RECIDIVISM RATES OF DIVERTED JUVENILE OFFENDERS*

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This paper reviews a project that was designed to evaluate a California law enforcement program to divert juveniles away from the juvenile justice system. The evaluated juvenile diversion program was run by the Juvenile Bureau of a large law enforcement department in California, using funds granted by the California Youth Authority, the state's office of juvenile corrections. The program emphasized diversion with referral to local, community-based counseling agencies, as opposed to simple diversion with no further action taken.

Probably the most distinctive component of the evaluated diversion program was the allocation of a portion of the grant monies for purchase of the services provided to referred offenders. The purchase of service contracts provided that for each diversion client served, the agency would receive a $50 fee for providing a minimum of 6 sessions of service, and another $150 if the client was not re-arrested within a 6-month period following the referral arrest. The intention was to motivate the agencies to orient their efforts toward keeping diversion clients out of trouble with the law. Purchase of service contracts did in fact lead to the provision of increased service to referred clients. Records from agency files showed that 100% of the subjects referred with purchase of service were actually contacted personally at least once. By contrast, only about 80% of the subjects referred without purchase of service were contacted personally.

The primary objective of the evaluation was to determine which of four possible arrest dispositions had the most beneficial impact on subsequent offense behavior. The four dispositions were 1)outright release--that is,

simple diversion without referral for additional services; 2)referral without purchase of service; 3)referral with purchase of service; and 4)normal petitioning through juvenile court, without detention.

There were two evaluation measures used to assess the impact of the four experimental conditions on the subsequent offense behavior of the project juveniles. These measures included 1)official re-arrest data, and 2)a self-reported index of involvement in delinquent activities. Re-arrest data were retrieved using a central, County-wide repository of juvenile arrest reports. The self-reported delinquency indices were obtained in the course of two waves of personal interviews with the juvenile offenders within the 18-month period following the referral arrest. The indices consisted of 18 behavior items for which a juvenile could be arrested. These included a broad range of status offenses, "victimless" offenses, and both minor and serious offenses against persons and property. Respondents were asked, in a modified card-sort technique, to indicate which if any of these things they had done subsequent to the arrest for which they entered the subject pool.

The project presented an opportunity to speak to two theoretical orientations much debated by social scientists and justice systems practitioners alike. These are 1)labeling or societal reaction theory, which emphasizes the role of official institutions in inadvertently encouraging illicit behavior, and 2)deterrence theory,an approach that examines the efficacy of official penalties in deterring antisocial behavior. These two orientations appear in many ways antithetical to each other. Labeling theory would imply that the more involvement delinquents have with agents of social control, the more likely they are to assume deviant identities, and to get into more trouble as a result. Deterrence theory would imply that the actions of social control agents penalizing deviant behavior are likely to deter individuals from further wrongdoing. Actually, these two orientations agree on a fundamental point, that social sanctions may influence the subsequent behavior of sanc-

tioned individuals. Proponents would tend to differ only on whether this influence tends to encourage or discourage further misbehavior. The commonality of these theories allowed us to address them both.

Due to the enlightened cooperation of the police administrators in charge of the program, it was possible to assign project offenders to the four treatment conditions on a random basis. The procedures used to accomplish this were simple. Except for being instructed to try to include moderately serious offenders into the referrable pool, juvenile officers were allowed to select offenders for the referrable pool using their normal discretionary criteria. After selection to the pool, offenders were assigned to disposition categories by the juvenile bureau commanders, who used lists of randomly generated dispositions. Because of delays in processing the purchase of service contracts through County bureaucracy, assignment of subjects to the refer with purchase condition began late. As a result, fewer subjects were assigned to this condition. At the end of the subject assignment period, 306 offenders had been randomly assigned to the four treatment conditions.

Halfway through the subject assignment period, evidence concerning possible selection bias was compiled from both police records and from informal conversations with station officers. This evidence suggested that in fact, officers were assigning some subjects to disposition on a non-random basis. The station personnel were reprimanded and forcefully re-instructed to adhere to the randomization procedures. After subject assignment was completed, subjects in the four conditions were compared to determine whether officers had succeeded in entering significant bias into the assignment process. Subjects were compared on age, sex, ethnicity, nature of referral offense and presence or absence of prior record. Although the differences across disposition are not significant with respect to most of these variables,

there is a statistically significant difference among groups on prior record. Officers over-assigned subjects with prior records to the most serious disposition, court petition. Fortunately, it was possible to adjust re-arrest scores for this bias. The adjustment for prior record will be discussed below.

FINDINGS

The earliest re-arrest data were collected and tabulated for a period of six months from the date of each subject's referral arrest. These six-month re-arrest data were examined for differences across disposition with respect to both simple recidivism, that is, the number of recidivists within the 6-month time period regardless of number of re-arrests, and with respect to multiple recidivism, which is the number of recidivists who were re-arrested more than once. These data are presented in Table 1. At six months after the referral arrest, 36% of the cohort of 306 subjects had been re-arrested at least once. The simple rates for each disposition were: counsel and release, 28%; refer without purchase, 32%; refer with purchase, 35%; and court petition, 48%. Although there was a tendency for more subjects to recidivate in the more serious dispositions, the trend was not statistically significant at the .05 level.

When multiple recidivism was calculated for the four disposition conditions, ignoring prior record, results were obtained which picked up and magnified the pattern suggested in the simple recidivism data. The proportion of recidivists re-arrested more than once across dispositions were: counsel and release, 6%; refer without purchase, 16%; refer with purchase, 20%; and court petition, 29%. These differences in proportions of re-arrested recidivists are statistically significant at the .05 level. The impact of police disposition appears more pronounced for multiple recidivists than for

simple recidivists, although in both cases the release condition shows the lowest re-arrest rates, the court petition condition shows the highest rates, and the two referral conditions show rates intermediate between those two.

Later re-arrest and self-reported delinquency data are now available for a period of about 15 months from the date of the referral arrest. At this later point, indicators of both simple and multiple recidivism show the earlier pattern that appeared most clearly in the figures for multiple recidivism. These data are summarized in Table 2. There are now statistically significant differences in simple as well as multiple recidivism. The rates are: for counsel and release, 37%; refer without purchase, 45%; refer with purchase, 58%; and court petition, 62%. Again, multiple recidivism reflects the same pattern of increasing re-arrests for the referral and court conditions. The proportions are: for counsel and release, 16%; refer without purchase, 27%; refer with purchase, 40%; and court petition, 41%.

The comparison of simple and multiple recidivism for both short-tern and long-term follow-up suggests that the impact of differences in police handling is manifest earlier in multiple recidivism than it is in simple recidivism figures. Evaluators may be well advised to select indicators of impact carefully, according to whether program effects are to be measured in the short or long term. In particular, we have found that multiple recidivism appears to be a more sensitive short-term indicator of the impact of police disposition decisions on juvenile re-arrest rates.

None of the figures presented so far have taken into account the likelihood that offenders with prior records of arrest were over-assigned to the most serious disposition, due to the deliberate non-adherence of some police referral officers to the random assignment procedures. The dimensions of this problem are described in Table 3, in which re-arrest means and fre-

quencies are presented cross-classified by disposition and the presence or absence of a prior record of arrest. A chi-square value significant at the .01 level indicates that subject assignment was biased so that prior offenders were over-represented in the court petition condition. The row means, describing re-arrest levels regardless of disposition, reveal that subjects with priors are, indeed, re-arrested twice as often.

When re-arrest patterns are expressed in terms of means rather than proportions, the pattern across dispositions seen so far remains the same. The pattern within categories of priors is slightly altered, especially for subjects with no priors. In both rows released subjects remain easily the least re-arrested, and subjects referred with purchase of service are re-arrested most often. To assess the importance of these differences, analysis of variance tests for main and interaction effects of disposition and prior record on re-arrests were performed. The presence of significant interaction effects here would make it difficult to discuss the impact of dispositions independent of prior record. As Table 4 shows, the F test for interaction is not significant at the .05 level. The F tests for disposition and prior record were both significant at the .01 level, on the other hand. Together, these factors explained about 11% of the variation in re-arrests; disposition explained 4% of the variance, prior record 6%.

A discussion of analytic solutions to such problems of selection bias in evaluation research designs appears in a recent article by Alwin and Sullivan, in the journal Sociological Methods and Research.* Because there is no significant interaction between these independent variables, it was

*Duane F. Alwin and Michael J. Sullivan. "Issues of Design and Analysis in Evaluation Research," Sociological Methods & Research, Vol. 4, No. 1, (August, 1975). Pp. 77-100.

possible to employ a covariance adjustment, which removes the effect of prior record from means on the criterion variable. The unadjusted means, which appear as column means in Table 3,increase with severity of disposition. Reflecting the bias of referral officers in over-assigning prior offenders to the court disposition, the adjusted means change in the expected directions: the mean for court petition falls relative to the means for the three other conditions. The adjusted mean for release is .85 re-arrests; refer without purchase, 1.31; refer with purchase, 1.95; court petition, 1.91. It is of interest to note that subjects in the refer with purchase condition are re-arrested most often of all subjects when means are adjusted for prior record. Also, there is a noteworthy disparity between the means for the court and refer with purchase conditions, on the one hand, and the release condition, on the other. Clearly the most effective treatment for reducing re-arrests was to release subjects outright.

To this point, although randomization and adjustment for selection bias have excluded the possibility that background characteristics might have accounted for differing re-arrest rates, it would remain unclear whether juveniles not released by the police become more involved in delinquent activities or whether they only find themselves arrested more often for the same level of delinquent activity engaged in by their released co-subjects. To address this problem, reports were obtained from the subjects themselves regarding their delinquent activity, for which they may or may not have been arrested.

Because subjects were contacted for interview about 15 months from the date of the referral arrest, the completion rate was disappointing. One hundred fifteen subjects were interviewed, about 38% of the subject pool. The primary reason for this low rate was unavailability of subjects due to mobility, rather than refusal. When respondents were compared with non-respondents, however, there were virtually no differences in age, sex, prior

record, nature of referral offense,mean number of subsequent offenses, and treatment condition. None of the small differences between the two groups were statistically significant. It is reasonable to conclude from this that the respondents adequately represent the entire subject pool.

Surprisingly, respondents across the four disposition conditions did not differ appreciably with respect to the number of illegal activities they reported. Table 5 presents the mean number of offenses reported by respondents, the mean number of times respondents were re-arrested, adjusted to control for prior record, and resulting proportion of offenses for which respondents were apprehended. Although the respondents' self-reports do tend to follow the trend across disposition categories for re-arrest, none of the self-report differences are statistically significant. Thus, higher re-arrest rates for the court and refer with purchase conditions cannot be attributed to increased delinquent activity on the part of offenders not previously released by the police. Speculation concerning differing re-arrest rates across dispositions should center instead on the possibility of increased responsive efficiency of social control agencies. That is, subsequent to arrest, offenders who have been brought to the attention of either community agencies or the courts are more likely to be re-arrested than their comrades who have been released outright, even though both groups of offenders subsequently engage in about the same level of offense behavior.

IMPLICATIONS

The theoretical implications of these findings appear straightforward in that the more severe societal responses did not deter subsequent re-arrests, but rather, tended to encourage them. Released offenders were re-arrested least often of the four groups. This tends to support the societal reaction focus of labeling theory, rather than the identity change focus, and it tends not to support deterrence theory. In addition, the use of both

official and self-reported delinquency indices provides more specific indication of the mechanism by which societal reaction tends to encourage re-arrest. Re-arrests are higher for non-released offenders not because their behavior differs from that of released offenders, but because their activities more often become known to authorities, and they are apprehended more often for the same level of offense behavior. This supports the increasing awareness that official crime and deviance rates are considerably affected by the policies of official social control agencies, aside from the activity of individuals subjected to official social control.

The implications for criminal justice evaluation are somewhat similar. In evaluating social programs, it is important to consider more than one indicator of impact, and to examine unofficial indicators as well as those routinely produced by the very office or sector being evaluated. In the course of this project, not only did we find that multiple recidivism proved to be a more sensitive short-term indicator of the impact of disposition on re-arrest, but that re-arrest data taken alone provided a misleading picture of the impact of police handling. Although first multiple recidivism and then simple recidivism showed that disposition alternatives do affect subsequent re-arrests, self-reported delinquency indices shed new light on the specific mechanism in this effect. Whereas the official figures might lead us to believe that more severe police handling aggravates delinquent behavior, unofficial interview figures show that this is not the case. Evaluators are well-advised to employ multiple indicators in the attempt to clarify some of the complex possibilities of program impact.

Finally, these data have rather surprising implications for police diversion programs and policy administrators. It may be that referral to community counseling agencies with purchase of service may actually result in higher official recidivism than outright release. However, none of the dispositions considered here had a significant impact on behavior, relative to

the other dispositions. Administrators can expect difficulty explaining to the public that re-arrest rates for diversion programs are a function of improved surveillance rather than of aggravated delinquent behavior, especially if public opinion favors eliminating expensive social programs. It is conceivable that increased surveillance could be adopted as the goal of diversion programs, although according to these data that would not reduce delinquent behavior compared to low surveillance techniques. However, if the goal of diversion programs is to reduce re-arrests, the present data would suggest that it may prove impossible to justify them on that basis. The disposition alternative that resulted in the lowest official recidivism was also the cheapest disposition--outright release.

TABLES

TABLE 1: OFFICIAL RECIDIVISM FREQUENCIES (6 months)

	Release	Refer w/o Purchase	Refer w. Purchase	Court Petition
Simple Recidivism (1+ re-arrests) x^2=7.52,<.10	23 (28%)	28 (32%)	19 (35%)	39 (48%)
Multiple Recidivism (2+ re-arrests) x^2=15.35,<.005	5 (6%)	14 (16%)	11 (20%)	24 (29%)
n=	81	88	55	82

TABLE 2: OFFICIAL RECIDIVISM FREQUENCIES (15 months)

	Release	Refer w/o Purchase	Refer w. Purchase	Court Petition
Simple Recidivism (1+ re-arrests) x^2=12.52,<.01	30 (37%)	40 (45%)	32 (58%)	51 (62%)
Multiple Recidivism (2+ re-arrests) x^2=41.73,<.001	13 (16%)	24 (27%)	22 (40%)	34 (41%)
n=	81	88	55	82

TABLE 3: SIMPLE OFFICIAL RECIDIVISM AT 15 MONTHS, BY DISPOSITION AND PRIOR RECORD

			Release	Refer w/o Purchase	Refer w. Purchase	Court Petition	overall
PRIOR RECORD	NO	Mean Re-arrests	.62	.98	1.25	.92	.93
		n=	40(28%)	45(32%)	32(23%)	24(17%)	141
	YES	Mean Re-arrests	1.00	1.64	2.80	2.71	2.05
		n=	28(22%)	36(28%)	20(15%)	45(35%)	129
		x^2=11.76,<.01	.75	1.22	1.82	1.95	1.40
		n=	68	81	52	69	270

TABLE 4: ANALYSIS OF VARIANCE: SIMPLE OFFICIAL RECIDIVISM AT 15 MONTHS WITH DISPOSITION AND PRIOR RECORD

Source of Variation	Sum of Squares	Df	F	Significance of F
Main Effects	138.002	4	8.488	.001
Prior Record	68.765	1	16.918	.001
Disposition	52.713	3	4.323	.006
Interaction Effects	22.262	3	1.826	.141
Explained Variation	160.264	7	5.633	.001
Residual	1064.913	262		
Total	1225.177	269		

TABLE 5: SELF-REPORTED DELINQUENCY AND RE-ARREST AT 15 MONTHS

	Release	Refer w/o Purchase	Refer w. Purchase	Court Petition	overall
Mean Self-Report	49.75	46.88	54.58	53.97	51.17
Mean Re-arrests	.94	1.41	2.56	1.71	1.62
Offenses Apprehended	1.9%	3.0%	4.7%	3.2%	3.2%
n=	28	33	24	30	115

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