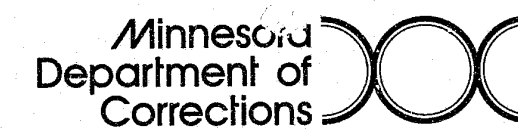


MINNESOTA COMMUNITY CORRECTIONS ACT EVALUATION



TECHNICAL REPORT:

PUBLIC PROTECTION

January, 1981

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NCJRS

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TABLE OF CONTENTS

LIST OF TABLES	i
LIST OF FIGURES	ii
I. INTRODUCTION	1
II. PUBLIC PROTECTION — ADULT OFFENDERS	3
A. Clarifying Issues for Analysis	3
1. Assumptions of the CCA	3
2. Specifying the Relevant Population	4
3. Follow-up Periods for Assessing Success/Failure	4
4. Summary of Issues to be Analyzed	7
B. Definition and Measurement of Public Protection	7
C. Research Design	10
1. Pretest-Posttest Design	10
a. Early Participants	10
b. Middle Participants	11
c. Recent Participants	11
2. The Inadequacies of Statistical Controls	13
D. Data Adjustments	15
1. Non-Comparable Follow-up Periods	15
2. Non-Comparable Proportions At-Risk	16
3. Non-Comparable Errors in Coding Non-Felonies	16
E. Decision Rules for Interpreting Results	17
1. Middle Participants	17
2. Early and Recent Participants	17
3. Probing Rival Explanations	18
F. A Search for Converging Evidence	18
1. Estimates of Eventual Failures	18
2. Aggregate Arrest Rates	21
G. Results	22
1. The Short-Term Impact of the CCA	23
2. The Long-Term Impact of the CCA	23
3. The Overall Impact of the CCA	28
4. Summary of Findings of CCA Impact Based on the Adult Offender Sample	35
5. Converging Evidence—Estimates of Eventual Success Rates	35
6. Converging Evidence—Aggregate Arrest Rates	39
III. PUBLIC PROTECTION — JUVENILE OFFENDERS	52
A. Special Problems in Juvenile Research	52
B. Juvenile Arrest Data	53
C. Methodology	54
D. Results	56
IV. SUMMARY AND CONCLUSIONS	69
REFERENCES	73

LIST OF TABLES

TABLE 1: Public Protection Analyses Feasible in Each CCA Area	6
TABLE 2: Summary of Public Protection Analyses Based on Adult Offenders	8
TABLE 3: Short-Term Impact of the CCA on Public Protection in Early Participants	24
TABLE 4: Short-Term Impact of the CCA on Public Protection in Middle Participants	25
TABLE 5: Short-Term Impact of the CCA on Public Protection in Recent Participants	26
TABLE 6: Short-Term Impact of the CCA on Public Protection in Ramsey and Hennepin Counties	27
TABLE 7: Long-Term Impact of the CCA on Public Protection in Early Participants	29
TABLE 8: Long-Term Impact of the CCA on Public Protection in Ramsey County	30
TABLE 9: Long-Term Impact of the CCA on Public Protection in Middle Participants	31
TABLE 10: Overall Impact of the CCA on Public Protection in Early Participants	32
TABLE 11: Overall Impact of the CCA on Public Protection in Ramsey County	33
TABLE 12: Overall Impact of the CCA on Public Protection in Middle Participants	34
TABLE 13: Summary of Public Protection Findings Based on the Adult Offender Sample	36
TABLE 14: Eventual Success Rates Estimated from the Split-Population Model	38
TABLE 15: Pre-CCA and Post-CCA Adult Arrest Rates -- Number of Arrests per 1,000 Adults At-Risk	40
TABLE 16: Pre-CCA and Post-CCA Juvenile Arrest Rates -- Number of Arrests per 1,000 Juveniles	57
TABLE 17: Percentage of Adult Offender Samples Treated in the Community	71

LIST OF FIGURES

FIGURE 1: Sources of Potential Threats to the Public	2
FIGURE 2: Summary of the Assumptions Linking the Community Corrections Act to Public Protection	9
FIGURE 3: Research Design for the Early Participants -- Pretest-Posttest Design with Non-Equivalent Control Group	12
FIGURE 4: Research Design for the Recent Participants -- Pretest-Posttest Design with Non-Equivalent Control Group	14
FIGURE 5: Example of Estimated and Actual Proportions of Failures Obtained from the "Split-Population" Model	20
FIGURE 6: Adult Felony Arrest Rates: CCA/non-CCA Counties	41
FIGURE 7: Adult Felony Arrest Rates: Dodge-Fillmore-Olmsted	42
FIGURE 8: Adult Felony Arrest Rates: Crow Wing-Morrison	43
FIGURE 9: Adult Felony Arrest Rates: Red Lake-Polk-Norman	44
FIGURE 10: Adult Felony Arrest Rates: Todd-Wadena	45
FIGURE 11: Adult Felony Arrest Rates: Region 3	46
FIGURE 12: Adult Felony Arrest Rates: Anoka	47
FIGURE 13: Adult Felony Arrest Rates: Region 6W	48
FIGURE 14: Adult Felony Arrest Rates: Blue Earth	49
FIGURE 15: Adult Felony Arrest Rates: Washington	50
FIGURE 16: Adult Felony Arrest Rates: Hennepin/Ramsey	51

FIGURE 17: Use of CCA and Non-CCA Arrest Data to Infer Effects of the CCA.	55
FIGURE 18: Juvenile Felony Arrest Rates: CCA/non-CCA Counties	58
FIGURE 19: Juvenile Felony Arrest Rates: Dodge-Fillmore-Olmsted	59
FIGURE 20: Juvenile Felony Arrest Rates: Crow Wing-Morrison	60
FIGURE 21: Juvenile Felony Arrest Rates: Red Lake-Polk-Norman	61
FIGURE 22: Juvenile Felony Arrest Rates: Todd-Wadena	62
FIGURE 23: Juvenile Felony Arrest Rates: Region 3	63
FIGURE 24: Juvenile Felony Arrest Rates: Anoka	64
FIGURE 25: Juvenile Felony Arrest Rates: Region 6W	65
FIGURE 26: Juvenile Felony Arrest Rates: Blue Earth	66
FIGURE 27: Juvenile Felony Arrest Rates: Washington	67
FIGURE 28: Juvenile Felony Arrest Rates: Hennepin/Ramsey	68

I. INTRODUCTION

The purpose of this report is to provide a thorough explanation of the evaluation of the impact of the Community Corrections Act (CCA) on public protection. The report is a supplement to the Minnesota Community Corrections Act Evaluation: General Report. Although considerable detail is provided in this report, additional information on the public protection evaluation can be found in two other sources. The Minnesota Community Corrections Act Evaluation: Research Design contains the conceptual overview for the evaluation which places the study of public protection in the context of the whole evaluation. Information on the sample on which the majority of public protection findings are based can be found in Technical Report: Adult Offender Sample.

A major responsibility of corrections policy is to protect the public from offender behaviors that threaten society. The evaluation's conceptual framework notes two interpretations of the expected contribution of the CCA to public protection. One argument is that the CCA can maintain public protection because the type of offender retained in the community will not commit crimes that threaten society. Another argument is that the CCA can increase public protection because community programs can better rehabilitate less serious offenders than can a prison environment. The evaluation of public protection explores both of these possibilities.

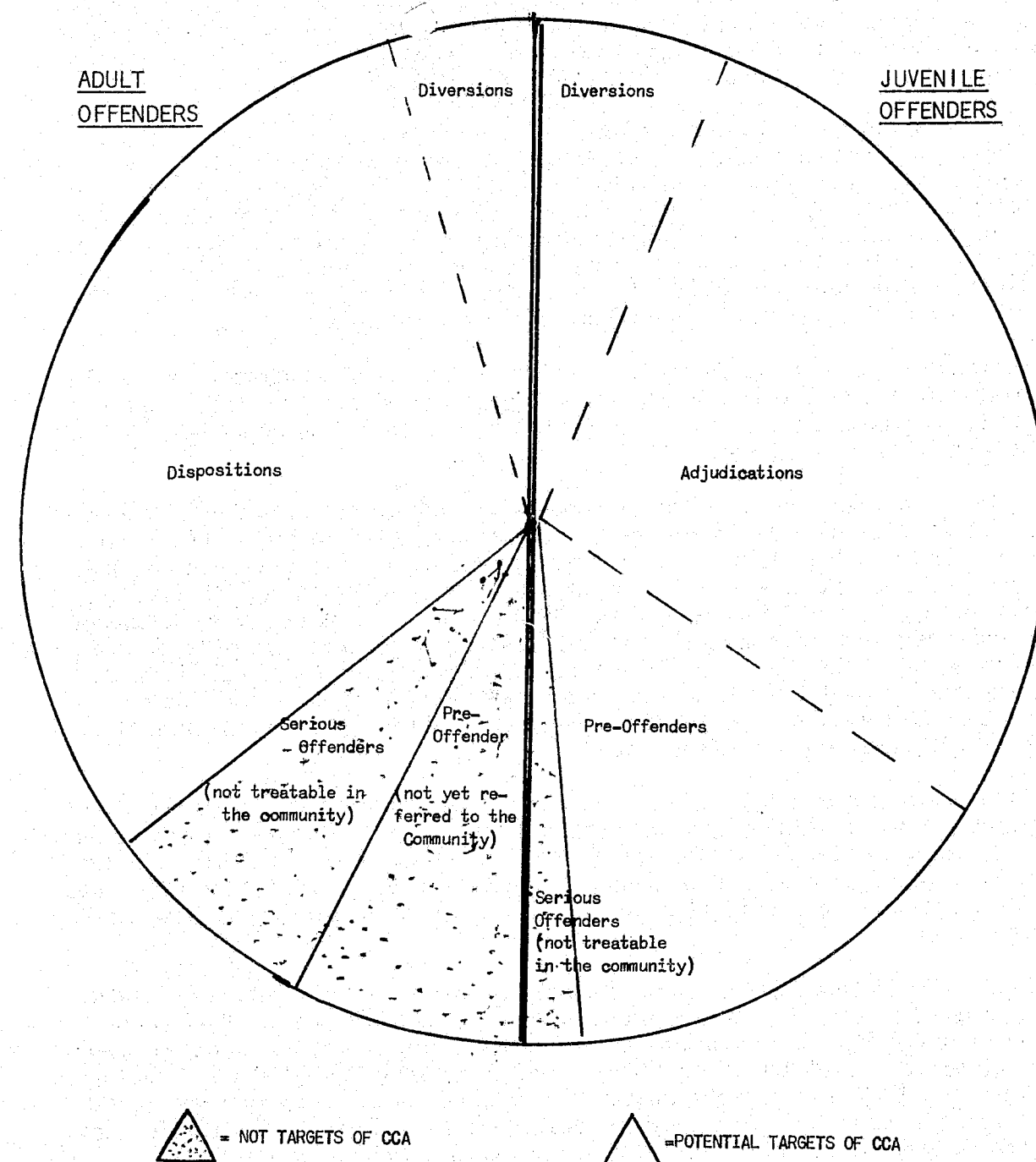
An investigation of the CCA's impact on public protection requires isolating the categories of offenders that could potentially be affected by the CCA. Figure 1 depicts the potential threats that are expected to be affected by implementation of the CCA and those that are not.

The CCA is not expected to affect the behaviors of two categories of offenders. First, serious adult offenders are expected to be committed to prison and treated there even with CCA participation. It is unreasonable, in other words, to conclude that the CCA is ineffective because serious offenders continue to commit crimes in the community. It is not a purpose of the CCA to deal with these offenders (except for parole supervision). Second, first-time adult offenders generally are not expected to be influenced by the CCA. The CCA has not developed programs to deal with adult pre-offenders, although juvenile prevention programs are common. Again, one cannot judge CCA effectiveness in terms of the number of adult first offenders (i.e. those coming directly from the pre-offender pool).

The implication drawn from Figure 1 is that it is inappropriate to assess CCA effectiveness by investigating aggregate crime rates. A portion of crimes or threats to society are accounted for by offenders who are not expected to be influenced by the CCA. Two major categories of offenders, however, are influenced by the CCA and should legitimately be investigated to see if threats by these groups have been affected.

The first group includes the 1) adult offenders treatable in the community. Adult offenders can be sentenced to a community sanction or they can be diverted to a community sanction prior to prosecution. Offenders treatable in the community are a source of threat to the public both during and after their supervision. Thus, an evaluation of the CCA must probe whether referring adults to the community creates an increased risk to society while they are treated locally and whether adult offenders are more likely to be "rehabilitated" after their local supervision/treatment.

FIGURE 1: Sources of Potential Threats to the Public



The second group includes 2) juvenile offenders treatable in the community. As with adults, juveniles may receive community dispositions or they may be diverted to community programs prior to adjudication. The behavior of these juveniles during and after their community supervision must be investigated to assess the impact of the CCA on public protection.

Two differences are apparent between juvenile and adult offenders. First, most serious juvenile offenders appear to be assumed to be treatable in the community. Charges are levied for all juveniles committed to state institutions, with only one minor exception — the state's Serious Juvenile Offender Program (SJO). Although the presumption of the CCA appears to be that all juveniles are treatable in the community, the DOC's development of the SJO and agreement not to charge per diems for its use is a recognition that some juveniles may be more appropriately placed in a state institution. This one program, however, is but a minor exception to the statement that all juvenile offenders are assumed to be treatable in the community.

A second difference is that juvenile pre-offenders, unlike adult pre-offenders, are targets of the CCA. A "pre-offender" is defined as someone who may (or may not) have exhibited potentially delinquent behavior or comes from an environment likely to promote delinquent behavior (e.g. family in crisis), but who has not been actually charged with an offense. The rationale behind prevention programs is that if the "pre-offender" can be treated at an early stage, later delinquency can be averted. It is, of course, difficult to assess if persons who have not yet committed crimes have been prevented from committing any crimes later because of preventive treatment.

Because of these differences in target population and because of differences in data availability, separate evaluations of Public Protection are conducted for adult and juvenile offenders.

II. PUBLIC PROTECTION-ADULT OFFENDERS

A. Clarifying Issues for Analysis

1. Assumptions of the CCA

Two very different arguments have been identified which suggest a linkage between the CCA and Public Protection. One argument is that less serious offenders can be treated safely in the community because they will not commit offenses that threaten the public. Essentially the argument is that prison incapacitation is unnecessary because less serious offenders do not pose a major risk to the community. This argument refers to the short-term effects of the CCA.

Another quite different argument is that, regardless of the short-term risk to the public, community treatment can better rehabilitate less serious offenders. In the long-term, community treatment pays off because less serious offenders have a better chance of being rehabilitated in the community than in a prison environment.

It is hypothesized that the combined short-term and long-term impact of the CCA should be an increase in public protection. If less serious offenders are unlikely to commit new offenses in the community and if they have a better chance of being rehabilitated, the expected overall impact of the CCA should be a net increase in public protection.

2. Specifying the Relevant Population

The Technical Report: Adult Offender Sample describes the cases sampled in each CCA area —offenders convicted of or diverted for felony offenses for a period of time before and after CCA entry. However, only a portion of the population sampled appears to be relevant to the issue of public protection. The offenders identified as targets of the CCA are: 1) all cases receiving community sanctions, and, 2) less serious offenders committed to prison. Serious state commitments are not targets of the CCA. No arguments have been made that the CCA ought to divert serious offenders to the community and that these cases would not pose a public risk; nor have any arguments been made that serious offenders can be better rehabilitated in the community. The CCA recognizes that certain categories of offenders should continue to be incarcerated. On the other hand, less serious state cases are CCA targets. The assumptions of the CCA are that these cases would not be public risks and they could be better rehabilitated if sentenced to a community alternative. Since the expected impact of the CCA is to reduce the proportion of less serious offenders committed and in so doing to affect positively public protection, analyses should include these cases. The behavior of serious state commitment, however, does not appear to be affected by the CCA. These cases should be committed. If these cases fail upon release, their failure should not be counted against the CCA.

The criteria used to determine which state cases to include in the public protection analyses are the measures used for the evaluation of appropriateness of sanctions. In particular, the Minnesota Sentencing Guidelines are used to categorize less serious and more serious offenders. An explanation of this measure and its justification can be found in Technical Report: Appropriateness of Sanctions.

3. Follow-up Periods for Assessing Success/Failure

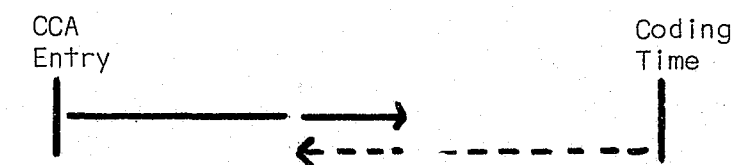
The evaluation of public protection requires follow-up periods for assessing the short-term, long-term, and overall effects of the CCA. The follow-up period for assessing the short-term impact is referred to as T_1 ; the period for assessing longer-term effects is referred to as T_2 ; the combined period is referred to as $T_1 + 2$.

T_1 for the state commitments is equivalent to their actual incarceration. Thus the state cases are incapacitated during T_1 . For the community cases the time period should be equivalent to the time such offenders would have been incarcerated had they been committed to a state institution. The argument, recall, is that these offenders need not be incarcerated because they will not be a significant threat while supervised in the community. If someone is given five years of probation, it may not be necessary to assess the offender's threat for five years. Had the person been incarcerated for twelve months, the offender probably would be returned to the community for parole supervision anyway after a year. Thus, the first twelve months in the community is the period during which this offender has the potential for being an additional threat.

The two alternatives available are to utilize a standard time period for all offenders in the sample, or to make the time period dependent upon the commitment offense (or charge, for diversions). The latter alternative assumes that offenders retained in the community would have been incarcerated for variable amounts of time. Given the diversity among community placements, it appears safest to assume they would be incarcerated for variable amounts of time. The release matrix utilized by the Minnesota Corrections Board provides a convenient tool to calculate expected incarceration time for individual offenders. The primary drawback of this tool is that it probably over-estimates incarceration time. In particular if a judge deemed an offender appropriate for the community, he may have limited the sentence had he incarcerated the offender. In addition, the type of person retained in the community

may have had the matrix time reduced by the Minnesota Corrections Board for mitigating reasons. Thus, the matrix time is likely to inflate the expected incarceration time. In order to utilize variable incarceration periods and at the same time compensate for the possible overestimation of time using the matrix, the minimum matrix time for each offender is assigned. This decision requires that each case be coded on offense severity and risk levels to enable a matrix placement. The technical report on the Adult Offender Sample contains the data collection instrument and coding instructions. Section D of the instrument contains the variables required for a matrix placement. A copy of the matrix is included with the coding instructions.

T_2 , the time period to assess rehabilitation, is at a minimum twelve months. This period begins after release for the state cases and after the estimated incarceration time for the community cases. A twelve-month follow-up is a compromise between two conflicting factors. First, it is desirable to have at least several post-CCA years from which to sample. It can be argued that the first year or so after entry is not yet representative of how an area operates with CCA participation. The longer the post-CCA period from which to sample, the more representative are cases of CCA participation. Second, it is desirable to have as long a follow-up period as possible. The longer the follow-up period, the more likely that findings are representative of the offenses that will eventually be committed. However, a long post-CCA period from which to sample cases and a long follow-up period are in conflict as the figure below demonstrates. The solid line represents the post-CCA period while the dashed line represents the follow-up period. Because both CCA entry and coding time are pre-determined, extending the post-CCA period shortens the followup period; extending the follow-up period shortens the post-CCA period.



The primary research relies on the fixed follow-up period. However, follow-up data are collected on most offenders for a much longer period of time. Some corroborating research utilizing variable follow-up periods is conducted, making it worthwhile to collect follow-up data as far as possible.

The requirement of a follow-up period affects the number of areas in which public protection can be assessed. Table 1 summarizes the issues that can be addressed in each area. Table 1 contains the number of post-CCA years available to study the short-term impact (T_1) and long-term impact (T_2) of the CCA. The first three areas (Dodge-Fillmore-Olmsted, Ramsey and Crow Wing-Morrison) entered early enough to permit several years of dispositions post-CCA from which to sample, and ample time for a follow-up assessment of behaviors during and after supervision. The middle participants are borderline cases. Behaviors during T_1 can be probed but there is only one and a half to two years of post-CCA dispositions to assess long-term rehabilitation. The last four areas to enter the CCA provide only one or less years of post-CCA dispositions to assess behaviors in the community (T_1). Long-term assessments are impossible; short-term assessments are tenuous. Although findings of CCA effectiveness in the recent participants are tenuous, it should be remembered that these areas serve an additional function as comparisons for the early participants.

TABLE 1: Public Protection Analyses Feasible in Each CCA Area

CCA Area	1972	1973	1974	1975	1976	1977	1978	1979	1980	Number of Post-CCA Years from which to Sample	
										For T ₂ Assessments	For T ₁ Assessments
Dodge-Fillmore- Olmsted			X							3 3/4	4 1/2
Ramsey			X							3 2/3	4 1/2
Crow Wing- Morrison			X							3 1/2	4 1/4
Red Lake-Polk- Norman					X					2 1/4	3
Todd-Wadena					X					1 2/3	2 1/2
Arrowhead Regional Corrections					X					1 2/3	2 1/2
Anoka					X					1 1/2	2 1/4
Region 6W						X				-	1 1/4
Blue Earth							X			-	1
Hennepin							X			-	1
Washington							X			-	1/2*
(Rock-Nobles)*							1.	2.	3.	-	-

* - Will not be included in analyses
 □ - Enough years for analysis
 □ - Enough years for tenuous analysis
 X - CCA entry

1. 4/1/78 is cutoff for including dispositions for T₂ assessments (at least two year follow-up)
2. 1/1/79 is cutoff for including dispositions for T₁ assessments (at least sixteen months follow-up)
3. Cutoff for coding the follow-up is 6/1/80

4. Summary of Issues to be Analyzed

Table 2 provides a summary of the issues to be analyzed, the population relevant for each analysis, the follow-up periods for each analysis, and the CCA areas that can be included in each analysis. Figure 2 provides a graphic presentation of the expected impact of the CCA on public protection in the short term, long term, and overall. It is important to emphasize that the evaluation assesses the impact of the Act on public protection, not the impact of community programs. The comparisons made are between the set of offenders sentenced before the CCA and the set of offenders sentenced after CCA entry. The post-CCA group is expected to contain a larger proportion of community-treated offenders. This relative increase in the community-treated group is expected to promote public protection. This test of the impact of the Act is not equivalent to an evaluation of community programs compared to state incarceration. The design does not compare one group of offenders sentenced to the community to a similar group of offenders sentenced to prison to determine which mode of corrections is more rehabilitative. The distinction is subtle but is very important for interpreting the results.

B. Definition and Measurement of Public Protection

Public protection is measured by the behaviors of offenders. The more that offenders are prevented from committing offenses, the more the public is protected. Offenders who do not commit further offenses are called "successes". The more that offenders commit further offenses, the less the public is protected. Offenders who commit further offenses are "failures". Since public protection is a positive goal to achieve, the goal is assessed in terms of a positive indicator (i.e. successes) rather than a negative indicator (i.e. failures).

Two issues are involved in determining what constitutes a success or a failure — 1) how serious must an offense be to consider that an offender has not succeeded; 2) should one base the assessment on arrest reports or actual convictions. For the purposes of this evaluation, an offender will be considered a success if he/she does not commit a felony. An offense must be as serious as a felony for the offender to be considered as not having succeeded.

Whether to use arrests or convictions poses a more difficult question. Both arrests and convictions are imperfect indicators of success/failure. Some offenders commit new offenses but are never caught, arrested, or convicted. Some offenders are arrested but may not have actually committed an offense. On the other hand, some offenders who do commit new offenses and are arrested may not be convicted for various reasons (e.g. insufficient evidence, plea bargaining, etc.). It was initially proposed that convictions provided a more reasonable indicator. It was believed that persons under supervision may be more likely to be suspected of crimes and, therefore, more likely to be arrested, when in fact they may not actually be committing more crimes. Since the CCA is expected to place more offenders under local supervision, arrests might increase simply because of higher levels of local supervision.

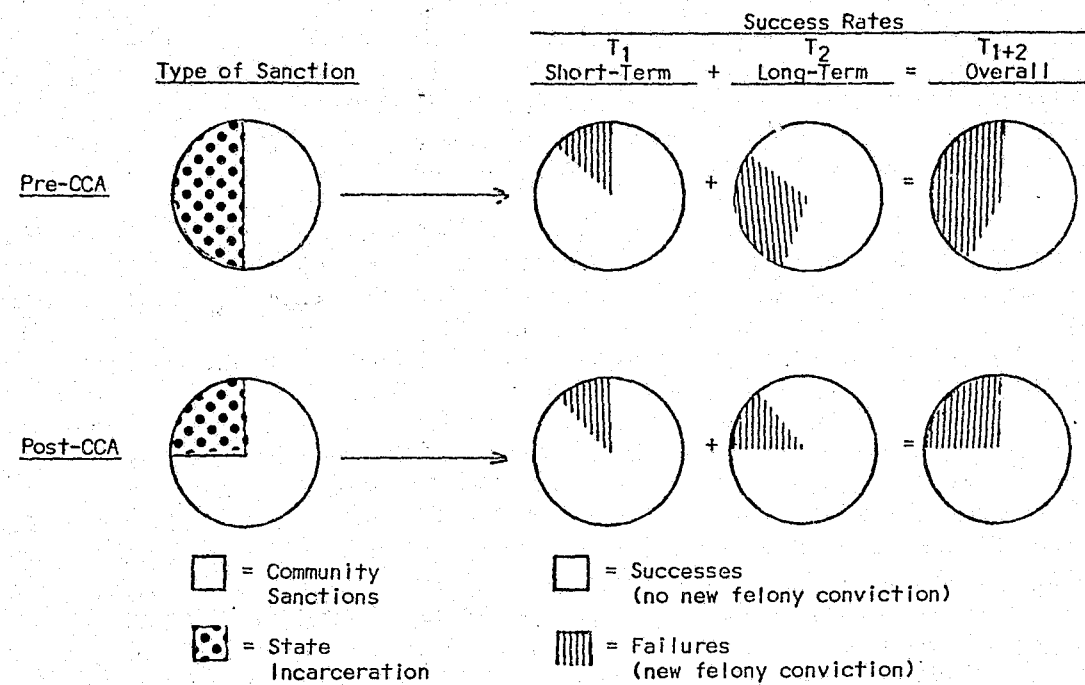
A number of outside reviewers disagreed with this position and argued that arrests are a better indicator — by the stage of conviction charges against many "real" offenders have been dropped. As a result both arrest and conviction data have been collected for the follow-up. In interpreting results, particularly possible contradictory findings between arrests and convictions, one should remember what is being measured. One is less interested in using a valid measure of success/failure levels than in using an indicator that validly measures change in success/failure. For example, should one

TABLE 2: Summary of Public Protection Analyses Based on Adult Offenders

<u>Issue</u>	<u>Relevant Population</u>	<u>Follow-Up Period</u>	<u>CCA Areas Included in Analysis</u>
1. Short-term risk to the public	1. Community Cases _____ 2. Less serious state cases _____	T_1 =estimated incarceration T_1 =actual incarceration	DFO, CWM, Ramsey, RPN, TW, ARC, 6W, Anoka, Hennepin, Blue Earth
2. Long-term rehabilitation	1. Community cases ^a _____ 2. Less serious state cases _____	T_2 =12 months (or longer) after estimated incarceration T_2 =12 months (or longer) after actual incarceration	DFO, CWM, Ramsey, RPN, TW, ARC, Anoka
3. Overall Effects on Public Protection	1. Community Cases _____ 2. Less serious state cases _____	$T_{1\&2}$ = continued incarceration plus 12 months (or longer) $T_{1\&2}$ =actual incarceration plus 12 months (or longer)	DFO, CWM, Ramsey, RPN, TW, ARC, Anoka

^aThe population and sample consists of those cases who have not failed by the end of T_1 . That is, of those who successfully complete their incarceration or community alternative, how many successfully complete the next year. Those who fail during T_1 are no longer available to fail in T_2 and therefore are excluded.

FIGURE 2 : Summary of the Assumptions Linking the Community Corrections Act to Public Protection



Expected Short-Term Impact of the CCA:

Assumptions: The CCA is expected to divert less serious offenders who should not be in prison to the community; the relative size of the community population should increase after CCA; this increase in the community population that is at-risk is not expected to pose an increased risk to the public.

Test: The proportion of successes during T_1 among less serious state and community cases will not decline after CCA entry.

Expected Long-Term Impact of the CCA:

Assumptions: Less serious offenders can be better rehabilitated by being treated in the community than in a prison environment; the proportion of offenders treated in the community is expected to increase after CCA entry; this increase in the community population should result in a larger proportion of rehabilitated offenders.

Test: The proportion of successes during T_2 among less serious state and community cases should increase after CCA entry.

Expected Overall Impact of the CCA:

Assumptions: Less serious offenders are unlikely to commit new offenses that threaten the public if retained in the community and they have a better chance of being rehabilitated; the CCA is expected to increase the proportion of less serious offenders retained in the community; since in the short term this increase in the community population is not expected to increase the public risk and in the long term should result in better rehabilitation, the net effect should be an increase in public protection.

Test: The proportion of successes during T_1 and T_2 should increase after CCA entry.

find that there is eighty percent success among community placements pre-CCA and ninety percent success post-CCA, one is more concerned with the inference that success rates have improved by ten percentage points than with describing levels of success pre- and post-CCA. The crucial point to remember in analysis is to find the indicator that best measures change in offenders' behaviors rather than changes in local reporting or court processing behaviors that themselves may be a result of CCA entry. The position adopted for the evaluation is that convictions probably are more stable overtime than arrests and, therefore, constitute a better measure of change. Most analyses are conducted using both measures. However, if findings diverge (which they generally do not) conclusions are based on conviction results. This position was unanimously accepted by the evaluation's Advisory Group.

During the data collection phase on the Adult Offender Sample (see Technical Report on the Adult Offender Sample) any reference to a new felony arrest and/or conviction was recorded. At the end of the data collection period coders checked Bureau of Criminal Apprehension records for all cases in the samples for evidence of a new felony arrest and/or conviction. Dates of arrest and conviction and Uniform Offense Codes for the arrest and conviction were coded. Coders were instructed to record the arrest and conviction information for an offender's first felony conviction which comes after the sentence for which the offender was sampled. For example, if an offender sentenced in November, 1976 was sampled, a coder recorded the first arrest and conviction that occurs after November, 1976. In addition, if an offender had a felony arrest that did not result in a conviction and occurred before the arrest that resulted in a felony conviction, that arrest date and Uniform Offense Code were recorded. Thus, one is able to measure an offender's first failure by his/her first arrest and by his/her first conviction. An arrest or conviction occurring within T_1 or T_2 is an indication of failure. When conviction is used as the indicator of failure, the date of the arrest that results in a conviction is used to determine whether the offense occurs during T_1 or T_2 because the arrest date is closest to the offense itself.

C. Research Design

1. Pretest-Posttest Design

The primary design used to evaluate public protection is a standard pretest-posttest design, comparing proportions of successes before and after CCA entry for the short term (T_1), long term (T_2) and combined periods ($T_1 + T_2$). A different variation of the design is used for counties that enter the Act at different times.

a. Early-Participants

The strongest design is used on the counties that entered the CCA first — Dodge-Fillmore-Olmsted, Crow Wing-Morrison, and Ramsey. One is able to utilize as comparison counties for these areas the counties that entered the CCA most recently. A design based on counties randomly assigned to be CCA and control groups is obviously out of the question. Moreover, project resources were exhausted collecting data only in CCA counties. Resources were not available to collect data in non-CCA areas. However, the areas that did not enter the CCA until 1978 can be used as comparisons for the areas that entered in 1974. The availability of the comparison counties enables one to control to some degree the possibility that the pattern of results found in the CCA areas is occurring elsewhere and is, therefore, not caused by the CCA. The research design literature refers to such a design as a pretest-posttest design with non-equivalent control groups. A thorough explanation of the strengths

and weaknesses of such a design can be found in Campbell and Stanley (1963) or Cook and Campbell (1976).

Figure 3 provides an explanation of the design for the early participants. As the technical report on the adult offender sample explains, cases have been sampled in each CCA area from 1972 through 1978. Since there are insufficient cases in each year to speak confidently about yearly values, a time-series design is not possible. However, when one aggregates the yearly samples into longer periods, the samples become large enough to provide relatively precise estimates of the population (generally at $\pm .05$). For the early participants, one aggregates the cases sentenced before CCA entry (mid-1974) and those sentenced after CCA entry.

One can treat the pre-CCA observations in the recent participants as comparisons for the early participants. One eliminates post-CCA observations (1978) in order to eliminate all CCA effects from the comparison data. The comparison observations are aggregated into periods comparable to the CCA area's pre-entry and post-entry periods. Because 1978 observations are removed from the comparison data, they are also removed from the CCA data to ensure that the CCA data and comparison data are comparable. For example, if 1978 observations remained in the CCA data but were removed from the comparison data and if something other than the CCA is affecting success rates in 1978, one might interpret a change in success rates post-CCA as due to the CCA when in fact the change is due to something else.

Three recent participants are available as comparisons for Dodge-Fillmore-Olmsted and Crow Wing-Morrison. Region 6W, Blue Earth and Washington entered the CCA approximately January, 1978. Their 1972 through 1977 data can be treated as comparison data for the two early participants.

Ramsey and Hennepin Counties are the two large urban counties and as such are somewhat different from the rest of the State. The 1972 through 1977 observations of Hennepin, the recent participant, can be treated as comparison data for Ramsey County whose entry is 1974.

Thus, success rates of offenders sentenced in the early participating areas will be calculated for T_1 , T_2 and $T_1 + T_2$ for periods before and after CCA entry. The changes found in the CCA areas are then compared to changes found between comparable periods in the recent participants to assess the likelihood that findings in the CCA areas are a result of the CCA or other factors.

b. Middle Participants

Four county areas entered the CCA in 1976. Because the entry dates of both the early and recent participants are so close to this time it is difficult to use other CCA counties for comparison purposes. The design is simply a pretest-posttest design without a control group. This design is weaker than the one described above because of the lack of a comparison for assessing rival explanations. The evaluation of public protection in Red Lake-Polk-Norman, Todd-Wadena, Arrowhead Regional Corrections and Anoka relies on comparisons of success rates before and after CCA entry. There is the possibility that any changes or lack of change discovered might be due to factors other than the CCA but without comparison data that possibility cannot be ruled out.

c. Recent Participants

Only the short-term effects of the CCA can be probed in the counties that entered the CCA most recently. Moreover, the analysis of short-term effects is tenuous because

FIGURE 3: Research Design for the Early Participants -- Pretest-Posttest Design with Non-equivalent Control Group

CCA County:	Year	1972	1973	1974	1975	1976	1977	1978
Dodge-Fillmore-Olmsted				X				
Crow Wing-Morrison				X				
Comparisons:								
6W	-----				-----			X
Blue Earth	-----				-----			X . . .
Washington	-----				-----			X . .
CCA County:								
Ramsey				X				
Comparison:								
Hennepin	-----				-----			X . . .

X = Area's CCA Entry

_____ = Time Period based on Early Participants' CCA Entry

----- = Comparison Time Period based on Early Participants' CCA Entry

. = Time Period Eliminated from Analysis to remove CCA Effects from the Comparison Data and to Make the CCA and Comparison Periods Comparable

post-CCA assessments are based on only one year of post-CCA sentences (1978). These tenuous assessments can be strengthened somewhat by incorporating data from the early participants as comparisons.

Figure 4 provides an explanation of the design for the recent participants. It is a pretest-posttest design with non-equivalent control groups. In this case, however, it is the early participants that are comparisons for the recent participants. The observations that are eliminated are the pre-CCA years of the early participants in an effort to eliminate effects due to the CCA and to detect effects that occur above and beyond the CCA.

It is clearly problematic to utilize CCA counties that have already joined the CCA as controls. A potential problem in the comparison is that the CCA in some way might insulate the county from other factors operating statewide. Arguments that the CCA might have interaction effects with other variables affecting success rates would be somewhat similar. While the CCA is expected to affect public protection, it does not appear that other factors operating statewide could not continue to affect offenders' follow-up behaviors. In the abstract, then, it appears appropriate to use early participants as comparisons for recent participants.

However, if one expects the CCA to affect a county in such a way that further change is not possible, it would not be legitimate to use these comparison counties. For example, if the CCA brings a county to a level beyond which it can not improve, the comparison would not be warranted. If this appears to be the case in any early participant, it is not used for comparison purposes.

Finally, if there is evidence that the CCA has started a trend (either upward or downward) in success rates in the early participants, the comparison data would in fact demonstrate a change that is a result of the CCA not other statewide factors. Since the purpose of comparison data is to demonstrate non-CCA changes (i.e. changes due to other factors), the early participants could not be used for comparison purposes. If the data from the early participants suggest the CCA has had no impact, or the impact has been a step increase at the time of entry, then data from the early participants are used for comparison purposes.

2. The Inadequacies of Statistical Controls

The pretest-posttest design with non-equivalent control group assumes that important non-CCA variables are in fact equivalent in the comparison areas. One alternative is to control statistically the variables that might be affecting outcomes. Statistical analyses were originally proposed as an alternative design. Such analyses when conducted appropriately are useful alternative or supplementary techniques to standard quasi-experimental designs. Three problems, however, led to the decision that statistical controls could not be appropriately conducted.

First, one must have data on variables that explain a large proportion of the variance in the dependent variable (success/failure). If the data on offenders cannot explain failure, then statistical controls are of little use. Major explanatory variables should not be omitted from the regression equation. Past efforts at explaining criminal behavior suggest that the data available on the adult offender sample are insufficient to explain much variance in success/failure.

FIGURE 4: Research Design for the Recent Participants -- Pretest-Posttest Design with Non-equivalent Control Group

CCA County:	Year						
	1972	1973	1974	1975	1976	1977	1978
6W							X
Blue Earth							X
Washington							X
<u>Comparisons:</u>							
Dodge-Fillmore-Olmsted			X	-----			----
Crow Wing-Morrison			X	-----			----
<u>CCA County:</u>							
Hennepin				-----			X ----
<u>Comparison:</u>							
Ramsey			X	-----			----

X = County's CCA Entry

----- = Time Period based on Recent Participants' CCA Entry

----- = Comparison Time Period based on Recent Participants' CCA Entry

..... = Time Period Eliminated from Analysis to Remove CCA Effects from the Comparison Data and to Make the CCA and Comparison Periods Comparable

The second problem relates to the dichotomous nature of the dependent variable -- success/failure. Standard regression methods are less appropriate than newer PROBIT techniques. Although PROBIT software is available, the third problem limits its use.

The third problem is that ordinary least squares regression may be inappropriate when entering the treatment variable (CCA/non-CCA) into a regression equation. If unmeasured variables affect both the treatment (CCA entry) and the outcome (failure rates), then regression assumptions would be violated. Also, the more that the same variables influence both the treatment and outcome, improving the specification of the outcome equation, increases the multicollinearity of the independent variables, again violating regression assumptions. If the data suggest that such problems exist two-stage-least-squares methods can be used to overcome them. However, two-stage-least-squares methods are not readily usable in conjunction with PROBIT analysis, limiting the ability to overcome them. These problems are well articulated in a recent draft manuscript by Achen (1980).

D. Data Adjustments

One adopts the strongest research design possible in an effort to control variables other than the treatment (here the CCA). When one utilizes randomly selected control groups, one has assurance that a variety of variables and errors are controlled. However, the middle participants have no comparisons and the comparisons available for other areas are not randomly selected. The possibility remains that there is error in the data that is creating or masking changes from before to after CCA entry. Errors that might exist have been hypothesized and the data checked to assess the likelihood that errors are present. When there is evidence that a source of error is present, the data are adjusted according to procedures outlined below.

1. Non-Comparable Follow-up Periods

The sampling plan was developed to see that most cases would have an adequate follow-up period. Thus, cases are sampled through 12/78 for assessing short-term behavior but sampled only through 3/78 to assess longer-term behaviors. Since coding on follow-up felonies was conducted in June, 1980, twenty-six post-sentence months of follow-up are available for the latest cases in the samples (i.e. for a case sentenced in 3/78 there are twenty-six months until June, 1980). However, for offenders who have unusually long periods of incarceration or estimated incarcerations, their T₁ and T₂ follow-up periods might not be completed by June of 1980 (see section A. 3 above for an explanation of the follow-up periods). If these cases had a full follow-up period, it is possible that they might fail. When there are cases in the post-CCA period whose follow-up periods are incomplete, success rates might be inflated because of the cases who have not yet had time to fail.

The pre- and post-CCA samples are examined to obtain the proportion of cases with inadequate follow-up periods. When the pre- and post-CCA periods are not comparable, the following adjustments are made:

1. find the number of cases with inadequate follow-up periods;
2. multiply this number by the period's failure percentage; (i.e. assume that these cases would fail by the end of the period at the same rate if given sufficient time)
3. reduce the number of successes by this number of estimated additional failures;
4. recalculate the success rate on this new estimate of successes.

These checks for non-comparable followups are conducted for areas when treated as CCA areas and when treated as comparison counties.

2. Non-Comparable Proportions At-Risk

The potential problem of cases not being at-risk is not present for the short-term analysis. Should a community case be revoked for something other than a new felony, the case is treated as a state case and the follow-up begins upon release. A potential problem is present for the long-term analysis. The long-term follow-up period begins after release for state cases and after the estimated incarceration time for community cases and is of one-year duration. If a person commits a new felony during this period, the case is considered a failure. If a person does not commit a new felony, the case is a success. Some cases, however, are not at-risk to fail. In particular, if a state case has a parole revoked during T_2 for something other than a new felony the case has not failed by our criteria but once incarcerated is not available to fail. Similarly if a community case has a probation revoked for something other than a new felony during this period, the case is not at-risk and not available to fail. If a large number of cases are not at-risk, success rates might be unduly inflated.

Data on the adult offender sample indicate whether a case is incarcerated during the majority of this one-year period following incarceration or estimated incarceration (T_2). These data are used to determine whether the proportion of cases who have not yet failed before and after CCA and who are not at-risk for the majority of the one-year period (i.e. who are incarcerated for some reason other than a new felony) is comparable. If the proportion not at-risk is not comparable before and after CCA:

1. determine the number not at-risk;
2. reduce the total number in the sample by this number;
3. reduce the number of sample successes by this number;
4. calculate the new success rate based on this new number of successes and new number in the sample.

This procedure removes from the sample those cases who have not failed but are not available to fail. These checks for non-comparable proportions not at-risk are conducted for counties when treated as CCA counties and when treated as comparison counties.

3. Non-Comparable Errors in Coding Non-Felonies

The majority of the follow-up data was coded from the Bureau of Criminal Apprehension (BCA). BCA records include the Uniform Offense Code (UOC) for the arrest and conviction. Coders were instructed to code arrests for felonies and convictions for felonies. If a UOC could be a felony, the arrest and/or conviction was coded. Some UOC's cover felony and non-felony offenses. For example, one cannot tell from a theft UOC whether the theft is a felony or misdemeanor. It is therefore possible that some of the felonies coded are in fact not felonies. This error should not be extensive because relatively few misdemeanors are reported to the BCA. The assumption is that this inevitable error in the follow-up data will be constant in the pre- and post-CCA periods and therefore will not affect changes in success rates. In order to assess the accuracy of this assumption, the following procedures are conducted for each CCA area:

1. determine the proportion of follow-up offenses that might possibly not be felonies for the pre- and post-CCA periods;

2. when this proportion is not comparable, eliminate the potential non-felonies from both the pre- and post-CCA failures;
3. recalculate the success/failure rates;
4. if conclusions do not differ from those based on the unadjusted rates, report the unadjusted rates;
5. if conclusions do differ from those based on the unadjusted rates, report the unadjusted rates with the caveat that findings might be the result of erroneously counting non-felonies as felonies.

The reason for relying on the unadjusted rates in that there is no accurate way to estimate the likelihood that certain categories of UOC's contain non-felonies. The removal of all questionable offenses would probably underestimate failures to a greater extent than their inclusion would overestimate failures.

E. Decision Rules for Interpreting Results

When one compares success rates before and after CCA entry one requires some criteria by which to determine whether the change indicates an increase in, decline in or maintenance of public protection. The criteria vary with the design employed.

1. Middle Participants

The design for the four county areas that join in the mid-1970's is a pretest-posttest design without control counties. Thus the only comparison is the pre-CCA and post-CCA success rates. One determines, then, whether the change is significant. The difference of proportions test (Z) indicates the likelihood that the pre- and post-CCA sample proportions represent the same or different populations. A Z that is significant at the .05 level indicates that the pre-CCA and post-CCA samples represent different populations; that is, the change is significant and is not likely to have occurred by chance.

2. Early and Recent Participants

The designs for the early and recent participants incorporate comparison counties. Thus, in addition to comparing pre-CCA and post-CCA success rates one compares the CCA change to changes occurring elsewhere. The following decision rules are applied when comparison county data are available:

Rule	Applicable To
1. When an area has one comparison the results in the CCA area must be significantly different from the comparison county to conclude that the CCA results are due to the CCA.	Hennepin compared to Ramsey Ramsey compared to Hennepin
2. When an area has two comparisons the results in the CCA county must be signifi-	Region 6W, Blue Earth and Washington compared to Dodge-Fillmore-Olmsted

cantly different in the same direction from both comparisons to conclude that CCA results are due to the CCA.

3. When an area has three comparisons the results in the CCA area must be significantly different in the same direction from two of the three comparisons to conclude that the CCA results are due to the CCA.

and Crow Wing-Morrison

Dodge-Fillmore-Olmsted and Crow Wing-Morrison compared to Region 6W, Blue Earth, and Washington.

The third decision rule is probably most open to debate. It permits one to conclude that the CCA has had an impact when one comparison county demonstrates similar results to the CCA findings. Research staff discussed several alternatives and eventually brought them to the Advisory Group for consideration. The third decision rule leads one to err in the direction of accepting a change (positive or negative) as due to the CCA, when in fact the CCA has no impact. More stringent rules, however, lead one to err in the direction of concluding there is no impact (positive or negative) when in fact there is. Research staff preferred the third decision rule. The evaluation Advisory Group also unanimously accepted the third rule. A difference-of-difference-of proportions test is utilized to determine whether CCA and comparison results are significantly different.

3. Probing Rival Explanations

The stronger the research design, the more likely that rival explanations are controlled, and the more likely that conclusions on CCA impact are correct. Since the comparison counties are not randomly selected and since some areas have no comparisons, the possibility remains that findings are not necessarily the result of the CCA but are due to other factors. Factors other than the CCA that might be creating or masking changes in levels of public protection are explored to the extent possible. While this procedure lacks the rigor and appeal of random control, to the extent that other explanations can be ruled out, the more strongly-based are the conclusions on CCA impact. In other words after various decision rules and statistical tests for interpreting results are applied the plausibility of rival explanations for these results is assessed. When no plausible rival explanations can be found, conclusions are more firmly-based; when rival explanations seem plausible, conclusions are less strong.

F. A Search for Converging Evidence

The primary design is a pretest - posttest design comparing the proportions of successes in samples of offenders sentenced before and after CCA entry. Two additional types of data are reported in an effort to find supporting evidence for the primary findings.

1. Estimates of Eventual Failures

One difficulty with the methodology described above is that it does not take full advantage of the follow-up data available. In particular, a person who has not failed by the end of T_2 is considered a success, even though the offender may fail at some

point beyond T_2 . Fixed, comparable follow-up periods are used to assure that pre-and post-CCA cases have an equivalent time period in which to fail.

A possibility exists that if cases were tracked for a longer period of time, eventual success rates would differ from the success rates obtained using fixed follow-up periods. Estimates of eventual success rates are particularly important because of the expected long-term contribution of the CCA. A one-year follow-up might be inadequate to detect the long-term, rehabilitative impact of the CCA. As a result efforts are made to estimate eventual success rates and to determine whether these estimates corroborate the overall findings based on fixed follow-up periods ($T_1 + 2$).

Michael Maltz (1980) has been developing methods to predict eventual failure/success rates using data from variable follow-up periods. For purposes here, the duration of the follow-up period for each individual is divided into one-month intervals. In addition, a "failure" is defined as the first felony offense by an individual after release into the community that leads to a conviction. The particular model of the recidivism process which is applied to the follow-up data for adult offenders is referred to as the "split-population" model. There are three basic assumptions through which this model is developed and applied:

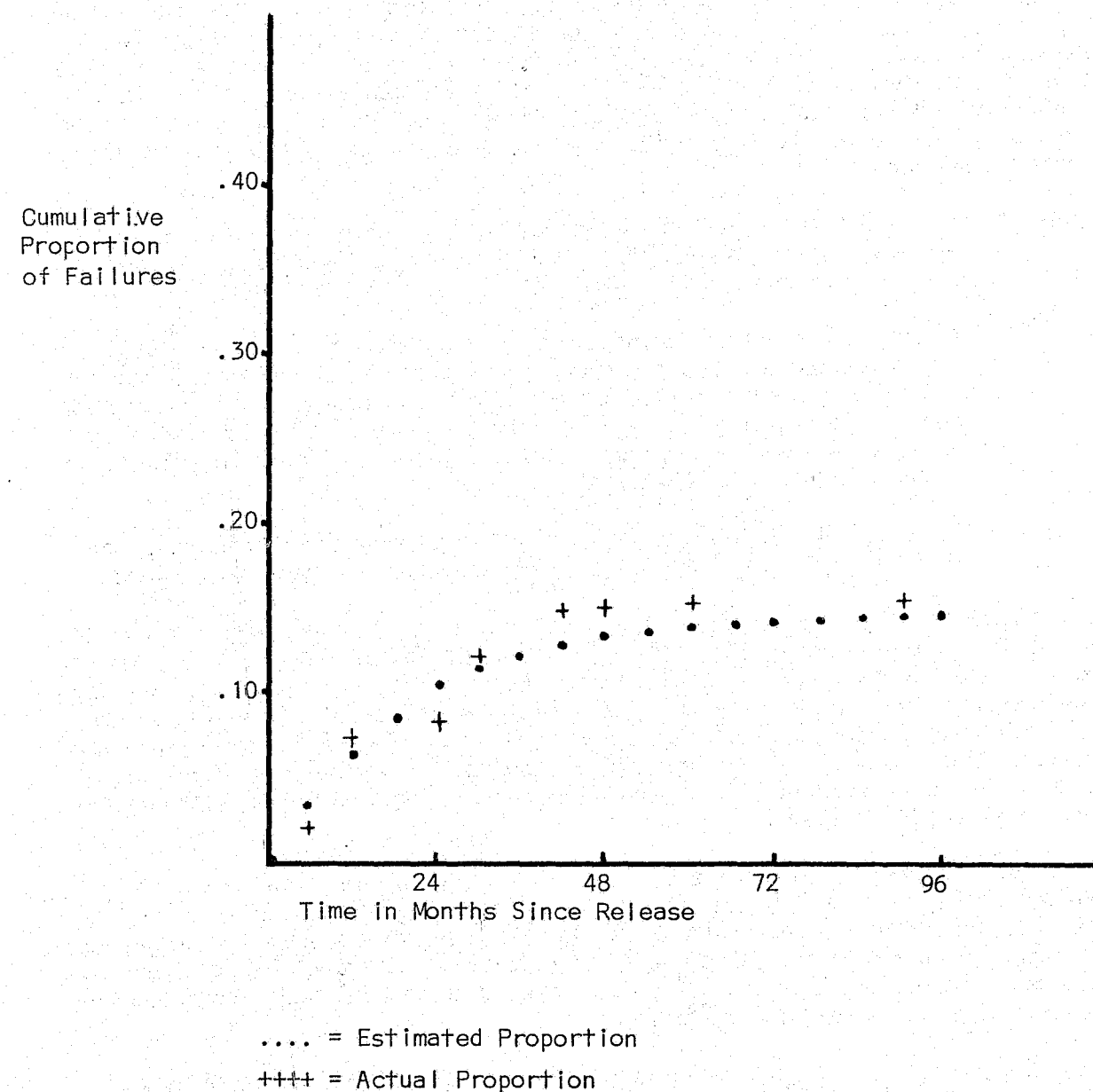
1. Out of a given group of offenders released into the community, two subgroups will emerge - one subgroup consisting of individuals who will not fail again, and a second subgroup whose members will all fail ultimately.
2. Within the subgroup consisting of the eventual failures, the (monthly) rate of failure is constant. At any given point during the follow-up, some number of individuals in this subgroup will not yet have failed. Assumption #2 means that at any such point during the follow-up, the number expected to fail within the next month is a constant proportion of the number of individuals within this subgroup who have not failed yet.
3. With respect to failure or non-failure, each member of the group under study is assumed to act independently of all other members of the group.

Each individual is observed to fail at some number of months after release, or else to exhibit no failure by the end of the follow-up period. Combining data for the observed pattern of failures/non-failures with the assumptions stated above, the modeling technique results in an estimate of the fraction of the total group who will ultimately fail. The constant rate of failure within this subgroup of eventual failures is also estimated. The estimates of the fraction of and rate of failure are based on the method of maximum likelihood. For a more rigorous explanation of the development of this model, see Maltz (1980).

The estimates arrived at using the split-population model are valid to the extent that the stated assumptions hold. Once the estimates for the fraction of failures and the failure rate have been obtained for a particular data set, the estimated (or expected) proportion of failures occurring by each successive month since release can be calculated. These proportions can be plotted over time, tracing out a curve such as that indicated by the dotted line in Figure 5.

At any given number of months since release, the height of this curve reflects the estimated proportion of the overall group having failed by this time. As one possible measure of how well this modeling technique applies to the data, the actual (observed) proportion of failures occurring by each successive month since release can also be

FIGURE 5: Example of Estimated and Actual Proportions of Failures Obtained from the "Split-Population" Model



calculated. These actual proportions are indicated by "+" in Figure 5. The degree to which the two sets of points coincide provides one indicator of how well the "split-population" model can describe the pattern of failures for a particular data set.

The model is estimated for the pre- and post-CCA periods for the early and middle participants. Recent participants are not analyzed using this technique because the follow-up period for many of the individuals post-CCA is considered too short to yield stable estimates. Recent participants are used as comparisons for the early participants, however. Time to failure is defined as the number of months between release into the community and the arrest date for the first post-release felony offense that leads to a conviction. Release dates are established as follows:

1. For individuals receiving community sanctions, the release date is the date of sentencing which resulted in the sanction.
2. For state cases, the release date is the sentencing date plus the time actually incarcerated.
3. Community cases revoked for something other than a felony during the T_1 follow-up period used in the pretest - posttest design are reconsidered state cases. Release begins with release from incarceration. This procedure is consistent with the other public protection analyses.
4. Community cases revoked after T_1 for something other than a new felony are considered to have successful exposure times up to the revocation.

Regardless of the release date, the latest date to which any individual was followed up was 3/1/80. For individuals arrested on or near this date, it is not always clear at the time data collection was stopped (6/1/80) whether or not this arrest resulted in a conviction.

No attempts are made to draw formal statistical inferences from the results of the split-population modeling technique. For example, no statistical tests are applied to determine whether the change in estimated eventual success rates before and after CCA are significant or whether the changes in a CCA area are significantly different from those in a comparison area. Statistical tests appear not to be appropriate for these data. Instead, results are used to indicate whether directions of change support or contradict the overall results based on the fixed follow-up period. When findings converge, conclusions on the impact of the CCA are more firmly based.

2. Aggregate Arrest Rates

In the next section on Juvenile offenders, juvenile arrest rates are reported to provide an indication of CCA impact on public protection. Arrest rates are used because of difficulties in collecting data on individual juvenile offenders. Aggregate arrest rates are recognized to be very imperfect indicators of public safety levels resulting from the CCA. This recognition is particularly true for adult arrests, many of which are not expected to be influenced by the CCA (see Figure 1 in Introduction). Data on adult arrest rates are collected to discover whether results converge with the findings based on samples of offenders. If findings do not converge, conclusions are based on sample data which is believed to provide a much better indication of public protection. If findings do converge, conclusions on CCA impact can be more firmly based for adults. Most important, evidence exists for the juvenile evaluation that arrest rate data provide an adequate measure of public protection.

Arrest data are available from the Bureau of Criminal Apprehension from 1973 through 1979. Numbers of Arrests for felony offenses are tabulated for all eighty-seven counties for each year. When an offense category could contain felonies and non-felonies, analysts estimate the proportion of the category that would be felonies. For adults, the number of felony arrests equals the number of arrests in the offense categories of murder, negligent manslaughter, rape, robbery, aggravated assault, burglary, unauthorized use of motor vehicle, arson, forgery, fraud, and stolen property, as well as 75% of the larceny arrests, 10% of the vandalism arrests, 75% of other sex offenses and 40% of the narcotics offenses.

Estimates of the adult population at-risk are obtained for each county for each year. The population at-risk for adults includes persons from the ages of 18 through 29, the age group which accounts for most arrests. Expanding the upper age limit includes more adults who might be arrested, but it masks year-to-year changes and makes the rate analysis less sensitive to change. Age estimates are based on recent estimates by the State Planning Agency and are used to revise previous estimates of expected county population in 1980. Projected population for other years is simply interpolated using three points in time: the 1970 census, the 1975 estimated population and the corrected 1980 projections.

Arrest rates are obtained by dividing the estimated number of felony arrests each year by the estimated population-at-risk. For multi-county CCA areas, the county data are pooled. For example, the total number of felony arrests in Dodge, Fillmore, and Olmsted in 1973 are divided by the total population at-risk in the three counties in 1973 to obtain an arrest rate for Dodge-Fillmore-Olmsted in 1973. The non-CCA comparison data are also pooled. Thus, for each year, the arrests in all counties that had not yet entered the CCA are divided by the combined population of all counties that had not yet entered the CCA. The non-CCA comparison plots exclude Ramsey and Hennepin data because their large sizes dominate results.

Arrest rates are plotted for each CCA area and for the CCA areas as a group. The non-CCA rates are also plotted for comparison purposes. CCA entry is marked on the plots. One then observes whether arrest rates increase or decline after CCA entry for the CCA area and compares the CCA results to what is happening statewide. Ramsey and Hennepin plots are compared to each other.

With only seven data points (i.e. 1973 through 1979) statistical tests cannot be used to determine whether pre-entry rates are different from post-entry rates or whether changes in a CCA area are different from state-wide changes. Plots are simply inspected to establish whether directions and general magnitude of arrest rate changes tend to corroborate the findings based on sample data.

G. Results

Results on the short-term, long-term and overall impact of the CCA on public protection vis a vis adult offenders are presented below. Results are provided for each CCA area because analyses are conducted by area. Emphasis in this report, however, is on statewide conclusions. The report on the Adult Offender Sample explains that cases are sampled in each CCA area only through 1978 because of the requirement of a follow-up period for assessing post-sentence offender behaviors. As a result, public protection results are not reported for two areas. Rock-Nobles enters the CCA in 1979. No post-CCA cases therefore are available. Washington entered the CCA on July 1, 1978, providing only six months of post-CCA cases. Analyses have been

conducted for Washington, but results are not presented. Six months of post-CCA cases are believed to be inadequate for making inferences on the impact of the CCA.

The results reported below are all based on felony convictions. In no case do results based on arrests provide contradictory conclusions. Adjusted percentages are explained in table footnotes when adjustments are made in the data according to the procedures explained in Section D above.

In the CCA areas for which the split-population model proved useful, the estimates of eventual success rates are reported and compared to the overall success rates based on fixed follow-up periods.

Finally, the arrest rate plots for CCA areas, individually and combined, are presented. The extent to which directions of change indicated by these plots corroborate findings based on samples of offenders is discussed.

1. The Short-term Impact of the CCA

The analysis of the short-term impact of the CCA assesses whether the CCA creates a public risk by reducing the number of offenders incapacitated in prison. Figure 2 in an earlier portion of this report depicts the underlying assumptions of the CCA. In the short term, the CCA is expected to divert more offenders to the community, thus reducing the proportion of less serious offenders committed. The expected short-term increase in the relative size of the community population is not expected to threaten public safety. The test of this assumption is to compare the success rate (i. e. the proportion of successes) among community and less serious state cases before and after CCA entry. The follow-up period for the short-term analysis (T_1) is explained in section 11. A. 3 above.

Pre- and post-CCA success rates are reported in Tables 3 through 6. In all CCA areas except Crow Wing-Morrison, the changes in success rates are not significant. The conclusion, therefore, is that in the short term public protection can be maintained with the Community Corrections Act. In Crow Wing-Morrison, however, an increase in public protection is detected. While this increase is small (+02.9%), it is significant in comparison to two control counties (6 West and Blue Earth) during comparable time periods (see Table 3). The conclusions for the recent participants are tentative because post-CCA success rates are based on only one year of post-CCA cases. With nine of ten areas demonstrating no change in success rates during the short-term follow-up, the evidence is very strong that the CCA does not increase risk to the public in the short term.

2. The Long-term Impact of the CCA

A second assumption is that in the long term the CCA can have a positive impact on public protection because community treatment can better rehabilitate less serious offenders. Again, Figure 2 contains a graphic presentation of this assumption. The expected increase in the relative size of the community population is expected to result in a larger proportion of rehabilitated offenders.

The test of this assumption is to compare success rates (i.e. the proportion of successes) among community and less serious state cases before and after CCA entry. The sample for this analysis differs from the short-term analysis. First, cases sentenced after 4/1/78 are excluded because of the longer follow-up requirement. As a result, the recent participants have an insufficient post-CCA period to be included in

TABLE 3: Short-Term Impact of the CCA on Public Protection in Early Participants

CCA AREA	Pre-CCA Success Rate	Post-CCA Success Rate	Pre-Post Change	Is CCA Change Significantly Different from Two of Three Comparison Counties? ^a	Conclusion
<u>Dodge-Fillmore-Olmsted</u>					
Percent	97.8	94.5		No	Maintain
Sample Size	(98)	(157)	-03.3		
<u>Crow Wing-Morrison</u>					
Percent	94.1	97.0	+02.9	Yes	Increase
Sample Size	(85)	(133)			
<u>COMPARISON AREA</u>					
<u>Region 6 West</u>					
Percent	100	94.6	-05.4		
Sample Size	(48)	(91)			
<u>Blue Earth</u>					
Percent	100	86.3	-13.7		
Sample Size	(59)	(112)			
<u>Washington</u>					
Percent	97.9	96.4	-01.5		
Sample Size	(86)	(153)			

a. Rules for interpreting comparison county data can be found in Section 11. E. 2 above. A Difference-of-Difference of Proportions test is used to determine whether the CCA change is significantly different from the change in a comparison area. A Z of ± 1.96 or greater indicates that a difference is significant at 95% confidence.

TABLE 4: Short-Term Impact of the CCA on Public Protection in Middle Participants

CCA AREA	Pre-CCA Success Rate	Post-CCA Success Rate	Pre-Post Change	Is Pre-Post Change Significant? ^a	Conclusion
<u>Red Lake-Polk-Norman</u>					
Percent	94.8	90.7	-04.1 ^b	No	Maintain
Sample Size	(99)	(140)			
<u>Todd-Wadena</u>					
Percent	95.7	96.7	+01.0	No	Maintain
Sample Size	(56)	(60)			
<u>Arrowhead Regional Corrections</u>					
Percent	95.6	95.7	+00.1	No	Maintain
Sample Size	(234)	(245)			
<u>Anoka</u>					
Percent	93.4	92.6	-00.8	No	Maintain
Sample Size	(221)	(202)			

a. A Difference of Proportions test is used to determine whether the pre-post change is significant. A Z of ± 1.96 or greater indicates a significant change at 95% confidence.

b. The decline in Red Lake-Polk-Norman could be accounted for by incorrectly coding some non-felonies. See Section 11. D. 3. The decline is not significant, however, and does not alter the conclusion of maintenance.

TABLE 5: Short-Term Impact of the CCA on Public Protection in Recent Participants^a

CCA AREA	Pre-CCA Success Rate	Post-CCA Success Rate	Pre- Post Change	Is CCA Change Significant?	Conclusion
<u>Region 6 West</u>					
Percent	96.4	97.7	+01.3	No	Maintain
Sample Size	(139)	(44)			
<u>Blue Earth</u>					
Percent	91.3	93.5	+02.2	No	Maintain
Sample Size	(171)	(46)			

a. Dodge-Fillmore-Olmsted and Crow Wing-Morrison are not used as comparisons for the recent participants because of the evidence that the CCA does appear to affect success rates in both areas. Although the yearly sample data lack precision, the time-series plots of both areas indicate a downward trend in T₁ success rates. This trend starts immediately in DFO but begins later in CWM. Because of the caveats on using early participants as comparisons explained in Section 11. C. 1 above, it was decided that DFO and CWM are not valid controls for what is happening state-wide without the CCA. It should be noted that if comparisons were used the conclusion would be an increase in public protection.

TABLE 6: Short-Term Impact of the CCA on Public Protection in Ramsey and Hennepin Counties

CCA AREA	Pre-CCA Success Rate	Post-CCA Success Rate	Pre- Post Change	Is CCA Change Significantly Different from Comparison County? ^a	Conclusion
<u>Ramsey</u>					
Percent	95.4	94.7	-00.7	No	Maintain
Sample Size	(180)	(233)			
<u>COMPARISON AREA</u>					
<u>Hennepin</u>					
Percent	94.8	95.3	+00.5		
Sample Size	(241)	(265)			

<u>CCA AREA</u>					
<u>Hennepin</u>					
Percent	95.3	93.6	-01.7	No	Maintain
Sample Size	(265)	(235)			
<u>COMPARISON AREA</u>					
<u>Ramsey</u>					
Percent	94.7	94.4	-00.3		
Sample Size	(233)	(197)			

a. Rules for interpreting comparison county data can be found in Section 11. E. 2 above. A Difference-of-Difference of Proportions test is used to determine whether the CCA change is significantly different from the change in a comparison area. A Z of ± 1.96 or greater indicates that a difference is significant at 95% confidence.

analysis. Second, offenders who fail during T_1 are no longer available to fail in T_2 and, therefore, are not part of the T_2 sample. This second assumption is tested with offenders who successfully complete their incarceration or community alternative (i.e. successfully complete T_1) in the early and middle participants. Thus, the sample sizes and the county areas reported in Tables 7 through 9 differ from those in Tables 3 through 6.

The long-term success rates are reported in Tables 7 through 9. Five of the seven CCA areas maintain public protection in the long term but do not demonstrate the hypothesized improvement. Dodge-Fillmore-Olmsted and Crow Wing-Morrison, on the other hand, experience a decline in offender success rates. Results from all seven areas suggest that the rehabilitation argument is not supported. With five of seven areas maintaining offender success rates, the statewide conclusion is that in the long term, public protection can be maintained but not improved with the CCA.

3. The Overall Impact of the CCA

The evaluation of public protection is less concerned with which of the CCA's assumptions is supported than with discovering the net impact of the CCA on public safety. Taking the short term and the long term into consideration, what is the overall contribution of the CCA to public protection?

The test for the overall impact is to compare success rates (i.e. the proportion of successes) before and after CCA entry during the combined short-term and long-term follow-up periods (T_{1+2}). The samples exclude cases sentenced after 4/1/78 because of the follow-up requirement. Therefore, the recent participants cannot be included in analyses. Tables 10 through 12 report success rates for the combined follow-up periods.

In all seven areas analyzed, the net impact of the CCA is to maintain public protection. The pattern is clear — overall, public protection is maintained but not improved with the CCA.

The maintenance of public protection for Dodge-Fillmore-Olmsted is based on a partial verification of the follow-up data. The original data indicated a very large, significant decline that was out of line with other CCA areas. Because of the unrepresentative decline, data were sent to Dodge-Fillmore-Olmsted for verification. Using additional data sources local personnel checked the post-CCA failures to see if any were not felonies and checked pre-CCA successes to see if the original coding had missed some failures. The partial verification has the potential to bias results because post-CCA successes and the comparison data have not been checked in a comparable manner. The analysis based on the partially verified data reduces the decline, primarily by changing a few pre-CCA cases originally coded as successes to failures. This decline is significant and continues to be the largest of all CCA areas, but the decline is no longer significant in comparison to two of three comparison areas. Although the comparison data may no longer be comparable, use of the comparison data produces the conclusion that the decline in success rates is not due to the CCA.

If the changes made in the Dodge-Fillmore-Olmsted data reflect random error this partial verification would bias results in the direction of inferring no public protection decline. That is, if errors are random, one would expect similar changes to be made to the post-CCA successes and to the comparison data; one would expect the reanalysis to produce conclusions virtually identical to the initial analysis. On the other hand, if

TABLE 7: Long-Term Impact of the CCA on Public Protection in Early Participants

CCA AREA	Pre-CCA Success Rate	Post-CCA Success Rate	Pre-Post Change	Is CCA Change Significantly Different from Two of Three Comparison Counties? ^a	Conclusion
<u>Dodge-Fillmore-Olmsted</u>					
Percent	94.6	87.2	-07.4	Yes	Decrease
Sample Size	(96)	(162)			
<u>Crow Wing-Morrison</u>					
Percent	97.3 ^b	93.2 ^{b,c}	-04.1	Yes	Decrease
Sample Size	(73)	(146)			
<u>COMPARISON AREA</u>					
<u>Region 6 West</u>					
Percent	100	87.2	-12.8		
Sample Size	(48)	(86)			
<u>Blue Earth</u>					
Percent	89.8 ^b	97.9	+08.1		
Sample Size	(49)	(97)			
<u>Washington</u>					
Percent	83.0	94.3	+11.3		
Sample Size	(84)	(167)			

- Rules for interpreting comparison county data can be found in Section 11. E. 2 above. A Difference-of-Difference of Proportions test is used to determine whether the CCA change is significantly different from the change in a comparison area. A Z of ± 1.96 or greater indicates that a difference is significant at 95% confidence.
- Percentage and sample size include adjustments for a non-comparable proportion of the sample not being at-risk to fail. See Section 11. D. 2 above.
- Percentage includes adjustment for cases with inadequate follow-up periods. See Section 11. D. 1 above.

TABLE 8: Long-Term Impact of the CCA on Public Protection in Ramsey County

CCA AREA	Pre-CCA Success Rate	Post-CCA Success Rate	Pre-Post Change	Is CCA Change Significantly Different from Comparison County? ^a	Conclusion
<u>Ramsey</u>					
Percent	88.7	88.5	-00.2	No	Maintain
Sample Size	(171)	(280)			
<u>COMPARISON AREA</u>					
<u>Hennepin</u>					
Percent	88.2	91.5	+03.3		
Sample Size	(228)	(252)			

a. A Difference-of-Difference of Proportions test is used to determine whether the CCA change is significantly different from the change in a comparison area. A Z of ± 1.96 or greater indicates that a difference is significant at 95% confidence.

TABLE 9: Long-Term Impact of the CCA on Public Protection in Middle Participants

CCA AREA	Pre-CCA Success Rate	Post-CCA Success Rate	Pre-Post Change	Is Pre-Post Change Significant? ^a	Conclusion
<u>Red Lake-Polk-Norman</u>					
Percent	94.2	92.1	-02.1	No	Maintain
Sample Size ^b	(94)	(96)			
<u>Todd-Wadena</u>					
Percent	100	100	00.0	No	Maintain
Sample Size ^b	(54)	(50)			
<u>Arrowhead Regional Corrections</u>					
Percent	93.1	91.6	-01.5	No	Maintain
Sample Size ^b	(224)	(174)			
<u>Anoka</u>					
Percent	95.7	91.0	-04.7 ^c	No	Maintain
Sample Size ^b	(207)	(122)			

a. A Difference of Proportions test is used to determine whether the pre-post change is significant. A Z of ± 1.96 or greater indicates a significant change at 95% confidence.

b. Sample includes cases who have not failed by the end of T₁; i.e. those who successfully complete their incarceration or community alternative.

c. See section below on converging evidence for possible reasons for this non-significant decline.

TABLE 10: Overall Impact of the CCA on Public Protection
in Early Participants

CCA AREA	Pre-CCA Success Rate	Post-CCA Success Rate	Pre- Post Change	Is CCA Change Significantly Different from Two of Three Comparison Counties? ^a	Conclusion
<u>Dodge-Fillmore-Olmsted</u>					
Percent	92.5	82.0	-10.5	No	Maintain ^e
Sample Size	(98)	(172)			
<u>Crow Wing-Morrison</u>					
Percent	89.8 ^b	88.2 ^{b,c}	-01.6	No ^d	Maintain
Sample Size	(79)	(153)			
<u>COMPARISON AREA</u>					
<u>Region 6 West</u>					
Percent	100	82.9	-17.1		
Sample Size	(48)	(91)			
<u>Blue Earth</u>					
Percent	85.7 ^b	84.5	-01.2		
Sample Size	(49)	(112)			
<u>Washington</u>					
Percent	81.2	90.9	+09.7		
Sample Size	(86)	(173)			

- a. Rules for interpreting comparison county data can be found in Section 11. E. 2 above. A Difference-of-Difference of Proportions test is used to determine whether the CCA change is significantly different from the change in a comparison area. A Z of ± 1.96 or greater indicates that a difference is significant at 95% confidence.
- b. Percentage and sample size include adjustments for a non-comparable proportion of the sample not at-risk to fail. See Section 11. D. 2 above.
- c. Percentage includes adjustment for cases with inadequate follow-up periods. See Section 11. D. 1 above.
- d. Crow Wing-Morrison is significantly different from Region 6 West and Washington but in opposite directions.
- e. The change based on arrests rather than convictions is significantly different from two of three comparison counties conclusions, however, are based on on conviction data. See Section 11. B. above.

TABLE 11: Overall Impact of the CCA on Public Protection
in Ramsey County

CCA AREA	Pre-CCA Success Rate	Post-CCA Success Rate	Pre- Post Change	Is CCA Change Significantly Different from Comparison County? ^a	Conclusion
<u>Ramsey</u>					
Percent	84.6	84.3	-00.3	No	Maintain
Sample Size	(180)	(294)			
<u>COMPARISON AREA</u>					
<u>Hennepin</u>					
Percent	83.7	87.2	+03.5		
Sample Size	(241)	(265)			

- a. A Difference-of-Difference of Proportions test is used to determine whether the CCA change is significantly different from the change in a comparison area. A Z of ± 1.96 or greater indicates that a difference is significant at 95% confidence.

TABLE 12: Overall Impact of the CCA on Public Protection
in Middle Participants

CCA AREA	Pre-CCA Success Rate	Post-CCA Success Rate	Pre- Post Change	Is Pre-Post Change Significant? ^a	Conclusion
<u>Red Lake-Polk-Norman</u>					
Percent	89.3	86.7	-02.6	No	Maintain
Sample Size	(99)	(102)			
<u>Todd-Wadena</u>	95.7	98.0	+02.3	No	Maintain
Sample Size	(56)	(51)			
<u>Arrowhead Regional Corrections</u>					
Percent	89.0	87.2	-01.8	No	Maintain
Sample Size	(234)	(183)			
<u>Anoka</u>					
Percent	89.5	82.5	-07.0	No ^b	Maintain ^b
Sample Size	(221)	(135)			

a. A Difference of Proportions test is used to determine whether the pre-post change is significant. A Z of ± 1.96 or greater indicates a significant change at 95% confidence.

b. The Z for Anoka is 1.91, almost significant at the .05 level. However, the section on converging evidence below notes that factors other than the CCA could explain the Anoka decline, reinforcing the conclusion of maintenance rather than decline.

the changes reflect a systematic reporting problem unique to Dodge-Fillmore-Olmsted's pre-CCA cases, then this partial verification of the data eliminates that unique, systematic error and revised results would be more accurate. Analysts have assumed that the changes made in the follow-up data correct systematic error unique to Dodge-Fillmore-Olmsted for two reasons. First, there is no plausible explanation for why Dodge-Fillmore-Olmsted should experience the atypical decline initially found. There is no plausible explanation for why Dodge-Fillmore-Olmsted should appear so different from the other CCA areas. Second, a careful review of the follow-up coding for all CCA areas suggests that underreporting to the BCA in 1972, 1973 and 1974 (DFO's pre-CCA years) appears to be a more extreme problem in Dodge-Fillmore-Olmsted. This reporting factor may not have been adequately controlled by the comparison counties in the original analysis.

4. Summary of Findings of CCA Impact Based on the Adult Offender Sample

Table 13 provides a summary of the public protection findings based on the Adult Offender Sample. In general, the short-term, long-term and overall impact of the CCA is to maintain public protection. The short-term data tend to offset the long-term declines in Crow Wing-Morrison and Dodge-Fillmore-Olmsted, producing overall conclusions of maintenance.

The assumption that the CCA does not increase the short-term risk to the public appears to be supported. The analysis of the long-term, however, provides no evidence of a rehabilitative effect. Although the long-term follow-up period tracks offenders for only one year past incarceration or estimated incarceration and although new felony convictions (or arrest) may be an imperfect measure of rehabilitation, the evidence available fails to support the assumption that the CCA can improve public protection. With only the short-term assumption supported, the net effect of the CCA is to maintain but not to increase public protection.

5. Converging Evidence— Estimates of Eventual Success Rates

The split-population model is applied to the follow-up data in the early and middle participants to estimate eventual success rates. The Pre-CCA data do not converge with the model in the comparison areas of Blue Earth and Region 6 West. Although estimates are available for Todd-Wadena, they appear too variable to be considered reliable. The model does provide estimates for:

1. Ramsey in comparison to Hennepin
2. Dodge-Fillmore-Olmsted in comparison to Washington
3. Crow Wing-Morrison in comparison to Washington
4. Red Lake-Polk-Norman
5. Arrowhead Regional Corrections
6. Anoka

Table 14 contains the estimated eventual success rates for these areas. The success rate change based on the fixed follow-up periods is included for comparison.

Some caveats are important for interpreting the results. The data used are sample data from populations of offenders, but inferences to the populations are not possible. Since it is not known whether parameter estimates are normally distributed, tests of statistical significance are inappropriate. Also, the validity of the estimates depend in part upon how well the data fit the model. The fit varies for the areas for which estimates are available. Generally, the estimated success rate findings corroborate more strongly the fixed follow-up findings the better the data fit the model.

TABLE 13: Summary of Public Protection Findings Based on the Adult Offender Sample

CCA Area	Short-Term Impact	Long-Term Impact	Overall Impact
Dodge-Fillmore-Olmsted	Maintain	Decrease	MAINTAIN
Crow Wing-Morrison	Increase	Decrease	MAINTAIN
Ramsey	Maintain	Maintain	MAINTAIN
Red Lake-Polk-Norman	Maintain	Maintain	MAINTAIN
Todd-Wadena	Maintain	Maintain	MAINTAIN
Arrowhead Regional Corrections	Maintain	Maintain	MAINTAIN
Anoka	Maintain	Maintain	MAINTAIN
Region 6 West	Maintain	N. A.	N. A.
Hennepin	Maintain	N. A.	N. A.
Blue Earth	Maintain	N. A.	N. A.
Summary State-wide	MAINTAIN	MAINTAIN	MAINTAIN

Results are generally corroborative, although there are exceptions. Also, changes tend to be more magnified with eventual success rates. The greater variability is not surprising. Not only is there a range of error surrounding the estimates, but the estimates are also based on sample data which themselves have a range of error.

The split-population model appears to be best suited for the Ramsey and Hennepin data. The data fit the model well and estimates appear reliable. In this case where the model appears to be most appropriate, results are supportive of the fixed follow-up findings. The conclusion based on the fixed follow-up data is that public protection is maintained in Ramsey. The estimated eventual success rate does show an increase but since Hennepin demonstrates an even larger increase the conclusion of maintenance remains. Unfortunately it is not possible to determine whether the increase in Hennepin is significantly greater than Ramsey's.

The model appears to be less appropriate, but usable, for the remaining four areas. For three of the five areas results are supportive. The Dodge-Fillmore-Olmsted and Washington results tend to corroborate the fixed follow-up results. Both analyses show a decline for Dodge-Fillmore-Olmsted but an increase in Washington. The Crow Wing-Morrison and Washington results also tend to corroborate the fixed follow-up results. Both analyses show virtually no change in Crow Wing-Morrison, but an increase in Washington. Although estimates could not be made for the comparison counties of Blue Earth and 6 West, evidence available indicates that the conclusions of maintenance in Dodge-Fillmore-Olmsted and Crow Wing-Morrison are supported. Also, the results for Arrowhead Regional Corrections support the negative (but not significant) decline based on the fixed follow-up data. The direction of change is the same but again the change is more magnified.

Anoka and Red Lake-Polk-Norman findings are less supportive. The divergence of Anoka results is explicable. In fact, the increase in estimated success rates is probably more accurate than the negative (although not significant) decrease based on fixed follow-ups. Because the changes based on the estimated eventual success rates tend to be more magnified in all areas, this Anoka increase should not be used to conclude there is a significant increase in public protection. However, the positive direction of change is probably more accurate than the negative direction.

Anoka provides an excellent example of the importance of probing rival explanations for changes. The decline in success rates based on fixed follow-ups in Anoka is almost significant and almost produces a conclusion of decline. However, other events occur in Anoka at the same time as CCA entry which probably affect success rates. The Welfare Fraud Unit was initiated and the major Crime Investigation Unit increased its activities just prior to CCA entry. The effect of these units is to increase arrests and convictions primarily after CCA entry. When one uses fixed follow-up periods, post-CCA cases are almost exclusively affected by these activities. Pre-CCA cases are affected much later in their follow-ups. This delayed effect is picked up when the eventual success rates are estimated because all follow-up data are used.

In Red Lake-Polk-Norman, estimated success rates jump dramatically but the fixed follow-up data provide a conclusion of maintenance. The Red Lake-Polk-Norman data do not fit the model well, producing less reliable estimates. This poor fit perhaps explains part of the divergence. Because of the potential for positive rather than neutral findings, however, the divergence will be explored further in the special Red Lake-Polk-Norman report.

TABLE 14: Eventual Success Rates Estimated from the Split-Population Model

<u>Area</u>	<u>Estimated Pre-CCA Success Rates</u>	<u>Estimated Post-CCA Success Rates</u>	<u>Estimated Pre-Post Change</u>	<u>Pre-Post Change Based on Fixed Follow-up</u>	<u>Do Findings Corroborate Fixed Follow-up Findings</u>
Ramsey compared to Hennepin	65.7 63.4	72.8 78.8	+ 7.1 +15.4	-00.3 +03.5	Yes
Dodge-Fillmore-Olmsted compared to Washington	81.7 66.4	77.0 84.3	- 4.7 +17.9	-10.5 +09.7	Yes
Crow. Wing-Morrison compared to Washington	81.9 66.4	81.9 84.3	00.0 +17.9	-01.6 +09.7	Yes
Arrowhead Regional Corrections	75.1	64.8	-10.3	-01.8	Yes
Anoka	74.5	86.8	+12.3	-07.0	No-Explicable
Red Lake-Polk-Norman	53.5	81.5	28.0	-02.6	No

In summary, estimates of eventual rates tend to support the findings based on fixed follow-up data. Pre-Post differences, both positive and negative, tend to be magnified. Whether these larger differences are, in fact, statistically significant, however, cannot be determined. The estimated eventual success rates help to elucidate a problem in the Anoka fixed follow-up data. Only in Red Lake-Polk-Norman are there clearly contradictory findings.

6. Converging Evidence — Aggregate Arrest Rates

Because arrest data were collected for the juvenile evaluation, adult arrest results are available as supplementary evidence. With arrest rate results, lower arrest rates are indicative of higher levels of public protection.

Table 15 contains a summary of the mean pre-CCA and post-CCA arrest rates for each area in comparison to non-CCA counties. The numbers reported are the number of arrests per 1,000 population-at-risk. Year of entry is treated as a post-CCA year. The non-CCA comparison data are aggregated into pre-CCA and post-CCA periods for each CCA area. The non-CCA data reported in this table and in the following figures include CCA counties except Ramsey and Hennepin prior to their entry. Purely non-CCA data also have been tabulated and provide virtually identical results. The percentage change found in the non-CCA counties is applied to each area's pre-CCA rate to predict the number of arrests had an area not participated in the CCA. The difference between the predicted and the actual provides an estimate of the impact of the CCA on arrest rates. Negative numbers indicate that the impact is negative -- there are more arrests with the CCA; positive numbers indicate a positive impact. Table 15 is a convenient summary of the data, but time-series plots are more informative because they reveal trends as well as levels. Also, the summary pre-CCA and post-CCA averages can be greatly affected by one deviant year while such deviations can easily be detected in a time-series plot. Figures 7 through 16 provide plots of arrest rates for each CCA area in comparison to the non-CCA counties. The plots of the CCA areas are expected to fluctuate more than the non-CCA plots because the latter are smoothed by aggregating data from more counties. Figure 6 contains a plot for the CCA areas as a group in comparison to non-CCA counties.

Inspection of Table 15 and Figures 6 through 16 suggests that there is an increase in arrest rates (i.e. a decline in public protection) after CCA. However, the figures indicate that this trend is occurring in non-CCA areas and generally appears to begin before CCA entry. While the differences between actual and predicted arrests in Table 15 suggest that the increases in CCA areas tend to be slightly greater than non-CCA areas, close inspection of the time-series plots generally does not reveal situations in which the CCA areas look noticeably different from non-CCA areas. The general conclusion, therefore, is that the CCA is having no consistent negative or positive effect on public protection. That is, public protection is maintained with the CCA.

A few deviations from these general remarks are worth noting. First, Ramsey's data (Figure 16) reveal no real trend pre- or post-CCA. Apparently Ramsey's data are subject to serious reporting problems in the early and middle 1970's, however, so the plots may contain considerable error. The Red Lake-Polk-Norman plots (Figure 9) demonstrate a sharp increase in arrests in 1975 and 1976 and then a decline. The decline is perhaps best described as a return to the pre-CCA mean, rather than a decline caused by the CCA. The Anoka data (Figure 12) clearly reveal the effects of the Welfare Fraud and Crime Unit activities mentioned in an earlier section. Anoka arrest rates follow exactly the non-CCA line through 1975. The level of arrests jumps

TABLE 15: Pre-CCA and Post-CCA Adult Arrest Rate -- Number of Arrests per 1,000 Adults At-Risk

CCA Area	Pre-CCA Arrests	Post-CCA Arrests	Predicted Post-CCA Arrests ^a	Predicted-Actual Arrests	Public Protection Conclusion
Dodge-Fillmore-Olmsted	15	22.1	17.9	-4.2	DECREASE
Non-CCA	13.5	16.1			
Crow Wing-Morrison	24	26.7	28.6	+1.9	INCREASE
Non-CCA	13.5	16.1			
Red Lake-Polk-Norman	14.3	14.8	15.8	+1.0	INCREASE
Non-CCA	14.8	16.4			
Todd-Wadena	7.5	11.2	8.3	-2.9	DECREASE
Non-CCA	14.8	16.4			
Arrowhead Regional Corrections	17	20.5	18.8	-1.7	DECREASE
Non-CCA	14.8	16.4			
Anoka	15.2	19.2	16.8	-2.4	DECREASE
Non-CCA	14.8	16.4			
Region 6 West	7.9	13.3	8.8	-4.5	DECREASE
Non-CCA	15	16.7			
Blue Earth	14.9	16.3	17.1	+ .8	INCREASE
Non-CCA	15.1	17.3			
Washington	9.2	11.8	10.5	-1.3	DECREASE
Non-CCA	15.1	17.3			
Ramsey	15.5	20.4	14.2	-6.2	DECREASE ^b
Hennepin	25.5	23.3			
Hennepin	22.2	26.8	21.6	-5.2	DECREASE ^b
Ramsey	19.9	19.3			

a

The predicted post-CCA value is obtained by multiplying the pre-CCA arrest rate by the percentage change found in the Non-CCA counties. The Non-CCA data exclude Ramsey and Hennepin Counties. The predicted value for Ramsey is based on the change in Hennepin and vice versa.

b

The size and perhaps even direction of changes in Ramsey and Hennepin could be due to reporting problems in Ramsey in the early 1970's

FIGURE 6: ADULT FELONY ARREST RATES

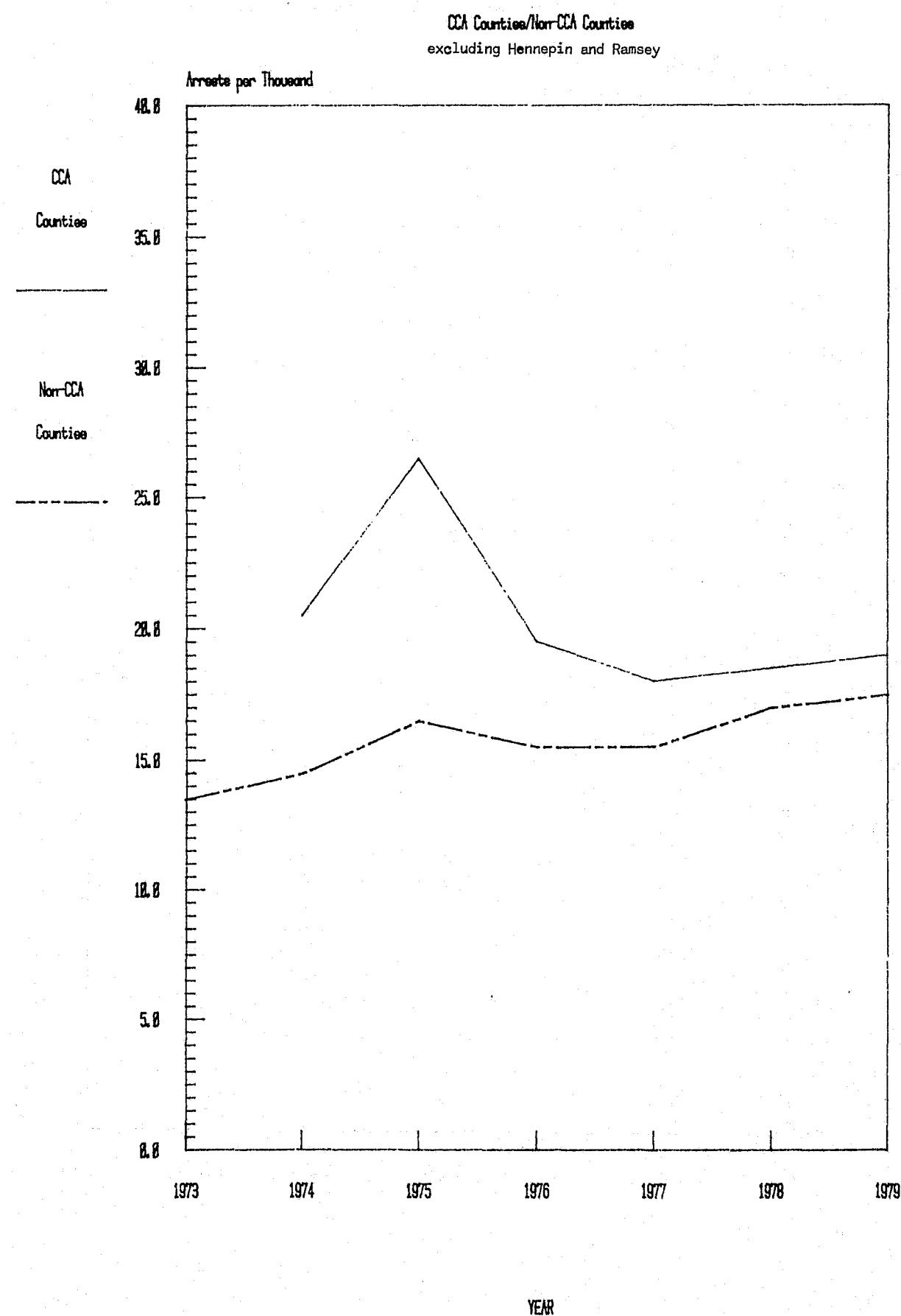


FIGURE 7: ADULT FELONY ARREST RATES

Dodge-Fillmore-Olmsted/Non-CCA Counties

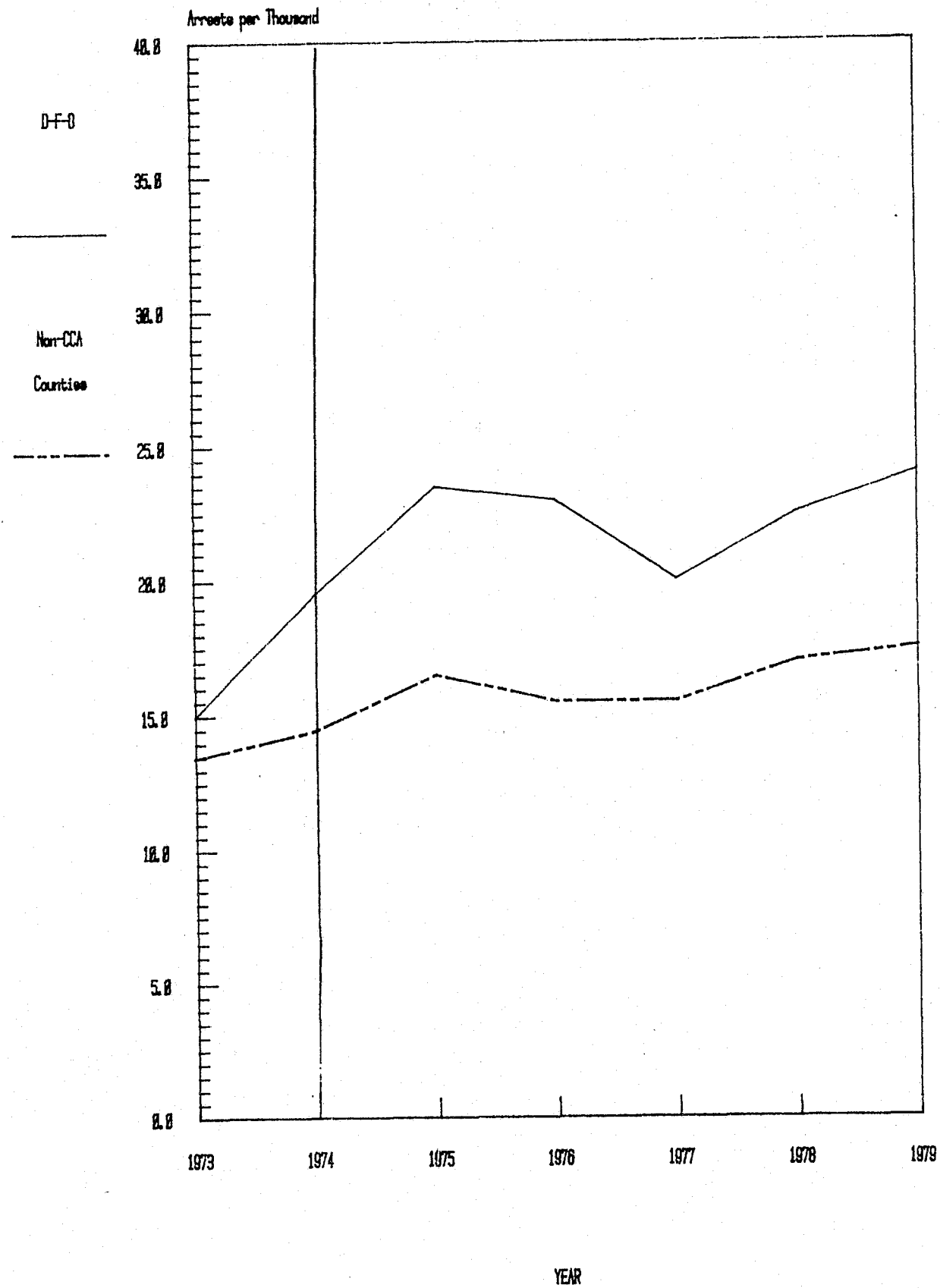


FIGURE 8: ADULT FELONY ARREST RATES

Crow Wing-Harrison / Non-CCA Counties

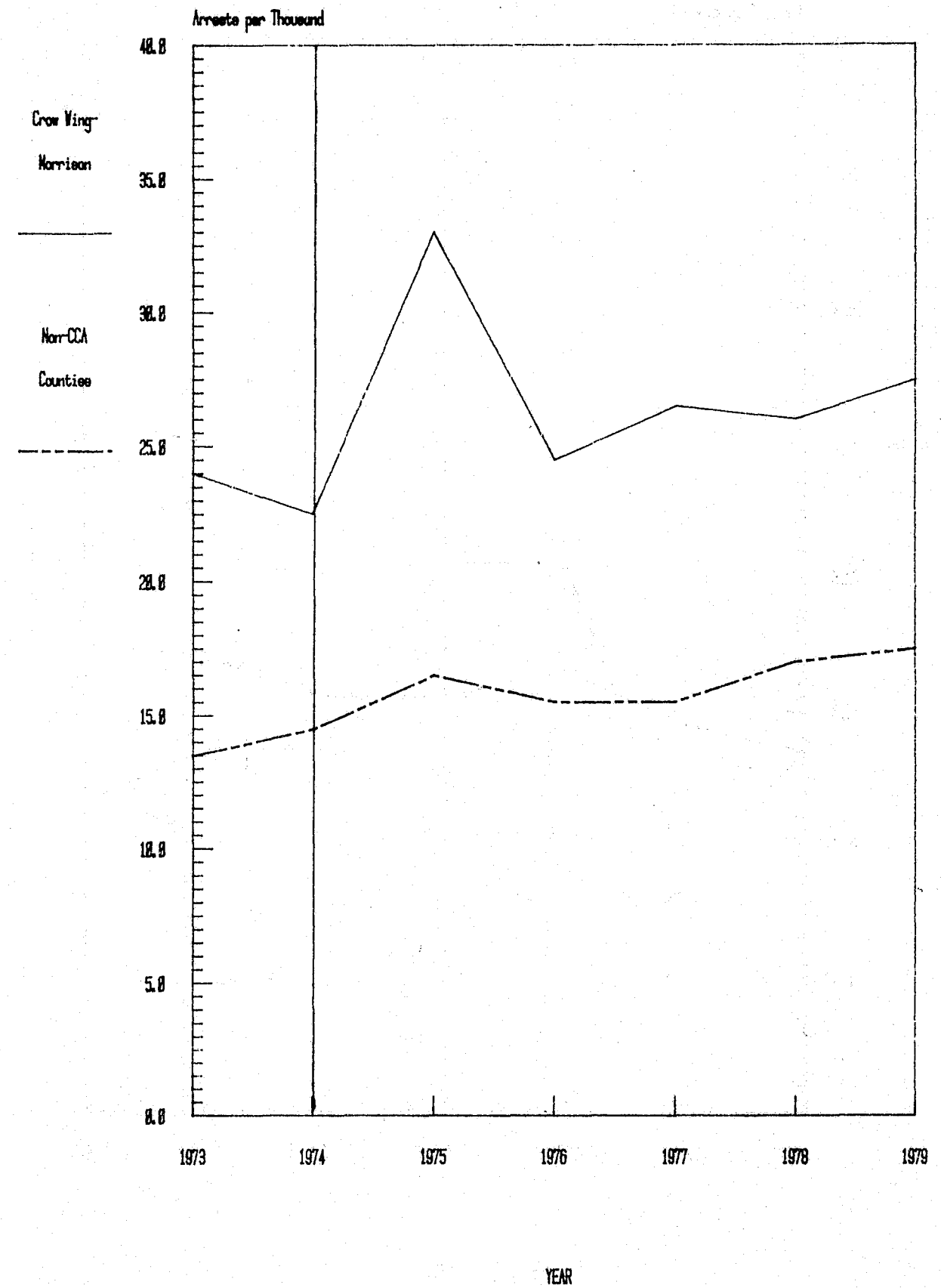


FIGURE 9: ADULT FELONY ARREST RATES

Red Lake-Polk-Norman/Narr-CCA Counties

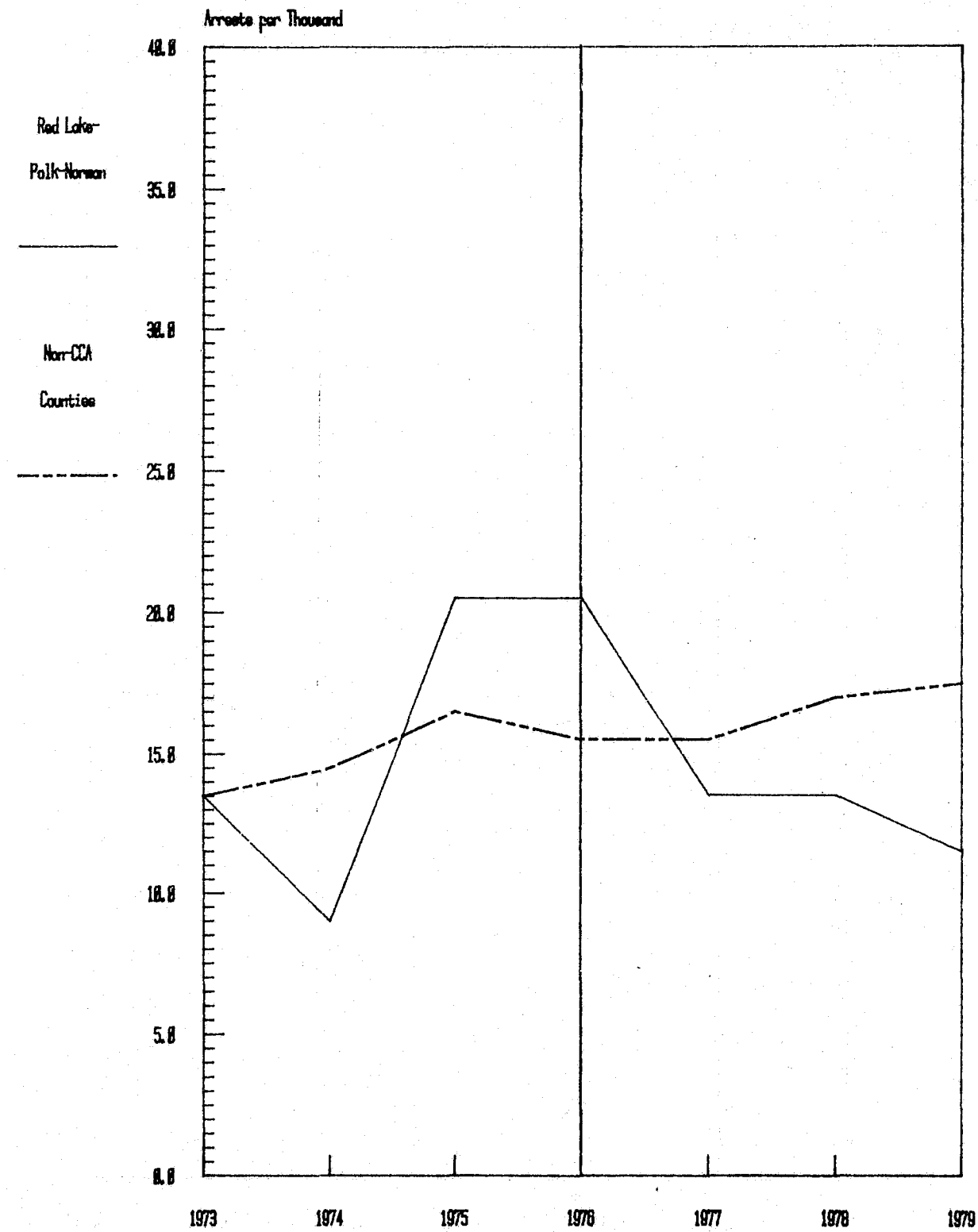
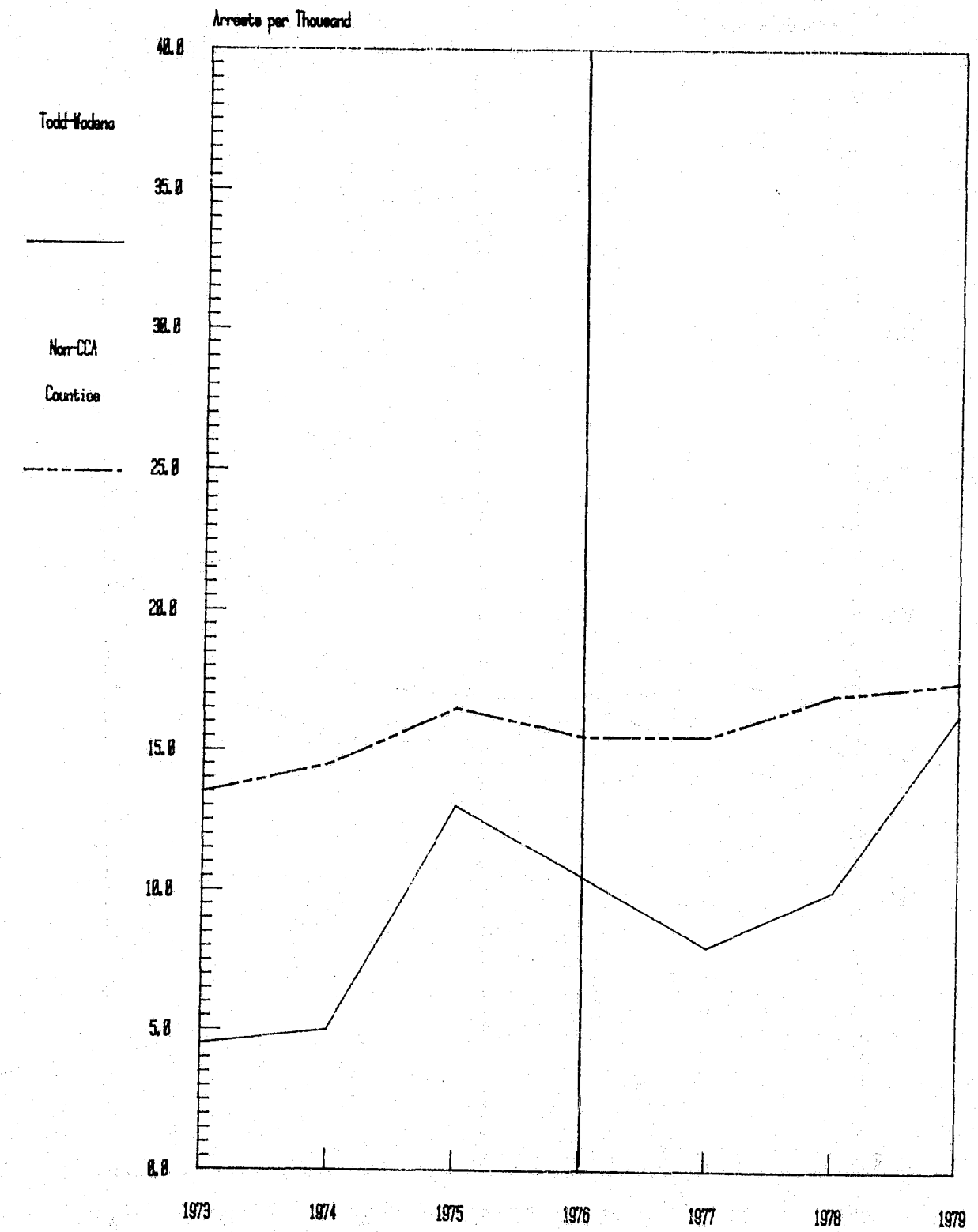


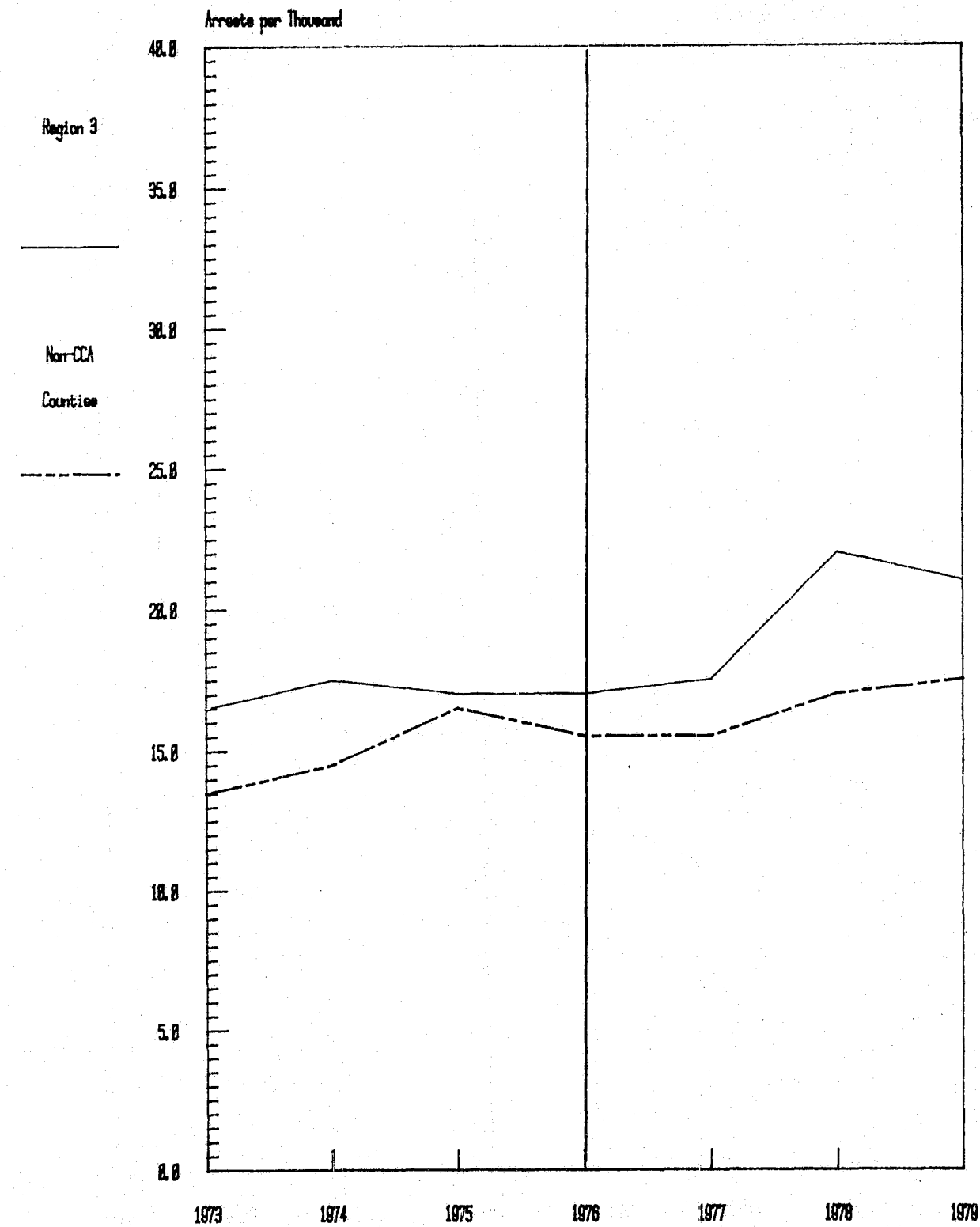
FIGURE 10: ADULT FELONY ARREST RATES

Todd-Madeno / Narr-CCA Counties



46
FIGURE 11: ADULT FELONY ARREST RATES

Region 3 / Non-CCA Counties



47
FIGURE 12: ADULT FELONY ARREST RATES

Anoka / Non-CCA Counties

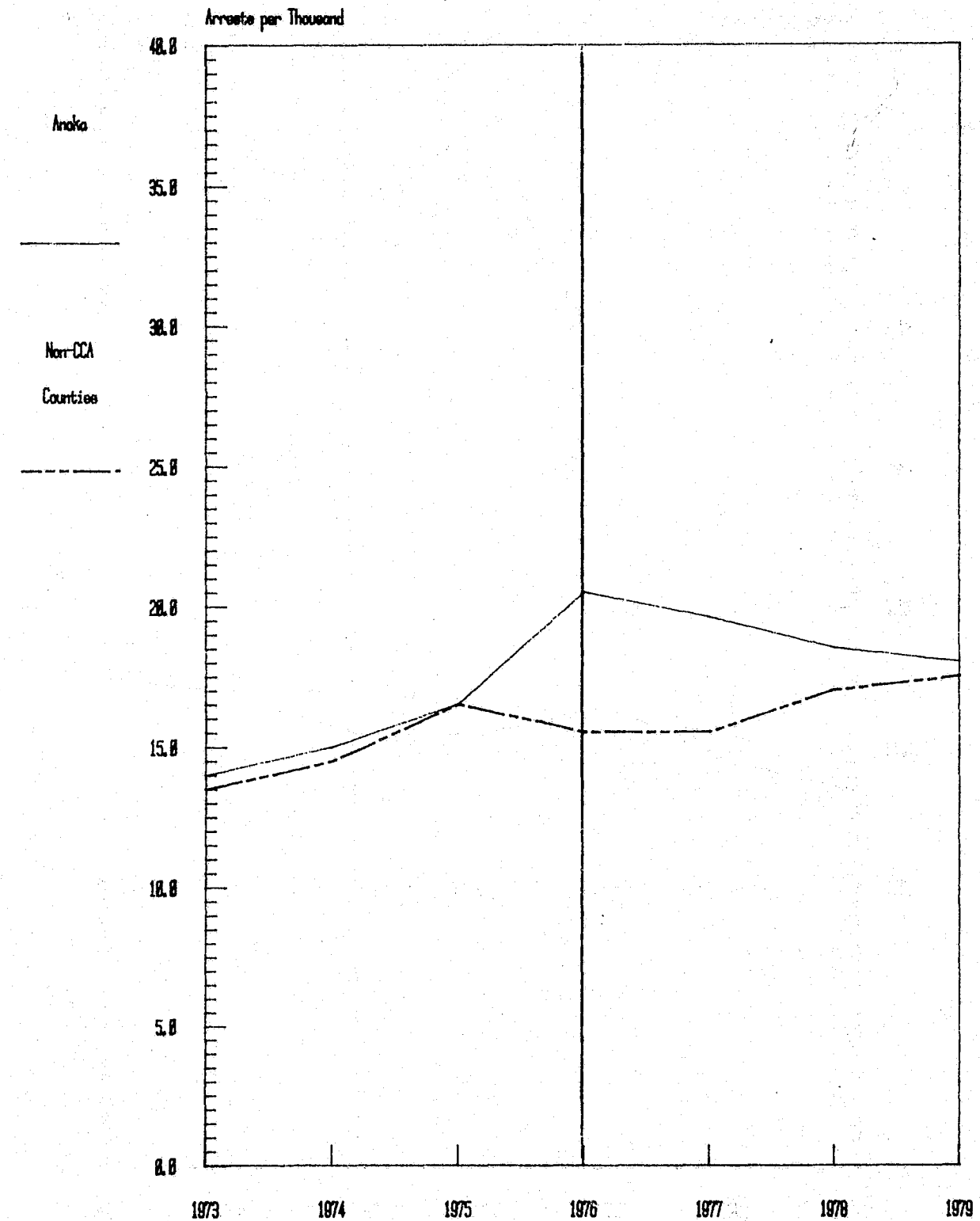


FIGURE 13: ADULT FELONY ARREST RATES

Region 6-W / Non-CCA Counties

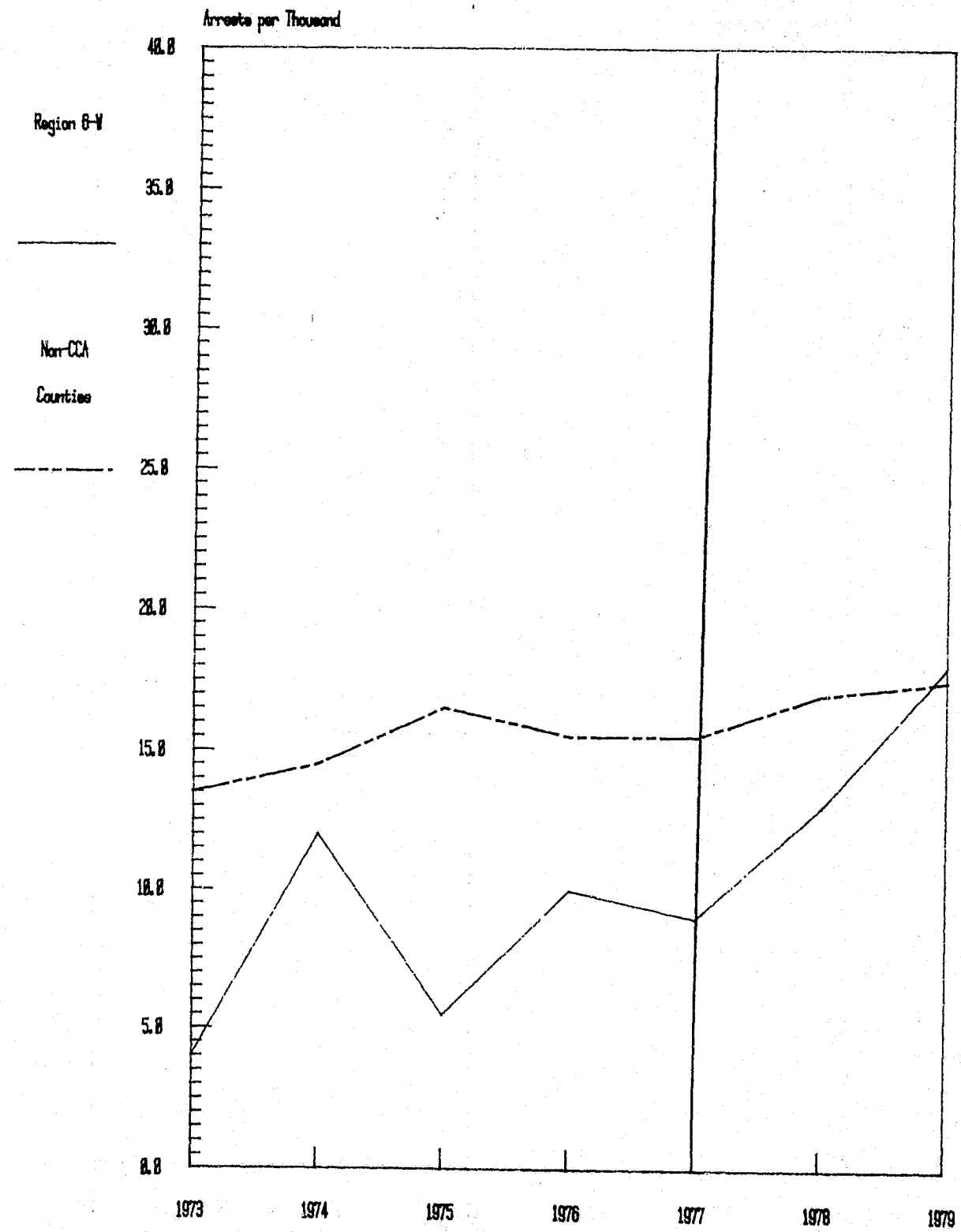


FIGURE 14: ADULT FELONY ARREST RATES

Blue Earth / Non-CCA Counties

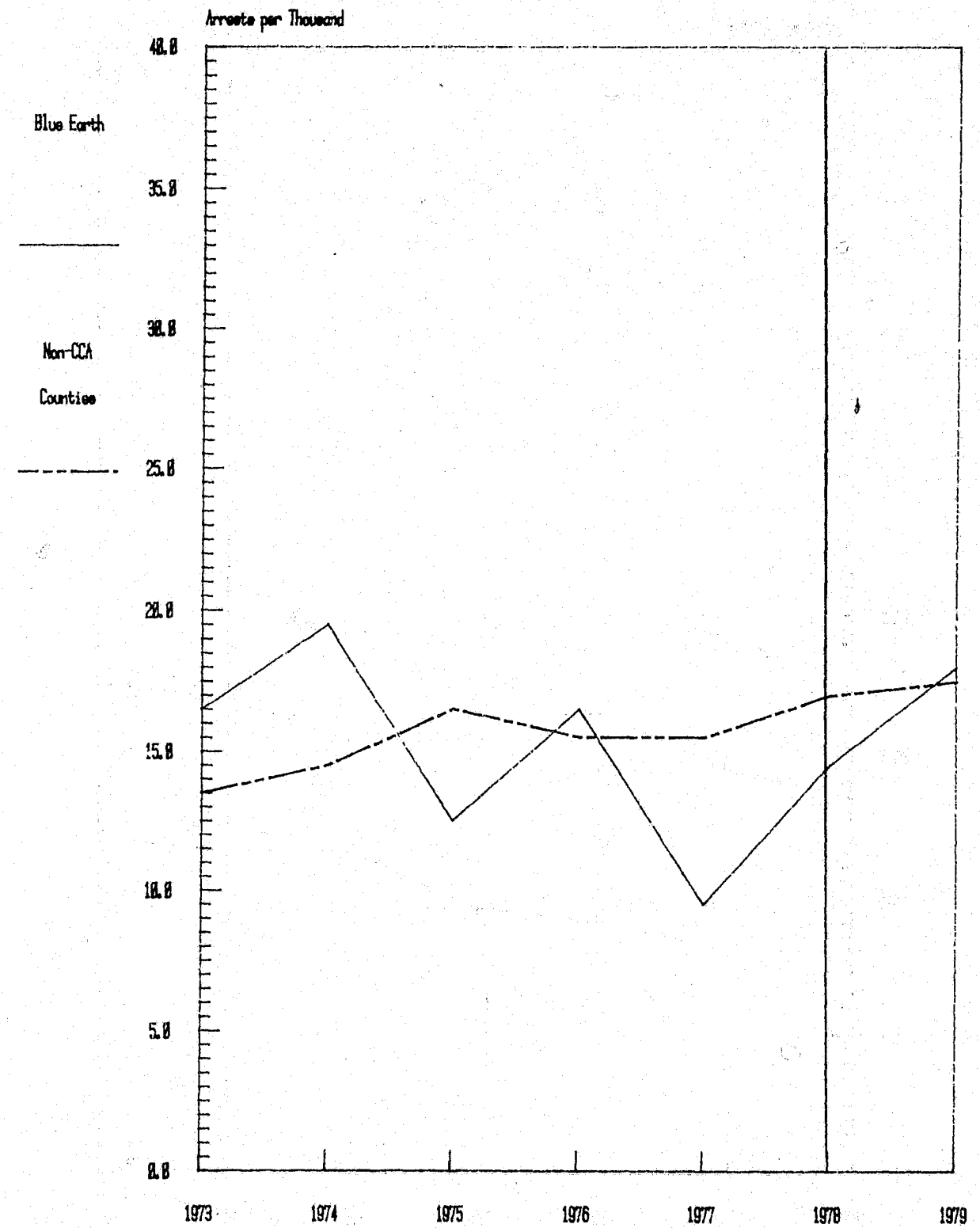


FIGURE 15: ADULT FELONY ARREST RATES

Washington / Non-CCA Counties

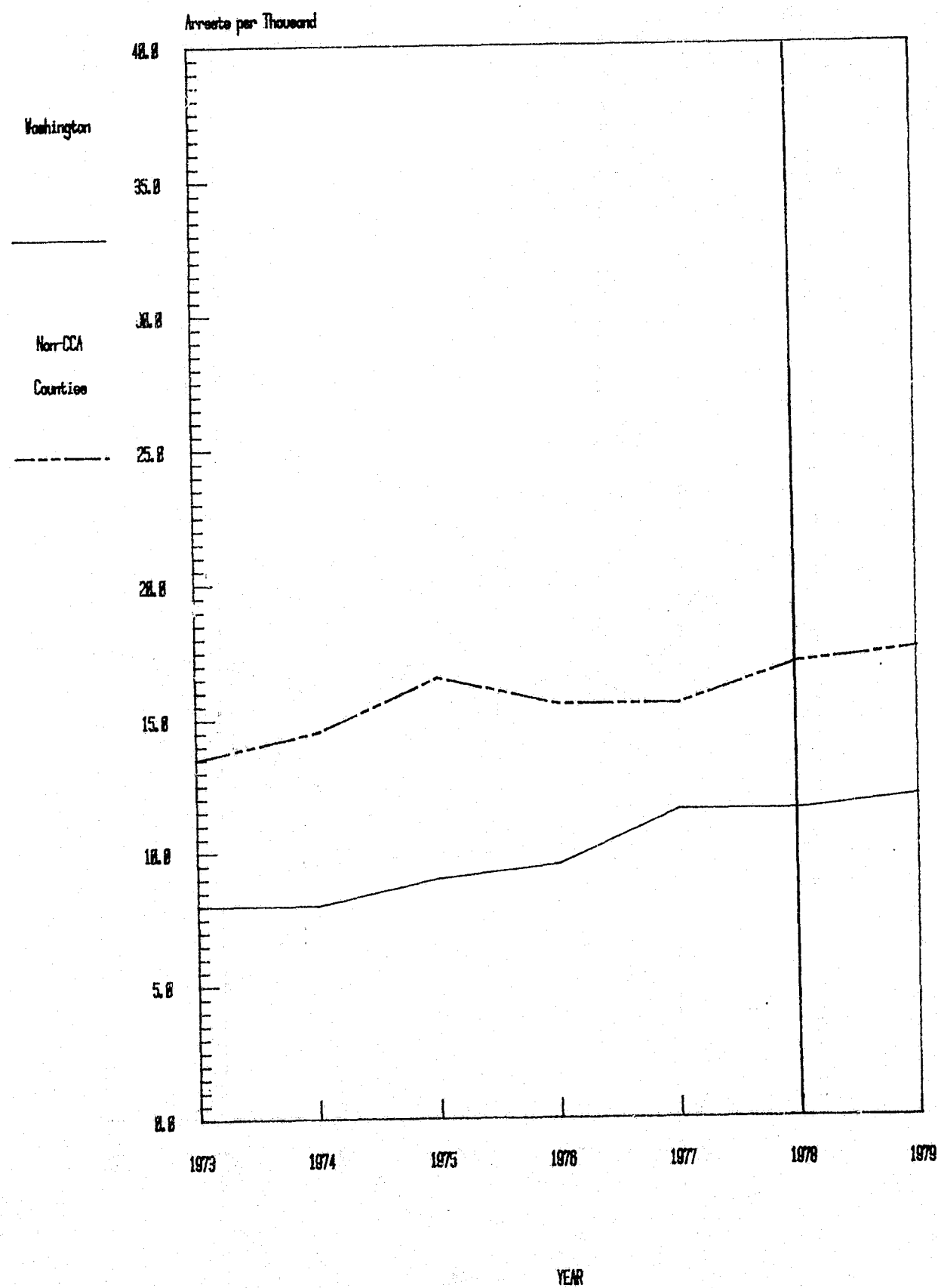
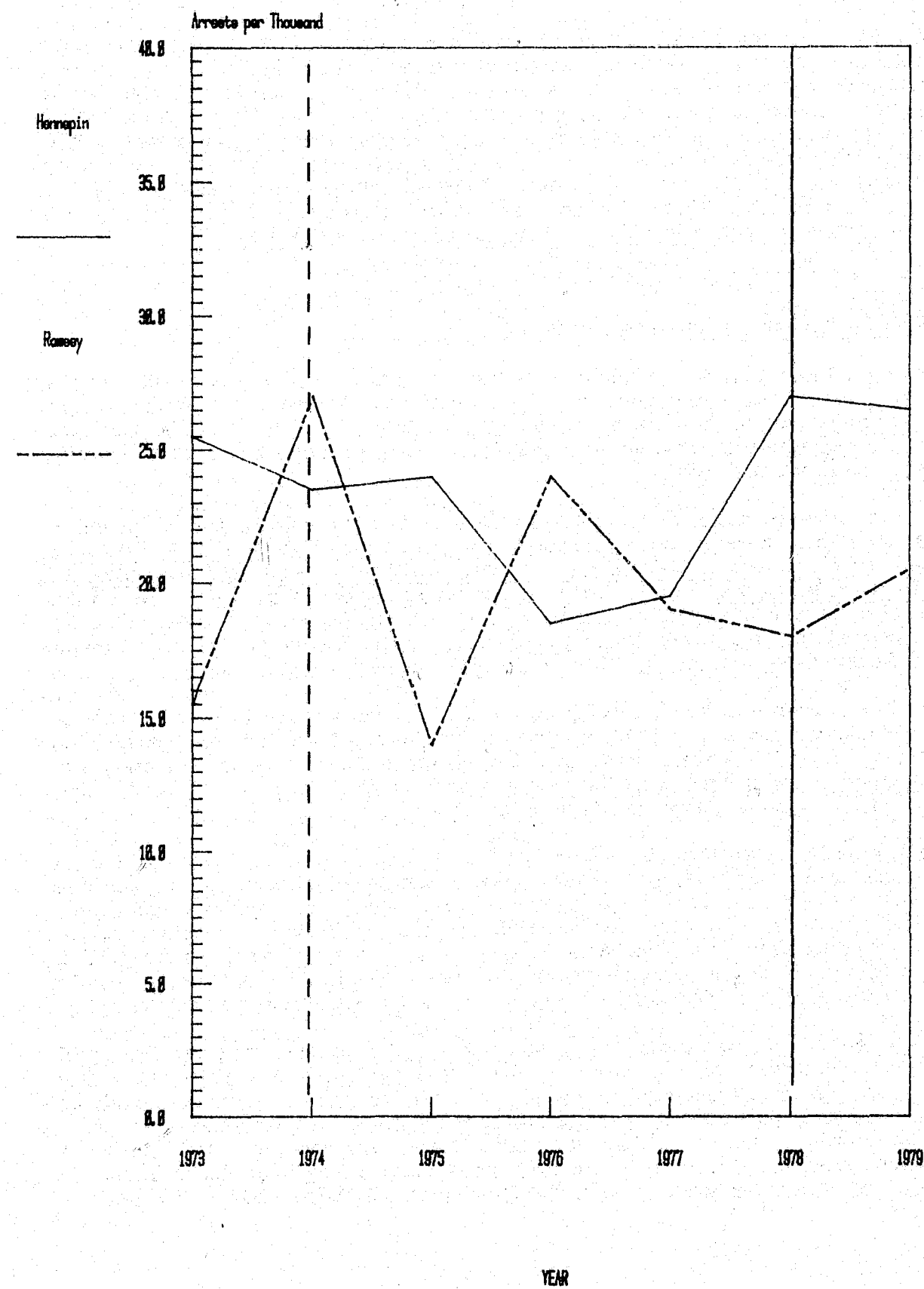


FIGURE 16: ADULT FELONY ARREST RATES

Hennepin / Ramsey



in 1976. After 1976 there is a slow return to the non-CCA trend line. Finally, the Blue Earth plot (Figure 14) suggests the possibility of a pre-CCA arrest rate decline but a post-CCA increase. With only two post-CCA observations, however, little more can be said.

Although inspection of the tables and figures is impressionistic, the evidence does appear to support the previous conclusion of maintenance. It is difficult to compare arrest rate and sample results for the recent participants. Their sample conclusions are very tentative and only two years of post-CCA arrest rates are available. Among the early and middle participants, the one area which produces a finding of a public protection decline based on sample data has the largest increases in arrest rates when compared to non-CCA counties (see the Dodge-Fillmore-Olmsted data in the last column of Table 15). Thus, the arrest rates do not contradict the conclusion that in general public protection is maintained but not increased with the CCA.

III. PUBLIC PROTECTION - JUVENILE OFFENDERS

A. Special Problems in Juvenile Research

The original intention was to handle juveniles in a manner as similar as possible to the adult study. It was anticipated that there would be data problems with a juvenile study, so searches of data sources were begun immediately. A number of anticipated and unanticipated problems emerged that led to the decision that tracking juvenile clients would not be feasible.

The first barriers discovered were difficulties in defining a population of juveniles committed to or diverted to the community. Without a clearly defined population, one cannot draw representative samples to study — without representative samples, all results are open to question. For the adult study, the Systems Rate Study contains the population of district court dispositions in all CCA areas from 1972 through 1978. No such population list is available for juveniles. As explained in the Research Design, procedures for defining the population are prohibitively expensive.

Even if resources existed to develop population lists from which to draw representative samples, further difficulties arise in obtaining information on the sampled juveniles. In five CCA areas records have been destroyed or sealed and in another two CCA areas accessibility to juvenile records is problematic. Even where records exist, information is generally more spotty than for adults.

One alternative suggested was to track individual juveniles in a few counties, shifting some resources from the adult study. For a variety of reasons outlined in the Research Design this alternative was rejected. The benefits of tracking juveniles in a few CCA areas is small — all findings could easily be challenged. The costs, on the other hand, are very high. One has to sacrifice roughly twice as much information on adults to obtain the same amount of information on juveniles. It is believed that to balance the adult and juvenile studies would result in two studies open to challenge.

Another alternative frequently suggested was to track clients in particular local programs. However, this evaluation is assessing the effectiveness of the CCA as a policy, not the effectiveness of individual community programs. A program could be quite successful while overall at the county level the CCA may not be, and vice versa. One has no way of knowing if data on clients in a few programs are representative of all county services. Successes in one program or service may be offset by failure in others. Also, most programs with usable client data exist after CCA entry, limiting

the inferences that could be made about changes due to the CCA. If one finds that ten percent of Program A's clients are failures (as defined in previous sections), what does one conclude regarding public protection? What is the basis of comparison? Finally, one may or may not be able to attribute the existence of the programs investigated to the CCA. In summary, program client data do not seem adequate for making inferences on CCA effectiveness in the area of public protection.

B. Juvenile Arrest Data

Although the research group began with the position that assessments of public protection should be linked to individual community placements, data problems led back to the necessity of using county-level arrest reports. When faced with this alternative, some advantages became apparent and some problems appeared less severe than initially assumed.

On the one hand, arrest data have several obvious advantages. They are readily available from the Bureau of Criminal Apprehension (BCA). They exist for all CCA and non-CCA counties. Data are available over a period of years before and after CCA entry. Use of these data does not require a shift of resources from the adult study.

On the other hand, use of arrest data involves several problems. First, arrests are an imperfect indicator of levels of public safety. An arrest for a felony offense does not necessarily mean that a felony offense has been committed. Second reported arrest data are subject to reporting errors and are generally considered unreliable. Third, county-level arrest rates may not accurately reflect the behavior of the target group of the CCA. In the adult study it was possible to isolate conceptually a target population and to draw representative samples from that population. The adult sample data provide an accurate representation of the behaviors of the target population. For juveniles it has not been possible to identify clearly the juvenile target population nor to draw representative samples of that population. Data are based on the total population-at-risk. Inferences are from the total population-at-risk to some ambiguous target population. The extent to which such inferences are warranted is unknown.

These problems while obviously present may not be entirely problematic. First, the inclusion of all non-CCA areas should help control the errors in the data. To the extent that reporting errors are present in both CCA and non-CCA data, these errors are controlled. Only if the errors affect systematically CCA or non-CCA counties would they be entirely uncontrolled. Second, faulty inferences from county-level arrest data seem less problematic with juveniles than with adults. The introductory section noted that there are major categories of adult offenders that are not targets of the CCA — serious offenders and pre-offenders. On the other hand, CCA areas include services for most juvenile offenders and generally provide extensive prevention and diversion services as well. If CCA programs are supposed to be preventing, diverting and correcting juveniles better than areas without CCA resources, some differences should emerge in arrest rates between CCA and non-CCA areas. The one category of serious juvenile offender not treatable in the community is so small (the serious juvenile offender program served thirty-one clients in 1978) that county-level arrest rates should not be influenced by this small group. Finally, the fact that adult arrest rate findings generally converged with sample results suggests that arrest data provide an adequate indication of changes in public protection.

Although one can correctly argue that reported arrests are affected by many factors other than CCA, this argument does not reduce the utility of arrest rates to infer effects of the CCA. It is important to keep in mind that a crucial aspect of this analysis is to discover what is happening state-wide. It could well be that arrest rates are rising in CCA areas, but if they are rising faster in non-CCA areas one would infer that the CCA has been effective. That is, the multitude of factors other than the CCA affecting arrest rates should be controlled by the inclusion of all non-CCA counties.

Another difficulty that some have with using arrest data is that reporting practices differ widely from county to county. However, the time-series design proposed below requires consistency within a county not across counties. That is, one is looking for changes in CCA areas that do not occur elsewhere. Patterns of change within a series rather than absolute levels across time series are what is being investigated. On the other hand, should reporting practices change state-wide (e.g. the BCA might institute or encourage new reporting policies), the resulting change in reported arrest rates would show up state-wide and would not be interpreted as a CCA effect.

The one remaining potential problem is that some unique factor affecting the reporting of or actual level of arrest rates coincides with CCA entry in a CCA area. Because the factor is unique, it would not be controlled by the inclusion of non-CCA comparison counties. For example, perhaps CCA entry coincides with a new police chief or sheriff who follows a new policy of pursuing and reporting more arrests. Researchers should try to identify with CCA personnel any such possible unique factors.

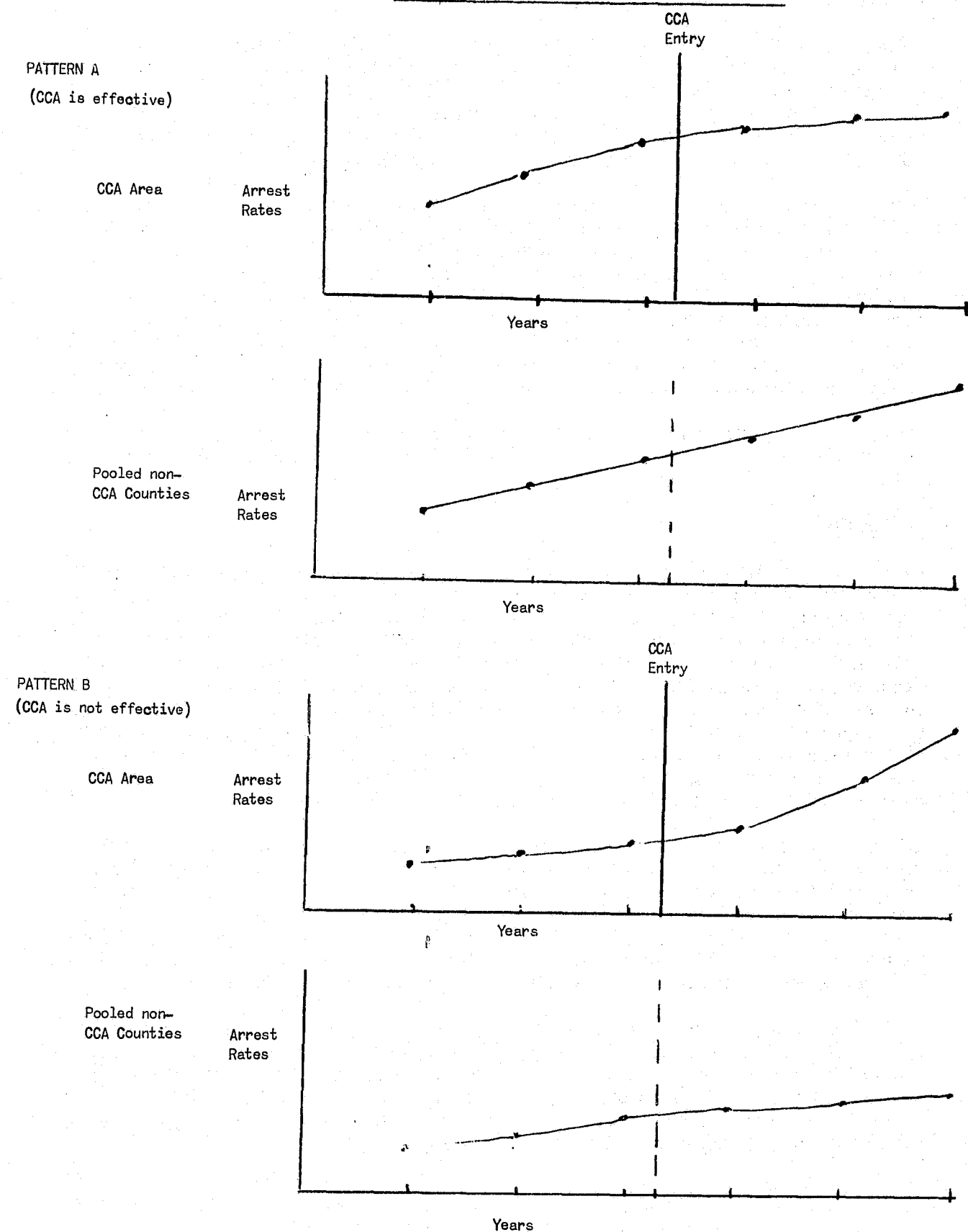
The key point to stress in this discussion is that the use of juvenile arrest data does not imply an assumption that the CCA should be influencing all arrests. Every reported arrest, for example, certainly does not indicate a failure of the CCA. Instead, the argument is that differences in changes in arrest rates between non-CCA and CCA areas can be used to infer CCA impact. Consider a couple of examples in Figure 17. In the first pattern, arrest rates have been rising in both the CCA and non-CCA areas. However, in the CCA area the rate of increase has slowed down after entry and is less sharp than in the non-CCA area. One would infer from such a pattern that CCA services (prevention, diversion, corrections) have reduced the increase in arrest rates. The second pattern suggests that the CCA has led to an increase in arrests. If juveniles diverted to the community are committing offenses during their supervision and are not being better rehabilitated with CCA resources, these phenomena should be detected in a greater rise in arrest rates in CCA than in non-CCA areas.

C. Methodology

Juvenile arrest rates are taken as a negative indicator of public protection — the higher the arrest rate, the less the public is protected. The major question to address for assessing the goal of public protection is whether the CCA has increased/decreased or maintained the juvenile arrest rate.

Juvenile arrest data are available from the Bureau of Criminal Apprehension from 1973 through 1979. Number of arrests for felony-type offenses are tabulated for all 87 counties for each year. When an offense category (e.g. larceny) could contain felonies and non-felonies, analysts estimate the proportion of the category that are likely to be felonies. For juveniles, the number of felony arrests equals the number of arrests in the offense categories of murder, negligent manslaughter, rape, robbery, aggravated assault, burglary, unauthorized use of motor vehicle, arson, forgery, fraud and stolen

FIGURE 17 - Use of CCA and Non-CCA Arrest Data to Infer Effects of the CCA



property as well as 25% of larceny arrests, 5% of vandalism arrests, 5% of other sex offenses and 20% of narcotics arrests.

Estimates of the juvenile population at-risk are obtained for each county for each year. The population at-risk for juveniles includes persons under 18. Age estimates are based on recent estimates by the State Planning Agency and are used to revise previous estimates of expected county population in 1980. Projected population for other years is simply interpolated using three points in time: the 1970 census, the 1975 estimated population and the corrected 1980 projections.

Arrest rates are obtained by dividing the estimated number of felony arrests each year by the estimated population-at-risk. For multi-county areas, the county data are pooled. For example, the total number of juvenile arrests for felony-type offenses in Crow Wing and Morrison counties in 1975 is divided by the combined juvenile population-at-risk in 1975 to obtain the 1975 arrest rate for Crow Wing-Morrison. The non-CCA comparison data are also pooled. Thus, for each year, the arrests in all counties that had not yet entered the CCA are divided by the combined population-at-risk of all counties that had not yet entered the CCA. The non-CCA data exclude Ramsey and Hennepin data, since their large volume of cases dominates results.

Arrest rates are plotted for each CCA area and for the CCA areas as a group. The non-CCA data are plotted for comparison purposes. CCA entry is marked on each plot.

D. Results

Conclusions on the impact of the CCA on public protection vis a vis juvenile offenders rely entirely on reported arrest rates. Figures 19 through 28 provide the arrest rate plots for each CCA area. Figure 18 contains the plot for the CCA areas as a group. The juvenile plots are similar to the adult ones. Table 16 provides information on pre-CCA and post-CCA arrest rates. The data in Table 16 should be interpreted in the same way as the adult data in Table 15. The non-CCA percentage change is applied to the pre-CCA arrest rate to obtain a predicted arrest rate had the CCA area not participated in the Act. The difference between the predicted rate and the actual rate is used as a rough indication of the impact of the CCA on public protection. If there are fewer arrests than predicted, public protection has increased; if there are more arrests than predicted, public protection has declined.

The data in Table 16 suggest that the increases in arrest rates tend to be somewhat greater for CCA areas than non-CCA counties. Arrest rates rise noticeably in Ramsey compared to Hennepin and decline noticeably in Hennepin compared to Ramsey. Unfortunately both findings rely on Ramsey data which are subject to serious reporting problems in the early and middle 1970's. Since the two time series (see Figure 28) are very similar from 1976 through 1979, the differences in arrest rates between Hennepin and Ramsey are probably largely explained by reporting errors in the early 1970's.

The arrest data generally suggest a decline in public protection. The problem is how to interpret those findings. In the adult area, sample data generally produced a conclusion of maintenance, while the arrest data indicate some small decreases in public protection. Is it possible that if adequate individual-level juvenile data were available that the conclusion would also be maintenance or even improvement, while the arrest data suggest a decline?

TABLE 16: Pre-CCA and Post-CCA Juvenile Arrest Rates -- Number of Arrests per 1,000 Juveniles

CCA Area	Pre-CCA Arrests/ Thousand	Post-CCA Arrests/ Thousand	Predicted Post-CCA ^a Arrests/ Thousand	Predicted - Actual Post-CCA Arrests	Public Protection Conclusion
Dodge-Fillmore-Olmsted Non-CCA	16 16.5	22.4 19.3	18.7	-3.7	DECREASE
Crow Wing-Morrison Non-CCA	19 16.5	24.9 19.3	22.2	-2.7	DECREASE ^b
Red Lake-Polk-Norman Non-CCA	5.8 18.5	7.3 19.3	6	-1.3	DECREASE
Todd-Wadena Non-CCA	1.8 18.5	6.9 19.3	1.9	-5.0	DECREASE
Arrowhead Regional Corrections Non-CCA	23.8 18.5	28 19.3	24.8	-3.2	DECREASE
Wadena Non-CCA	25.7 18.5	28.5 19.3	26.8	-1.7	DECREASE
Region 6 West Non-CCA	5.6 18.6	6.7 19.3	5.8	-.9	DECREASE
Blue Earth Non-CCA	26.3 18.7	27 19.5	27.4	+4	INCREASE ^c
Washington Non-CCA	14.5 18.7	20 19.5	15.1	-4.9	DECREASE
Ramsey Hennepin	34 50	39 47	32	-7.0	DECREASE ^d
Hennepin Ramsey	49 36	45 43	58.3	+13.3	INCREASE ^d
SUMMARY					
STATE-WIDE					DECREASE

^a

The predicted post-CCA value is obtained by multiplying the pre-CCA arrest rate by the percentage change found in the non-CCA counties. The non-CCA data exclude Ramsey and Hennepin Counties. The predicted value for Ramsey is based on the change in Hennepin and vice versa.

^b The 1977 rate is excluded from the actual Crow Wing-Morrison rate because it appears deviantly high (see Figure 20). The rise in arrest rates reported is therefore less than if the 1977 values were included.

^c The 1975 rate is excluded from the pre-CCA Blue Earth rate upon which the predicted rate is based because it appears deviantly low (see Figure 26). Had the 1975 values remained in the pre-CCA rate, the predicted rate would be lower and the conclusion would have been a decrease in public protection.

^d The sizes of the decrease in Ramsey and the increase in Hennepin are probably exaggerated because of reporting problems in Ramsey in the early 1970's.

FIGURE 18: JUVENILE FELONY ARREST RATES

CCA-Counties/Non-CCA Counties
excluding Hennepin and Ramsey

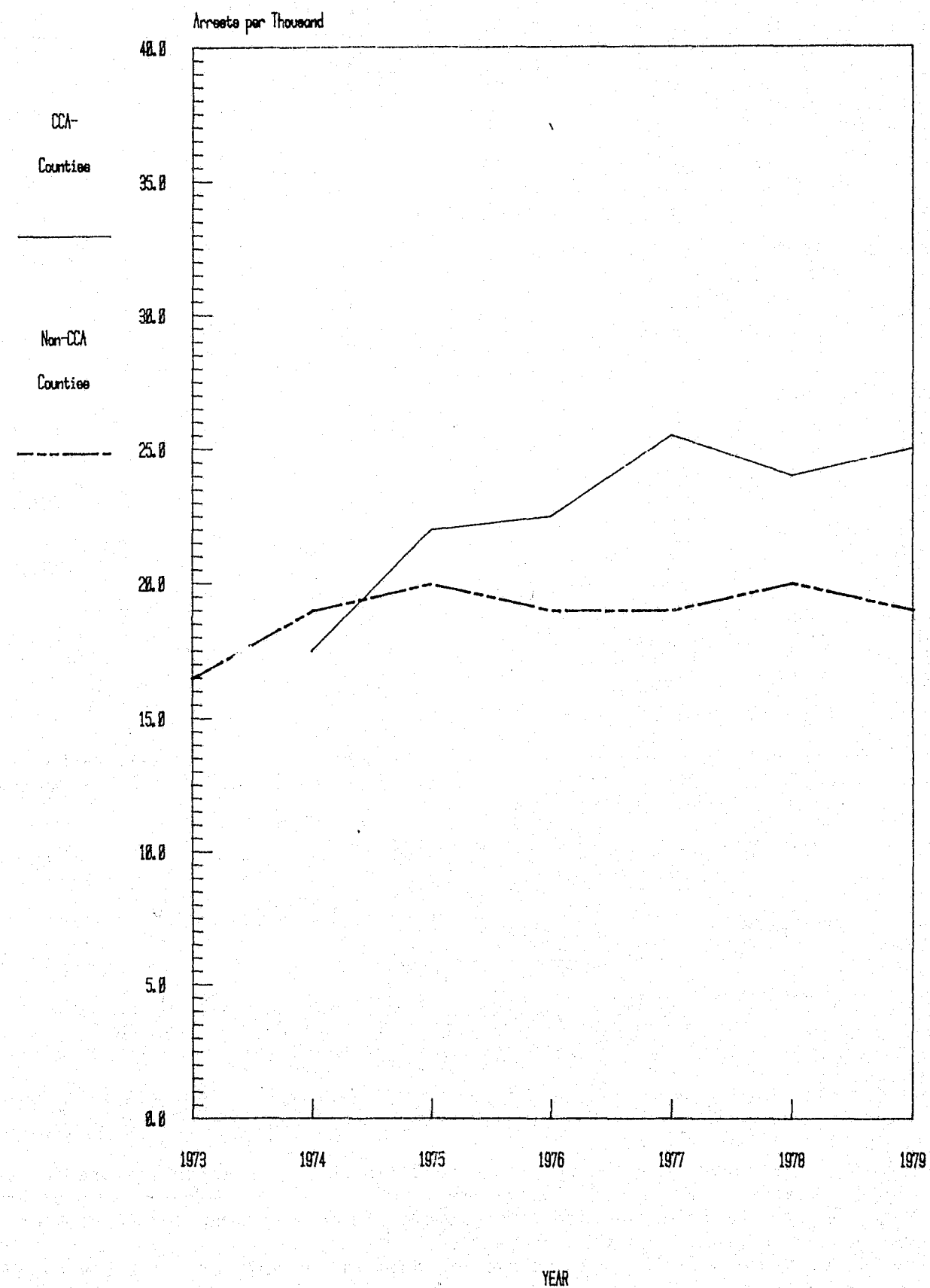


FIGURE 19: JUVENILE FELONY ARREST RATES

Dodge-Fillmore-Olmsted/Non-CCA Counties

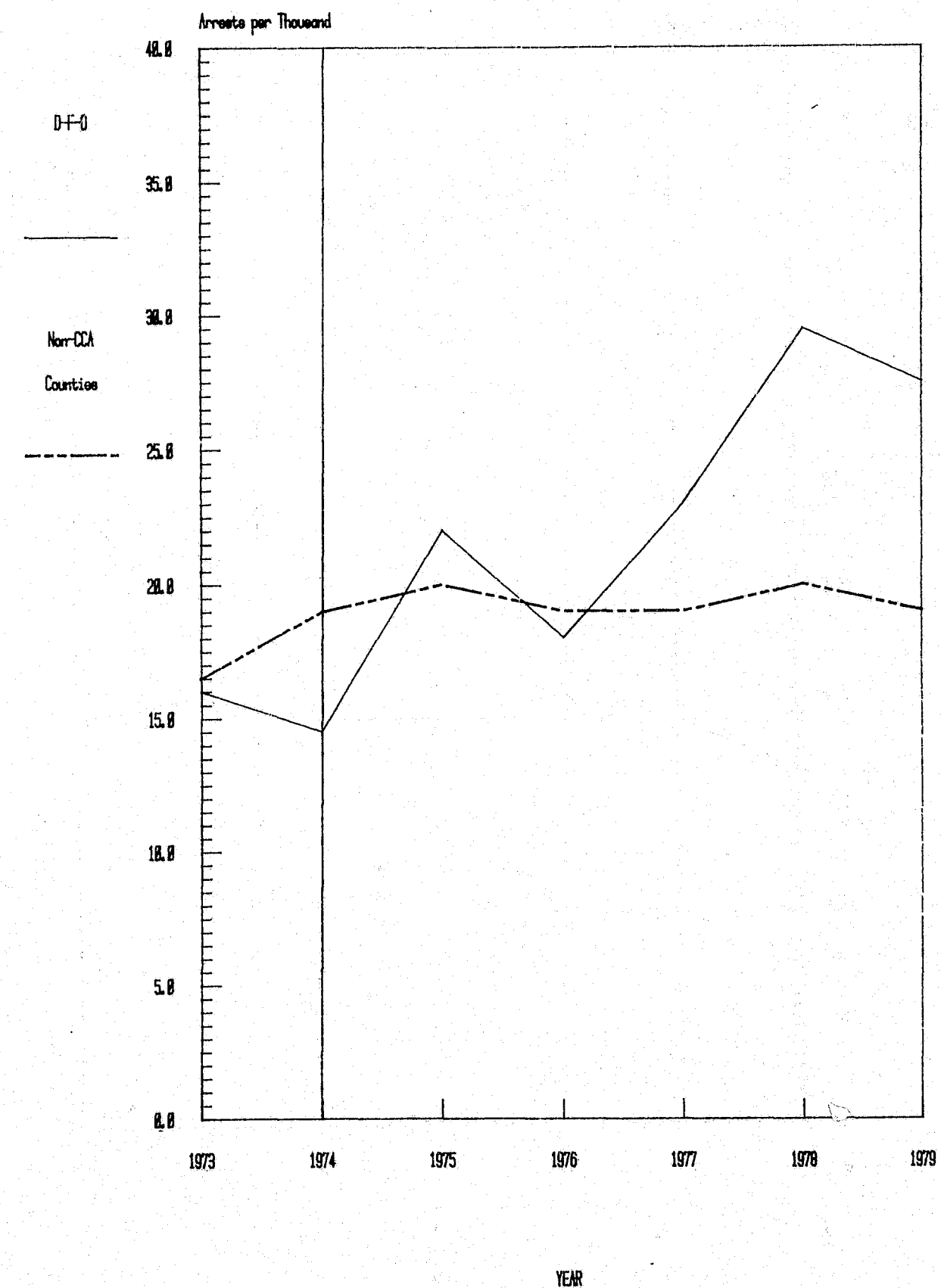


FIGURE 20: JUVENILE FELONY ARREST RATES

Crow Wing-Harrison/Non-CCA Counties

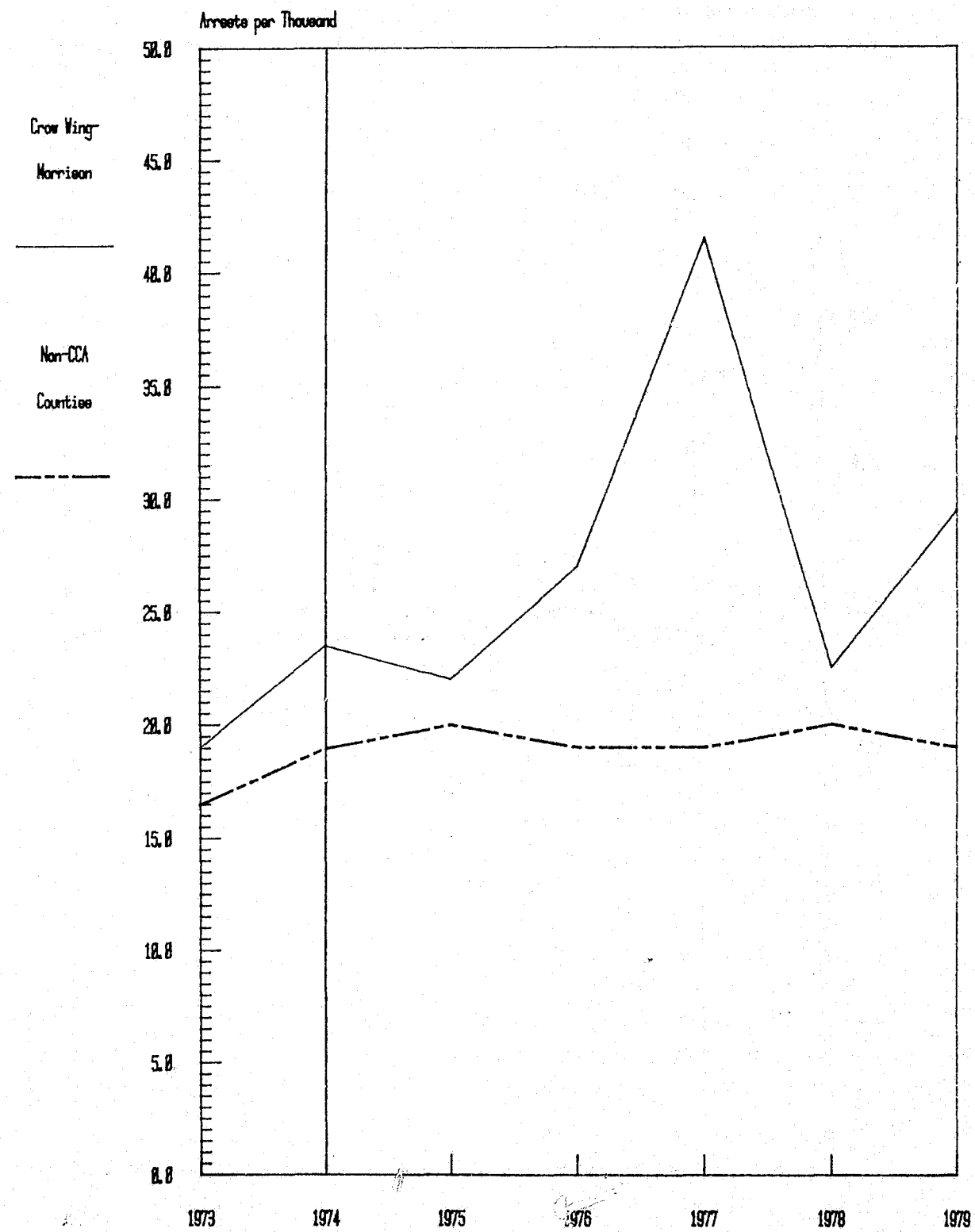


FIGURE 21: JUVENILE FELONY ARREST RATES

Red Lake-Polk-Norson/Non-CCA Counties

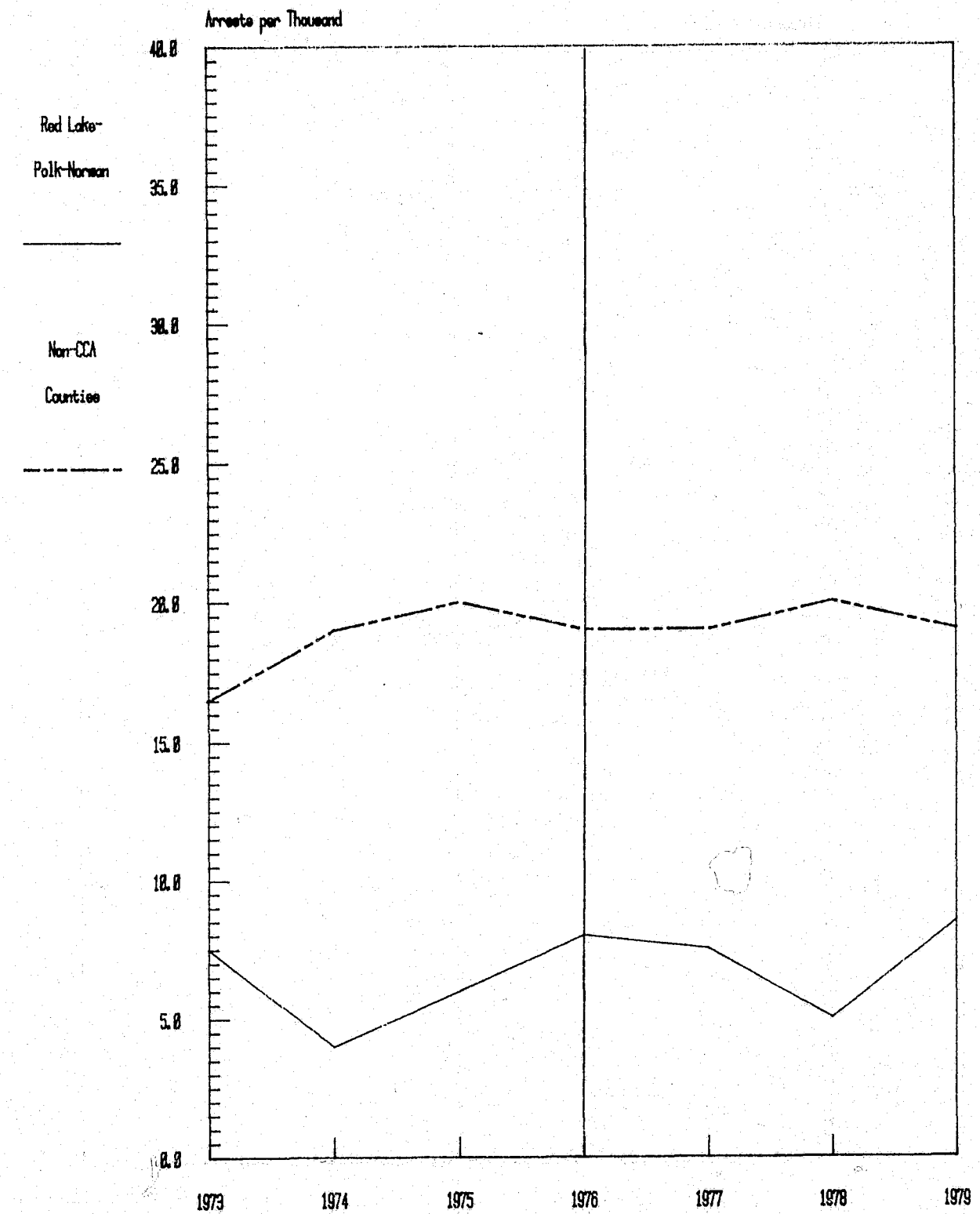


FIGURE 22: JUVENILE FELONY ARREST RATES

Todd-Madison/Non-CCA Counties

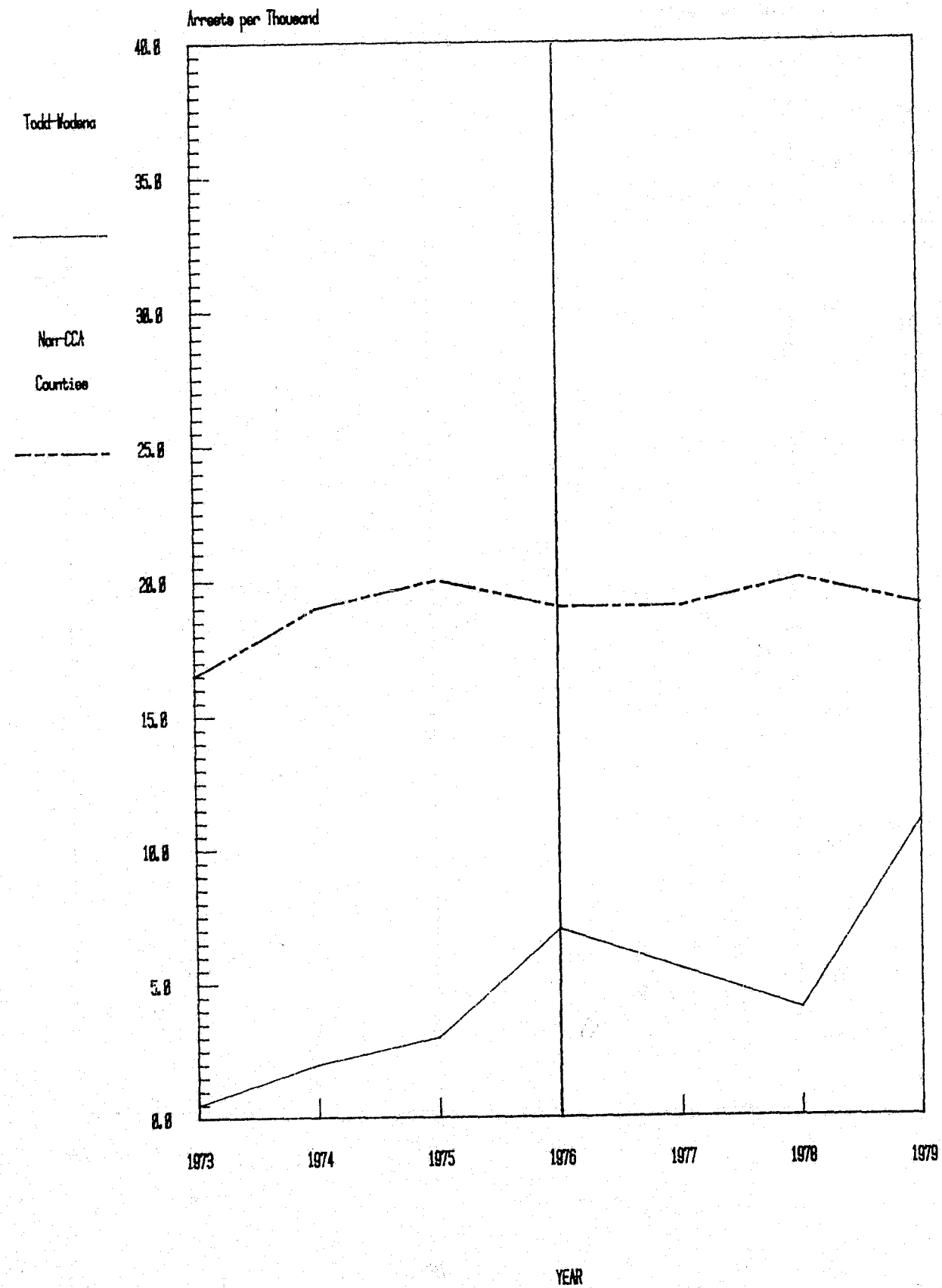


FIGURE 23: JUVENILE FELONY ARREST RATES

Region 3 / Non-CCA Counties

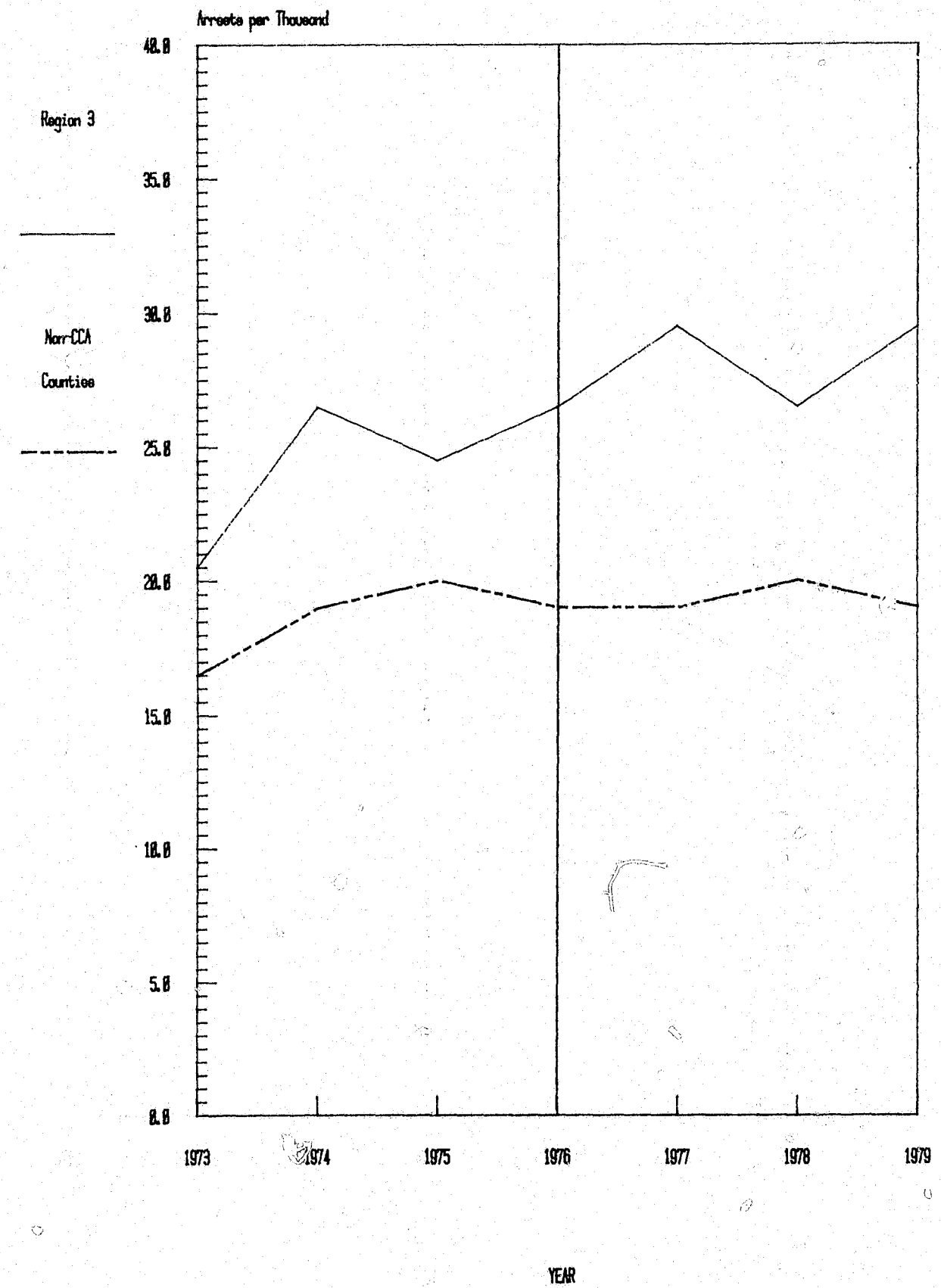


FIGURE 24: JUVENILE FELONY ARREST RATES

Anoka / Non-CCA Counties

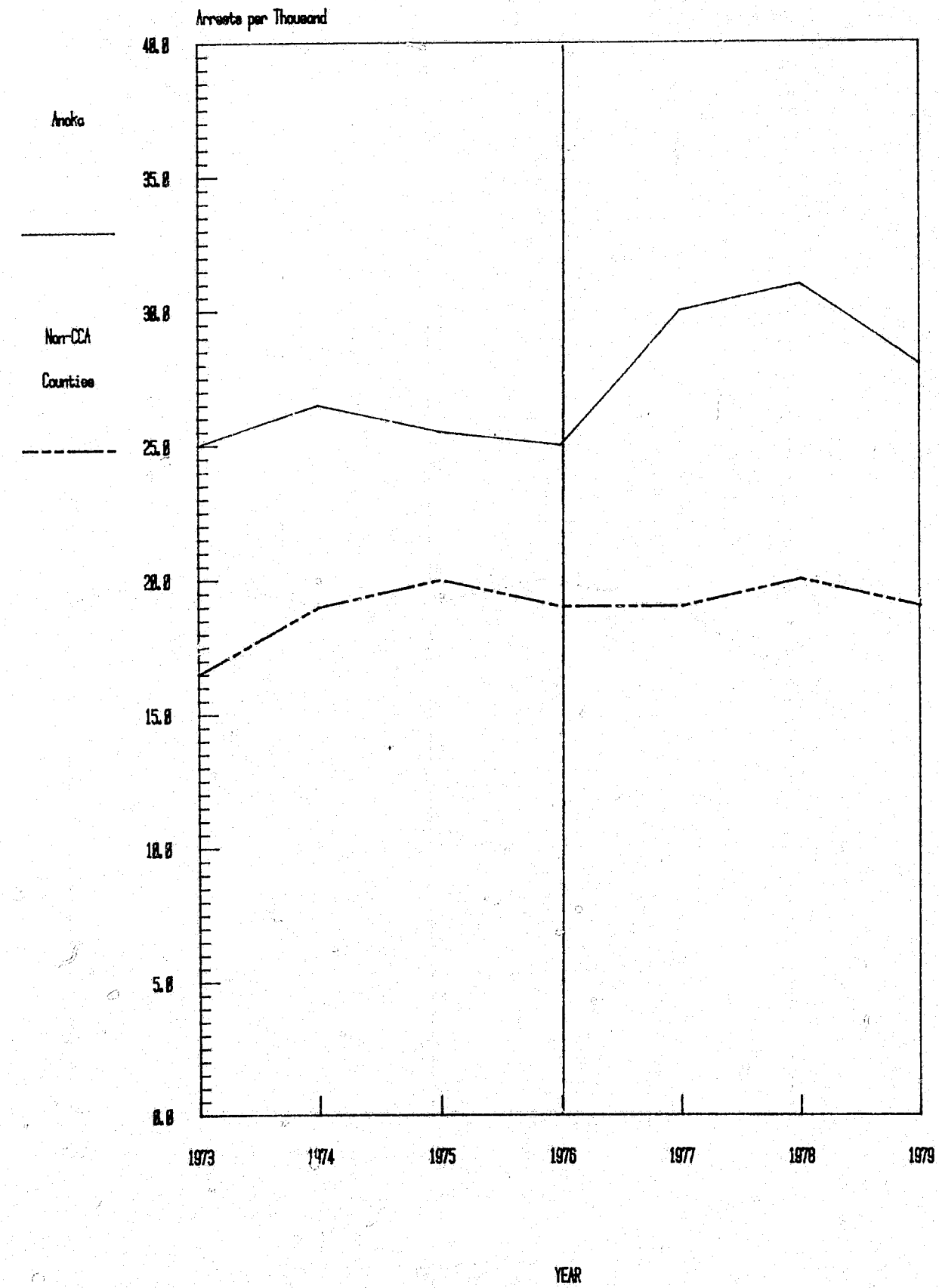


FIGURE 25: JUVENILE FELONY ARREST RATES

Region 6-W / Non-CCA Counties

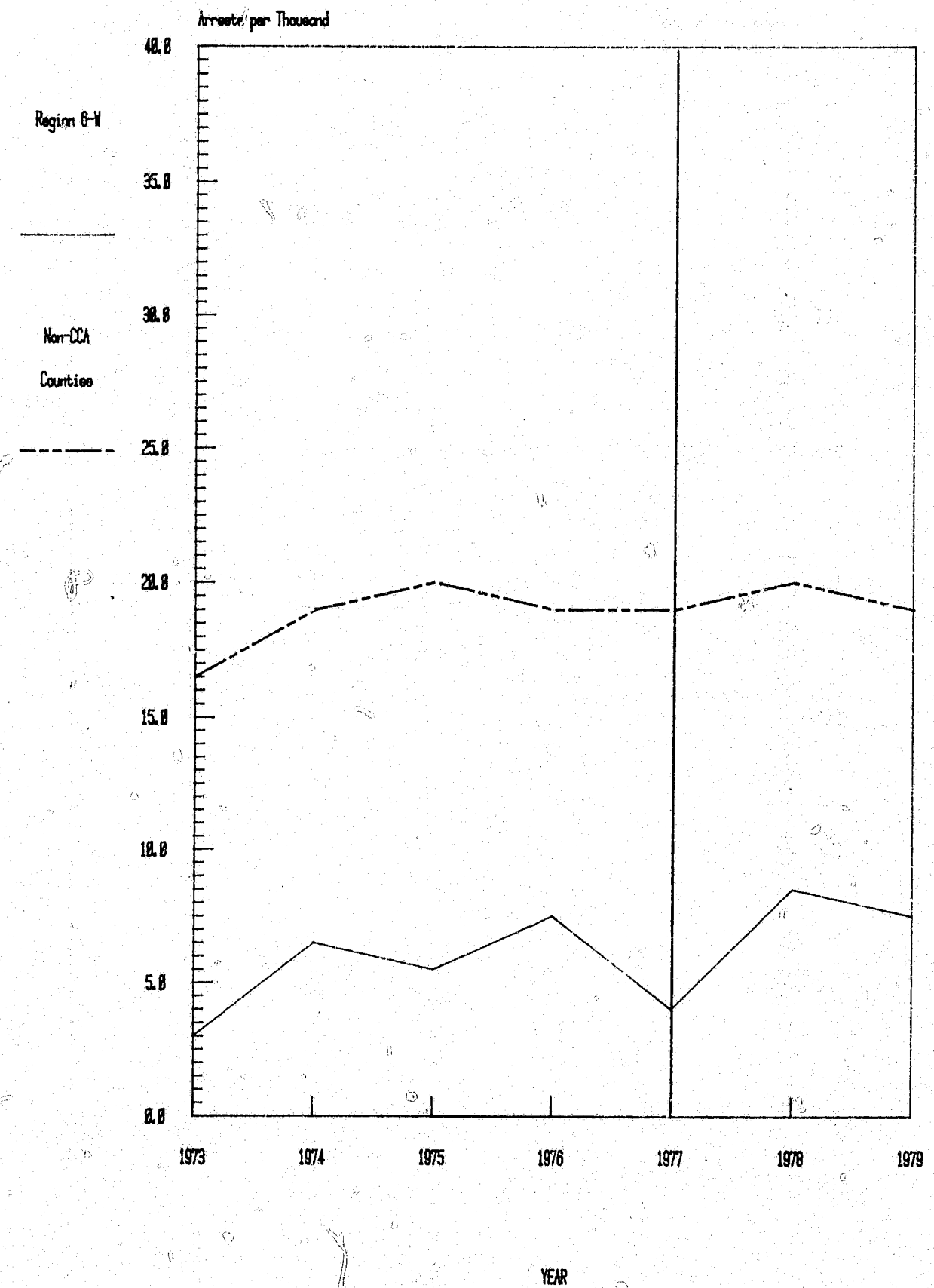


FIGURE 26: JUVENILE FELONY ARREST RATES

Blue Earth / Non-CCA Counties

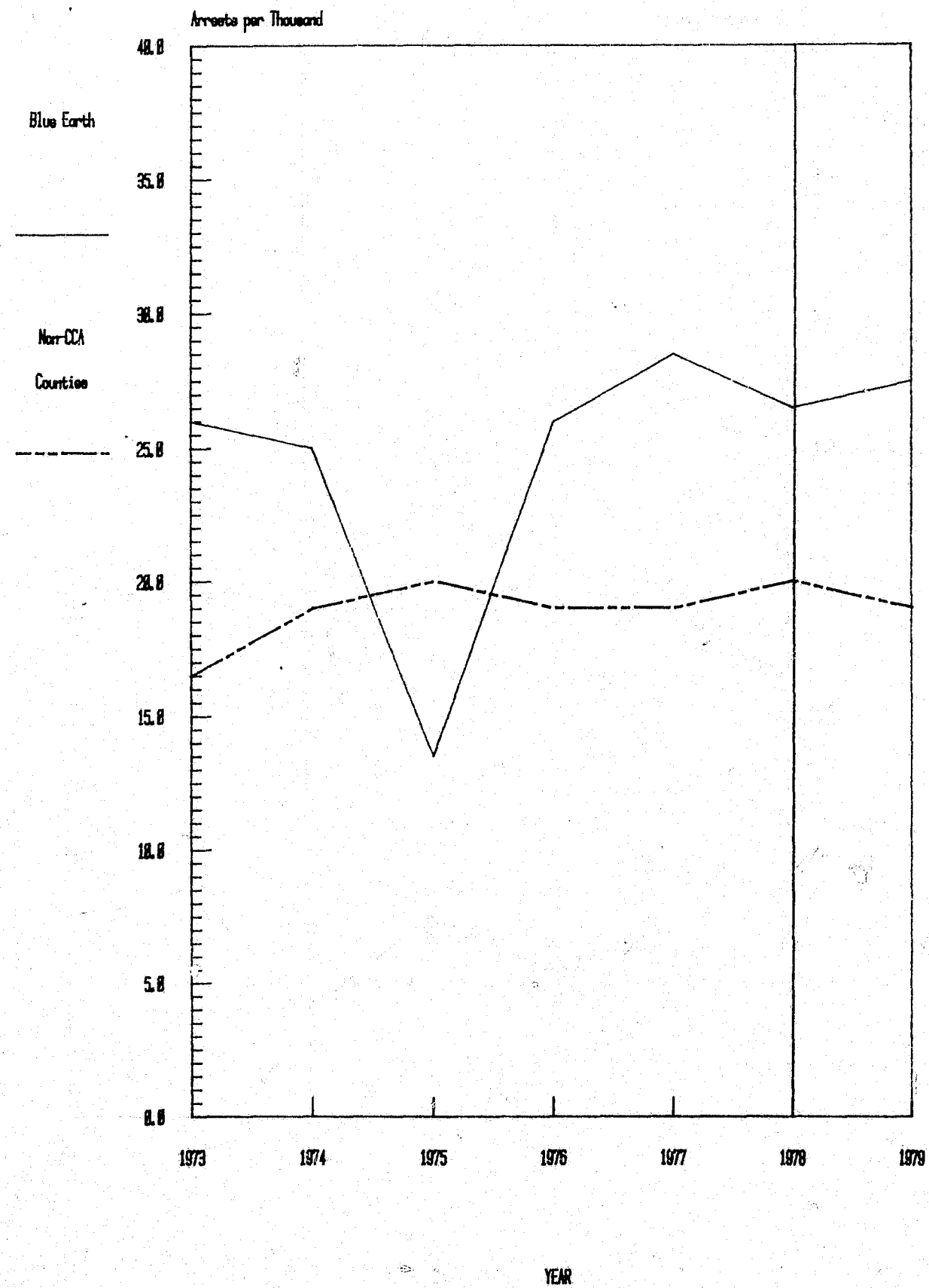


FIGURE 27: JUVENILE FELONY ARREST RATES

Washington / Non-CCA Counties

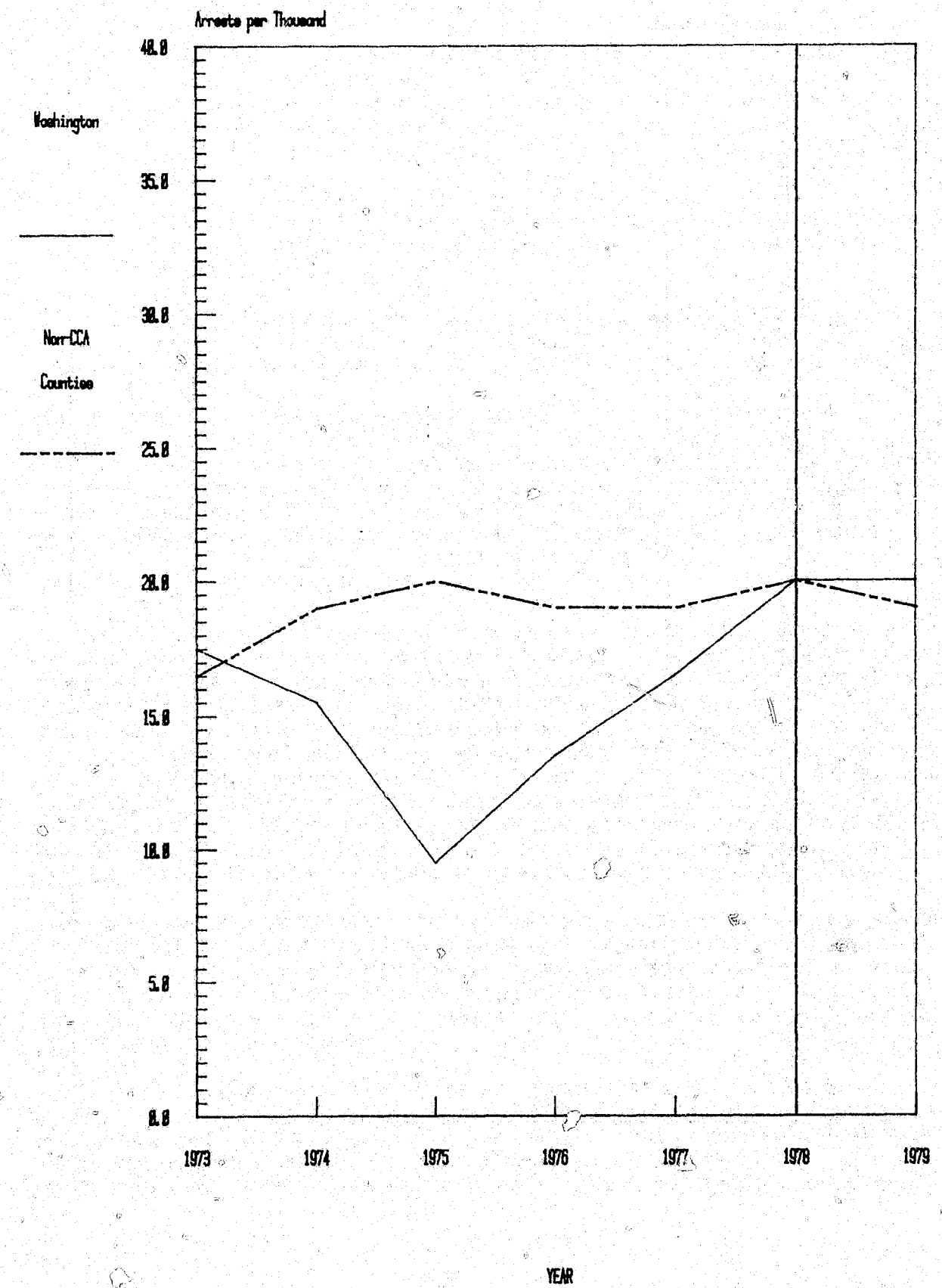
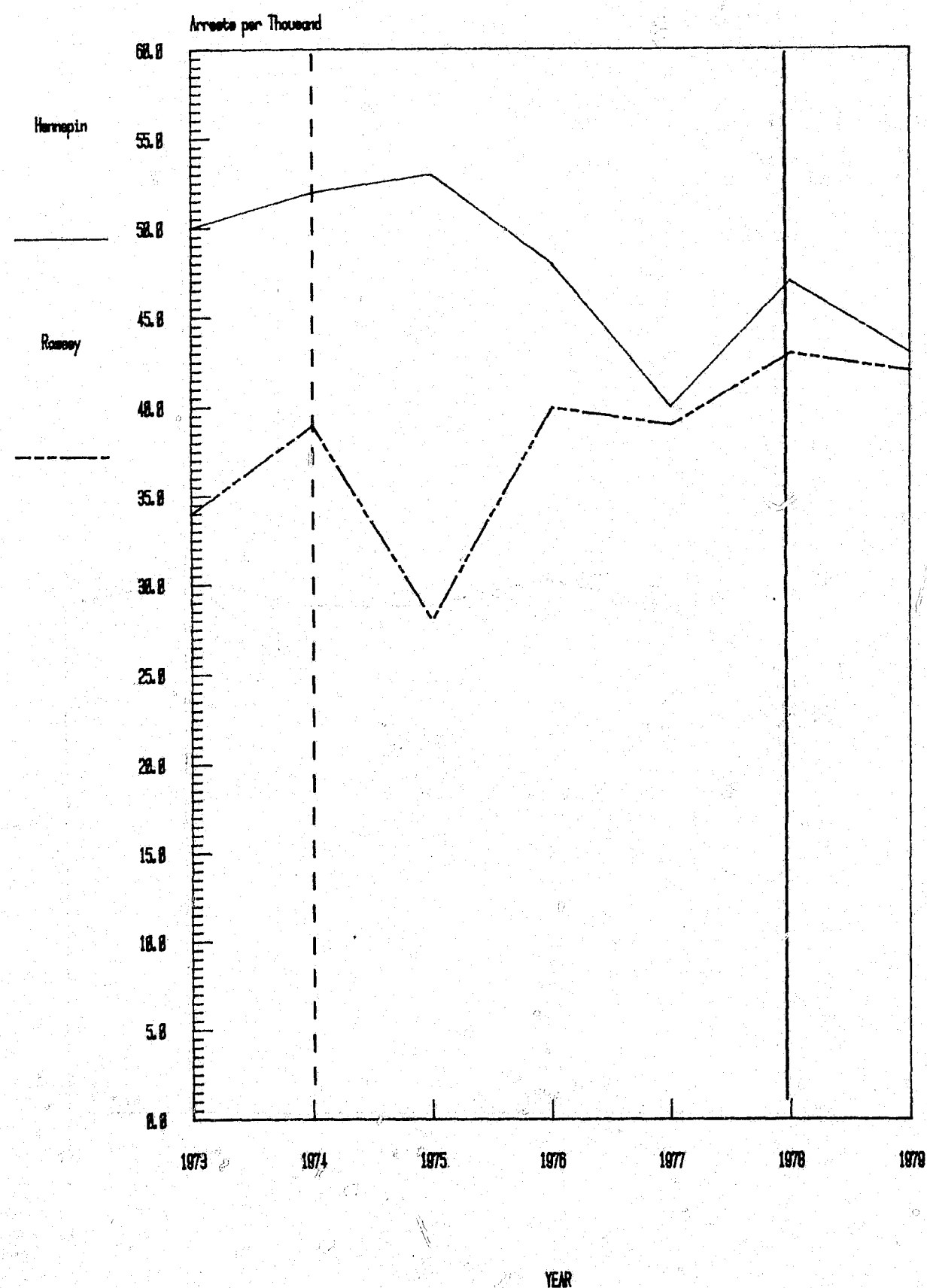


FIGURE 28: JUVENILE FELONY ARREST RATES

Hennepin / Ramsey



Because of data unreliability, researchers are not sufficiently confident to conclude that the CCA has led to a decrease in public protection. This position is strengthened by the fact that the adult arrest data also tended in a negative direction. On the other hand, the following points should not be ignored:

1. For reasons mentioned above, juvenile arrest data are more likely to reflect the behaviors of the target population than adult arrest rates. Discrepancies between sample data from the target population and arrest data based on the entire population-at-risk are more likely to exist for adults because the target population is a small subset of the population-at-risk.
2. The negative effect is more uniform and the size of impact greater in almost all areas for the juvenile data compared to the adult data (compare Tables 15 and 16).
3. It is perhaps suggestive that all areas demonstrate a decrease in public protection except Hennepin and Blue Earth - - the two areas in which juvenile commitments increase rather than decrease after CCA entry.

The juvenile arrest data suggest that the impact of the CCA may be a decline in public protection. In nine of eleven areas arrest rates tend to increase more than in non-CCA counties. Some of these increases are very small, however. Because of the numerous problems associated with arrest rate data, the evidence is certainly not strong that the impact of the CCA has been to reduce public protection. On the other hand, there is certainly no evidence that the CCA has had a positive impact.

IV. SUMMARY AND CONCLUSIONS

The evaluation of public protection provides extensive evidence that public protection is maintained with the Community Corrections Act. Conclusions regarding adult offenders are the most firmly based. Data on samples of adult offenders indicate that during short-term, long-term and combined follow-up periods, public protection is maintained. Two supporting analyses provide corroborating evidence. With only one exception, estimates of eventual success rates tend to support the conclusions based on fixed follow-up periods. Aggregate data on arrest rates in all CCA and non-CCA counties also support the conclusions based on sample data. Conclusions regarding juvenile offenders are less firmly based because only arrest data are available for analysis. The evidence available, however, indicates that the increase in arrest rates in CCA counties may be somewhat greater than that found in non-CCA counties.

The arguments relating the Community Corrections Act to public protection assume that the relative number of offenders treated in the community will increase after CCA entry. On the one hand, arguments are found that this increase will not threaten public safety (i.e. public protection can be maintained). On the other hand, arguments are found that this increase will improve public protection because community treatment is more rehabilitative.

Conclusions of maintenance support the assumption that the CCA does not threaten public safety but fail to support the contribution of rehabilitation. However, the assumptions can be tested adequately only if the relative size of the community population is, in fact, increasing. If the relative number of offenders treated in the community is not increasing, the hypothesized basis for influencing public protection is absent.

Table 17 provides data on the relative size of the group of offenders treated in the community rather than incarcerated before and after CCA entry. In the three recent participants and Todd-Wadena, the expected increase is absent. In the other six areas the relative size of the community group is increasing as expected. The question of whether this increase can be attributed to the CCA can be avoided for now. The major issue here is whether this increase is associated with a change in public protection.

Since the relative size of the community-treated group of offenders does tend to increase after CCA entry, strong evidence is available that this increase does not threaten public safety. While one must recognize that the actual numbers of offenders diverted from prison tend to be small and many of those offenders are incarcerated locally, the fact that treating more offenders in the community does not significantly increase the public risk is an important finding.

There is equally strong evidence that the increase in the relative size of the community-treated group does not increase public protection. The rehabilitation argument is obviously difficult to assess adequately. Better indicators and longer follow-ups are desirable. However, the fact that in all areas analyzed there is virtually no indication of a long-term, positive impact on public protection indicates that the contribution of rehabilitation is not present.

To conclude that the CCA maintains but does not improve public protection does not necessarily indicate that community corrections is no more rehabilitative than state incarceration. Once again it is important to recall that this evaluation assesses the Act or policy and not community corrections programs. It is in fact possible that for a given type of offender community treatment is more rehabilitative than state incarceration but this individual effect fails to show up at the level of policy evaluation.

The evaluation data are unable to determine the relative contributions of community treatment versus state incarceration. It is possible that community treatment is not more rehabilitative. It is equally possible that community treatment is more rehabilitative but that the policy is not contributing to public protection through greater levels of rehabilitation. Several issues relate to this possibility. First, most CCA areas have traditionally treated the majority of their felons in the community. The felons that the CCA is diverting to the community are the "more serious" of the less serious target population. One possibility is that community placement is more rehabilitative for the offenders traditionally treated in the community but may not be for the somewhat more serious offenders who may have been diverted to the community by the CCA.

A second important point is that diverting offenders to the community can have only a marginal impact on failure rates. The data from most areas indicate that state commitments do fail more frequently than community placements, although figures vary greatly across areas. For purposes of illustration, assume that five percent of the community placements fail during the short-term, ten percent fail the following year, for an overall failure rate of fifteen percent. Assume state cases fail at a rate of thirty percent for one year after release. Further assume that the differences in failure rates is due to the mode of corrections (community versus state) rather than the type of offender. If the CCA is diverting ten offenders to the community a year, the short-term risk to the public might increase by none or only one (five percent X ten) offense. Overall if those ten cases went to prison, the expected failures would be three, (ten X thirty percent) but would be 1.5 (ten X fifteen percent) if treated in the

TABLE 17: Percentage of Adult Offender Samples^a Treated in the Community

CCA Area	Pre-CCA	Post-CCA	Pre-Post Change
Dodge-Fillmore-Olmsted			
Percent	89.2	92.2	+03.0
Sample Size	(88)	(236)	
Crow Wing-Morrison			
Percent	70.1	83.3	+13.2
Sample Size	(85)	(201)	
Ramsey			
Percent	73.4	90.2	+16.8
Sample Size	(180)	(430)	
Red Lake-Polk-Norman			
Percent	72.9	96.9	+24.0
Sample Size	(99)	(140)	
Todd-Wadena			
Percent	97.6	90.0	-07.6
Sample Size	(56)	(60)	
Arrowhead Regional Corrections			
Percent	84.2	90.3	+06.1
Sample Size	(234)	(245)	
Anoka			
Percent	73.8	78.9	+05.1
Sample Size	(221)	(202)	
Region 6 West			
Percent	93.8	90.9	-02.9
Sample Size	(139)	(44)	
Blue Earth			
Percent	88.2	78.3	-09.9
Sample Size	(171)	(46)	
Hennepin			
Percent	84.9	79.1	-05.8
Sample Size	(505)	(235)	

a. The sample numbers reported are for community and less serious state cases and include cases through 1978.

community. For an individual offender chances of failure might be twice as great in prison. From the perspective of the policy, community treatment has negligible and probably not noticeable impact of numbers of successful offenders (i.e. 1.5 offenses).

A third important point is that CCA areas provide an opportunity for only a marginal shift in the offender population from prison. CCA areas have traditionally treated the majority of their felons in the community. The category of less serious felons who continue to be incarcerated at the time of CCA entry is relatively small. As a result actual numbers retained in the community are relatively small. Even for Red Lake-Polk-Norman where the largest shift to the community is evident, estimates of numbers retained in the community are approximately eight a year. If one applies the hypothetical failure rates of thirty percent and fifteen percent used above, at most the CCA would be responsible for one additional success a year. Use of those failure rates to obtain one additional success no doubt provide an over estimate because it does not control for the fact that state cases tend to be more serious and therefore more likely to fail whether incarcerated or treated in the community.

The point of these examples is that it is possible for an individual offender to have a greater chance of rehabilitation in the community but for the CCA as a policy to have no impact. In counties where the number of offenders who remain to be diverted from prison is small, where those that remain to be diverted are the more serious of the target population who may be less likely to be rehabilitated and where failure rates can only be marginally reduced; it is unlikely that positive (or negative) effects can be detected. Any small increases or decreases in numbers of successes are unlikely to be detected at the level of policy evaluation.

The conclusion that the CCA maintains but does not improve public protection holds for areas which have traditionally treated the majority of felons in the community. In other contexts it is not known whether diverting to the community large numbers of felons with perhaps higher failure rates would increase the public risk or whether it would improve public protection through rehabilitation. Most important the finding that the CCA does not improve public protection does not imply that the majority of felons who have always been given community sanctions could be equally rehabilitated in prison.

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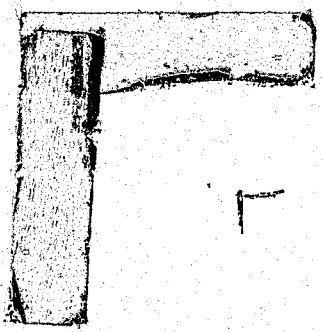
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