males who had no felony charges prior to enrollment, 12% were charged with serious offenses while they were working in the NYC, while in the functionally equivalent time period 10% of the control males who had no serious offense records prior to application acquired one after application; similarly, 19% of the year-round and 23% of the control males who had no misdemeanor police contacts before enrollment/application were charged with minor offenses while in the NYC and after they applied to the program. Moreover, when the interval between enrollment and the date of the offense record check is used for the enrollees—a period directly comparable with the subsequent time period for the controls—the proportion of the year-round males who committed their first serious offense during this time was 16% (12% did so in the shorter exposure period *while* in the program) and the proportion who committed their first minor offense was 25% (19% did so *while* in the program), compared with the same statistics reported above for the control males of 10% and 23% who, after application to the program, committed their first serious and minor offenses, respectively.

Of perhaps equal importance as the indices of "delinquency reduction" utilized above in studying the relationship between NYC participation and police contacts is the program's effect on discouraging youths who were *non-delinquent* prior to enrolling from *becoming* delinquent after entering. That is, it may be more reasonable to expect that NYC participation would be more successful in *preventing* delinquency than in *reducing* it. While it is obviously a less demanding and more modest task to prevent youths who have not become the objects of police action and attention from doing so than it is to discourage already delinquent youths from persisting in their misconduct, the former accomplishment would nonetheless be substantial. Unfortunately, however, there was no evidence that this occurred as a result of the Neighborhood Youth Corps: of the year-round males without an offense record prior to enrollment, 24% acquired one, *i.e.*, *became* delinquent *while* working in the NYC, the very same proportion as the previously non-delinquent control males who became delinquent after applying to the program. With respect to the previously non-delinquent females, 98% of the year-round enrollees continued to be non-

delinquent while in the NYC as did 98% of the control females after application. Finally, among both males and females who were non-delinquent prior to enrollment/application, there were no significant differences in the proportion who became delinquent subsequently: among the males, one-third YR (31%), 24% C and 22% SO did so, while among the females less than 3% of any sample subsequently became delinquent.

THE DETROIT DATA

An analysis of the police records similar to the above was performed for the same typology of random samples of interviewed Negroes (after excluding a small number of white cases) who were enrolled in and applied to the Detroit in-public school Neighborhood Youth Corps program: 161 year-round enrollees (51 males, 110 females), 239 summer-only enrollees (132 males, 107 females),⁸ and 124 controls (38 males, 86 females). There are three reasons that less confidence should be placed in the Detroit than in the Cincinnati data, two of which deal with the control group and the other with the Master Record File, discussed below.

First, because a sample of financially eligible applicants not admitted to the NYC was not available in Detroit, applicants who, prior to the date of sample selection in the summer of 1966, had been rejected because of over-income were utilized as the control subjects. It was desirable to use the smallest possible over-income cutoff point that would identify an adequate sample size of controls in order to minimize serious poverty status differences between them and the experimental (YR and SO) youths, who of course had met the financial eligibility criteria. A family over-income of \$1,500 as the cutoff point yielded 245 names of applicants who were utilized as the control sample in Detroit. While the control group in Detroit represents a slight compromise, the average family income by which they exceeded the financial eligibility requirements of the Neighborhood Youth Corps was only \$648, confirming that the financial background of the two experimental groups—year-round and summer-only enrollees—was not substantially less than that of the control group in

⁸ The summer-only sample consisted of Negroes who worked in the 1966 summer program operated by Total Action against Poverty, rather than by the Detroit Public Board of Education, because the latter sponsor did not anticipate a reduction in their Fall 1967 quota which would make it necessary to terminate a substantial number of the enrollees who worked in the NYC during their 1966 summer program.

Detroit. Family income differentials would only be serious if they were such as to identify distinct social class categories, because of the known and documented class-related variation in behavior patterns. It is submitted that discrepancies in financial background of what amounts to a few hundred dollars annually are not likely to cause or account for differentials in the present criterion and that, therefore, no injury has been done by accepting the minimally over-income Detroit youths as valid controls. It would indeed be difficult to argue, for example, that the Detroit controls from 6-member households whose annual family income exceeded the eligibility requirement by \$673 were, in their behavior, attitudes, motivation, values, etc., discernably different and presumably "better" than those from even less fortunate families. Even when the average family over-income of the Detroit controls was related to size of household, it never reached as much as \$1,000.

Secondly, out of the population of 245 controls, only 140, or 57%, were interviewed.⁹ While the completion rate for the Detroit control group is admittedly low, it should be noted that it represents the entire eligible universe rather than a sample proper selected from a larger population; thus the interview-to-universe ratio is quite high for the controls in Detroit: 1 out of less than every 2 controls in the population was interviewed. In addition, almost without exception the reason for not obtaining interviews with the Detroit controls was that they could not be located in time or had moved and were therefore physically inaccessible—reasons which, a priori, would not distinguish the un-interviewed controls from those who were. Thus, despite a low completion rate, a high interview/population ratio and the absence of self-selection factors served to maximize the representativeness of those controls in Detroit who were interviewed.

Finally, the charges listed in the juvenile Master Record File in Detroit did not contain all known recorded police contacts but only those—presumably representing the more serious offenses—which were filtered through the individual police precincts after they had been recorded in the latter's record. Thus, in Detroit all of the charges recorded at the precinct level, for one reason or another, were not routinely made known to and incorporated into the Master Record File, which was the source of data collection in the present study. How-

⁹ All of the eligible summer-only enrollees and 70% of the sample of year-round names selected were interviewed.

ever, the structure of the present analysis is less concerned with the absolute number of police contacts or delinquency typology than it is with the comparative profile of police contacts among the controls and experimental youths, to which the same procedural limitations were applicable. Thus, since there is no reason to believe that the under-recording of the number of juvenile police contacts and their typology is not randomly distributed among the experimental and control groups, *i.e.*, since there is no systematic bias in recording juvenile police contacts among the youth samples, the utilization of this data in Detroit is not viewed as problematic. With the preceding remarks by way of introduction and qualification, the Detroit findings may now be summarized below.

Because the number of youths in each group type in Detroit who had a known offense record was initially restricted because of this last mentioned procedural consideration, segmentation of data—holding constant offense status and typology prior to enrollment/application in order to measure change in subsequent periods—was limited. *The gross effects in Detroit, however, are the same as those in Cincinnati, revealing no effect of program participation on reducing or preventing delinquency.* Among the males, prior to enrolling in the Neighborhood Youth Corps 17% YR, 21% SO and 21% C had been charged with felonies, 2% YR, 10% SO and 18% C with misdemeanors, and 17% YR, 24% SO and 29% C had an offense record. In other words, in the pre-enrollment/application period the controls were equally as, if not more, delinquent than the experimental youths. Yet, the proportion of control males who committed felonies (11%), misdemeanors (5%) or any offenses (13%) after application was no different from that of the year-round males who committed the same offenses while working in the NYC, or from the proportion of summer-only males who incurred this typology of offenses after enrolling in the program.

Among the females in Detroit, there were no significant differences in the proportion who previously were charged with serious offenses (7% YR, 17% SO, 9% C), with misdemeanors (5% YR, 18% SO, 7% C) or with any offenses (10% YR, 26% SO, 14% C). Nor were there any salient differences in the proportion of controls who subsequently committed felonies (4%), misdemeanors (none) or any offenses (4%) compared with year-round females who were charged with felonies (2%), misdemeanors (2%), or any offenses (4%)

while working in the NYC, or compared with summer-only females (5% felonies, 8% misdemeanors, 11% any offenses) after entering the program. Among both male and female youths in Detroit who were non-delinquent prior to enrollment/application, almost all of them continued to remain free of an offense record subsequently: among the males, 92% YR *while* in the program and in the entire subsequent period, 93% controls and 92% of the summer-only after joining the program did so; and among the females 97% *while* in the program, 99% controls and 90% of the summer-only after joining the program continued to remain non-delinquent.

SUMMARY

Separate analyses of the police records of year-round and summer-only enrollees who worked in the in-school Neighborhood Youth Corps programs in Cincinnati and Detroit compared with those of control youths who applied to the program revealed that NYC participation, among both males and females, was unrelated to delinquency

prevention or reduction. Examination of the gross and net effects of program participation disclosed no evidence that working in the program made enrollees with a previous offense record less likely to continue to commit offenses while they were working in the program, in any way had a positive effect on particular types of offenders, or reduced overall the number of police contacts or specific kinds of offensive behavior. Nor, among enrollees who had no previous offense record prior to enrollment, did the program dissuade them from entering the ranks of delinquency more so than was the case with the controls in the absence of program participation. In neither city was there any indication that NYC participation had an effect on reducing criminality on the part of enrollees while the youths were working in the program or after they left it.

Assuming that police contacts are a valid index of variation in illegal behavior, then the putative importance of anti-poverty programs that consist largely of the creation of work opportunities in reducing criminality among juveniles and young people may be more illusive than real.