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March 14, 2007

Members of the Legislative Budget
and Audit Committee:

In accordance with the provisions of Title 24 of the Alaska Statutes, the attached report is submitted for your review.

USE OF RECIDIVISM RATES BY STATE AGENCIES
RECIDIVISM RATES FOR THE
ALCOHOL SAFETY ACTION PROGRAM

March 13, 2007

Audit Control Number

06-30035B-07

The purpose of this audit was to calculate and analyze recidivism rates for the state's Alcohol Safety Action Program (ASAP) as part of a larger audit on the use of recidivism rates by state agencies *Use of Recidivism Rates by State Agencies, Overview of Current Practices, February 23, 2007* (Audit Control No. 06-30035A-07). A contractor, the Urban Institute, consulted on research design and performed advanced statistical analysis. Additionally, the Urban Institute produced an ASAP recidivism report that forms the basis for conclusions contained in this report. The Urban Institute's report is included as Appendix A.

The audit was conducted in accordance with generally accepted government audit standards. Fieldwork procedures utilized in the course of developing the research conclusions are discussed in the Objectives, Scope, and Methodology.

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OBJECTIVES, SCOPE, AND METHODOLOGY

In accordance with Title 24 of the Alaska Statutes and a special request by the Legislative Budget and Audit Committee, we have conducted an audit on the use of recidivism rates by state rehabilitation programs.

To facilitate dissemination of the results, the review of recidivism rates for state rehabilitation programs is divided into three separate reports. One report covers the overall conclusions regarding the use of recidivism rates by state programs, recommends standards for the calculation of recidivism rates, and concludes as to the adequacy of data collection by state agencies. Two separate reports, including this one, report the calculation and analysis of recidivism rates for the Alcohol Safety Action Program (ASAP) and Sex Offenders.

Objectives

This report will calculate and analyze criminal recidivism rates for ASAP. Specifically, recidivism rates for FY 02 ASAP clients will be calculated and analyzed for the following groups and outcomes:

- An overall recidivism rate for all ASAP clients; rates for those clients that completed the program; rates for those that did not complete; and, rates for those that never came into an ASAP office to be assessed.
- An overall recidivism rate for all clients screened as needing substance abuse education; rates for those that completed education; and, rates for those that did not complete.
- An overall recidivism rate for all clients screened as needing substance abuse treatment; rates for those that completed treatment; and, rates for those that did not complete.

This report will also report on variables found to affect the likelihood of recidivating. Further, the audit will report demographic information useful in understanding ASAP.

Scope

ASAP serves adult misdemeanants convicted in Alaska of an alcohol-related offense where the offender was court-ordered to attend ASAP. Specifically, those offenders with a new ASAP case during FY 02 and a judgment dated prior to July 1, 2002 were selected for review. Offense data (arrests and convictions) were collected for the period beginning July 1, 1986 through March 31, 2006. Additionally, program and demographic data specific to the FY 02 qualifying case were collected.

ASAP's program statistics show 7,255 clients were served in 12 sites[1], during FY 02. However, the scope of FY 02 ASAP clients was limited in that five ASAP offices, representing approximately 17 percent of the FY 02 ASAP clients (1,256 clients), were closed. Reliable data were not available for closed ASAP offices. The limitation did not prevent the calculation of recidivism rates since 83 percent of client files were available for sampling.

ASAP Locations Included in Scope	ASAP Locations Not Included in Scope
Anchorage	Dillingham
Homer/Kenai	Seward
Matanuska-Susitna Valley	Kodiak
Fairbanks	Bethel
Juneau	Kotzebue
Ketchikan	

Methodology

The professional services of the Urban Institute were procured for assistance in designing a sampling plan; designing a database for collection of recidivism information; and statistical expertise in calculating recidivism rates. The Urban Institute's full report on ASAP recidivism rates, including methodology, can be found as Appendix A of this report.

All program and reoffense data were collected by legislative auditors. In addition to reviewing ASAP client files, data from the following information systems was utilized:

- APSIN (Alaska Public Safety Information System Network), Department of Public Safety
- ASAP (Alcohol Safety Action Program) Database, Department of Health and Social Services

Audit procedures were applied to gain assurances over the reliability of the system data.

Research Design: The ASAP recidivism study did not include independent comparison groups. Independent comparison groups – groups of individuals convicted of similar crimes that did not go through ASAP – were not possible, since all people convicted of misdemeanor crimes involving drugs or alcohol are court-ordered into ASAP. Instead, ASAP participants were split into comparison groups based on program outcomes. Those that completed ASAP were compared to those that did not complete and to those that were never assessed. Under the circumstances, this represented the best alternative research design.

[1] Kenai and Homer ASAP offices were subsequently merged into one office operated out of Kenai serving both Kenai and Homer locations.

Although comparing recidivism rates by program outcome was the best alternative, it limits the ability to make definitive conclusions based on the resulting recidivism rates. ASAP clients self-select into three different, program outcome groups: (1) complete; (2) not complete; and (3) never assessed. The same factors that lead a person to complete treatment may be the same factors that also reduce recidivism (e.g. being employed and being in a relationship). Similarly, the same factors that lead a person to not complete treatment may also contribute to a person's likelihood of recidivating (e.g. poor mental health and unemployment).

While the study design was unable to isolate the impact of completing ASAP on recidivism, it did provide recidivism information by outcome that will aid in evaluation of the program. Further, the following analysis provides valuable insight on the subsequent criminal behavior of ASAP participants that may prove useful for policy makers.

Other important components of the recidivism study design are as follows:

Time period reviewed: ASAP clients, with new ASAP cases in FY 02, were selected as the group to be studied. This time period was sufficient to allow for the evaluation of behavior during at least two years post-treatment.

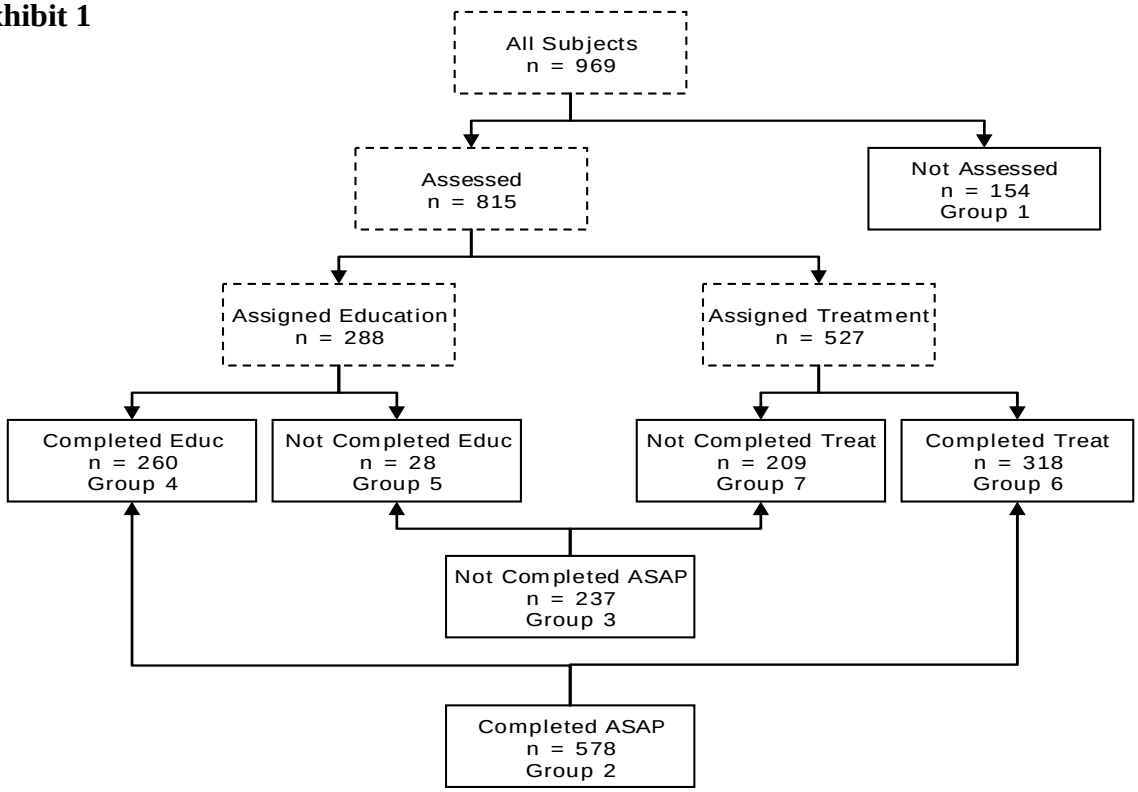
Sample size and methodology: Sampling procedures were used to create a representative subset universe for ASAP clients.[2] The sample was selected from lists of clients provided by each ASAP location. Excluding the locations and clients identified, on page 2; 5,526 ASAP clients were identified as starting the ASAP process during FY 02. The sample was selected by using a random number generator and included 969 subjects.[3] The 969 clients were broken into groups depending on their program outcome. Subgroups are identified in Exhibit 1 on the next page.

ASAP's socio-demographic and treatment data were obtained from client files for each of the 969 subjects. After ASAP data were entered into a database, the final data were provided to the Urban Institute for statistical analysis. The Urban Institute calculated recidivism rates; performed regression analysis to identify whether any of the variables were correlated to an increased/decreased likelihood of recidivating; and, calculated survival curves and hazard rates for the different program outcomes. A detailed description of the Urban Institute's methodology is included in Appendix A of this report.

[2] The listings of clients obtained from the ASAP offices had 473 fewer clients than reported by the offices in their FY 02 program statistics.

[3] The original sample included 987 subjects. In order to perform statistical analyses, it was necessary to exclude 18 subjects from the analysis due to missing information on key measures.

Exhibit 1



Recidivism Measures: Four recidivism measures were used: (1) percent of ASAP clients that were rearrested for any crime; (2) percent of ASAP clients rearrested for an alcohol/drug-related crime; (3) percent that were reconvicted for any crime; and, (4) percent that were reconvicted for an alcohol/drug-related crime. All four were measured within 42 months of the qualifying ASAP judgment.

Socio-demographic and treatment variables: Demographic and treatment variables were evaluated to determine whether each resulted in an increased or decreased likelihood of recidivism. Variables were selected for analysis, based on the availability of data and an indication that variables have been shown to impact recidivism rates in other recidivism studies. The variables selected for review are listed below:

- Age
- Race
- Gender
- Education attainment
- ASAP location
- Prior criminal history
- Alcohol and substance abuse education
- Alcohol and substance abuse treatment

ORGANIZATION AND FUNCTION

This section of the audit describes the organization and function of the state's Alcohol Safety Action Program as administered by the Department of Health and Social Services, Division of Behavioral Health.

Department of Health and Social Services

Created under Alaska Statute (AS) 18.05.010, the department was established to administer the laws and regulations relating to the promotion and protection of public health. The department is responsible for a wide variety of health and social service programs.

Division of Behavioral Health

In July 2003, the department's Division of Alcoholism and Drug Abuse and its mental health section were merged, creating a new Division of Behavioral Health (DBH). Services previously administered by the Division of Alcohol and Drug Abuse are now carried out under DBH. In addition, DBH is responsible for implementing state laws which protect and promote the well-being of Alaskans who experience mental illness. The mental health section of DBH includes the Alaska Psychiatric Institute. Further, DBH also administers the state's Alcohol Safety Action Program.

Alcohol Safety Action Program (ASAP)

ASAP was created in the 1970s as part of a federal move to prevent people from driving while intoxicated. Safety action programs were unique in that they attempted to integrate the services of the courts, law enforcement, substance abuse treatment facilities, and educational programs.

Alaska began its program in 1977 with one site located in Anchorage. By 1992, the number of sites had grown to 15 located throughout the state. By 2003, reductions in funding left ASAP offices located in only the following six sites: Anchorage, Fairbanks, Ketchikan, Juneau, Kenai/Homer, and the Matanuska-Susitna Valley.

The mission of ASAP is to screen, refer, and monitor both adult and juvenile offenders ensuring they complete the substance abuse education or treatment program that is prescribed by the courts, Division of Motor Vehicles, and/or Division of Juvenile Justice. ASAP's staff screen each person to assess patterns of alcohol and drug use. A determination is made, using standard assessment tools, as to whether substance abuse education or treatment is needed. If the initial screening by ASAP staff is not conclusive, the client is referred for a more comprehensive evaluation. ASAP staff communicates with providers on the compliance of

its clients with the prescribed education or treatment. In the event of noncompliance, ASAP's staff files petitions with the court to revoke probation.

Substance Abuse Treatment in Alaska

Private and public treatment agencies provide substance abuse treatment services throughout the State. DBH is the state agency responsible for licensing and inspecting the facilities for compliance with prescribed standards. Part of the approval process includes an on-site survey of the facilities to determine if they are in compliance with state standards. On-site surveys are performed at least once every two years by division health facility surveyors.

Private substance abuse treatment facilities are for profit organizations who charge a fee for their service. Public facilities operate on a sliding fee schedule, meaning that clients are charged a fee based on their income levels. Public facilities may be subsidized through division grants. Treatment facilities that receive a state grant must provide services to clients regardless of their ability to pay. Most of the clients seeking services from public substance abuse treatment facilities do so in order to comply with court requirements – many of which are directed to treatment through ASAP. DBH's management estimates that upwards of 80 to 90 percent of public facility caseloads are court-directed.

UNDERSTANDING AND INTERPRETING RECIDIVISM

Webster's Dictionary defines recidivism as "a failure to maintain a higher state." There are two common ways of using the term recidivism: criminal and clinical. Generally, criminal recidivism is the tendency for a person to lapse back into criminal behavior. Clinical recidivism describes the tendency for a person to lapse into abusive pretreatment behavior (such as substance abuse). This audit focuses on criminal recidivism as it applies to state rehabilitation programs. Criminal recidivism is often expressed as a rate; the fraction of the population that experienced at least one failure in a specific time period.

As a general rule, recidivism rates help measure the success of a program, if prior criminal behavior is one of the key characteristics of a program's participants. Rehabilitation programs that serve such populations strive, in part, to help its participants become productive, law-abiding citizens. Recidivism rates communicate the degree to which a program's participants become "restored" and maintain their restored condition. A limitation of criminal recidivism rates is that it only measures criminal behavior. Other goals of rehabilitation programs are not covered.

Recidivism rates—given that they are limited in their focus—give a glimpse of a program's outcomes, but by no means, provide the entire picture. As an example, many substance abuse clients are court-directed into treatment programs—programs focused on helping clients become free from alcohol and drugs. Criminal recidivism is one measure of effectiveness. However, measures of effectiveness for substance abuse treatment providers also include many other factors such as: clinical recidivism, employment status, health care, education/training level, and an ability to maintain positive relationships. Because criminal recidivism rates are restricted to only one of the program's outcome measures, criminal recidivism rates should be evaluated in conjunction with other program measures when evaluating effectiveness of the program as a whole.

Measurement Issues – Recidivism Components

The purpose of calculating recidivism rates should drive the calculation methodology. What types of management decisions will the rates hope to answer? Decisions of how to define each component are intrinsically tied to the purpose of calculating the recidivism rates.

Before a recidivism rate can be measured, the following components must be defined:

- ✓ what constitutes a "relapse into criminal behavior;"
- ✓ period for review; and
- ✓ population to be studied.

Defining "relapse into criminal behavior:"

There are three common ways to measure someone's return to criminal behavior (also referred to as "failure"). The three common measures of recidivism are: rearrest, reconviction, and reincarceration. In practice, the availability and reliability of data—as well as the amount of resources available to collect and analyze the data—is important to the decision of what measures to use. Commonly-cited advantages and disadvantages of each measure are described below.

Measure	Advantages	Disadvantages
Rearrest	Rearrest is a better indicator of the offender's conduct than conviction, because plea-bargaining can reduce the severity of the charges to which an offender is ultimately convicted.	Standards for arrest are less rigorous than for conviction. Rearrest may overstate criminal behavior because arrested individuals may be innocent.
Reconviction	Reconviction is a relatively solid measure since it is based on a finding of guilt in court. This measure reduces the possibility of overstating criminal behavior.	Reconviction may understate recidivism, as not all crimes are prosecuted. Further, plea-bargaining can reduce the severity of a charge.
Reincarceration (Remand)	Reincarceration can be useful in studies looking at the costs associated with recidivism, since costs to incarcerate individuals are usually readily available.	An offender may be reincarcerated because of a new crime or for technical violations and incarceration data may not identify this difference. Terms of probation are not the same among offenders so what constitutes a technical violation differs between offenders.

Each of the measures has its advantages and disadvantages. No one measure of recidivism is considered the industry standard. Using multiple measures of recidivism improves the validity of results.

Defining the period to be reviewed:

The follow-up period should provide enough time to ascertain the population's propensity to relapse into criminal behavior. The length of time chosen for review should take into consideration the availability of data and the recidivism measures to be used. It should be noted that when using reconviction as a measure, it can take years for a case to make it through the adjudication process. Minimum follow-up periods of at least two years for rearrest and reincarceration and three years for reconviction is desired. Typically, longer follow-up periods are more desirable than shorter follow-up periods.

Defining the population to be studied:

Populations are often heterogeneous – consisting of many distinct traits and behaviors. When calculating recidivism rates, treating a heterogeneous group as one group ignores the impact that distinct traits and behaviors have on the rates. For example, substance abusers are often referred to as a single population of people. In reality, substance abusers are individuals with different addictions and criminal behaviors. If the purpose of calculating recidivism rates is to make decisions on how best to treat substance abusers to reduce subsequent criminal acts, it may provide more useful information to calculate recidivism rates for the subgroups of the population.

Defining comparison groups is another important aspect of defining the population – see Exhibit 2. Comparing recidivism rates of a treatment group to a nontreatment group provides a way to evaluate the effectiveness of the treatment (or rehabilitation program). A detailed understanding of characteristics in both the treatment group and the comparison group are important. This understanding will help ensure differences in recidivism rates between the groups are caused by the treatment instead of other factors.

Exhibit 2

Random v. Non-Random Comparison Groups

Comparison groups are valuable in measuring the impact of intervention (i.e. treatment) on recidivism. The results from a group of subjects who received an intervention are compared to the group that did not receive it. The differences in recidivism rates are then associated with the intervention.

In theory, the best way to control the impact of other variables is to assign subjects, randomly, to the intervention group and the control group. Random assignment will ensure that characteristics and traits occur randomly in each of the two groups. This will help prevent differences in recidivism rates between the two groups, based on some characteristic or trait.

In practice, recidivism methodologies rarely include random assignment of subjects. Ethical, legal, and public safety concerns usually prevent withholding an intervention (i.e. treatment) for the purposes of research.

On large populations, recidivism rates are often calculated for a representative sample. Statistically, the sample size is determined by population size, the allowable error[4] rate, and the desired confidence level.[5]

Finding ways to reduce recidivism

Producing recidivism rates for rehabilitation programs is a starting point for finding ways to reduce recidivism. Identifying factors that increase or decrease the likelihood of recidivating is the end goal. With this information, policy-makers and program managers can make program and policy changes that best protect the public and reduce further victimization.

Factors that may impact recidivism rates are often referred to as variables. Variables can be static, not able to be altered (e.g. age at first arrest) or dynamic, which can be changed throughout one's life (e.g. attitude and drug use). There are no absolutes when it comes to finding which variables may impact recidivism. Recidivism analysis involves collecting data for those variables which tend to impact recidivism. Through statistical analysis, as discussed in more detail on the following page, variables are evaluated to determine their significance.

Variables often found to affect recidivism rates include: race, age, gender, level of education, prior criminal history, and a history of substance abuse. Other variables, such as completion of treatment programs and community supervision, may also impact recidivism rates. Statistical analysis, such as regression analysis, makes it possible to determine to what degree variables predict recidivism.

Calculating and interpreting recidivism rates

The most common methods of calculating recidivism rates are gross rates, survival curves, and life tables (hazard rates). Additionally, regression analysis can be used to analyze the degree to which variables impact the likelihood of recidivating.

Gross recidivism rates are calculated by taking the number of people, who recidivated, divided by the total number of the population. Gross rates are simple to calculate and the results are easily understood.

A disadvantage of using gross rates is they do not account for individuals being in the follow-up period for differing lengths of time. Gross rates assume the follow-up period is the same for everyone in the population. If individuals in a population are at risk for differing periods, gross rates do not work well.

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[4] Allowable error is the maximum percent (acceptable difference) between the true population rate and the sample rate. For example: if the error rate is set at five percent, and the results conclude that 25 percent of the sample has a certain characteristic, it would be acceptable if the actual rate in the population fell between plus/minus five percent (between 20 and 30 percent).

[5] The confidence level is how confident you are the true population rate falls within the acceptable difference (acceptable error rate). A confidence level of 95 percent means for every hundred times a sample was taken from the population, five would produce results that exceeded the allowable error rate.

A more complex method of calculating recidivism is survival curves and hazard rates. A survival curve is a statistical method that can gauge recidivism for individuals who are at risk of failure for differing periods of time. The survival curve is plotted as a line graph with the vertical axis indicating the percentage of persons that have *not* failed (proportion surviving) and the horizontal axis indicating days since judgment. Each point on the curve indicates the percentage of persons who survived to a specific point in time.

Exhibit 3

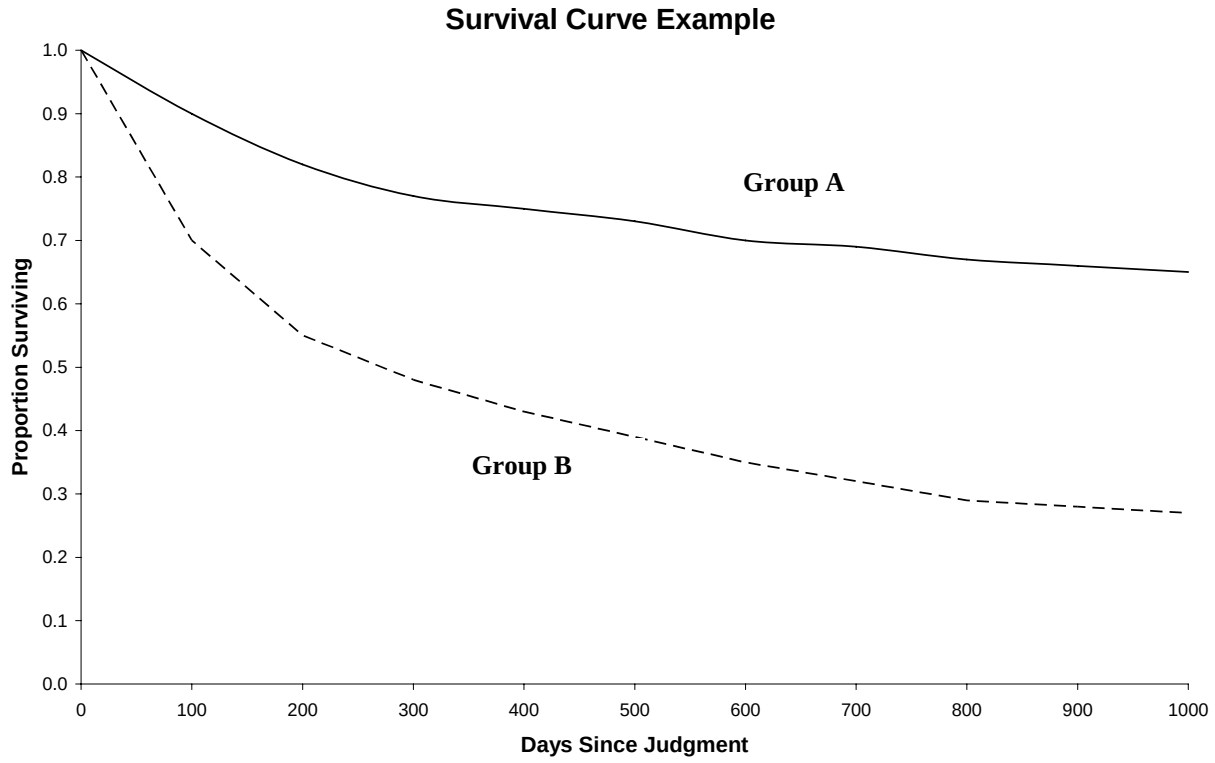


Exhibit 3, above, provides an example of a survival curve for subjects in Group A and for subjects in Group B. Both Group A and Group B have the steepest part of their curves during the first 200 days. This shows that the failure rate is highest during this time. As the rate of failure decreases, survival curves flatten out. The placement of the curve for Group A shows, at all times, a smaller percent of Group A failed when compared to Group B, since the curve for Group A is always higher than the curve for Group B.

Hazard rates are a statistical method used to report instantaneous rates of failure (the opposite of surviving). Hazard rates are often prepared in conjunction with survival curves to further understand the rate of failure during specific spans of time.

Regression is used to identify factors that significantly increase or decrease a person's likelihood of recidivating. Using regression, statisticians can control, through advanced

statistical models, the impact of other variables. This allows the statistician to identify the impact of a specific variable.

Survival curves, hazard rates, and regression, are advanced statistical analyses. The application of these techniques requires statistical expertise and specialized statistical software.

Comparing recidivism results is difficult

Few recidivism studies can be directly compared due to variations in study populations and calculation methodologies. Laws governing crimes, sentencing, treatment, incarceration, and community supervision/probation are defined differently by states/counties; thereby, limiting the ability to directly compare recidivism results. Further, variations in calculation methodology (length of follow-up period, recidivism measures, statistical processes) contribute to differences in recidivism rates.

When comparing recidivism rates, it is important to understand the differences between the groups studied. Understanding the differences between the groups will help determine if the method of comparing groups is equivalent to comparing the proverbial apples to apples, apples to slightly different apples, or apples to oranges. If the calculation methodology does not describe the groups in sufficient detail to make this determination, then recidivism results should not be compared.

No comparisons should be made between recidivism studies that are not accompanied by a detailed description of populations studied and the calculation methodology. Comparing recidivism results, without an understanding of the study's design and methodology, could lead to incorrect conclusions. Although direct comparisons between recidivism studies are infrequent and imprudent, due to differences in methodology, general trends can be determined. From these general trends, the extent of recidivism can be understood.

In Exhibit 4, on the following page, recidivism rates for ASAP, as calculated in this report, are compared to those calculated for a similar program in the State of Virginia. This comparison helps show the challenges to comparing recidivism studies.

Recidivism rates tend to be understated

All recidivism rates tend to be understated since not all crimes are reported; not all reported crimes result in arrest; and, not all arrests result in prosecution. Recidivism rates are also understated due to incomplete criminal history data.

Within Alaska, the State's criminal history database—maintained by the Department of Public Safety (DPS)—is missing an estimated 5 percent of recent criminal history and up to 15 percent of historical criminal history. The missing arrest information is due to local law

Exhibit 4

Alaska's ASAP compared to Virginia's ASAP

Alaska's ASAP recidivism results, from this report, are compared to recidivism rates for the State of Virginia's program released in August 2004. Important components to consider when comparing recidivism results are emphasized below.

As a first step, the differences between Alaska's and Virginia's programs and populations studied are identified. Virginia's program serves only those people convicted of driving under the influence (DUI) offenses. In contrast, Alaska's program serves people convicted of any misdemeanor crime involving the use of alcohol or drugs. Virginia's ASAP provides case management services including education and treatment. Alaska's program is a screening and monitoring agency. All treatment and education is provided by local providers.

As a second step, key differences in research methodologies are highlighted:

Recidivism Components	Virginia's ASAP	Alaska's ASAP
Populations Studied	People convicted of DUI crimes who received services from ASAP during the period 1997 through 2000	People convicted of any misdemeanor crime involving alcohol or drugs who entered ASAP during FY 02
Definition of Reoffense	Two categories: DUI crimes and any moving offense	Two categories: Any crime and any alcohol/drug-related crime
Recidivism Measures	One measure: Reconviction	Two measures: rearrest and reconviction
Time Period Reviewed	After a client completed services through 2003	From the point of judgment through March 2006

The differences noted above act to qualify the recidivism results, providing information necessary to interpret and compare the results. Alaska had a higher rate of recidivism when comparing reconvictions for alcohol/drug-related crimes to Virginia's rate of reconviction for DUI crimes. Alaska's ASAP recidivism rate was 19.9 percent compared to Virginia's ASAP rate of 11 percent.

Alaska had a lower rate of recidivism when comparing reconvictions for any crime to Virginia's ASAP reconvictions for any moving offense. Alaska's ASAP recidivism rate for any crime was 44.1 percent. Virginia's ASAP rate was 60 percent for any moving offense.

enforcement agencies failing to report arrest data, correctional facilities failing to report fingerprints and related charge data, and/or the Alaska Court System failing to report court disposition data. Additionally, until recently, a person's criminal history was deleted from the database when DPS became aware that a person had died.

Another factor that causes recidivism rates to be understated is a lack of out-of-state criminal history. Other than the Federal Bureau of Investigation's (FBI) database that links states' criminal history databases, there is no dependable way to find whether a person was arrested, convicted, or incarcerated in another state. The FBI restricts access to this information and rarely makes it available for research purposes. However, when it is made available, the FBI requires fingerprints as the means of matching a suspect and these fingerprints must be no older than two years. Fingerprints are difficult, if not impossible, to obtain as part of a research project.

Conviction data made available by court systems throughout the country, can be searched; however, the completeness and accuracy of the data has not been established. Further, matching people is difficult since only a few data-fields are available. This requires follow-up with the specific court system to obtain more detailed information to ensure the correct person has been identified.

SUMMARY RECIDIVISM RESULTS

This section of the audit summarizes the calculation and analysis of ASAP recidivism rates. Specifically, the following recidivism rates are reported:

- An overall recidivism rate for all ASAP clients; rates for those clients that completed the program; those that did not complete; and, those that never came into an ASAP office to be assessed.
- An overall recidivism rate for all clients screened as needing substance abuse education; rates for those that completed education; and, those that did not complete.
- An overall recidivism rate for all clients screened as needing substance abuse treatment; rates for those that completed treatment; and, those that did not complete.

Additionally, this section of the audit summarizes the key factors that impact an ASAP client's likelihood of recidivating.

Overall recidivism rates and key findings

Exhibit 5, on the following page, provides the recidivism rates for the 969 individuals in our sample.[6] Rearrest and Reconviction for Any Crime included any misdemeanor or felony-type charge. Minor offenses such as traffic tickets were excluded. Rearrest and Reconviction for an Alcohol/Drug Crime is limited to a crime specifically mentioning drugs or alcohol in the charge title.[7] Rates were calculated over the 42-month period after each client's judgment date.

The key recidivism findings are as follows:

- Overall, 52.6 percent of ASAP clients were rearrested for any crime within 42 months of their ASAP judgment date, and 44.2 percent had a new conviction over the same time period.
- Overall, 24.0 percent of ASAP clients were rearrested for an alcohol/drug-related offense within 42 months of their judgment date, and 19.9 percent were convicted of a new alcohol/drug-related offense during the same period of time.

[6] Based on the sample methodology, we are 95 percent confident that rates identified above represent the recidivism rates for the FY 02 ASAP clients with an allowable error rate of five percent.

[7] Reoffenses categorized as alcohol/drug-related were limited to offenses that specifically involved alcohol or drugs, as identified by the title of the charge (i.e. driving while intoxicated).

Exhibit 5: Recidivism Rates by Program Outcome and Type of Crime

	Any Crime		Alcohol/Drug Crime	
Outcome Groups	**Rearrest**	**Reconviction**	**Rearrest**	**Reconviction**
Overall (All ASAP clients)	52.6%	44.2%	24.0%	19.9%
Completed	42.7%	34.3%	19.9%	17.0%
Not Completed	67.9%	59.5%	30.8%	24.5%
Never Assessed	66.2%	57.1%	29.2%	24.0%
Assessed for Education	36.1%	28.1%	15.6%	13.2%
Completed Education	32.7%	24.6%	14.2%	12.3%
Not Completed Education	67.9%	60.7%	28.6%	21.4%
Assessed for Treatment	57.6%	48.9%	27.1%	22.4%
Completed Treatment	50.9%	42.1%	24.5%	20.8%
Not Completed Treatment	67.9%	59.3%	31.1%	24.9%

Key conclusions regarding variables that impact recidivism of ASAP clients

As discussed in the Objectives, Scope, and Methodology section of this report, ASAP clients included in this recidivism analysis were compared by program outcome. Because ASAP clients self-select into the program outcome groups, unmeasured variables not identified or discussed in this report may significantly impact recidivism rates.

Recidivism measured for the 42 months post-judgment

Rearrest for Any Crime

- ASAP clients who completed alcohol and substance abuse education were *less* likely to recidivate than those that were never assessed.[8]
- ASAP clients that complete treatment were *less* likely, on average, to recidivate than those that were never assessed. However, the difference was not statistically significant.
- ASAP clients with a greater number of prior arrests were *more* likely to recidivate.
- Older subjects were *less* likely to recidivate.

[8] "Never assessed" means that a person was court-ordered into the program but never showed up at the ASAP office for an assessment.

- Race and gender were not significantly related to the risk of rearrest after controlling for other factors.

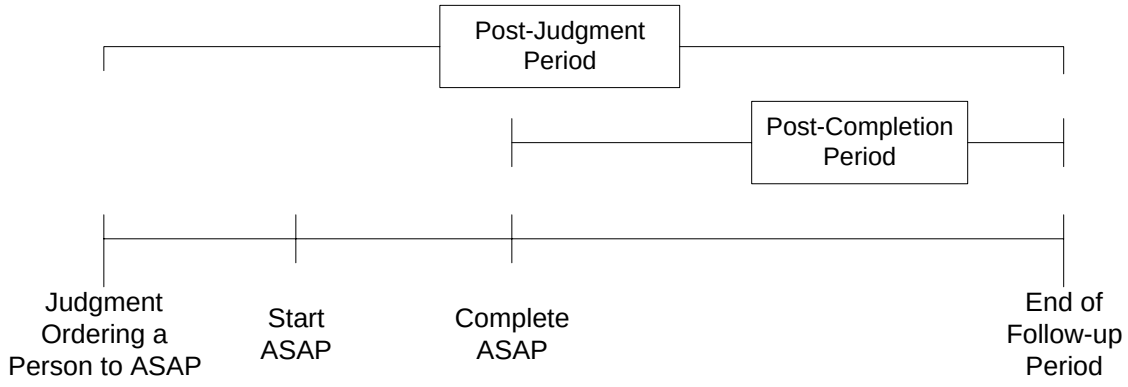
Reconviction for Any Crime

- ASAP clients with greater numbers of prior arrests for person or society offenses or a greater number of prior convictions were *more* likely to be reconvicted during the follow-up period.
- ASAP clients with a greater number of prior convictions for offenses against society were *less* likely to be reconvicted after controlling for the other effects.
- Race and gender are not significantly related to reconviction recidivism

Rearrest or Reconviction for an Alcohol/Drug Crime

- ASAP clients who complete alcohol and substance abuse education were *less* likely to be rearrested or reconvicted for an alcohol/drug crime than those never assessed.
- Older ASAP clients were *less* likely to be rearrested or reconvicted for an alcohol/drug crime.
- White ASAP clients were *less* likely to be rearrested or reconvicted for an alcohol/drug crime than clients in the "other" race category.
- ASAP clients with a greater number of prior crimes against society were *more* likely to be rearrested for an alcohol/drug crime.

Post-Judgment versus post-completion recidivism



- Among individuals who completed ASAP (education or treatment), there was no significant difference between pre-completion[9] and post-completion[10] risk of rearrest or reconviction for any crime.

[9] The period of time between judgment and completing the required substance abuse treatment or education.

- For those individuals that completed ASAP (education or treatment), the hazard rates, measured after completion, were not significantly different than the hazard rates measured after judgment. This indicates that impact of the program is realized immediately upon entering the program rather than upon completion of the program.

Key conclusions regarding survival times of ASAP clients

- Survival times for clients that did not complete treatment or did not complete education were similar to the survival times for those never assessed.
- While the recidivism rates between those that completed treatment and those who were not assessed are not statistically different, the time period in which the failures occur is markedly different. Twenty-five percent of "not assessed" clients were estimated to fail within 4 months, and 50 percent were expected to fail in less than 16 months. In contrast, 25 percent of "completed treatment" clients were estimated to fail within 12 months, and 50 percent were expected to fail within approximately 41 months.

Analysis of Recidivism Results

The purpose of this audit is to calculate recidivism rates for ASAP clients and to identify variables that significantly increase or decrease the likelihood of recidivating. The effectiveness of the research design was limited by the lack of groups against which to compare recidivism rates. All members of the study were court-directed to complete the program and rates of recidivism were calculated based on program outcome (completed, did not complete) and what type of services were received (education or treatment).

In evaluating ASAP, criminal recidivism rates are only one measure of effectiveness. Other measures include, in part, an increased ability to maintain employment, better relations with family members, and better health. Currently, the ASAP program does not collect data for these types of variables. For those ASAP clients that attend state-subsidized substance abuse treatment, DHSS does have a database capable of capturing data for these types of variables. However, at the time of this report, the database was not complete and no procedures have been implemented to ensure the accuracy of the data.

ASAP appears to be appropriately screening its population to identify persons that have an alcohol/drug problem that requires treatment. This is evidenced by the much lower recidivism rate of those that complete education versus those that do and do not complete treatment. However, there was a marked discrepancy between ASAP locations—on the percentage of people directed to education versus treatment—which may indicate other factors are influencing the screening decision such as availability of treatment services.

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[10] The period of time from completing the required substance abuse treatment or education and the end of the follow-up period.

DETAILED RECIDIVISM RESULTS

The detailed recidivism results are based on the Urban Institutes detailed ASAP recidivism report, included as Appendix A.

Detailed recidivism rates

As shown in Exhibit 5 on page 16, for arrests of any crime, ASAP clients had a 52.6 percent rate of recidivism within three and a half years from their respective judgment dates. While, overall 52.6 percent of the individuals ordered into ASAP recidivated, the effect of ASAP cannot be isolated without true comparison groups – groups of like individuals that were not required to complete ASAP. Therefore, it is difficult to use the 52.6 percent recidivism as a gauge of effectiveness. In other words, it is impossible to evaluate whether 52.6 percent rate of rearrest for any crime is lower than what would have been experienced had the clients not gone through ASAP.

ASAP clients that completed education had the lowest rate of rearrest/reconviction for any crime and for an alcohol/drug-related crime. Lower rates of recidivism for education clients makes sense since those assessed as needing education are those considered not to have an alcohol/drug dependency problem. Those without an alcohol/drug dependency problem would be *less* likely to recidivate, holding all other factors constant.

Those ASAP clients that were never assessed – never came into the ASAP office to be evaluated – had a similar rate of arrest for any crime (66.2 percent) as those that did not complete education/treatment (67.9 percent). These rates are much higher when compared to those that completed treatment/education (42.7 percent). However, again it is not possible to conclude that those who had treatment/education led to a reduced rate of recidivism, without true comparison groups. The same factors that contributed to a person finishing treatment may be the same characteristics that helped prevent the person from recidivating.

Those ASAP clients that did not complete treatment have the highest rate of recidivism for a new alcohol/drug crime. Treatment clients would be expected to have a higher rate of recidivism than their education counterparts because they have been identified as having an alcohol/drug dependency problem. It would also follow that those clients that complete the treatment program would have a lower rate of recidivism than clients that did not complete treatment, assuming treatment helps address the alcohol/drug problem.

Recidivism rates by program outcome and demographic variable

Exhibit 6, below, summarizes the percentage arrested for *any crime within 42 months* by program outcome and demographic variable. The recidivism rates are expressed as a percentage of the number of people in each category. For example, there were 81 people who were never assessed in Anchorage. Of those people, 56 were arrested within 42 month which is 69 percent of the total. The rates should be interpreted with caution as percentages are greatly influenced by small population sizes.

Similar tables for arrest for an alcohol/drug crime, conviction for any crime, and conviction for alcohol/drug-related crimes are included in the Urban Institute detailed report (see Appendix A). Additionally, tables with the same information sorted by ASAP-site are included in Appendix A.

Exhibit 6: Proportion Arrested For Any Crime Within 42 Months of Judgment

	Program Outcome					
	Never Assessed	Completed Education	Did Not Complete Education	Completed Treatment	Did Not Complete Treatment	All Program Outcomes
ASAP Location						
Anchorage (509)	69.1%	32.0%	75.0%	50.8%	64.2%	54.2%
Fairbanks (161)	68.3%	33.8%	37.5%	51.9%	70.6%	49.7%
Juneau (84)	64.7%	24.0%	100.0%	34.8%	68.8%	46.4%
Kenai/Homer (96)	50.0%	28.9%	80.0%	56.3%	66.7%	47.9%
Ketchikan (36)	0.0%	46.2%	*	81.8%	80.0%	63.9%
Mat-Su (83)	57.1%	42.1%	*	47.5%	88.2%	55.4%
Age at Judgment						
18-25 (261)	72.9%	43.2%	72.7%	61.0%	79.5%	61.3%
26-35 (268)	71.2%	36.8%	37.5%	48.4%	70.0%	54.9%
36-45 (287)	58.3%	25.7%	85.7%	52.0%	68.9%	51.6%
Over 45 (153)	50.0%	21.2%	100.0%	38.0%	45.2%	35.9%
Race						
Alaska Native (289)	75.9%	33.3%	83.3%	65.8%	69.6%	65.7%
White (557)	55.9%	31.9%	61.1%	45.1%	66.0%	46.0%
Other (123)	56.3%	35.9%	75.0%	53.7%	69.6%	52.0%
Education Level						
< 12th Grade (141)	66.7%	42.3%	75.0%	64.4%	88.9%	67.4%
12th Grade/GED (402)	74.5%	29.4%	91.7%	57.6%	60.8%	54.2%
Some College (325)	72.7%	33.6%	50.0%	38.5%	67.2%	45.2%
Unknown (101)	56.4%	32.0%	0.0%	50.0%	71.4%	49.5%
Gender						
Female (236)	59.1%	25.3%	100.0%	55.8%	60.0%	47.9%
Male (733)	67.4%	35.7%	64.0%	49.1%	70.4%	54.2%

* Indicates zero cases in cell.
Shading above indicates the variable was statistically significant.
() Identifies the total clients in each category that were selected as part of our sample of ASAP clients.

Recidivism rates by treatment provider

Exhibit 7, below, provides recidivism rates (arrest for any crime) for the ASAP clients that completed treatment at the biggest treatment providers in terms of number of ASAP clients served. The treatment providers included in Exhibit 7, served approximately 77 percent of the ASAP clients who completed treatment. All the providers below had at least 20 clients complete treatment.

Exhibit 7: Recidivism Rates Among Completers Served by Select Treatment Providers

	Any Crime		Alcohol/Drug Crime	
Substance Abuse Treatment Provider	**Rearrest**	**Reconviction**	**Rearrest**	**Reconviction**
Salvation Army (69)	48.9%	42.2%	24.4%	22.2%
Starting Point (46)	45.2%	32.3%	12.9%	12.9%
Alaska Human Services (32)	28.0%	20.0%	16.0%	8.0%
Out-Of-State (28)	40.9%	31.8%	22.7%	18.2%
RITE (23)	60.0%	45.0%	40.0%	30.0%
Genesis House (23)	33.3%	28.6%	14.3%	14.3%

( ) indicate the number of clients

Variables that impact the likelihood of recidivating

The purpose of the regression analysis was to determine whether specific variables contribute to an increased likelihood of recidivating. Identifying significant variables is immensely important in understanding program outcomes and in making changes to improve program effectiveness.

The term "statistically significant" refers to whether the magnitude of the change in the outcome variable (recidivism rates), that is attributed to a specific variable or combination of variables, exceeds a predefined threshold. For the purposes of this study, the threshold for statistical significance is two standard deviations. As an example, if statistical tests/models show that age causes a change in the likelihood of recidivating greater than two standard deviations, the variable is said to be statistically significant.

The following variables were found to significantly impact an ASAP client's likelihood of being rearrested:

- Completed substance abuse education
- Age
- Number of prior arrests

ASAP clients that completed ASAP education were *less* likely to be rearrested than clients who were not assessed. In contrast, ASAP clients that completed treatment were not *less* likely to be rearrested than clients who were not assessed. ASAP clients with a greater

number of prior arrests were *more* likely to be rearrested and older subjects were *less* likely to be rearrested. Race and gender were not significantly related to the risk of rearrest after controlling for other factors.

The variables that impact an ASAP client's likelihood of being reconvicted were different than those found to impact being rearrested. The following variables impact the likelihood of being reconvicted during the follow-up period:

- Completed substance abuse education
- Age
- Number of prior arrests for crimes against person
- Number of prior arrests for crimes against society
- Number of prior convictions
- Number of prior convictions for crimes against persons
- Number of prior convictions for crimes against society

ASAP clients with a greater number of prior arrests for person or society offenses, or a greater number of prior convictions, were *more* likely to be reconvicted. ASAP clients with a greater number of prior convictions for offenses against society were *less* likely to be reconvicted. Gender and race did not significantly impact the likelihood of being reconvicted.

The variables that significantly impact an ASAP client's likelihood of being rearrested for an alcohol/drug related crime are as follows:

- Completed substance abuse education
- Older age
- Race (White compared to non-White/non-Natives)
- Number of prior arrests for crimes against society

ASAP clients that completed substance abuse education were *less* likely to be rearrested or reconvicted for an alcohol/drug crime than those that were never assessed. All types of alcohol/drug crimes were categorized as crimes against society, so it is not surprising that ASAP clients with a greater number of prior crimes against society were *more* likely to be rearrested for a new alcohol/drug crime. White ASAP clients were *less* likely to be rearrested or reconvicted for an alcohol/drug crime than clients from the "Other" race category (i.e. Black, Hispanic).

The variables that significantly impact the likelihood of reconviction for an alcohol/drug crime are the same variables noted above as significant for rearrest except that the number of prior arrests for crimes against society was not significant.

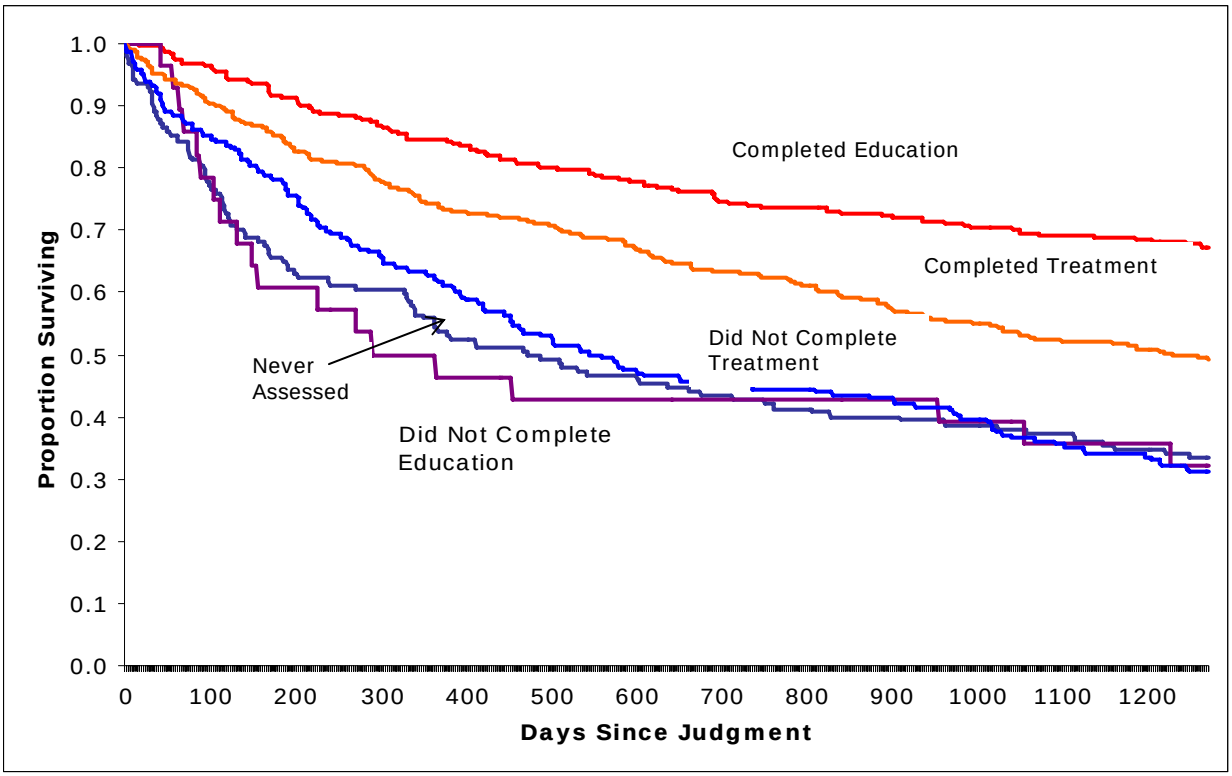
Recidivism as described by survival curves and hazard rates

The statistical methods of survival analysis and life tables were used to examine the differences between the following five outcome measurements:

- Never assessed
- Completed education
- Not complete education
- Completed treatment
- Not complete treatment

In general, survival analysis shows the cumulative portion of ASAP clients who survived (were not arrested or were not convicted depending on which recidivism measure is being studied) over a period of time. Exhibit 8, below, plots the rate of survival in the form of a curve using the groups defined above. The curves provide insight into the pattern of reoffending over time. Differences between the curves illustrate the differences in timing and magnitude of rearrest/reconviction.

Exhibit 8 Survival Plot of Arrests For Any Crime Within 42 Months of Judgment



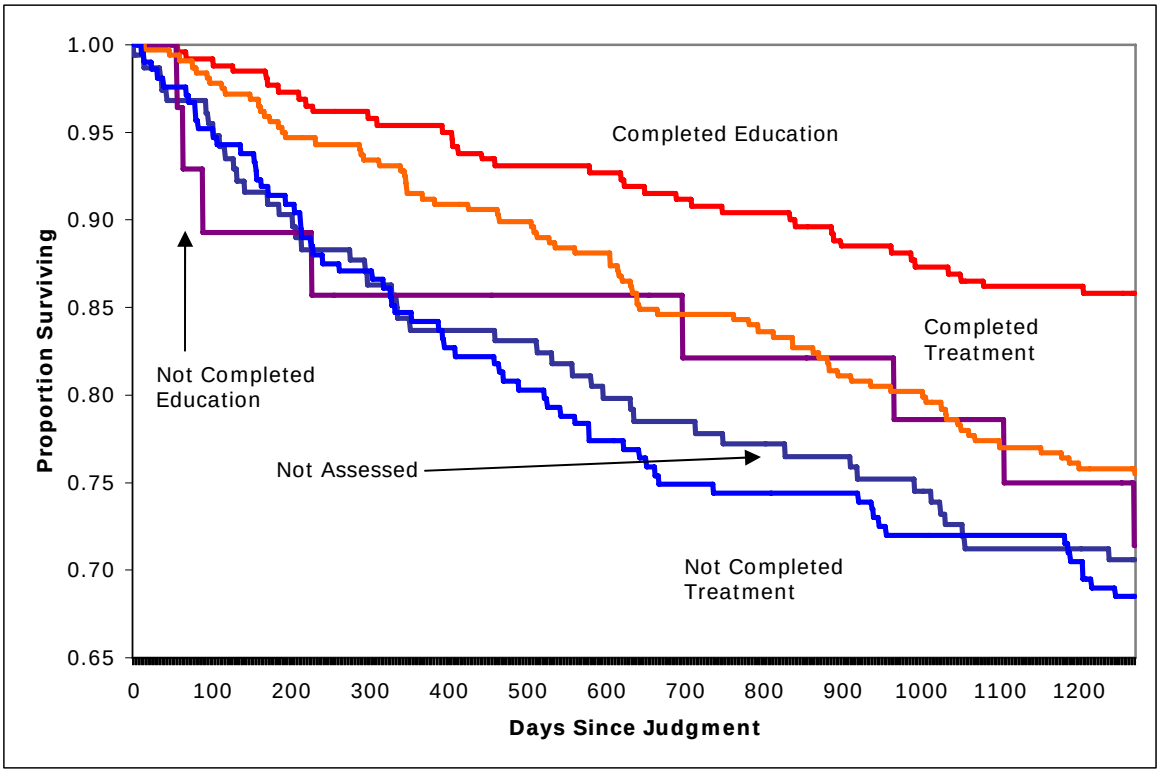
The survival curves of those not assessed and noncompleting either treatment or education are virtually indistinguishable. The stair-stepped appearance of the noncompleting education curve is a consequence of the small size of the group.

The survival curves of those that complete education and those that complete treatment are distinctive, both from each other and from the noncompleters and not assessed. Those that complete education are those *most* likely to survive. The slopes of the curves indicate that the period of time that most of the ASAP clients fail is the first 400 days.

The curves also show that half of the ASAP clients that were never assessed would be expected to be rearrested within approximately 16 months (approximately 500 days). In contrast, only 25 percent of the clients that completed education would be expected to have a new arrest within 23 months (approximately 700 days) of judgment.[11]

The survival curve for rearrest for an alcohol/drug crime within 42 months of judgment is plotted at Exhibit 9. The curves are markedly different than those for arrest for any crime. In order to view the differences between the curves, the scale on the Y axis of the chart was reduced so that the curves could be magnified. Use caution when comparing the curves in Exhibit 8 to those in Exhibit 9 recognizing the different measurement scales.

Exhibit 9 Survival Plot of Alcohol/Drug Arrests Within 42 Months of Judgment



Again, those ASAP clients that complete education show the highest rate of survival and those that complete treatment show the second highest rate of survival. The stair stepped appearance of the "not complete education" category is due to the small number (28 clients)

[11] Appendix A of this report calculates hazard rates, in addition to survival curves as a means of further understanding the information provided by the survival curves.

in the category. The survival curves for “not complete treatment,” and “not complete education,” don’t become distinguishable until approximately 400 days but then converge again at around 1,100 days.

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DESCRIPTIVE STATISTICS

This section of the audit describes the sample of ASAP clients studied as part of the recidivism analysis. Detailed descriptive statistics provide greater insight into the various groups of people served by ASAP and help identify key differences between comparison groups that may contribute to differences in recidivism rates. The demographics and outcomes described in this section were reviewed as part of the recidivism analysis to determine which variables significantly increase or decrease a client's likelihood of recidivating.

Analysis of ASAP clients

As described more fully in the Objectives, Scope, and Methodology section of the audit, the recidivism rates were calculated for a sample of FY 02 ASAP clients. Rates were compared for subgroups of ASAP clients, based on whether clients were assessed as needing substance abuse education or treatment and whether the clients completed the requisite education/treatment. Exhibit 10, below, outlines the sample by subgroup – "n" means the number of clients.

Exhibit 10

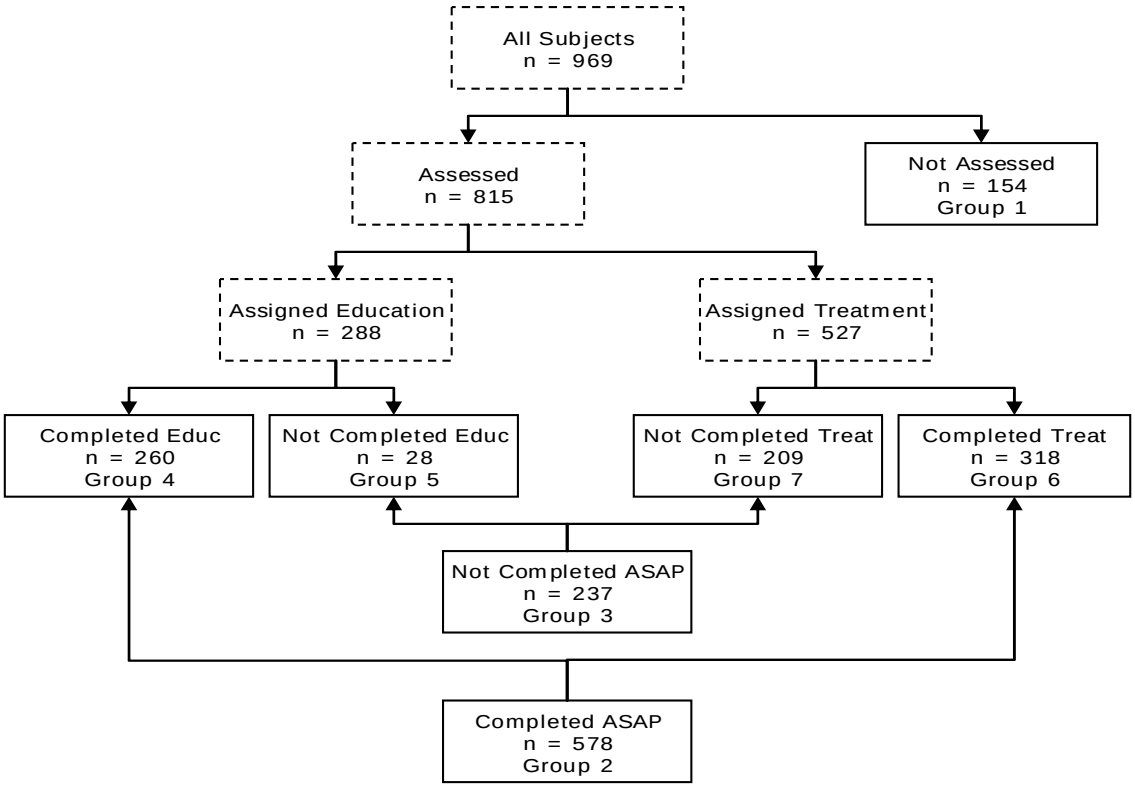

Exhibit 11, below, provides basic descriptive statistics for the sample of FY 02 ASAP clients. The statistics show, in part:

- More than half of the ASAP clients were assigned to the Anchorage ASAP office.
- More than 57 percent were white; nearly 30 percent were identified as Alaska Natives or American Indians.
- Most of the clients (76 percent) were male.
- On average, clients had 1.5 prior arrests and 1 prior conviction.

Exhibit 11 – Analysis Sample Measures and Descriptive Statistics

ASAP office where subject assigned for assessment		Average Number of Prior Arrests	
Anchorage	52.5%	All Crimes	1.48
Fairbanks	16.6%	Crimes against persons	0.36
Juneau	8.7%	Crimes against property	0.18
Kenai/Homer	9.9%	Crimes against society	0.49
Ketchikan	3.7%		
Mat-Su	8.6%	Average Number of Prior Convictions	
		All Crimes	1.01
Race/ethnicity		Crimes against persons	0.19
Alaska Native/American Indian	29.8%	Crimes against property	0.12
White	57.5%	Crimes against society	0.40
Other	12.7%		
Educational attainment		Average Age at ASAP judgment date	34.31
< 12th grade	16.2%		
12th grade/GED	46.3%	Gender	
Some college	37.4%	Female	24.4%
		Male	75.6%

Further analysis was done for the group of ASAP clients that attended treatment to gain an understanding of completion rates by treatment provider. The completion rates for the

Exhibit 12 – Completion Rates for Select Treatment Providers

Provider	Clients Served	Clients Not Completed	Clients Completed	Percent Completed
Unknown	83	78	5	6.0%
Salvation Army	69	24	45	65.2%
Starting Point	46	15	31	67.4%
Alaska Human Services	32	7	25	78.1%
Out-Of-State	28	6	22	78.6%
RITE	23	3	20	87.0%
Genesis House	23	2	21	91.3%
Mat-Su Council Recovery Center	18	6	12	66.7%
Gastineau Human Services	16	5	11	68.8%
The Recovery Connection	16	5	11	68.8%
Cook Inlet Tribal Corporation	15	5	10	66.7%

treatment providers that served 70 percent of the clients are identified in Exhibit 12 on the previous page. These providers were selected because they served the largest number of clients.

Additional descriptive statistics for the sample's 969 ASAP clients are shown in Exhibit 13 on the next page. This exhibit shows that the percentage of ASAP clients that were never assessed varied by ASAP location, with Fairbanks having the highest percentage (25 percent) and Kenai/Homer and Ketchikan having the lowest percent (6 percent). The high percentage of clients not coming into the ASAP office may be attributed to a reluctance on behalf of the Fairbanks' prosecutor to act on petitions to revoke probation filed with the prosecutor by the Fairbanks' ASAP office. Per the ASAP Fairbanks office administrator, for several years (including FY 02) the prosecutor's office stopped enforcing petitions to revoke probation (PTRP). Lax enforcement typically leads to increased noncompliance. This practice has subsequently been changed and PTRPs are currently being enforced by the prosecutors office, in the event a person does not show up for the ASAP assessment.

There was also a discrepancy between the percent of clients directed to education versus treatment. In Anchorage, 21 percent of clients were assessed as needing to attend education courses and 63 percent were assessed as needing treatment. The Mat-Su location also had a comparatively high percent of its clients assessed as needing treatment (69 percent) when compared to 23 percent needing education. The split between education and treatment was reversed in Fairbanks with 47 percent assessed as needing education compared to only 28 percent needing treatment. The difference may be linked to the availability of treatment providers and/or it may be linked to the types of cases/offenders that are directed to ASAP in each community.

Exhibit 13 –Demographics for Sample of ASAP Clients by Program Site

	Anchorage		Fairbanks		Juneau	
	Number of Clients	Percent of Clients	Number of Clients	Percent of Clients	Number of Clients	Percent of Clients
Program Outcome						
Never Assessed	81	16%	41	25%	17	20%
Education						
Completed	97	19%	68	42%	25	30%
Did Not Complete	12	2%	8	5%	3	4%
Treatment						
Completed	185	37%	27	17%	23	27%
Did Not Complete	134	26%	17	11%	16	19%
Total	509	100%	161	100%	84	100%
Age at time of Judgment						
18-25	125	25%	41	26%	28	33%
26-35	155	30%	49	30%	17	20%
36-45	148	29%	50	31%	29	35%
over 45	81	16%	21	13%	10	12%
Total	509	100%	161	100%	84	100%
Race						
Alaska Native/ American Indian	170	33%	48	30%	29	35%
White	253	50%	96	60%	48	57%
Other	86	17%	17	10%	7	8%
Total	509	100%	161	100%	84	100%
Education Level						
< 12th Grade	73	14%	12	7%	12	14%
12th Grade or GED	219	43%	57	36%	29	35%
Some College or Training	180	36%	49	30%	30	36%
Unknown	37	7%	43	27%	13	15%
Total	509	100%	161	100%	84	100%
Gender						
Female	138	27%	33	20%	16	19%
Male	371	73%	128	80%	68	81%
Total	509	100%	161	100%	84	100%

Exhibit 13 –Continued

Ketchikan		Kenai/Homer		Mat-Su		Total	
Number of Clients	Percent of Clients	Number of Clients	Percent of Clients	Number of Clients	Percent of Clients	Number of Clients	Percent of Clients
2	6%	6	6%	7	8%	154	16%
13	36%	38	40%	19	23%	260	27%
0	0%	5	5%	0	0%	28	3%
11	30%	32	33%	40	48%	318	33%
10	28%	15	16%	17	21%	209	21%
36	100%	96	100%	83	100%	969	100%
12	33%	30	31%	25	30%	261	27%
9	25%	21	22%	17	20%	268	28%
8	22%	24	25%	28	34%	287	29%
7	20%	21	22%	13	16%	153	16%
36	100%	96	100%	83	100%	969	100%
13	36%	17	18%	12	15%	289	30%
18	50%	73	76%	69	83%	557	57%
5	14%	6	6%	2	2%	123	13%
36	100%	96	100%	83	100%	969	100%
10	28%	16	17%	18	22%	141	15%
20	55%	42	44%	35	42%	402	42%
4	11%	35	36%	27	32%	325	33%
2	6%	3	3%	3	4%	101	10%
36	100%	96	100%	83	100%	969	100%
4	11%	28	29%	17	20%	236	24%
32	89%	68	71%	66	80%	733	76%
36	100%	96	100%	83	100%	969	100%

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APPENDIX

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RESEARCH REPORT March 2007

Recidivism Among Persons Court Ordered to Alaska's Alcohol Safety Action Program: Analysis of a Statewide Cohort

Mark Coggeshall

research for safer communities

URBAN INSTITUTE
Justice Policy Center

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CHAPTER 1.
Introduction

The Alaska Division of Legislative Audit (DLA) contracted with the Urban Institute (UI) Justice Policy Center for technical consultation on a study of recidivism among persons who were court referred to Alaska's Alcohol Safety Action Plan (ASAP) diversion program. UI provided the DLA with three services: (1) assistance with the selection of a simple random sample of the approximately 5,600 ASAP clients who were eligible for the study; (2) development of a Microsoft Access database application to assist DLA staff with data collection; and (3) data analysis and reportage. This report summarizes the data analysis performed by UI.

The purpose of the study is to examine whether the ASAP program affects the recidivism behavior, as indicated by new arrests and convictions, of those who are directed to complete it by the courts. In Alaska, the ASAP program is structured so that persons referred to the program by a court are required to appear to one of several ASAP offices in the state for needs assessment. Those persons who are assessed are referred to either drug and alcohol education or drug and alcohol treatment. Once a person is referred to either education or treatment, they may choose from among several dozen providers of ASAP services. Those persons who seek services may or may not complete the ASAP program successfully.

The study was conceived and conducted retrospectively. The only no-treatment comparison subjects (i.e., those who did not receive any ASAP treatment or education services) available for the analysis are those persons who, once court referred to ASAP, did not appear at an ASAP office for assessment (Group 1). This is because of the reporting requirements of the program: the state ASAP offices report on persons who are assessed, and the service providers report on persons who complete the program successfully but not on persons who enroll in the program but do not complete it. Consequently, in the study data it is not possible to identify those persons who were assessed by an ASAP office but never enrolled in an ASAP program. That is, the study does not include measures of the amount or dosage of ASAP services (either treatment or education) that subjects received.

All comparison subjects are, therefore, comparison subjects because they chose not to be assessed. That is, they self-selected into the comparison group. This fact poses a serious problem for the analysis because any observed differences between the recidivism of comparison subjects (i.e., those who were not assessed) and the recidivism of treatment subjects (i.e., those who were assessed) may be attributable to either (1) the effects of ASAP or (2) antecedent differences (e.g., motivation to change or reduce problem substance use) between the comparison and treatment subjects that may have affected both their decision to be assessed and their decision to commit a new offense. The purpose of the study is to estimate the effect of ASAP net of any antecedent differences among the subjects, so this ambiguity poses a significant problem.

Despite our efforts we were unable to devise a fully satisfactory analytic remedy for this selection problem. One common approach developing such a remedy would be to estimate a model of the selection process itself (i.e., the decision subjects made to be assessed or not) and then include the estimates from that model in a second-stage model of the outcome of interest (i.e., recidivism). However, we were unable to model the selection process well enough to justify using this approach. Our attempts to apply this technique were hampered by two factors. First, only demographic and criminal history measures were available and, collectively, these measures were not sufficient to develop a satisfactory model of the selection process. Second, approximately 16 percent of the subjects were not assessed leaving many fewer comparison subjects than treatment subjects. Consequently, we report recidivism estimates that are unadjusted for any effects of self-selection. The reported differences in recidivism between the subjects who were not assessed and the subjects who were assessed may be due to unmeasured attributes (e.g., motivation to behavioral change) rather than to the ASAP program itself.

We find that subjects who completed the ASAP educational program were less likely to recidivate during the follow-up period than were subjects who were never assessed. This was true regardless of whether recidivism was measured as a new arrest or as a new arrest leading to conviction. Recidivism was noticeably less common among subjects who completed the ASAP treatment program as compared with subjects who were never assessed, but this difference was not statistically significant after controlling for subject demographics and criminal history. Recidivism patterns among subjects who were assessed but who did not complete the ASAP program were similar to those among subjects who were never assessed.

When persons are court-ordered to complete ASAP, they are instructed to have their needs assessed by persons who are screeners for the ASAP program. Those who are identified as having lower risk for continued involvement with alcohol and/or drugs are assigned to complete the ASAP education program. Those judged to be at higher risk are assigned to the ASAP treatment program. The education program is less onerous than the treatment program as evidenced by the fact that some of the subjects assigned to it completed it within two weeks and the majority of subjects assigned to education completed it. The finding that subjects who completed education were less likely to recidivate than those who were never assessed is consistent with two explanations: (1) the ASAP education program ameliorated the criminal tendencies of those who completed it; or (2) the ASAP screening process reliably identifies persons at low-risk for recidivism and assigns them to education. Based on our familiarity with the research literature with interventions that attempt to reduce recidivism risk among persons identified as at-risk for future offending, we find the second explanation to be more plausible than the first. It is important to note, however, that the empirical evidence from this study is consistent with both explanations.

The remainder of this report is structured as follows: Chapter 2 describes the research questions, analysis plan, and sample data, Chapter 3 describes the analysis and findings in detail, and Chapter 4 discusses the findings and offers conclusions and recommendations for additional research.

CHAPTER 2.
Methods

This chapter summarizes the analysis plan, data processing, sample attrition, our aborted attempt to develop an inverse probability weight, and provides descriptive statistics on the key measures used in the analysis. All data processing and analysis was performed using software developed by the SAS Institute (http://www.sas.com). Unless otherwise noted, all statistical significance testing was conducted using two-tailed tests at the 95% confidence level (α=.05).

ANALYTIC OVERVIEW

The purpose of this research is to examine whether the ASAP program affects recidivism. This was to be done by analyzing recidivism rates for several groups of ASAP clients. Initially, the research design called for the analysis of clients who completed ASAP treatment, as well as comparing this group to other clients. In total, seven groups were specified: (1) Clients who were never assessed by the ASAP program; (2) clients who completed their assigned ASAP program (i.e., treatment or education); (3) Clients who did not complete their assigned ASAP program (i.e., treatment or education); (4) Clients who completed ASAP drug and/or alcohol education; 5) Clients who did not complete drug and/or alcohol education; (6) Clients who completed drug and/or alcohol treatment; and (7) Clients who did not complete drug and/or alcohol treatment. Figure 1 illustrates the relationships among the groups and the number of subjects in each group.[1] Since Group 2 (i.e., ASAP completers) is composed of subjects in Groups 4 and 6 and Group 3 (i.e., ASAP non-completers) is composed of subjects in Groups 5 and 7, the inferential portion of the analysis was composed on the set of mutually exclusive groups (i.e., Group 1 and Groups 4-7).

The analysis focused on two questions. First, is the post-judgment recidivism behavior, as measured by arrests and convictions for new offenses, different among persons who never appear for ASAP assessment (Group 1) than among those who were assessed (Groups 4-6) in the 42 months after the courts directed them to ASAP? Second, is the recidivism behavior of those who completed the ASAP education program (Group 4) different than that of those who completed the ASAP treatment program (Group 6) during the 24 months after they completed their ASAP assignments?

The sample included new ASAP cases during the fiscal year 2002, with judgments effective by the end of June 2002. This allowed enough time passage whereby clients would be assessed, and completed or be done with treatment for more than two years. Because the programs have not changed materially over the past five years, the 2002 fiscal year was determined to be representative. Moreover, the client must be 18 years old within three

[1] Only 28 subjects failed to complete the ASAP education program (Group 5). The small sample size accounts for the relative inefficiency (i.e., imprecision) of the estimates for these subjects in the remainder of the report.

months of the judgment date. Only clients convicted of a misdemeanor offense are included in this study.

The study used four measures of recidivism: (1) a new arrest for any offense; (2) a new arrest for an alcohol- or drug-related offense (e.g., driving while intoxicated, other alcohol- or drug-related traffic offenses, possession of marijuana or other illicit drugs); (3) a new arrest leading to conviction for any offense; and (4) a new arrest leading to conviction for an alcohol- or drug-related offense.. Statistical analysis was performed to identify whether select variables contributed to an increased likelihood of recidivism. The information on recidivism is obtained from several sources, including the Department of Public Safety database (APSIN), court records, and ASAP probation files. Measurement of recidivism began on the date the client's ASAP case was opened and continued through March 31, 2006.

There were a total of 5,526 files for ASAP clients. Due to time and cost, sampling procedures were used to create a representative subset of the population for analysis. The sample was compiled by creating separate lists of clients from each of six ASAP locations;[2] a simple random sample of clients was selected from the client pool using a pseudo-random number generator. The final sample included 969 subjects; this sample size was determined to be appropriate for this research effort.

In addition to client files specifying the ASAP program completion, demographic information was made available. Descriptive elements include: name, date of birth, race, education level, and gender. The treatment elements include the type of treatment, the provider, and the date treatment was completed.

DATA PROCESSING

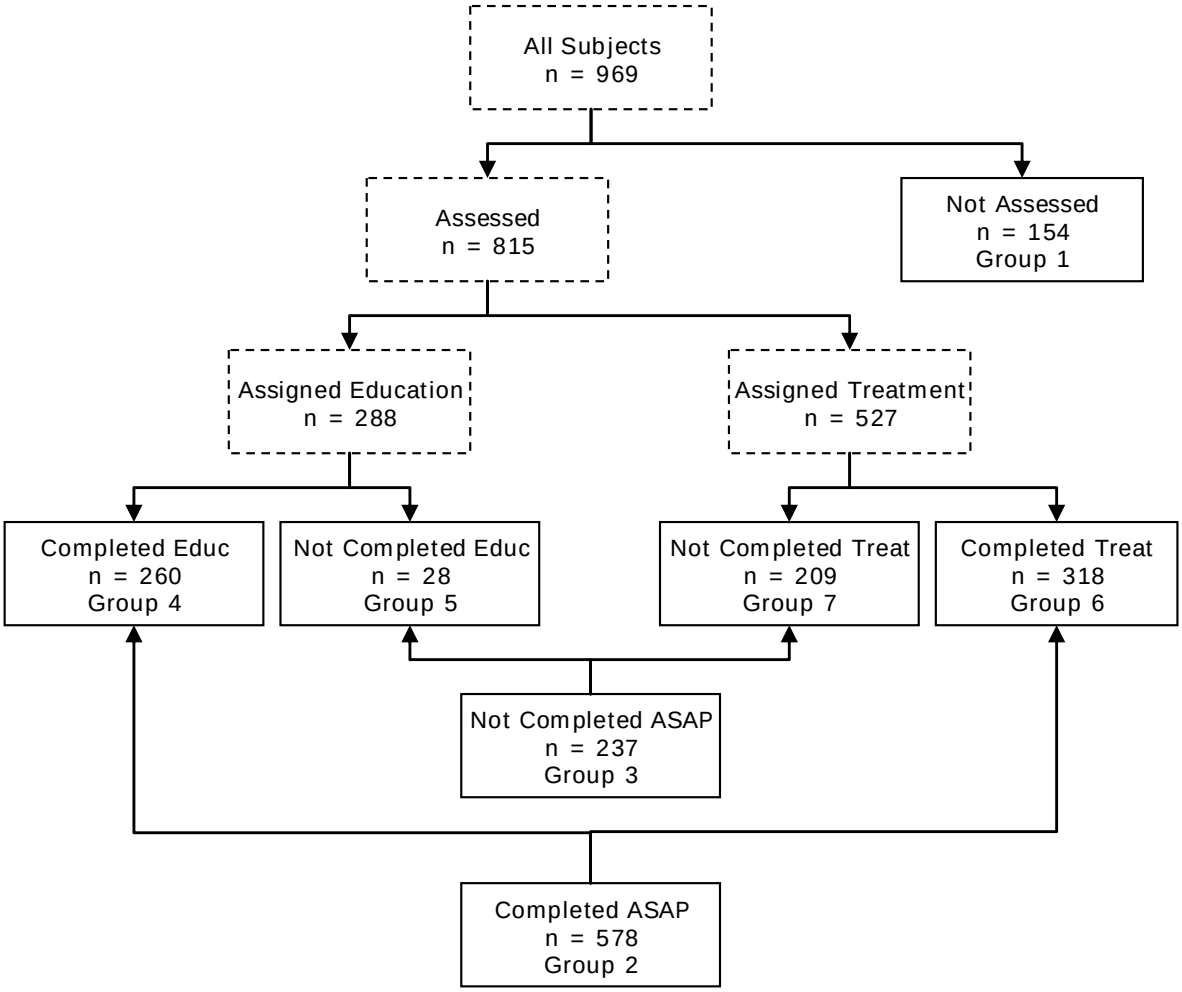
Auditors with the Division of Legislative Audit (DLA) assembled data that included information about 987 persons court referred to Alaska's Alcohol Safety Action Program (ASAP) and their adult criminal histories as provided by the Alaska Department of Public Safety. Most of the subjects were court-referred to ASAP during fiscal year 2002 (July 1, 2001 through June 30, 2002) and all were at least 18 years of age within 90 days of that court judgment date. Ninety subjects had judgment dates between 1990 and June 30, 2001 but appeared at an ASAP office for assessment during fiscal year 2002. Since some of these subjects waited years for assessment, we repeated portions of the analysis with these 90 early referral subjects excluded and, except where noted in Chapter 3, found that the exclusion of these subjects did not affect our findings.

The auditors assembled a total of 10,150 criminal history records, one record for each arresting charge faced by one of the subjects on or after July 1, 1986. We used the offense codes established for the FBI's National Incident-Based Reporting System (NIBRS) to categorize each charge (FBI, 2000). The NIBRS codes incorporate a seriousness scale such

[2] During FY 2002, there were more than six active ASAP offices throughout the state where clients were screened. By the time data collection for this study began in FY 2006, only six of those offices were still in operation, and the archived client records from the offices that closed were either unavailable or judged to be unreliable by DLA auditors. This means that the sample of subjects assembled for this study is representative of ASAP clients from the six offices that contributed client data to the sampling frame and is not representative of ASAP clients statewide.

that the code for homicide is ranked as more serious than forcible sex offenses, which are more serious than robbery and so on. We used the NIBRS codes to sort the charge records in descending order of seriousness and the top (i.e., most serious) charge from each of the 6,624 arrests was retained for analysis.

Figure 1. Analysis Groups and Sample Sizes



Since the arrest records were only comprehensive after 1986, we retained a uniform period of criminal history for each subject. Inspection showed that the data contained a minimum of 1,336 days of adult criminal history for each subject. This figure, which represents more than 3.7 years, is the number of days elapsed between July 1, 1986 and the earliest judgment date in February 1990.[3] For all subjects, arrests made more than 1,336 days prior to their ASAP judgment date were excluded from the analysis.[4]

When reviewing the criminal history records, it became apparent that some records were not responses to criminal activity. The following categories of criminal history records were

[3] The minimum criminal history interval increases to 5,479 days if the early referral cases are excluded.
[4] If a subject were arrested more than 1,336 days prior to their ASAP judgment date but convicted in connection with the arrest *less than* 1,336 days prior to their judgment date, the conviction was excluded from the analysis.

excluded from the analysis: placements into protective custody, probation or parole revocations, and technical violations committed while on pretrial release or post-conviction supervision. Additionally, records from warrant arrests were excluded from the analysis. The date that an arrest warrant is issued may be far removed from the time that the offense that prompted the warrant. Because of the ambiguity, we excluded this category of records as well.

Records where the disposition date was identical to the judgment date were also excluded as a measure of criminal history. These records describe the qualifying arrest or conviction, rather than a true prior crime. Moreover, only arrest dates were treated as failure dates, regardless of whether or not the case resulted in a conviction. For example, in a case in which a client was rearrested, and convicted of a new crime, the arrest date is recorded as the date of failure, rather than the conviction disposition date. The timing of case dispositions can be a result of a number of factors unrelated to the recidivism risk posed by the offender. For this reason, the conviction outcome measures are characterized as indicating that the subject had an arrest that lead to a conviction rather than that they simply had a new conviction. Using arrest dates to measure the timing of conviction failures has the effect of reducing the differences between the arrest- and conviction-related recidivism measures. This means that the analysis of the conviction outcomes is somewhat more likely to yield findings similar to those from the analysis of the arrest outcomes.

As stated previously, March 31, 2006 was the final date for the follow-up period. Therefore, any known arrests that took place after this date are not recorded in this analysis as recidivism, but arrests that occurred prior to the cutoff date and ended in a conviction *after* the cutoff date but before June 20, 2006 were counted as recidivism events on both arrest and conviction measures. During the follow-up period, eight clients died without recidivating; these clients were recorded as right-censored cases (i.e., the *CENSORED variables were coded equal to 1) with the follow-up period ending on the date of death.

SAMPLE ATTRITION

The original sample included 987 subjects. In order to perform statistical analysis to determine the affect of ASAP on recidivism, it was necessary to exclude subjects who did not meet the requirements of this study. Eighteen subjects were excluded from the analysis due to missing information on key measures: sixteen of the subjects were missing race and two of these subjects were also missing gender. One subject was missing date of birth (from which age of the subject was computed). An additional subject was coded as having been assessed as completing their ASAP obligation, but the type of program they completed—education or treatment–was unknown. The missing information prevented the subject from being categorized as a member of any of the seven analysis groups, and therefore the subject was excluded. An analysis was conducted to determine whether the 18 excluded subjects, some of whom were missing data more than one measure, differed from the 969 retained subjects on any of the measures relevant to the analysis.[5]

[5] Data on educational attainment were missing for 115 of the original 987 subjects. Rather than lose so many subjects from the analysis, we excluded the variable. We determined that variable was not essential to the analysis after estimating logistic regression models of the recidivism outcomes and finding that, after controlling

This analysis used chi-square tests to contrast the groups on categorical measures (e.g., group membership, race) and *t* tests to contrast the groups on continuous measures (e.g., age). The analysis showed that non-assessed subjects (Group 1) were overrepresented among the 18 excluded subjects but that the differences between two groups were not greater than would be expected by chance.[6] The exclusion of these subjects left 88 early referral subjects in the analysis sample.

OUTCOME MEASURES

The ASAP judgment date was used as the beginning of the follow-up period for all subjects, and each subject's follow-up period spanned 3.5 years (i.e., 1,279 days) from that date. A supplemental analysis was undertaken of the subjects who completed their ASAP obligation (i.e., those in Groups 4 and 6). For this analysis the follow-up period began at the date of ASAP completion and extended for up to two years (i.e., 731 days).

Of the 969 subjects, sixty-one completed their ASAP assignments and did not have a new arrest during follow-up but had post-completion follow-up periods less than two years (i.e., 731 days) in duration. Seventy-two subjects had no post-completion convictions but had truncated post-completion follow-up periods. These were subjects who took longer than average to complete their assignments and, by virtue of this delay, shortened their own post-completion follow-up periods.[7] If there were a strong correlation between the length of time required to complete ASAP (TIME2COMPLETE) and the number of days between judgment and a recidivism event, either any new arrest (TIME2ARST) or any new conviction (TIME2CONV), any comparison of the 42-month recidivism behavior of the completers (i.e., Groups 4 and 6) with their post-completion recidivism may be misleading if these subjects with truncated follow-up periods were retained in the analysis. In this sample, however, the correlation between completion time and survival time is not statistically significant ($\alpha = 0.05$, two-tailed test).[8] Thus, the subjects whose post-completion follow-up periods were less than two years were retained in the analysis.

WEIGHT ESTIMATION

As discussed previously, subjects who were not assessed effectively were self-selected into the comparison group (Group 1). This created the issue of endogeneity, in which a variable in the model may be correlated with other factors relegated to the error term. To

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for the effects of the other demographics (e.g., age, sex, race/ethnicity), educational attainment was not a statistically significant predictor of recidivism.

[6] It is unsurprising that the subjects in Group 1 were overrepresented among those subjects excluded for missing data. The case files created during the ASAP assessment process were an important source of data for the study. Since the Group 1 subjects were never assessed, no case files were created for them.

[7] The 260 subjects who completed education (Group 4) completed in an average of 223 days (standard deviation = 387 days). The 318 subjects who completed treatment (Group 6) completed in an average of 617 days (standard deviation = 512 days). By contrast, the 72 subjects who completed either treatment or education but whose post-completion follow-up period was less than two years required an average of 1,185 days from judgment to complete their ASAP program (standard deviation = 298).

[8] Specifically, the correlations between TIME2COMPLETE and TIME2ARST and TIME2CONV were 0.05 ($P = 0.20$) and 0.09 ($P = 0.04$), respectively among the 578 subjects who completed the ASAP program.

adjust for the endogeneity problem, a logistic regression model was estimated with a dummy variable identifying members of Group 1 specified as the dependant variable. Several independent variables were specified as predictors: the site (five categories—Anchorage, Fairbanks, Juneau, Kenai/Homer, and Ketchikan—plus Mat-Su as the omitted reference category), race (two categories—Alaska Native/American Indian and White—plus other as the omitted reference category), gender, and a count of prior arrests for property offenses (PRIORARSTPTY). These independent variables were selected to maximize model fit while minimizing subject attrition due to missing data.[9]

All independent variables were statistically significant, but the model explained only about 5 percent of the variation in the dependent variable. The parameter estimates indicated that ASAP subjects sent to Fairbanks for assessment were significantly less likely to be assessed than subjects who were sent to Mat-Su. Males were less likely than females to be assessed; Alaska Natives were less likely than persons of 'Other' ethnic groups (i.e., other non-whites in this case) to be assessed. Finally, subjects with more prior arrests for property offenses were less likely to be assessed than those with fewer property priors. The logistic regression model was estimated to generate for each subject a predicted probability that the subjects were not assessed. The inverse of the predicted probabilities was used to create an inverse probability weight following an approach commonly used to adjust for missing data (Robins, Rotnitzky, and Zhao, 1995). Unfortunately, as suggested by the low percentage of the variation explained by the model, the resulting weight variable was badly skewed. We might have truncated the outlying weight values and continued, but we were dissuaded by yet another consideration. The suitability of the inverse probability approach is contingent on whether an ignorability assumption is met. Conditional on the covariates, the selection mechanism (represented by the weight in this case) must be uncorrelated with the other unmeasured influences on the selection process (e.g., the subject's motivation to change their behavior). Given the small amount of variation in the selection process that our model was able to explain, we were unconvinced that we had adequately modeled the selection process to the point where the ignorability assumption had been met. Rather than risk muddling the results with an inadequate remedy, we decided to proceed with the analysis without attempting to correct for the selection problem.

MEASURES

Table 1 provides basic descriptive statistics on the analysis sample. More than half of the subjects were assigned to be assessed at the Anchorage site. More than 57 percent were white;

[9] Several alternative specifications of this model were examined. The best alternative specification included age as a predictor in lieu of PRIORARSTPTY. The -2 log likelihood (-2LL) of the model that included both age and PRIORARSTPTY was 752.9. Excluding age increased the -2LL value to 756.5. Since the models are nested and differ by only one parameter, the difference in the -2LL values is distributed as chi-square with 1 degree of freedom. The chi-square value (756.5 – 752.9 = 3.6) is not statistically significant. The Hosmer and Lemeshow goodness-of-fit test partitions the data into deciles and uses a chi-square test to examine how well the model reproduces the observed outcome. The Hosmer and Lemeshow value for the model that included PRIORARSTPTY but excluded age was somewhat lower (χ^2=2.03, df = 7, P(>χ^2)=0.96) than that for the model that included both variables (χ^2=8.67, df = 8, P(>χ^2)=0.37). Moreover, the age parameter was not statistically significant when PRIORARSTPTY was also included, suggesting that the two variables were explaining much of the same variance in the outcome.

nearly 30 percent were identified as Alaska Natives or American Indians. Most of the subjects (76 percent) were male. On average they had experienced 1.48 prior adult arrests and 1.01 prior convictions. Overall, 53 percent were rearrested within 42 months of their ASAP judgment date, and 44 percent had a new arrest that lead to conviction over the same time span. Twenty-four percent of the subjects were rearrested for an alcohol-or drug-related offense within 42 months of their judgment date, and 19.9 percent were convicted of a new alcohol- or drug-related offense during the same period of time.

Table 1. Analysis Sample Measures and Descriptive Statistics (n = 969)

Variable	Label	Percentage or Mean, se	
Attributes			
SITE	ASAP office where subject was to have been assessed	Anchorage	52.5%
		Fairbanks	16.6%
		Juneau	8.7%
		Kenai/Homer	9.9%
		Ketchikan	3.7%
		Mat-Su	8.6%
RACEREC	Race/ethnicity	Alaska Native/ American Ind.	29.8%
		White	57.5%
		Other	12.7%
EDUCREC	Educational attainment (n = 868)	< 12th grade	16.2%
		12th grade/GED	46.3%
		Some college	37.4%
EARLYREFERRAL	Judgment date prior to FY 2002?	Yes	9.1%
		No	90.9%
MALE	1 = male; 0 = female	Male	75.6%
		Female	24.4%
AGE	Integer age on ASAP judgment date	mean	34.31
		std. error	0.36
TIME2COMPLETE	Time (in years) between judgment date and completion date (n = 587)	mean	1.20
		std. error	0.06
PRIORARST	Count of prior arrests	mean	1.48
		std. error	0.07
PRIORARSTPER	Count of prior arrests for crimes against persons	mean	0.36
		std. error	0.03
PRIORARSTPTY	Count of prior arrests for crimes against property	mean	0.18
		std. error	0.02
PRIORARSTSOC	Count of prior arrests for crimes against society	mean	0.49
		std. error	0.03
PRIORARSTTRF	Count of prior arrests for traffic offenses	mean	0.22
		std. error	0.02
PRIORCONV	Count of prior convictions	mean	1.01
		std. error	0.05
PRIORCONVPER	Count of prior convictions for crimes against persons	mean	0.19
		std. error	0.02
PRIORCONVPTY	Count of prior convictions for crimes against property	mean	0.12
		std. error	0.02
PRIORCONVSOC	Count of prior convictions for crimes against society	mean	0.40
		std. error	0.02
PRIORCONVTRF	Count of prior convictions for traffic offenses	mean	0.15
		std. error	0.01

Outcomes			
ARST42CENSORED	Arrested within 42 months of ASAP judgment? 1 = No; 0 = Yes	Yes	52.6%
		No	47.4%
CONV42CENSORED	Arrest ending in conviction within 42 months of ASAP judgment? 1 = No; 0 = Yes	Yes	44.1%
		No	55.9%
ARST42AODCENSORED	Arrested on alcohol/drug charge within 42 months of ASAP judgment? 1 = No; 0 = Yes	Yes	24.0%
		No	76.0%
CONV42AODCENSORED	Arrest ending in conviction on alcohol/drug charge within 42 months of ASAP judgment? 1 = No; 0 = Yes	Yes	19.9%
		No	80.1%
ARST24CENSORED	Arrested within 24 months of ASAP completion? 1 = No; 0 = Yes	Yes	26.8%
		No	73.2%
CONV24CENSORED	Arrest ending in conviction within 24 months of ASAP completion? 1 = No; 0 = Yes	Yes	19.4%
		No	80.6%
ARST42TIME	Number of days between ASAP judgment and first arrest, if any, within 42 months	mean	819.39
		std. error	16.19
CONV42TIME	Number of days between ASAP judgment and first arrest, if any, within 42 months that lead to conviction	mean	889.05
		std. error	15.73
ARST42AODTIME	Number of days between ASAP judgment and first alcohol/drug arrest, if any, within 42 months	mean	1090.94
		std. error	12.02
CONV42AODTIME	Number of days between ASAP judgment and first arrest, if any, within 42 months that lead to alcohol/drug conviction	mean	1123.83
		std. error	11.09
ARST24TIME	Number of days between ASAP completion and first arrest, if any, within 24 months	mean	569.57
		std. error	10.27
CONV24TIME	Number of days between ASAP completion and first arrest, if any, within 24 months that lead to conviction	mean	602.39
		std. error	9.38

CHAPTER 3.
Results

The analysis proceeded in three stages. During the first stage, we used logistic regression to model the binary outcome variables to identify which of the covariates were related to each outcome.[10] The second stage of the analysis used Kaplan-Meier and life tables methods to examine the duration outcome variables.[11] This stage yielded non-parametric estimates of the survival curves and of the hazard rates over specified intervals. In the third and final stage, we estimated a Cox regression model of recidivism among the subjects who completed ASAP to test whether their post-completion recidivism risk differed from their pre-completion recidivism risk.[12]

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[10] Logistic regression is a widely used technique for estimating inferential models of binary outcomes (i.e., those that may take only one of two possible values, yes or no, true or false). For continuous outcomes (i.e., those that do not have fixed minimum or maximum values), a different type of regression is typically used wherein the outcome variable is modeled directly, a linear relationship is assumed between the outcome and one or more predictor variables, and the parameter estimates associated with each predictor are estimated by the non-iterative least squares method. This type of regression, known as ordinary least squares (OLS) regression is not appropriate for binary outcomes because it imposes neither an upper nor a lower limit on the values of the outcome estimated from the model and because it assumes a linear relationship between the outcome and the predictors. To avoid these problems, logistic regression models are designed to produce estimates of the logged odds that the outcome variable takes one of its two possible values. If the outcome variable is Y and takes values of 1 or 0, the odds of Y is equal to the probability $Y = 1$ divided by the probability that $Y = 0$. The logged odds of Y are equal to the natural logarithm of the odds of Y. By specifying the model in terms of the odds, the predicted values of the model are bounded between 1 and 0. The log transformation makes the distribution of the predicted values appropriately non-linear (Menard, 1995). Aside from the transformation of the expected outcome, logistic regression and OLS regression models are similar in form.

[11] The Kaplan-Meier and life table methods are non-parametric approaches to the analysis of duration data. Non-parametric methods do not require the analyst to make assumptions about the distribution of the outcome of interest, recidivism, in our case. This is advantageous since, if such an assumption is required, the validity of the results is conditional on the appropriateness of the assumption. Our application of these methods is purely descriptive; we do not use them as a foundation for hypothesis testing. For each observed failure time, k, the Kaplan-Meier method yields an estimate of the probability that a subject from the sample would survive to time k with appropriate adjustments for any subjects whose follow-up period was censored (i.e., their follow-up period ended before they were observed to recidivate) at a time prior to k. Unlike the Kaplan-Meier method, which uses the observed event times to establish the boundaries for the intervals, the life table method permits the analyst to establish arbitrary boundaries. Survival estimates are then computed at the midpoint of each boundary under the assumption that the distribution of censored observations is uniform over the interval (Allison, 1995). In exchange for this assumption, the life table method permits the computation of hazard rates (i.e., instantaneous failure rates) at the midpoint of each interval. We chose to use the Kaplan-Meier estimates as the basis for survival curves and the life table method to estimate hazard rates.

[12] Cox regression is a widely used framework for estimating inferential models of duration data (i.e., models where the outcome is the length of time until some event occurred). It is widely used because of its flexibility, being readily adaptable to handle time-dependent covariates (i.e., independent variables with values that vary, within subjects, over the course of the follow-up period) and periods where subjects are not at risk of an event. Cox regression is characterized as a semi-parametric procedure because it assumes a constant hazard for subjects with a value of zero on all of the covariates (Allison, 1995). This turns out to be weak assumption, however, since there need not be any subjects fitting that description.

DESCRIPTIVE ANALYSIS

Before beginning the inferential modeling, we estimated the proportion of subjects who recidivated during the 42 months following their ASAP judgment dates and, for those who completed ASAP, the proportion recidivating during the 24 months following ASAP completion (see Table 2 and Table 3). This simple analysis showed that the proportion of non-assessed subjects (Group 1) with a new arrest or conviction was nearly identical to the proportion of non-completers (Groups 3, 5, and 7) recidivating. Approximately, 66 percent of the non-assessed subjects and the non-completers were rearrested within 42 months of their judgment dates, and approximately 59 percent of the same subjects experienced a new arrest that lead to conviction during the same time period. By contrast, only 43 percent of the ASAP completers were rearrested and only 34% were arrested and convicted during the post-judgment follow-up period. Unlike the non-completers and the non-assessed subjects, there was a notable difference in the proportion of education completers (Group 4) who failed as compared with the treatment completers (Group 6), with the former being less likely to re-offend than the latter. The same pattern held when we examined recidivism over the 24 months following completion for these two groups, and when we restricted our focus to new arrests and convictions related to alcohol or other drugs (AOD) (see Table 3).

We also computed the proportion of subjects who recidivated for each of several crossed combinations of several of the categorical measures (e.g., group, education, race, site, and sex). The results of this analysis, which we repeated for each of the four post-judgment recidivism measures (i.e., arrest, AOD arrest, conviction, and AOD conviction), are included in Appendix A. We also computed client completion rates and recidivism rates for 16 selected providers of ASAP treatment (see Appendix A).

LOGIT MODELS OF BINARY OUTCOMES

Having established the basic recidivism rates of the groups, we estimated logistic regression models of each of the four binary outcome variables. The estimation of each model began with the estimation of a full model that included dummy variables[13] to represent the groups of interest, MALE, RACEREC, AGE, and all ten of the criminal history measures (see Table 1 for a description of these measures). Since the criminal history measures are highly correlated many were not significant in these initial models. From this point, we iteratively dropped insignificant criminal history variables from the model in search of a parsimonious model that fit the data as well as the full model. We retained the group dummies and the demographic terms in these models even when they were not significant to illustrate the relationships between the covariates and the outcomes. Estimates of the model parameters are reported as odds ratios rather than beta values, which are more difficult to interpret.

[13] A dummy variable is a binary variable that is set equal to one (1) to indicate that a condition is true and zero (0) to indicate it is false.

Table 2. Proportion Recidivating by Group and Outcome

	Proportion recidivating within…			
	…42 months of ASAP judgment		…24 months of ASAP completion	
	Proportion	std. error	Proportion	std. error
New Arrest				
All Subjects	0.526	0.016		
Not Assessed (Group 1)	0.662	0.038		
Completers (Group 2)	0.427	0.021	0.268	0.018
Non-Completers (Group 3)	0.679	0.030		
Completed Education (Group 4)	0.327	0.029	0.227	0.026
Not Completed Education (Group 5)	0.679	0.090		
Completed Treatment (Group 6)	0.509	0.028	0.302	0.026
Not Completed Treatment (Group 7)	0.679	0.032		
New Arrest Leading to Conviction				
All Subjects	0.441	0.016		
Not Assessed (Group 1)	0.571	0.040		
Completers (Group 2)	0.343	0.020	0.194	0.016
Non-Completers (Group 3)	0.595	0.032		
Completed Education (Group 4)	0.246	0.027	0.158	0.023
Not Completed Education (Group 5)	0.607	0.094		
Completed Treatment (Group 6)	0.421	0.028	0.223	0.023
Not Completed Treatment (Group 7)	0.593	0.034		

Note: Shaded cells denote post-completion recidivism rates that are undefined because some of the subjects in the reference group did not complete the ASAP program.

Table 3. Proportion with New Alcohol/Drug Offenses by Group and Outcome

	Proportion recidivating within 42 months of ASAP judgment	
	Proportion	std. error
New Alcohol/Drug Arrest		
All Subjects	0.240	0.014
Not Assessed (Group 1)	0.292	0.037
Completers (Group 2)	0.199	0.017
Non-Completers (Group 3)	0.308	0.030
Completed Education (Group 4)	0.142	0.022
Not Completed Education (Group 5)	0.286	0.087
Completed Treatment (Group 6)	0.245	0.024
Not Completed Treatment (Group 7)	0.311	0.032
New Arrest Leading to Alcohol/Drug Conviction		
All Subjects	0.199	0.013
Not Assessed (Group 1)	0.240	0.035
Completers (Group 2)	0.170	0.016
Non-Completers (Group 3)	0.245	0.028
Completed Education (Group 4)	0.123	0.020
Not Completed Education (Group 5)	0.214	0.079
Completed Treatment (Group 6)	0.208	0.023
Not Completed Treatment (Group 7)	0.249	0.030

Post-Judgment Recidivism

Models of the 42-month binary outcomes—ARST42CENSORED, ARST42AODCENSORED, CONV42CENSORED, and CONV42AODCENSORED—were estimated with dummy variables indicating membership in groups 4-7 with the non-assessed subjects (Group 1) serving as the omitted reference group. The full model of the 42-month arrest outcome included an intercept, dummy variables for Groups 4-7, MALE, AGE, RACEREC, and the ten criminal history variables. Based on the results of this model, a second model was estimated in which only one of the ten criminal history variables—

PRIORARST—was retained. A difference of log likelihoods test showed that the fit of the second more parsimonious model was not significantly worse than that of the first model (-2LL = 1135.19-1125.51 = 9.68; d.f. = 9; $P(>\chi^2) = 0.37$), so the second model was retained for interpretation (see Table 4). The model indicates that: (1) subjects who completed ASAP education (Group 4) were less likely than subjects who were not assessed (Group 1) to recidivate during the follow-up period; (2) subjects who completed ASAP treatment (Group 6) were *not* significantly less likely than subjects who were not assessed (Group 1) to recidivate; (3) subjects with a greater number of prior arrests were more likely to recidivate, and (4) older subjects were less likely to recidivate. Race and sex were not significantly related to recidivism after controlling for the other factors. Subjects in Groups 5-7 were not significantly more or less likely to recidivate than were the non-assessed subjects in Group 1. Hosmer and Lemeshow goodness-of-fit tests showed that all three models reproduced the data acceptably well ($\chi^2 = 1.31$; d.f. = 8; $P(>\chi^2) = .99$) and the model explained 19 percent of the variation. The results of the analysis of arrests over the 42-month follow-up period were substantively identical when the model was re-estimated after excluding the subjects whose judgment dates were prior to FY 2002.[14] We also considered an alternative specification of this model to examine whether the location of the ASAP office to which the subjects were referred for screening (SITE) was related to recidivism risk. See Appendix B for an explanation of this model.

Table 4. Arrest Within 42 Months of Judgment: Logistic Regression Estimates

		95% Confidence Interval	
Effect	**Odds Ratio**	**Lower Limit**	**Upper Limit**
Group4	0.50	0.31	0.80
Group5	1.70	0.69	4.22
Group6	0.77	0.49	1.19
Group7	1.17	0.72	1.89
Male	1.20	0.86	1.67
Age	0.97	0.96	0.98
Alaska Native vs. Other	1.26	0.78	2.05
White vs. Other	0.91	0.59	1.41
PriorArst	1.57	1.41	1.76

To make the implications of the model easier to interpret, we used the model to estimate the probability of recidivism (i.e., a new arrest during the 42-month follow-up period) for 'typical' subjects in each analysis group. We computed the mean of the binary and continuous covariates (e.g., MALE , AGE, and PRIORARST) within each group and used those means to define our typical subjects.[15] Groups 1 and 7 included approximately equal proportions of Alaska Natives and whites, so we estimated predicted probabilities of a new arrest for a typical Alaska Native and for a typical white for each of these two groups. Most subjects in

[14] Here and elsewhere in this report the phrase 'substantively identical' is intended to mean that the inferences about the significance of each term in the model were the same and that the direction of the significant effects was the same. In this case, only three terms—GROUP4, AGE, and PRIORARST—were jointly significant in any of the models, and the direction of these three effects was the same in all of the models.

[15] For example, if 75 percent of the subjects in a group were male, the typical subject for that group would have a value of 0.75 on MALE since MALE = 1 if the subject is male and 0 if the subject is female. This makes for a hypothetical subject who is neither fully male nor fully female but who is typical of subjects in the group with respect to biological sex.

the other groups were white, so we estimated predicted probabilities of typical whites for those groups. Age has an important effect on recidivism risk, but the size of the effect is difficult to apprehend from the odds ratio alone. So, for each of the five groups, we computed values of AGE at the 25th, 50th, and 75th percentiles, and estimated the probability of recidivism at each of these percentile levels.

Table 5 displays the results of this analysis for the model of post-judgment arrests. It shows that a 'typical' 24-year-old Alaska Native who was never assessed (Group 1) has 0.79 probability of being rearrested with 42 months of judgment. By contrast, if the same hypothetical subject were 14 years older at the time of judgment, their risk of recidivism would be 0.71.[16] A 23.5 year old white subject who completed ASAP education (Group 4) has a 0.38 probability of failure. A 24 year old white subject who is never assessed (Group 1) has nearly twice the recidivism risk (0.73).

Table 5. Predicted Probability of Arrest Among 'Typical' Subjects

Group	Age	Race	Predicted Probability of Recidivism
1	24	Alaska Native	0.79
1	31	Alaska Native	0.75
1	38	Alaska Native	0.71
1	24	White	0.73
1	31	White	0.69
1	38	White	0.64
4	23.5	White	0.38
4	33.5	White	0.31
4	42.5	White	0.26
5	21.5	White	0.75
5	30	White	0.70
5	39	White	0.64
6	26	White	0.57
6	35	White	0.51
6	42	White	0.46
7	27	Alaska Native	0.80
7	36	Alaska Native	0.76
7	41	Alaska Native	0.73
7	27	White	0.75
7	36	White	0.70
7	41	White	0.66

We used an analogous procedure to estimate a logistic model of arrests leading to conviction during the 42 months following the judgment date (CONV42CENSORED). The specification of the full model was identical to that used for the full model of the post-judgment arrest outcome variable. Based on the results of this model, a second model was estimated in which four of the ten criminal history variables—PRIORARSTPER, PRIORARSTSOC, PRIORCONV, and PRIORCONVPER—were retained. A difference of log likelihoods test showed that the fit of the second more parsimonious model was not significantly worse than that of the first model (-2LL = 1115.20 – 1109.91 = 5.29; d.f. = 6;

[16] Note that subjects with ages of 24, 31, and 38 were used in Group 1 because those values represent the 25th, 50th, and 75th percentiles, respectively, of AGE within that group. The effects that were included in the model (see Table 3) but not in Table 4 are held constant at their within-group mean values across subjects. That is, the PRIORARST term is constant for all the Group 1 subjects in Table 4 and for all the Group 4 subjects, but the value of PRIORARST for a Group 4 subject is not equal to the value of PRIORARST for a Group 1 subject.

$P(>\chi^2) = 0.51$), so the second model, which explained 20 percent of the variation in the outcome, was retained for interpretation (see Table 6). The GROUP4, AGE, and all four retained criminal history terms are significant. Subjects with greater numbers of prior arrests for person or society offenses, or a greater number of prior convictions, were more likely to be convicted again during the follow-up period. Note, however, that subjects with a greater number of prior convictions for offenses against society (PRIORCONVSOC) (e.g., drug offenses, weapon offenses, prostitution, DWI, disorderly conduct) were *less* likely to be convicted again after controlling for the other effects in the model. Sex (MALE), race (RACEREC), and the dummy variables for groups 5-7 were not significant. As was the case with the model of post-judgment arrests, omitting the early referral subjects from the sample lead to substantively identical findings except that PRIORARSTSOC dipped slightly below the statistical significance threshold.

Table 6. Conviction Within 42 Months of Judgment: Logistic Regression Estimates

		95% Confidence Interval	
Effect	**Odds Ratio**	**Lower Limit**	**Upper Limit**
Group4	0.50	0.31	0.80
Group5	1.85	0.77	4.43
Group6	0.85	0.55	1.32
Group7	1.25	0.78	2.00
Age	0.97	0.95	0.98
Male	1.12	0.80	1.58
Alaska Native vs. Other	1.10	0.68	1.78
White vs. Other	0.79	0.51	1.22
PriorArstPer	1.46	1.08	1.97
PriorArstSoc	1.44	1.01	2.06
PriorConv	1.68	1.35	2.09
PriorConvSoc	0.53	0.35	0.80

We repeated the analysis of predicted probability of failure for the same set of 'typical' subjects used before. The results (see Table 7) are similar to those found in the analysis of post-judgment arrests.

We also conducted a similar analysis of the measures of alcohol- and drug-related (AOD) arrest within 42 months of judgment (ARST42AOD) and AOD arrest leading to conviction within 42 months of judgment (CONV42AOD). Overall, the results of this analysis were similar to those from the foregoing analysis of all arrests and convictions. Subjects who completed the ASAP education program (GROUP4) were significantly less likely than subjects who were never assessed (GROUP1) to experience an AOD arrest or conviction. None of the other group dummy variables was statistically significant in the AOD models. The results of the analysis of the AOD outcomes are presented in Appendix C.

Table 7. Predicted Probability of Conviction Among 'Typical' Subjects

Group	Age	Race	Predicted Probability of Recidivism
1	24	Alaska Native	0.71
1	31	Alaska Native	0.65
1	38	Alaska Native	0.59
1	24	White	0.63
1	31	White	0.57
1	38	White	0.51
4	23.5	White	0.29
4	33.5	White	0.22
4	42.5	White	0.17
5	21.5	White	0.67
5	30	White	0.60
5	39	White	0.52
6	26	White	0.48
6	35	White	0.40
6	42	White	0.34
7	27	Alaska Native	0.74
7	36	Alaska Native	0.67
7	41	Alaska Native	0.63
7	27	White	0.67
7	36	White	0.59
7	41	White	0.55

Post-Completion Recidivism

Next, we estimated models of recidivism within 24 months of ASAP completion using an analogous approach with 587 subjects who completed their ASAP assignments. For these models, we included only one group dummy variable (GROUP4) making Group 6 the omitted reference category. We also included two new covariates—EDUCREC and TIME2COMPLETE—in our full model specifications. The measure of educational attainment was missing for 37 of the subjects who completed ASAP, so we retained it in the model only when it was statistically significant or when it improved the overall model fit. The final models of the post-completion arrest and conviction outcomes explained only approximately seven percent of the variance in their outcomes. This indicates that the overall fit of the post-completion outcome models was substantially worse than that of the post-judgment outcome models. One or more factors that explain post-completion failure were not measured by the study.

The reduced model of post-completion arrests (ARST24CENSORED) included the demographic terms, the Group 4 dummy variable, and two criminal history measures: prior arrests (PRIORARST) and prior convictions for crimes against society (PRIORCONVSOC). The fit of the reduced model was not significantly worse than that of the full model (-2LL = 594.63 – 581.79 = 12.84; d.f. = 8; $P(>\chi^2) = 0.12$). The reduced model showed that, after controlling for the other effects, recidivism risk among subjects in Group 4 was significantly different from that among subjects in Group 6 (see Table 8). Older subjects were less likely to recidivate. The two criminal history effects were also significant: Subjects with more prior convictions (PRIORCONV) were more likely to recidivate after completion, and subjects with more prior convictions for crimes against society (PRIORCONVSOC) were *less* likely to recidivate after completion. The measure of time elapsed between ASAP judgment and ASAP

completion (TIME2COMPLETE) approached, but did not exceed, the statistical significance threshold. When the 44 early referral subjects were excluded the model was substantively identical.

Table 8. Arrest within 24 Months of Completion: Logistic Regression Estimates

		95% Confidence Interval	
Effect	**Odds Ratio**	**Lower Limit**	**Upper Limit**
Group4	0.60	0.38	0.95
Male	1.20	0.76	1.88
Age	0.98	0.96	1.00
Alaska Native vs. Other	0.85	0.44	1.64
White vs. Other	0.63	0.36	1.09
12th Grade/GED vs. < 12th Grade	0.61	0.34	1.09
Some College vs. < 12th Grade	0.65	0.35	1.18
Time2Complete	0.83	0.69	1.01
PriorConv	1.62	1.28	2.06
PriorConvSoc	0.57	0.36	0.89

We used the estimates from the model to repeat our analysis of the same set of 'typical' subjects from Groups 4 and 6 that we examined before. The results, shown in Table 9, indicate that, holding education constant, the predicted recidivism risk of a younger white subject who completes education (Group 4) is comparable to that of an older white subject who completes treatment (Group 6).

Table 9. Predicted Probability of Arrest Within 24 Months Among 'Typical' Subjects

Group	Age	Race	Education	Predicted Probability of Recidivism
4	23.5	White	< 12th Grade	0.31
4	23.5	White	Some College	0.22
4	33.5	White	< 12th Grade	0.27
4	33.5	White	Some College	0.19
4	42.5	White	< 12th Grade	0.24
4	42.5	White	Some College	0.17
6	26	White	< 12th Grade	0.40
6	26	White	Some College	0.30
6	35	White	< 12th Grade	0.36
6	35	White	Some College	0.27
6	42	White	< 12th Grade	0.33
6	42	White	Some College	0.24

Next, we modeled arrests leading to convictions within 24 months of ASAP completion. In the reduced model selected for this outcome age (AGE) dipped below the statistical significance threshold, prior arrests (PRIORARST) and prior convictions for crimes against society (PRIORCONVSOC) emerged as a significant criminal history measures, and one of the educational attainment (EDUCREC) contrasts (i.e., that between subjects with some college and the omitted reference category—subjects who did not complete grade 12) reached significance (see Table 10). The reduced model was not a significantly worse fit for the data than the full model (-2LL = 499.52 – 494.15 = 5.37; d.f. = 8; $P(>\chi^2) = 0.72$). Excluding the early referral subjects yielded a substantively identical model.

Table 10. Conviction Within 24 Months of Completion: Logistic Regression Estimates

Effect	Odds Ratio	95% Confidence Interval Lower Limit	Upper Limit
Group4	0.55	0.32	0.92
Male	1.43	0.85	2.41
Age	0.99	0.97	1.01
Alaska Native vs. Other	1.19	0.59	2.42
White vs. Other	0.61	0.33	1.13
12th Grade/GED vs. < 12th Grade	0.65	0.35	1.21
Some College vs. < 12th Grade	0.48	0.25	0.94
Time2Complete	0.73	0.57	0.92
PriorArst	1.28	1.09	1.50
PriorConvSoc	0.67	0.43	1.03

For the analysis of 'typical' subjects for the model of post-completion convictions, we added new subjects with one of two levels of educational attainment to examine the effect of that term. The results (see Table 11) indicate that educational attainment may be associated with differences in recidivism risk that are great enough to be of interest to decision makers. For example, a 23.5 year old white subject without a high school diploma has an estimated recidivism risk of 0.22. The same subject with some college or post-secondary training has a risk of 0.12. These estimates should be viewed with some skepticism, however, in light of rather poor fit of the model to the data.

Table 11. Predicted Probability of Conviction Within 24 Months Among 'Typical' Subjects

Group	Age	Race	Education	Predicted Probability of Recidivism
4	23.5	White	< 12th Grade	0.22
4	23.5	White	Some College	0.12
4	33.5	White	< 12th Grade	0.20
4	33.5	White	Some College	0.11
4	42.5	White	< 12th Grade	0.17
4	42.5	White	Some College	0.09
6	26	White	< 12th Grade	0.28
6	26	White	Some College	0.16
6	35	White	< 12th Grade	0.26
6	35	White	Some College	0.14
6	42	White	< 12th Grade	0.24
6	42	White	Some College	0.13

NON-PARAMETRIC DURATION MODELS

Having examined the binary outcomes, we switched our focus to duration outcome measures. We used non-parametric Kaplan-Meier methods to examine the survival times of the subjects and life table methods to estimate hazard rates over specified intervals.

We examined the time elapsed between ASAP judgment date and a new arrest (ARST42TIME) over a 42-month follow-up period and estimated survival times at the 25th and 50th percentiles for each analysis group (see Table 12).[17] The 95 percent confidence

[17] We did not apply the life table methods to the AOD recidivism measures since were too few AOD failures to produce reasonable estimates. Fewer than 25 percent of the subjects in groups 4 and 6 experienced an AOD arrest or conviction during the follow-up period.

intervals around these estimates are quite large, and it was not possible to estimate median (i.e., 50th percentile) survival times for Group 4 because fewer than half of the subjects failed during the follow-up period. The estimates show that half of the subjects who were never assessed (Group 1) would be expected to be rearrested within 474 days (approximately 16 months) of judgment. The confidence interval around this estimate indicates that a more robust interpretation of the data would be that half of the non-assessed subjects would be expected to be rearrested within 11 to 25 months (i.e., 336 to 751 days) of their judgments.[18] By contrast, the estimates suggest that only 25 percent of the subjects who completed education (Group 4) would be expected to have a new arrest within 23 months of judgment.

Table 12. Kaplan-Meier Estimates of Median Time to Arrest, by Group

			95% Confidence Interval	
Group	Percentile	Estimate	Lower Limit	Upper Limit
1	50	474	336	751
1	25	114	80	165
4	50	*	*	*
4	25	695	508	1073
5	50	327.5	149	1228
5	25	108.5	69	271
6	50	1238	964	*
6	25	348	288	513
7	50	545	446	874
7	25	205	147	262

* Indicates sample data would not support estimation.

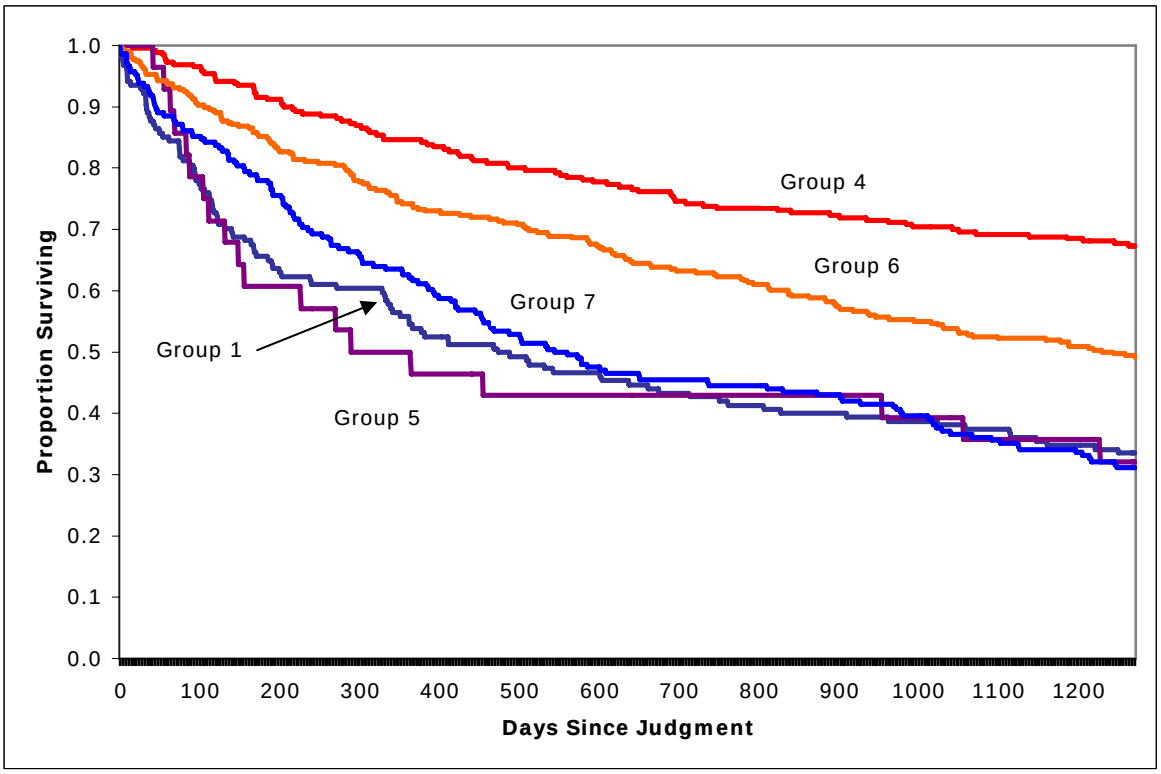
Figure 2 depicts cumulative proportion of subjects who were not arrested is plotted over the 42-month follow-up period by group.[19] The figure shows that the survival curves of the groups of non-assessed (Group 1) and non-completing subjects (Groups 5 and 7) are virtually indistinguishable, which indicates that the selection problem in the study design is acute. The stair-stepped appearance of the survival curve for Group 5 is a consequence of the small size of the sample (n = 28). The survival plots of the two groups of completers (Groups 4 and 6) are distinctive, however, both from each other and from the non-completers and non-assessed groups. Once the first 100 days after judgment have elapsed, the survival functions of the groups of completers are clearly distinct with the proportion of Group 4 subjects surviving remaining greater than that of Group 6 for the duration of the follow-up period.[20]

[18] More formally, a 95 percent confidence interval indicates that if the study were repeated numerous times, drawing a new random sample of subjects for each repetition, we would expect to find that the median survival time was within the confidence interval in 95 percent of the repetitions.

[19] The survival plots are not adjusted for any covariates such as criminal history or socio-demographics that are related to recidivism risk.

[20] We also estimated Kaplan-Meier estimates and survival plots for time to post-judgment conviction (CONV42TIME). Since the estimates are similar to those for the post-judgment arrest outcome, we have omitted them from the report in the interest of brevity. Appendix C includes a survival plot of time to post-judgment AOD arrest (ARST42AODTIME).

Figure 2. Survival Plot of Arrests Within 42 Months of Judgment



A survival plot of post-completion arrests (ARST24TIME) shows that the survival curves for Groups 4 and 6 do not diverge noticeably until approximately 250 days after program completion (see Figure 3).

We also used life-table methods to estimate the hazard rate (i.e., the instantaneous rate of failure [arrest, in this case]) for each group.[21] We estimated hazard rates for both post-judgment arrests and for post-completion arrests. For the post-judgment arrests, we estimated the hazard rate for days 1-30, 31-60, 61-90, 91-180, 181-365, 366-547, 548-730, and 731-1279.[22] For the post-completion arrests, we estimated hazard rates for all but the last of these intervals, which was outside the 24-month post-completion follow-up period. The results, which are estimated at the midpoint of each interval, are shown in Table 13 for post-judgment arrests and in Table 14 for post-completion arrests.

As noted before, the hazard rate is an instantaneous rate of failure. For subjects in Group 1, the hazard rate over the first 30 days after judgment is 0.00262, indicating that if there were 100,000 subjects in Group 1, we would expect 262 of them to fail each day, on average, during the first month after judgment. The hazard rate for Group 1 starts high and remains constant through the first six months after judgment. During months 6-18, the hazard rate drops to approximately half of its level over months 1-6. During months 18-24, it drops by

[21] The drawback to life-table methods, in contrast to Kaplan-Meier methods, is that the life-table method requires that the hazard rate be estimated over arbitrarily selected intervals with the consequence that the results are necessarily conditional on the choice of intervals (Allison, 1995).

[22] The number of days in each successive interval grows larger from one interval to the next to compensate for the diminishing number of subjects surviving to each successive interval. The data would not support estimation of reasonably robust hazard rates over narrow intervals near the end of the follow-up period.

approximately half once again and seems to hold that level for the remainder of the follow-up period. For subjects in Group 7, the pattern is similar except that the first notable decrease in the hazard rate is during month 3 rather than in months 4-6. The pattern for Group 5 may be somewhat different, but the confidence intervals are wide due to the small number of subjects (n = 28). Recidivism risk in Group 4 may be somewhat lower during month 1 than during month 2. In general, with the possible exception of Group 4, the hazard rate seems to reach its maximum shortly after judgment and decline monotonically with each succeeding interval. The post-completion hazard rates for Groups 4 and 6 evince a similar pattern (see Table 14).

Figure 3. Survival Plot of Arrests Within 24 Months of Completion

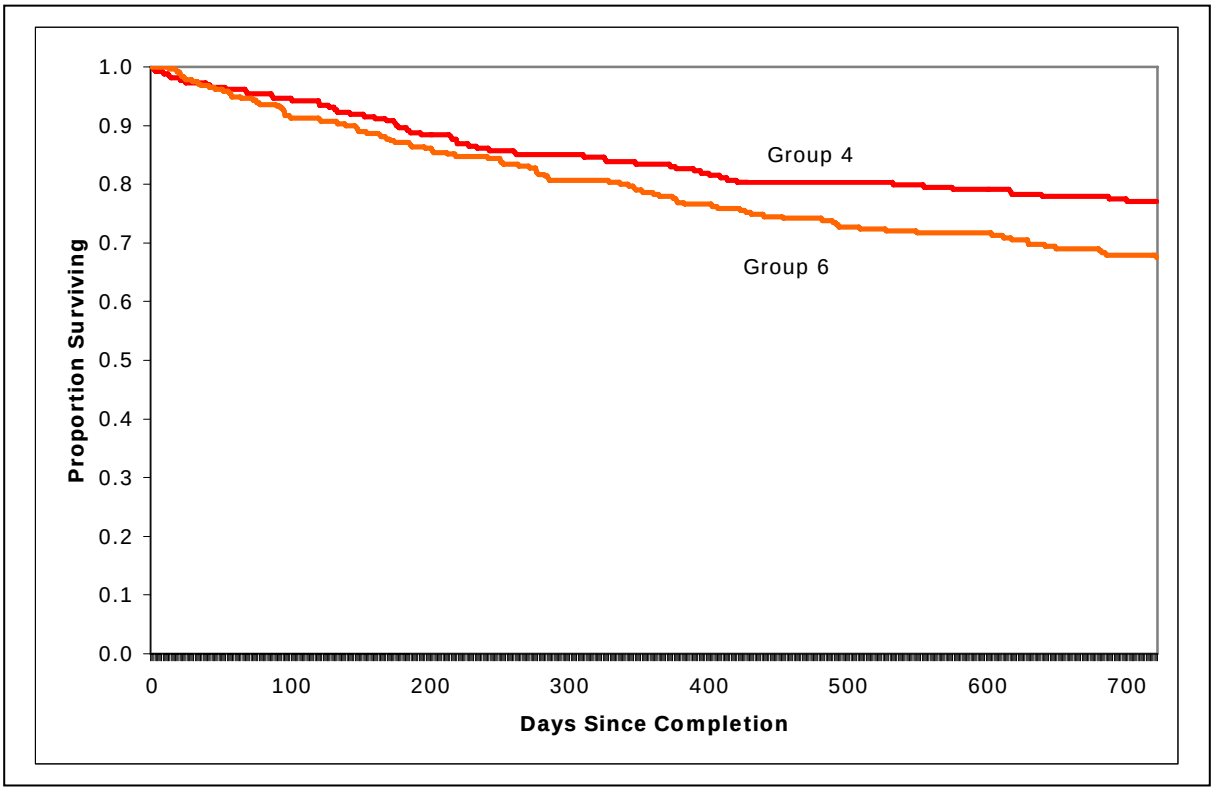


Table 13. Hazard Rates for Arrest after Judgment, by Group

	Interval Limits			Hazard	95% Confidence Intvl.	
Group	Lower	Midpoint	Upper	Rate	Lower Limit	Upper Limit
1	0	15.5	30	0.00262	0.00114	0.00409
1	31	46	60	0.00269	0.00110	0.00427
1	61	76	90	0.00183	0.00047	0.00319
1	91	136	180	0.00227	0.00135	0.00320
1	181	273.5	365	0.00100	0.00053	0.00147
1	366	457	547	0.00086	0.00037	0.00134
1	548	639.5	730	0.00048	0.00010	0.00087
1	731	1005	1278	0.00044	0.00021	0.00067
4	0	15.5	30	0.00012	0.00000	0.00037
4	31	46	60	0.00078	0.00016	0.00141
4	61	76	90	0.00013	0.00000	0.00039
4	91	136	180	0.00063	0.00030	0.00097
4	181	273.5	365	0.00042	0.00023	0.00062
4	366	457	547	0.00036	0.00017	0.00055
4	548	639.5	730	0.00036	0.00016	0.00055
4	731	1005	1278	0.00018	0.00010	0.00026
5	0	15.5	30	0.00000	.	.
5	31	46	60	0.00247	0.00000	0.00589
5	61	76	90	0.00556	0.00013	0.01098
5	91	136	180	0.00285	0.00037	0.00533
5	181	273.5	365	0.00144	0.00004	0.00284
5	366	457	547	0.00044	0.00000	0.00130
5	548	639.5	730	0.00000	.	.
5	731	1005	1278	0.00052	0.00000	0.00111
6	0	15.5	30	0.00124	0.00054	0.00194
6	31	46	60	0.00088	0.00027	0.00149
6	61	76	90	0.00079	0.00021	0.00138
6	91	136	180	0.00079	0.00044	0.00114
6	181	273.5	365	0.00075	0.00050	0.00099
6	366	457	547	0.00041	0.00022	0.00061
6	548	639.5	730	0.00050	0.00027	0.00072
6	731	1005	1278	0.00045	0.00032	0.00058
7	0	15.5	30	0.00207	0.00095	0.00320
7	31	46	60	0.00192	0.00079	0.00306
7	61	76	90	0.00091	0.00011	0.00171
7	91	136	180	0.00110	0.00058	0.00163
7	181	273.5	365	0.00123	0.00081	0.00164
7	366	457	547	0.00119	0.00073	0.00165
7	548	639.5	730	0.00051	0.00018	0.00084
7	731	1005	1278	0.00069	0.00044	0.00093

Table 14. Hazard Rates for Arrest after Completion, by Group

	Interval Limits			Hazard	95% Confidence Intvl.	
Group	Lower	Midpoint	Upper	Rate	Lower Limit	Upper Limit
4	0	15.5	30	0.00088	0.00023	0.00153
4	31	46	60	0.00040	0.00000	0.00085
4	61	76	90	0.00054	0.00001	0.00106
4	91	136	180	0.00060	0.00028	0.00093
4	181	273.5	365	0.00039	0.00020	0.00058
4	366	457	547	0.00024	0.00008	0.00039
4	548	639.5	730	0.00019	0.00005	0.00034
6	0	15.5	30	0.00083	0.00025	0.00140
6	31	46	60	0.00089	0.00027	0.00150
6	61	76	90	0.00057	0.00007	0.00108
6	91	136	180	0.00077	0.00042	0.00111
6	181	273.5	365	0.00059	0.00037	0.00082
6	366	457	547	0.00044	0.00023	0.00065
6	548	639.5	730	0.00036	0.00016	0.00056

SEMI-PARAMETRIC DURATION MODELS

A secondary research question for this study was whether the post-completion recidivism risk differed from the pre-completion recidivism risk among those subjects who successfully completed the program. To examine that question, we estimated semi-parametric Cox regression models of the four 42-month continuous outcomes—ARST42TIME, ARST42AODTIME, CONV42TIME, and CONV42AODTIME—for the sample of 578 completers (i.e., Group 2).[23] A time-dependent variable—COMPLETED—was included in this model, which was equal to 1 for the period of time after each subject's completion date and 0 otherwise. The coefficient on this variable, which is computed by the software during model estimation, will indicate whether the post-completion hazard rate differed from the pre-completion hazard rate. We also included the Group 4 dummy variable to control for the differences in risk that lead the ASAP screeners to assign some subjects to education and others to treatment.

Model estimation proceeded in the familiar manner. We began with full models that included the covariates of interest, basic demographics, and all ten measures of criminal history. We reduced the model by eliminating non-significant criminal history measures until we had identified a parsimonious model that fit the data as well as the full model. Table 15 displays estimates from the parsimonious model of arrests during the 42-month follow-up period. This model, which equaled the full model in fit (-2LL = 2891.436 – 2888.12 = 3.32; d.f = 8; $P(>\chi^2) = 0.91$), indicates that the pre-completion and post-completion hazard rates were not significantly different after controlling for prior arrests, prior arrests for crimes against persons, age, sex, race, and the screener's assessment (GROUP4). The model estimates are expressed as hazard ratios, which equal to the hazard rate for a subject with a

[23] Cox regression models are sensitive to 'ties' (i.e., subjects who fail after identical periods of time). For these models, we specified the 'exact' method for handling the few ties observed in the sample. This is the most computationally intensive method, but it yields the more efficient estimates than the other procedures for handling ties available in the SAS software (Allison, 1995).

value of k on the variable divided by the hazard rate for a subject with a value of $k - 1$ on the variable after controlling for the other variables in the model. The hazard ratios for the criminal history measure indicate that the hazard rate increases 19 percent for each prior arrest (PRIORARST) and an additional 22 percent for each prior arrest for a crime against a person (PRIORARSTPER). Since GROUP4 is a dummy variable, which only takes two values, its hazard ratio indicates that the hazard rate for subjects assigned to education was only 74 percent of the hazard rate for subjects assigned to treatment after controlling for the other factors. Neither sex nor race was statistically significant.

Table 15. Model of Arrests Among ASAP Completers (n = 578)

Variable	Hazard Ratio	Wald Chi-Square	P(>Chi-Square)
Completed	0.80	1.70	0.1924
Group4	0.74	4.20	0.0404
Age	0.98	10.93	0.0009
Male	1.11	0.52	0.4700
Alaska Native vs. Other	1.00	0.00	0.9856
White vs. Other	0.85	0.79	0.3729
PriorArst	1.19	16.72	<.0001
PriorArstPer	1.22	4.52	0.0336

A similar model of arrests leading to conviction during the 42-month follow-up period (i.e., CONV42TIME) yielded similar findings. Once again, the more parsimonious model matched the fit of the full model (-2LL = 2336.70 – 2328.37 = 8.33; d.f = 8; $P(>\chi^2) = 0.40$), and once again the time-dependent COMPLETED term was not statistically significant (see Table 16). The most notable difference was that prior convictions for crimes against society (PRIORCONVSOC) emerged as significant. Note that the effects of the two criminal history measures in this model are opposite in sign: The conviction hazard increases with each additional prior arrest (PRIORARST) but decreases with each additional prior conviction for a crime against society (PRIORCONVSOC).

Table 16. Model of Convictions Among ASAP Completers (n = 578)

Variable	Hazard Ratio	Wald Chi-Square	P(>Chi-Square)
Completed	0.74	2.68	0.1014
Group4	0.65	6.42	0.0113
Age	0.98	7.91	0.0049
Male	1.08	0.20	0.6544
Alaska Native vs. Other	1.13	0.28	0.5974
White vs. Other	0.80	1.17	0.2794
PriorArst	1.26	34.25	<.0001
PriorConvSoc	0.72	6.66	0.0099

We also estimated a model of AOD arrests among ASAP completers (see Table 17) and a model of AOD convictions among ASAP completers (see Table 18). The two models are generally similar. ASAP completion status (COMPLETED) is not statistically significant in either model indicating that subjects were no more likely to experience an AOD failure during ASAP than after completing the program. Subjects who completed the ASAP education program (GROUP4) were significantly less likely to have a new AOD arrest. The GROUP4 parameter approached, but did not reach, statistical significance in the model of AOD

convictions. White subjects were less likely than subjects of 'Other' ethnic origin to have an AOD arrest or conviction. Younger subjects were less likely to experience a new AOD conviction, but the AGE parameter did not reach statistical significance in the model of AOD arrests.

Table 17. Model of AOD Arrests Among ASAP Completers (n = 578)

Variable	Hazard Ratio	Wald Chi-Square	P(>Chi-Square)
Completed	0.92	0.14	0.7080
Group4	0.63	4.68	0.0306
Age	0.99	2.71	0.0999
Male	1.35	1.79	0.1805
Alaska Native vs. Other	0.84	0.36	0.5480
White vs. Other	0.59	4.51	0.0337
PriorConv	1.13	2.69	0.1012

Table 18. Model of AOD Convictions Among ASAP Completers (n=578)

Variable	Hazard Ratio	Wald Chi-Square	P(>Chi-Square)
Completed	0.91	0.14	0.7062
Group4	0.64	3.70	0.0543
Age	0.98	4.33	0.0375
Male	1.20	0.59	0.4405
Alaska Native vs. Other	0.79	0.56	0.4547
White vs. Other	0.55	4.99	0.0256
PriorConv	1.12	2.18	0.1397

KEY FINDINGS

- Recidivism patterns among subjects who were never assessed (Group 1) were similar to recidivism patterns for subjects who were assessed but failed to complete the ASAP program. This suggests that the Group 1 subjects differed from the subjects who completed the ASAP program (Groups 4 and 6) at baseline (i.e., before they began ASAP) on one or more unmeasured factors (e.g., motivation to change their behavior) that affected both the probability of assessment and recidivism risk. Contrasts between the recidivism behavior of ASAP completers (Groups 4 and 6) and non-completers (Groups 1, 5, and 7) will yield poor estimates the effectiveness of the ASAP program.
- Subjects who completed ASAP education (Group 4) were less likely to recidivate than subjects who were never assessed (Group 1).
- Subjects who completed ASAP treatment (Group 6) were not significantly less likely to recidivate than subjects who were never assessed (Group 1).
- Among subjects who completed ASAP (education or treatment), we detected no significant difference between pre-completion and post-completion risk of arrest or conviction. We also found no difference between the pre-completion and post-completion risk of AOD arrest or conviction. On average, subjects who eventually completed ASAP successfully were no more likely to recidivate *after* receiving the ASAP program in full than *during* the program.

CHAPTER 4.
Discussion & Conclusion

Science advances through falsification rather than affirmation. Evidence is assembled to show that rival explanations are inconsistent with the data until only a single explanation remains intact. This study of recidivism among persons court ordered to Alaska's ASAP program succeeds as a description of the subjects' subsequent offending, but, by virtue of its design, it fails to eliminate the key rival hypotheses and clear the path to an unequivocal inference about whether ASAP reduces recidivism.

To estimate the effect of ASAP on recidivism, we would like to be able to plausibly assert that the only relevant difference between the subjects in Group 1 and the subjects in Groups 4-7 is that only the latter subjects who did participated in ASAP. If that assertion were plausible, the difference between the recidivism rate of the Group 1 subjects and the recidivism rate of the subjects in Groups 4-7 would be a reasonable estimate of the effect of ASAP participation on recidivism. In fact, however, that assertion seems implausible. The resemblance between the recidivism rates between the Group 1 subjects and the assessed subjects who did not complete ASAP (Groups 5 and 7) and notable differences between the recidivism patterns of the ASAP completers (Groups 4 and 6) and non-completers suggests that there were substantial unmeasured differences between the non-assessed subjects (Group 1) on the one hand and the assessed subjects on the other that were strongly related to their recidivism behavior. These unmeasured differences might include social control influences from family and friends encouraging the subjects to change their substance use patterns, the subjects' internal desire to change their behavior, and their employment history.

Keeping in mind this key weakness in the design of the study, the key descriptive finding is that subjects who complete the ASAP education component (Group 4) are less likely to recidivate than subjects who do not appear for assessment by the ASAP screeners (Group 1). Subjects who complete the ASAP treatment component (Group 6) are also less likely to recidivate, on average, than those who were not assessed, but that difference was not statistically significant.[24] The recidivism patterns of the subjects who did not complete ASAP (Groups 5 and 7) were indistinguishable from the recidivism patterns of those who were never assessed (Group 1).

At least two explanations fit these findings, and neither may be discounted based on this analysis. The first explanation is that the ASAP program reduces recidivism risk and, furthermore, the education component is more effective in this regard than the treatment component. The second explanation is equally valid and more plausible. It notes that persons believed to be at low-risk of future offending are assigned to the ASAP education component. Since the assignment process is non-random, it is plausible to attribute the differences in

[24] These findings remained unchanged when we removed the 90 subjects who were court-ordered to ASAP prior to FY 2002 but were not assessed until that fiscal year.

recidivism rates between Groups 4 and 6 to the ability of the ASAP screeners to reliably assign the low-risk persons to the education program. Net of the effect of the ASAP screening process, the differences between the completers and the non-completers may be attributable to self-selection: Persons most ready to desist from crime, who have the lowest levels of antecedent risk, and the highest levels of pro-social support are more likely to both refrain from new offending and to complete the program.

We attempted to remedy one of the barriers to inference (i.e., the fact that subjects self-selected into the non-assessed group (Group 1) by applying a statistical adjustment based on inverse probability weighting (Wooldridge, 2002). Applying the technique requires the estimation of a robust first-stage model of the selection process (i.e., the factors that lead some persons to complete the assessment while others do not) that is used to create weights to adjust the remainder of the analysis. We found that we were unable to estimate a satisfactory first-stage model using the available data and ultimately abandoned the effort.

A secondary component of our analysis examined the recidivism behavior of those subjects who completed their ASAP assignments over the 24 months following their completion date. The results of this analysis showed no significant difference between the pre-completion and post-completion hazard rates among the 578 subjects who completed ASAP after controlling for criminal history, demographics, and the outcome of the ASAP assessment process (i.e., assignment to either education or treatment). This finding is also consistent with both rival explanations (i.e., 'ASAP reduces recidivism' versus 'ASAP assessment diverts low-risk subjects to education').If ASAP does reduce recidivism, it could be that this effect is most potent immediately after the program begins and less potent, and therefore less easily detected, after the program is completed.

We also found no significant differences between the post-completion recidivism rates of the education completers as compared to the treatment completers. This analysis was not as satisfactory as the examination of post-judgment recidivism, however. The post-completion models were able to explain only seven percent of the variance in the outcome, whereas the post-judgment models accounted for nearly 20 percent of the variance. This difference is likely attributable to omitted variables. The data elements collected for the study were selected to predict recidivism in sample of drug- or alcohol-involved misdemeanants. Within that sample, the subjects who completed the program comprise a low-risk sub-sample. Modeling recidivism within that sample in a more satisfactory way would likely require information about time-dependant covariates (e.g., employment status over time, marital status over time, peer associations, and family supports) that are unlikely to be available for any retrospective study like this one.

If another study of recidivism among ASAP clients were undertaken a few changes should be considered. First, an assessment should be made of the extent to which the ASAP education and treatment programs are delivered in a uniform manner by the several dozen service providers throughout the state. Is there a standard written curriculum used by all of the providers? Are the instructors and therapists licensed or certified? Are there uniform requirements for successful completion? If the programs are not reasonably standardized, any study that assesses them as though they are will conflate variation in the substance of the programs and their delivery with the effects of the programs themselves.

Second, any change that improves the richness of the data available to the study would also increase the confidence that can be placed in the results. One way to improve the richness of the data would be to introduce a brief questionnaire into the assessment and service delivery process. One version of the questionnaire would be designed to be completed once at assessment by each ASAP client. This questionnaire might ask the client to report on risk factors such as employment status, recent employment history, health status, recent health history, family support, and motivation to change their substance use behaviors. A second version of the questionnaire would be distributed to the ASAP service providers, who would ask clients to complete the form each time they appeared for services. This version of the form would simply ask the client to report on any changes in the risk factors that had occurred since they last completed the form. Each completed form would be dated so that a review of the clients' treatment record would provide a richer set of time-dependent data on a wider set of risk factors than were available to the present study.

The third and most important change to consider is an approach to identifying suitable comparison subjects who did not receive ASAP services but who were eligible to do so. Stakeholders within the criminal justice system often object to proposals to conduct experimental evaluations of programs like ASAP. Often the objection is that the comparison subjects, who are randomly selected to not receive the sentences given to them by the courts, are allowed to escape punishment.

One approach to answering this objection would be to assign the comparison subjects to a 'handling as usual condition.' Persons court ordered to ASAP would be assessed as they are at present and recommended for either education or treatment on the basis of that assessment. After the recommendation is known, the subjects would be randomly assigned to receive either the recommended ASAP services (i.e., education or treatment) or a period of 'substance use monitoring' (e.g., intermittent or random urinalysis and/or regular visits with a probation officer) of similar duration. The random assignment procedure would ensure that the groups are equivalent at baseline. The two main drawbacks to this design are: (1) the significant expense associated with setting up the random assignment procedure and ensuring its integrity and (2) the estimates of the effect of ASAP would be relative to the monitoring condition rather than to no treatment at all. An alternative would be to assign subjects (at random and after assessment) to a wait list condition wherein they would be ordered to start their recommended ASAP program at some designated time in the future (say, 12 months after the recommendation is made). This design would yield a comparison between ASAP and no-treatment at the cost of imposing a relatively short follow-up period during which the subjects would be monitored for recidivism.

Both of these experimental designs (i.e., the monitoring design or the wait-list design) would need to be conducted prospectively rather than retrospectively and both would be much more costly than the present study. On the positive side, both would yield much more positive conclusions about the effectiveness of ASAP than the present study was able to do. If there were naturally occurring wait-list conditions, wherein subjects seeking ASAP services are placed on wait lists by service providers who lack open slots for them, we see no way to conduct a quasi-experimental study (i.e., one where subjects are *not* randomly assigned to conditions) that is likely to yield more definitive findings than the present study has.

Appendix A.
Additional Descriptive Analysis

This appendix contains the remainder of the descriptive analysis of post-judgment recidivism referenced in Chapter 3. Tables 19-22 show the proportion of subjects with a recidivism event by analysis group. Tables 23-26 show the proportion of subjects with a recidivism event by ASAP office location (SITE). Tables 27 and 28 show completion and recidivism rates, respectively, for 16 selected providers of ASAP treatment services.

Table 19. Proportion Arrested Within 42 Months of Judgment, by Group

	Program Outcome					
	Never Assessed (Group 1)	Completed Education (Group 4)	Did Not Complete Education (Group 5)	Completed Treatment (Group 6)	Did Not Complete Treatment (Group 7)	All Program Outcomes
ASAP Location						
Anchorage	0.691	0.320	0.750	0.508	0.642	0.542
Fairbanks	0.683	0.338	0.375	0.519	0.706	0.497
Juneau	0.647	0.240	1.000	0.348	0.688	0.464
Kenai/Homer	0.500	0.289	0.800	0.563	0.667	0.479
Ketchikan	0.000	0.462	*	0.818	0.800	0.639
Mat-Su	0.571	0.421	*	0.475	0.882	0.554
Age at Judgment						
18-25	0.729	0.432	0.727	0.610	0.795	0.613
26-35	0.712	0.368	0.375	0.484	0.700	0.549
36-45	0.583	0.257	0.857	0.520	0.689	0.516
Over 45	0.500	0.212	1.000	0.380	0.452	0.359
Race						
Alaska Native	0.759	0.333	0.833	0.658	0.696	0.657
White	0.559	0.319	0.611	0.451	0.660	0.460
Other	0.563	0.359	0.750	0.537	0.696	0.520
Education Level						
< 12th Grade	0.667	0.423	0.750	0.644	0.889	0.674
12th Grade/GED	0.745	0.294	0.917	0.576	0.608	0.542
Some College	0.727	0.336	0.500	0.385	0.672	0.452
Unknown	0.564	0.320	0.000	0.500	0.714	0.495
Gender						
Female	0.591	0.253	1.000	0.558	0.600	0.479
Male	0.674	0.357	0.640	0.491	0.704	0.542

* Indicates zero cases in cell.

Table 20. Proportion with an AOD Arrest Within 42 Months of Judgment, by Group

	Program Outcome					
	Never Assessed (Group 1)	Completed Education (Group 4)	Did Not Complete Education (Group 5)	Completed Treatment (Group 6)	Did Not Complete Treatment (Group 7)	All Program Outcomes
ASAP Location						
Anchorage	0.346	0.113	0.333	0.249	0.269	0.246
Fairbanks	0.268	0.132	0.375	0.333	0.471	0.248
Juneau	0.176	0.200	0.333	0.174	0.250	0.202
Kenai/Homer	0.167	0.158	0.000	0.250	0.267	0.198
Ketchikan	0.000	0.154	*	0.273	0.400	0.250
Mat-Su	0.286	0.211	*	0.200	0.529	0.277
Age at Judgment						
18-25	0.354	0.173	0.455	0.325	0.432	0.307
26-35	0.327	0.158	0.000	0.209	0.333	0.243
36-45	0.194	0.086	0.286	0.250	0.297	0.216
Over 45	0.222	0.154	0.500	0.180	0.129	0.170
Race						
Alaska Native	0.380	0.154	0.833	0.315	0.293	0.315
White	0.186	0.132	0.000	0.201	0.319	0.190
Other	0.250	0.179	0.750	0.341	0.348	0.293
Education Level						
< 12th Grade	0.300	0.269	0.250	0.378	0.389	0.340
12th Grade/GED	0.426	0.137	0.500	0.252	0.324	0.269
Some College	0.136	0.121	0.100	0.197	0.250	0.175
Unknown	0.236	0.120	0.000	0.167	0.286	0.198
Gender						
Female	0.364	0.067	0.333	0.256	0.400	0.237
Male	0.280	0.173	0.280	0.241	0.283	0.241

* Indicates zero cases in cell.

Table 21. Proportion Convicted Within 42 Months of Judgment, by Group

	Program Outcome					
	Never Assessed (Group 1)	Completed Education (Group 4)	Did Not Complete Education (Group 5)	Completed Treatment (Group 6)	Did Not Complete Treatment (Group 7)	All Program Outcomes
ASAP Location						
Anchorage	0.617	0.216	0.667	0.400	0.552	0.446
Fairbanks	0.561	0.250	0.375	0.481	0.647	0.416
Juneau	0.529	0.240	1.000	0.261	0.625	0.405
Kenai/Homer	0.500	0.263	0.600	0.531	0.600	0.438
Ketchikan	0.000	0.462	*	0.727	0.700	0.583
Mat-Su	0.429	0.211	*	0.400	0.765	0.434
Age at Judgment						
18-25	0.688	0.321	0.636	0.571	0.750	0.548
26-35	0.596	0.281	0.250	0.396	0.583	0.448
36-45	0.472	0.171	0.857	0.400	0.622	0.422
Over 45	0.389	0.192	1.000	0.280	0.323	0.281
Race						
Alaska Native	0.671	0.282	0.833	0.575	0.598	0.574
White	0.441	0.225	0.500	0.358	0.585	0.366
Other	0.563	0.308	0.750	0.463	0.609	0.463
Education Level						
< 12th Grade	0.600	0.385	0.500	0.556	0.778	0.589
12th Grade/GED	0.681	0.225	0.833	0.496	0.559	0.475
Some College	0.500	0.234	0.500	0.287	0.531	0.338
Unknown	0.491	0.240	0.000	0.417	0.714	0.426
Gender						
Female	0.500	0.160	1.000	0.477	0.540	0.398
Male	0.583	0.281	0.560	0.401	0.610	0.454

* Indicates zero cases in cell.

Table 22. Proportion with an AOD Conviction Within 42 Months of Judgment, by Group

	Program Outcome					
	Never Assessed (Group 1)	Completed Education (Group 4)	Did Not Complete Education (Group 5)	Completed Treatment (Group 6)	Did Not Complete Treatment (Group 7)	All Program Outcomes
ASAP Location						
Anchorage	0.284	0.093	0.250	0.211	0.224	0.204
Fairbanks	0.244	0.118	0.250	0.333	0.412	0.224
Juneau	0.176	0.160	0.333	0.130	0.125	0.155
Kenai/Homer	0.000	0.132	0.000	0.188	0.200	0.146
Ketchikan	0.000	0.154	*	0.182	0.400	0.222
Mat-Su	0.143	0.211	*	0.175	0.353	0.217
Age at Judgment						
18-25	0.271	0.173	0.273	0.312	0.318	0.261
26-35	0.269	0.123	0.000	0.176	0.267	0.198
36-45	0.194	0.057	0.286	0.190	0.243	0.174
Over 45	0.167	0.135	0.500	0.140	0.129	0.144
Race						
Alaska Native	0.304	0.077	0.500	0.301	0.228	0.253
White	0.169	0.121	0.000	0.157	0.266	0.160
Other	0.188	0.179	0.750	0.293	0.261	0.252
Education Level						
< 12th Grade	0.267	0.269	0.250	0.333	0.250	0.284
12th Grade/GED	0.319	0.118	0.333	0.223	0.265	0.221
Some College	0.136	0.112	0.100	0.156	0.234	0.154
Unknown	0.200	0.040	0.000	0.083	0.143	0.139
Gender						
Female	0.318	0.053	0.000	0.244	0.300	0.199
Male	0.227	0.151	0.240	0.194	0.233	0.199

* Indicates zero cases in cell.

Table 23. Proportion Arrested Within 42 Months of Judgment, by ASAP Site

	ASAP Location						
	Anchorage	Fairbanks	Juneau	Kenai/ Homer	Ketchikan	Mat-Su	All Sites
Program Outcome							
Never Assessed	0.691	0.683	0.647	0.500	0.000	0.571	0.662
Completed Education	0.320	0.338	0.240	0.289	0.462	0.421	0.327
Did Not Complete Education	0.750	0.375	1.000	0.800	*	*	0.679
Completed Treatment	0.508	0.519	0.348	0.563	0.818	0.475	0.509
Did Not Complete Treatment	0.642	0.706	0.688	0.667	0.800	0.882	0.679
Age at Judgment							
18-25	0.640	0.537	0.571	0.633	0.750	0.560	0.613
26-35	0.594	0.551	0.235	0.429	0.667	0.529	0.549
36-45	0.534	0.500	0.448	0.417	0.625	0.571	0.516
Over 45	0.309	0.286	0.600	0.381	0.429	0.538	0.359
Race							
Alaska Native	0.647	0.604	0.690	0.706	0.769	0.750	0.657
White	0.474	0.448	0.333	0.425	0.556	0.522	0.460
Other	0.535	0.471	0.429	0.500	0.600	0.500	0.520
Education Level							
< 12th Grade	0.671	0.667	0.667	0.688	0.900	0.556	0.674
12th Grade/GED	0.557	0.456	0.448	0.571	0.550	0.629	0.542
Some College	0.467	0.510	0.400	0.257	0.750	0.519	0.452
Unknown	0.568	0.488	0.462	0.667	0.000	0.000	0.495
Gender							
Female	0.514	0.485	0.250	0.464	0.250	0.471	0.479
Male	0.553	0.500	0.515	0.485	0.688	0.576	0.542

* Indicates zero cases in cell.

Table 24. Proportion with an AOD Arrest Within 42 Months of Judgment, by ASAP Site

	ASAP Location						
	Anchorage	Fairbanks	Juneau	Kenai/ Homer	Ketchikan	Mat-Su	All Sites
Program Outcome							
Never Assessed	0.346	0.268	0.176	0.167	0.000	0.286	0.292
Completed Education	0.113	0.132	0.200	0.158	0.154	0.211	0.142
Did Not Complete Education	0.333	0.375	0.333	0.000	*	*	0.286
Completed Treatment	0.249	0.333	0.174	0.250	0.273	0.200	0.245
Did Not Complete Treatment	0.269	0.471	0.250	0.267	0.400	0.529	0.311
Age at Judgment							
18-25	0.352	0.293	0.179	0.267	0.250	0.320	0.307
26-35	0.258	0.224	0.235	0.143	0.444	0.176	0.243
36-45	0.182	0.300	0.172	0.167	0.250	0.321	0.216
Over 45	0.173	0.095	0.300	0.190	0.000	0.231	0.170
Race							
Alaska Native	0.294	0.333	0.310	0.353	0.308	0.500	0.315
White	0.182	0.219	0.125	0.151	0.278	0.246	0.190
Other	0.337	0.176	0.286	0.333	0.000	0.000	0.293
Education Level							
< 12th Grade	0.356	0.250	0.167	0.375	0.500	0.333	0.340
12th Grade/GED	0.256	0.298	0.310	0.262	0.100	0.371	0.269
Some College	0.189	0.224	0.133	0.057	0.500	0.148	0.175
Unknown	0.243	0.209	0.154	0.000	0.000	0.000	0.198
Gender							
Female	0.261	0.242	0.188	0.179	0.000	0.235	0.237
Male	0.240	0.250	0.206	0.206	0.281	0.288	0.241

* Indicates zero cases in cell.

Table 25. Proportion Convicted Within 42 Months of Judgment, by ASAP Site

	ASAP Location						
	Anchorage	Fairbanks	Juneau	Kenai/ Homer	Ketchikan	Mat-Su	All Sites
Program Outcome							
Never Assessed	0.617	0.561	0.529	0.500	0.000	0.211	0.571
Completed Education	0.216	0.250	0.240	0.263	0.462	0.400	0.246
Did Not Complete Education	0.667	0.375	1.000	0.600	*	*	0.607
Completed Treatment	0.400	0.481	0.261	0.531	0.700	0.000	0.421
Did Not Complete Treatment	0.552	0.647	0.625	0.600	0.429	0.000	0.593
Age at Judgment							
18-25	0.592	0.488	0.500	0.567	0.750	0.360	0.548
26-35	0.458	0.449	0.235	0.381	0.667	0.529	0.448
36-45	0.446	0.400	0.345	0.417	0.375	0.429	0.422
Over 45	0.198	0.238	0.600	0.333	0.429	0.462	0.281
Race							
Alaska Native	0.559	0.563	0.621	0.647	0.615	0.583	0.574
White	0.364	0.344	0.271	0.384	0.556	0.406	0.366
Other	0.465	0.412	0.429	0.500	0.600	0.500	0.463
Education Level							
< 12th Grade	0.616	0.583	0.417	0.625	0.900	0.389	0.589
12th Grade/GED	0.466	0.421	0.414	0.548	0.500	0.571	0.475
Some College	0.344	0.388	0.367	0.200	0.500	0.333	0.338
Unknown	0.486	0.395	0.462	0.667	0.000	0.000	0.426
Gender							
Female	0.442	0.303	0.250	0.429	0.250	0.353	0.398
Male	0.447	0.445	0.441	0.441	0.625	0.455	0.454

* Indicates zero cases in cell.

Table 26. Proportion with an AOD Conviction Within 42 Months of Judgment, by ASAP Site

	ASAP Location						
	Anchorage	Fairbanks	Juneau	Kenai/ Homer	Ketchikan	Mat-Su	All Sites
Program Outcome							
Never Assessed	0.284	0.244	0.176	0.000	0.000	0.143	0.240
Completed Education	0.093	0.118	0.160	0.132	0.154	0.211	0.123
Did Not Complete Education	0.250	0.250	0.333	0.000	*	*	0.214
Completed Treatment	0.211	0.333	0.130	0.188	0.182	0.175	0.208
Did Not Complete Treatment	0.224	0.412	0.125	0.200	0.400	0.353	0.249
Age at Judgment							
18-25	0.296	0.268	0.179	0.233	0.250	0.200	0.261
26-35	0.206	0.184	0.176	0.095	0.444	0.176	0.198
36-45	0.149	0.280	0.103	0.083	0.125	0.286	0.174
Over 45	0.160	0.095	0.200	0.143	0.000	0.154	0.144
Race							
Alaska Native	0.247	0.271	0.207	0.235	0.231	0.417	0.253
White	0.150	0.208	0.104	0.110	0.278	0.188	0.160
Other	0.279	0.176	0.286	0.333	0.000	0.000	0.252
Education Level							
< 12th Grade	0.301	0.250	0.167	0.313	0.500	0.167	0.284
12th Grade/GED	0.219	0.263	0.207	0.167	0.100	0.314	0.221
Some College	0.161	0.204	0.133	0.057	0.250	0.148	0.154
Unknown	0.135	0.186	0.077	0.000	0.000	0.000	0.139
Gender							
Female	0.232	0.212	0.125	0.143	0.000	0.118	0.199
Male	0.194	0.227	0.162	0.147	0.250	0.242	0.199

* Indicates zero cases in cell.

Table 27. Completion Rates for Selected Treatment Providers

Provider	Clients Served	Clients Not Completed	Clients Completed	Percent Completed
Unknown	83	78	5	6.0%
Salvation Army				
Salvation Army	2	1	1	50.0%
Salvation Army Adult Rehabilitation Program	10	6	4	40.0%
Salvation Army Clitheroe Center	57	17	40	70.2%
Starting Point	46	15	31	67.4%
Alaska Human Services	32	7	25	78.1%
Out-Of-State	28	6	22	78.6%
RITE	23	3	20	87.0%
Genesis House	23	2	21	91.3%
Mat-Su Council Recovery Center - Alaska Family Services	18	6	12	66.7%
Gastineau Human Services	16	5	11	68.8%
The Recovery Connection	16	5	11	68.8%
Cook Inlet Tribal Corporation (CITC)				
CITC - The Ernie Turner Center	7	2	5	71.4%
CITC - The Ernie Turner Center, Alaska North Addiction	3	1	2	66.7%
CITC SAS Mobile Treatment Unit	4	2	2	50.0%
CITC - Clare Swan	1	0	1	100.0%

Table 28. Recidivism Rates Among Completers Served by Selected Treatment Providers

	Percent of Completers Recidivating			
Provider	Any Arrest	AOD Arrest	Any Conviction	AOD Conviction
Unknown	80.0%	20.0%	40.0%	20.0%
Salvation Army				
Salvation Army	100.0%	100.0%	100.0%	100.0%
Salvation Army Adult Rehabilitation Program	50.0%	25.0%	50.0%	25.0%
Salvation Army Clitheroe Center	47.5%	22.5%	40.0%	20.0%
Starting Point	45.2%	12.9%	32.3%	12.9%
Alaska Human Services	28.0%	16.0%	20.0%	8.0%
Out-Of-State	40.9%	22.7%	31.8%	18.2%
RITE	60.0%	40.0%	45.0%	30.0%
Genesis House	33.3%	14.3%	28.6%	14.3%
Mat-Su Council Recovery Center - Alaska Family Services	66.7%	25.0%	50.0%	25.0%
Gastineau Human Services	45.5%	27.3%	27.3%	18.2%
The Recovery Connection	27.3%	18.2%	27.3%	9.1%
Cook Inlet Tribal Corporation (CITC)				
CITC - The Ernie Turner Center	80.0%	80.0%	80.0%	80.0%
CITC - The Ernie Turner Center, Alaska North Addiction	100.0%	100.0%	100.0%	100.0%
CITC SAS Mobile Treatment Unit	100.0%	50.0%	100.0%	50.0%
CITC - Clare Swan	100.0%	0.0%	100.0%	0.0%

Appendix B.
Location and Enforcement Vigor

The Division of Legislative Audit (DLA) suggested that SITE, the categorical variable indicating which ASAP office screened each subject and assigned them to receive either education or treatment, may be related to recidivism risk. More specifically, DLA hypothesized that variation between Alaska's major cities in the vigor with which local prosecutors pursue penalties against persons who fail to complete their ASAP obligations may affect the risk profile of the subjects who never appear for ASAP assessment. In cities where violations of ASAP judgments are prosecuted vigorously, the subjects who were never assessed (i.e., Group 1) may represent a higher-risk group than the never assessed subjects in other cities where prosecutorial vigor is low. We tested this hypothesis by creating dummy variables representing five of the six ASAP offices that contributed subjects to the sample leaving the Anchorage subjects as the reference group. We then created four additional dummy variables representing the interaction of the site-specific dummy variables and GROUP1 for four of the six sites.[25] For example, the Fairbanks x Group 1 interaction dummy was equal to 1 for subjects referred to the Fairbanks ASAP office who were never assessed (i.e., SITE = Fairbanks and Group1 = 1).

We found no evidence to support the hypothesis that SITE affected recidivism risk We estimated two additional logistic regression models of arrests within 42 months of judgment. Each of these models included all of the terms retained in the model reported in Table 4. In addition to those terms, the first of these models also included the dummy variables for the five sites with Anchorage serving as the reference category. The second model included the terms from Table 4 plus the five site dummy variables and the four interaction dummy variables contrasting SITE and GROUP1. Table 299 displays odds ratios from the first of these two models. Results from the second model were substantively identical; none of the site dummy variables or the interaction dummy variables reached statistical significance in either model. Moreover, the other terms in the model remained substantively unchanged; GROUP4, AGE, and PRIORARST remained statistically significant and none of the other terms were statistically significant.

[25] We omitted the Group 1 subjects in Anchorage to serve as a reference category and omitted the Group 1 subjects in Ketchikan because there were only two such subjects and including the Ketchikan × Group 1 interaction term caused convergence problems.

Table 29. Arrest within 42 Months of Judgment: Logistic Regression Estimates with Site Dummies

		95% Confidence Interval	
Effect	**Odds Ratio**	**Lower Limit**	**Upper Limit**
Group4	0.48	0.30	0.77
Group5	1.72	0.69	4.28
Group6	0.73	0.46	1.15
Group7	1.12	0.69	1.83
Male	1.18	0.84	1.64
Age	0.97	0.96	0.98
Alaska Native vs. Other	1.25	0.77	2.03
White vs. Other	0.89	0.57	1.38
Fairbanks	1.01	0.67	1.52
Juneau	0.89	0.53	1.49
Kenai/Homer	1.11	0.68	1.81
Ketchikan	1.91	0.87	4.17
Mat-Su	1.37	0.81	2.31
PriorArst	1.57	1.41	1.76

Appendix C.
Analysis of AOD Recidivism

This appendix describes the results of our analysis of the alcohol- and drug-related (AOD) recidivism measures. In general, the results of this analysis were similar to those of the analysis of recidivism in all offense categories. We used the same approach to model estimation that we used with the other recidivism measures. We began by estimating a full logistic regression model that included four group dummies, subject demographics, and ten measures of criminal history. Then, we discarded non-significant criminal history measures until we were left with a more parsimonious model that fit the outcome as well as the full model.

The first AOD outcome examined was arrests within 42 months of ASAP judgment date (ARST42AODCENSORED). The fit of the reduced model was not significantly worse than that of the full model (-2LL = 1013.67 – 1005.40 = 8.27; d.f. = 9; $P(>\chi^2) = 0.51$). We found that subjects who completed the ASAP education program (GROUP4) were less likely than subjects who were never assessed (GROUP1) to be arrested for a new AOD offense (e.g., DWI, drug possession or distribution) during the 42 months following their ASAP judgment (see Table 30). None of the parameters associated with the other group dummy variables (i.e., GROUP5, GROUP6, and GROUP7) reached statistical significance. We also found that younger subjects were somewhat more likely than older subjects to have a new AOD arrest. These findings are fully consistent with our analysis of all post-judgment arrests. Race/ethnicity was not significantly associated with risk of post-judgment arrest, but we did find that white subjects were less likely than subjects of 'other' ethnic origin to have a new *AOD* arrest following their ASAP judgment date. All types of AOD offenses were categorized as crimes against society, so it is not surprising that subjects with greater numbers of prior crimes against society (PRIORARSTSOC) were more likely to have a new AOD arrest during the follow-up period.

Table 30. AOD Arrest Within 42 Months of Judgment: Logistic Regression Estimates

		95% Confidence Interval	
Effect	**Odds Ratio**	**Lower Limit**	**Upper Limit**
Group4	0.54	0.32	0.92
Group5	1.13	0.46	2.82
Group6	0.98	0.62	1.54
Group7	1.18	0.74	1.88
Male	0.99	0.69	1.43
Age	0.97	0.96	0.99
Alaska Native vs. Other	0.93	0.57	1.50
White vs. Other	0.60	0.38	0.95
PriorArstSoc	1.27	1.08	1.50

We also used the reduced model to estimate the probability of recidivism (i.e., AOD arrest) for typical subjects in the different groups (see Table 31). The model indicated that a young (age 24) Alaska Native who was never assessed (Group 1) would have 0.37 probability of being rearrested on an AOD charge within 42 months of ASAP judgment. By contrast, an older (age 42) white subject who completed ASAP education would have a 0.10 probability of a new AOD arrest.

Table 31. Predicted Probability of AOD Arrest Within 42 Months Among 'Typical' Subjects

Group	Age	Race	Predicted Probability of Recidivism
1	24	Alaska Native	0.37
1	31	Alaska Native	0.33
1	38	Alaska Native	0.29
1	24	White	0.28
1	31	White	0.24
1	38	White	0.21
4	23.5	White	0.15
4	33.5	White	0.12
4	42.5	White	0.10
5	21.5	White	0.30
5	30	White	0.25
5	39	White	0.21
6	26	White	0.25
6	35	White	0.21
6	42	White	0.18
7	27	Alaska Native	0.39
7	36	Alaska Native	0.34
7	41	Alaska Native	0.31
7	27	White	0.30
7	36	White	0.25
7	41	White	0.22

Our analysis of AOD arrests leading to conviction (CONV42AODCENSORED) lead to nearly identical findings (see Table 32). The fit of the reduced model was not significantly worse than that of the full model (-2LL = 930.92 – 920.56 = 10.36; d.f. = 9; $P(>\chi^2) = 0.32$). The only substantive difference in findings was that the number of prior arrests for crimes against society (PRIORARSTSOC) approached, but did not reach, statistical significance. The statistically significant parameters in the reduced logistic regression model of AOD convictions were GROUP4, AGE, and the parameter contrasting white subjects were those of 'other' ethnic origin.

Table 32. AOD Conviction Within 42 Months of Judgment: Logistic Regression Estimates

		95% Confidence Interval	
Effect	Odds Ratio	Lower Limit	Upper Limit
Group4	0.56	0.32	0.98
Group5	0.96	0.36	2.60
Group6	0.99	0.61	1.61
Group7	1.13	0.69	1.85
Male	0.99	0.67	1.45
Age	0.98	0.96	0.99
Alaska Native vs. Other	0.86	0.52	1.43
White vs. Other	0.59	0.37	0.95
PriorArstSoc	1.18	1.00	1.39

When we repeated our analysis of recidivism probabilities for typical subjects using the model of AOD convictions, we obtained a similar pattern of probabilities (see Table 33) as we had before when estimating probabilities of AOD arrest. Naturally, however, the probability of conviction was lower than that for arrest.

Table 33. Predicted Probability of AOD Conviction Within 42 Months Among 'Typical' Subjects

Group	Age	Race	Predicted Probability of Recidivism
1	24	Alaska Native	0.30
1	31	Alaska Native	0.26
1	38	Alaska Native	0.23
1	24	White	0.23
1	31	White	0.20
1	38	White	0.17
4	23.5	White	0.13
4	33.5	White	0.11
4	42.5	White	0.09
5	21.5	White	0.22
5	30	White	0.19
5	39	White	0.16
6	26	White	0.21
6	35	White	0.18
6	42	White	0.15
7	27	Alaska Native	0.31
7	36	Alaska Native	0.27
7	41	Alaska Native	0.24
7	27	White	0.24
7	36	White	0.20
7	41	White	0.18

We also estimated a Kaplan-Meier survival plot of alcohol- and drug-related (AOD) arrests during the post-judgment follow-up period (see Figure 4). The scale on the vertical axis of the figure spans from 0.65 to 1.00 rather than from 0 to 1. This change of scale was necessary to make the lines appear distinct. Like Figure 2, which plots all arrests during the follow-up period, Figure 4 shows that AOD arrests were least common among subjects who completed ASAP education (Group 4) and most common among subjects who were never assessed (Group 1) and those who failed to complete the ASAP treatment successfully (Group 7).

Figure 4. Survival Plot of AOD Arrests Within 42 Months of Judgment

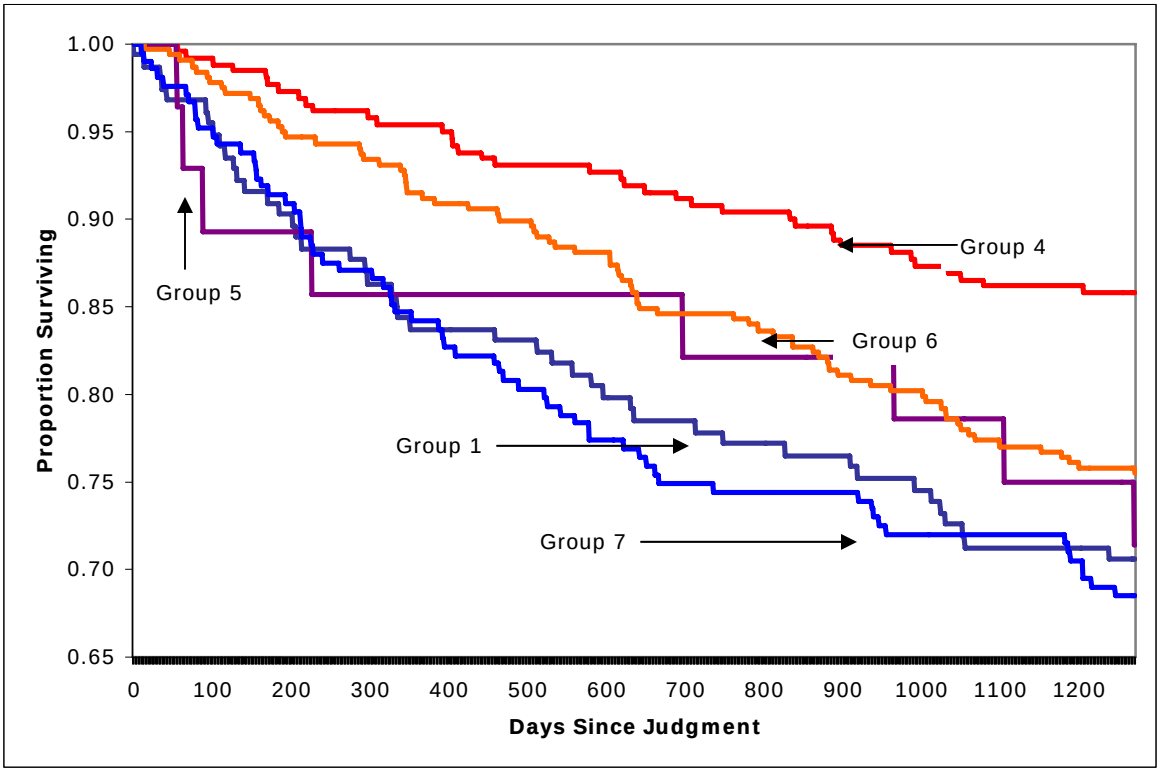


Table 34 displays the hazard rates of AOD arrest during the follow-up period. It is analogous in presentation and purpose to the hazard rates for all post-judgment arrests displayed in Table 13. Note that many of the confidence intervals include zero, and it was not possible to estimate several of the confidence intervals because no AOD arrests were observed during the interval.

Table 34. Hazard Rates for AOD Arrest After Judgment, by Group

	Interval Limits			Hazard	95% Confidence Intvl.	
Group	Lower	Midpoint	Upper	Rate	Lower Limit	Upper Limit
1	0	15.5	30	0.00042	0.00000	0.00101
1	31	46	60	0.00066	0.00000	0.00142
1	61	76	90	0.00000	.	.
1	91	136	180	0.00069	0.00024	0.00114
1	181	273.5	365	0.00044	0.00018	0.00071
1	366	457	547	0.00013	0.00000	0.00028
1	548	639.5	730	0.00027	0.00005	0.00049
1	731	1005	1278	0.00018	0.00007	0.00028
4	0	15.5	30	0.00000	.	.
4	31	46	60	0.00013	0.00000	0.00038
4	61	76	90	0.00013	0.00000	0.00038
4	91	136	180	0.00017	0.00000	0.00034
4	181	273.5	365	0.00013	0.00003	0.00023
4	366	457	547	0.00013	0.00003	0.00024
4	548	639.5	730	0.00014	0.00003	0.00025
4	731	1005	1278	0.00010	0.00005	0.00016
5	0	15.5	30	0.00000	.	.
5	31	46	60	0.00121	0.00000	0.00359
5	61	76	90	0.00256	0.00000	0.00612
5	91	136	180	0.00000	.	.
5	181	273.5	365	0.00022	0.00000	0.00065
5	366	457	547	0.00000	.	.
5	548	639.5	730	0.00023	0.00000	0.00069
5	731	1005	1278	0.00025	0.00000	0.00054
6	0	15.5	30	0.00010	0.00000	0.00030
6	31	46	60	0.00021	0.00000	0.00050
6	61	76	90	0.00021	0.00000	0.00051
6	91	136	180	0.00032	0.00011	0.00054
6	181	273.5	365	0.00024	0.00011	0.00036
6	366	457	547	0.00019	0.00007	0.00031
6	548	639.5	730	0.00024	0.00010	0.00037
6	731	1005	1278	0.00021	0.00013	0.00028
7	0	15.5	30	0.00047	0.00000	0.00099
7	31	46	60	0.00033	0.00000	0.00078
7	61	76	90	0.00083	0.00010	0.00155
7	91	136	180	0.00046	0.00014	0.00077
7	181	273.5	365	0.00044	0.00022	0.00067
7	366	457	547	0.00036	0.00015	0.00057
7	548	639.5	730	0.00028	0.00009	0.00047
7	731	1005	1278	0.00016	0.00007	0.00025

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April 5, 2007

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LEGISLATIVE AUDIT

Legislative Budget and Audit Committee
Division of Legislative Audit
P.O. Box 113300
Juneau, AK 99811-3300

Dear Pat Davidson:

Thank you for the opportunity to respond to the preliminary audit reports on:

Department of Health and Social Services, Use of Recidivism Rates by State Agencies, Overview of Current Practices, February 23, 2007

Department of Health and Social Services, Use of Recidivism Rates by State Agencies, Recidivism Rates for the Alcohol Safety Action Program, March 13, 2007

Department of Health and Social Services, Use of Recidivism Rates by State Agencies, Recidivism Rates for Alaska Sex Offenders, March 8, 2007

The Department of Law understands that these audits have not been approved, as of yet, by the Budget and Audit Committee, that they are not final and as such are confidential. The Department of Law views all measures of how well it is conducting its responsibility to protect the public as an important and worthwhile undertaking, and thanks you for the opportunity to comment on these audits. First addressing *Department of Health and Social Services Use of Recidivism Rates by State Agencies, Overview of Current Practices*, criminal recidivism is both a measure of treatment programs and a measure of calculating the function of the criminal justice system. If a shoplifter is arrested, convicted, and sentenced to three days in jail and he or she does not commit another criminal offense, this lack of recidivating tells little of the effectiveness of a treatment program because the program was not mandated in the sentence. On the other hand, it may be that the three day incarceration or the public opprobrium of going through the criminal justice system was successful in preventing future criminal behavior. The audit uses recidivism as a measure of treatment programs ordered by the court and does not consider other possible reasons that an offender may not commit a subsequent

criminal offense. For example, age has been identified as one of the most significant factors in recidivism independent of program participation for most violent crimes.

The audit also describes the varying lengths of time used in calculating the period of time that is being measured for the recidivism study. There are varying bench marks from which the time clock can start to run; the incident, arrest, time of conviction, completion of incarceration, completion of a treatment program, or end of probation. Each of these beginning points effects whether an event is calculated as a failure.

A brief comment or explanation needs to be made about the *Use of Recidivism Rates by State Agencies, Recidivism Rates for the Alcohol Safety Action Program* audit. The audit indicates the percentage of ASAP clients that were never accessed for treatment or education was highest in Fairbanks at 25%. This is compared to a statewide average of 16%. The audit states "[T]he high percentage of clients not coming into the ASAP office may be attributed to a reluctance on behalf of the Fairbanks prosecutor to act on petitions to revoke probation filed with the prosecutor by the Fairbanks' ASAP Office." The audit goes on to say that the practice of not filing petitions has now changed as it well should. The future will tell if this changed practice will affect the recidivism rate. The failure to file a petition to revoke probation may not be the source of the failure of an offender to appear for an assessment. The failure to appear for an assessment is in the sole control of the offender. A petition to revoke probation comes after the offender fails to appear for the assessment. It is possible that offenders would know of a practice of the district attorney's office not to file petitions, but such knowledge is unlikely.

Finally, in *Use of Recidivism Rates by State Agencies, Recidivism Rates for Alaska Sex Offenders*, under the chapter heading "Summary Recidivism Results", the audit says:

> "[I]n at least three instances, offenders recommitted sex crimes which were not prosecuted. Subsequent sex crimes were treated as violations of their probation/parole."

Page 21.

If a sex offender is on probation and the public can be better served by an agreement to admit a probation revocation, it would be surprising if the prosecutor didn't take this option. For example, if an offender had 10 years of suspended time of incarceration and committed a subsequent sexual assault that was for a C felony offence calling for a presumptive sentence of a 2 years and the prosecutor believed that isolation was an important consideration, he or she may well have chosen to proceed with a probation violation. Also, the burden of proof at a probation revocation is by a preponderance of the evidence, while at trial it is beyond a reasonable doubt. The evidence may be such that the lesser burden can be reached but not the greater. Again, the public interest would be better served by the probation revocation rather than through

a trial. This may mean that data is not easily captured to calculate a recidivism rate, but that the public is better served.

Thank you for the opportunity to respond to the issues in these audits.

Sincerely,

TALIS J. COLBERG
ATTORNEY GENERAL

By: Richard A. Svobodny
Deputy Attorney General

Cc: Talis Colberg, Attorney General

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April 5, 2007

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RE: Response to Preliminary Audit, Use of Recidivism Rates by State Agencies, Recidivism Rates for the Alcohol Safety Action Program (ASAP), March 13, 2007

Dear Ms. Davidson:

Thank you for the opportunity to respond to the Preliminary Audit, Use of Recidivism Rates by State Agencies, Recidivism Rates for the Alcohol Safety Action Program (ASAP). There are no recommendations or findings in the audit to respond to at this time, but it is clear that the Division of Legislative Audit went to considerable effort to develop recidivism rates for the ASAP program.

The recidivism information contained in the audit is very interesting and will be of considerable use as the department reviews the goals of the ASAP program. We intend to use the recidivism information contained in the audit for assessing the value of the ASAP program for the future.

Sincerely,

for Karleen K. Jackson, Ph.D.
Commissioner

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APR 11 2007
LEGISLATIVE AUDIT

State of Alaska

Department of

Public Safety

Sarah Palin, Governor
Walt Monegan, Commissioner

April 9, 2007

Ms. Pat Davidson
Alaska State Legislature
Legislative Budget and Audit Committee
P.O. Box 11330
Juneau, AK 99811-3300

Ms. Davidson;

Thank you for giving the Department of Public Safety the opportunity to review the three part legislative audit report on:

Department of Health and Social Services, Use of Recidivism Rates by State Agencies, Overview of Current Practices, February 23, 2007

Department of Health and Social Services, Use of Recidivism Rates by State Agencies, Recidivism Rates for the Alcohol Safety Action Program, March 13, 2007

Department of Health and Social Services, Use of Recidivism Rates by State Agencies, Recidivism Rates for Alaska Sex Offenders, March 8, 2007

The report includes the following recommendation relative to the Department of Public Safety:

The Commissioner of the Department of Public Safety (DPS), as chair of the criminal justice information advisory board, should reestablish the board as a first step towards integrating the State's criminal justice systems.

We will identify appropriate board members as specified in AS 12.62.100, and will schedule a meeting as soon as possible. It is likely that this will not occur until after the legislature is dismissed.

Again, thank you for the opportunity to review and comment on the above named reports.

Sincerely,

Walt Monegan
Commissioner

Office of the Commissioner
450 Whittier Street - Juneau, AK 99811 - Voice (907) 465-4322 - Fax (907) 465-4362

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ALASKA COURT SYSTEM

State of Alaska

Christine E. Johnson
DEPUTY ADMINISTRATIVE DIRECTOR

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April 5, 2007

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APR 09 2007
LEGISLATIVE AUDIT

Pat Davidson
Legislative Auditor
Alaska State Legislature
Legislative Budget and Audit Committee
Division of Legislative Audit
PO Box 113300
Juneau AK 99811-3300

Re: Legislative Budget and Audit Committee Audit Reports

Dear Ms. Davidson:

I am responding to your request for comments on three preliminary audit reports:

> *Department of Health and Social Services, Use of Recidivism Rates by State Agencies, Overview of Current Practices, February 23, 2007*
>
> *Department of Health and Social Services, Use of Recidivism Rates by State Agencies, Recidivism Rates for the Alcohol Safety Action Program, March 13, 2007*
>
> *Department of Health and Social Services, Use of Recidivism Rates by State Agencies, Recidivism Rates for Alaska Sex Offenders, March 8, 2007*

Regarding the first report, the court system agrees with the recommendation to reestablish the criminal justice information advisory board (CJIAB). Agency staff have done an excellent job laying the groundwork for a statewide information-sharing project by educating themselves about integrated justice technology, national standards, and best practices through MAJIC. But they cannot move forward without leadership and support at the policy level.

The other two recommendations in this report are directed at other agencies, and the court system has no basis for agreeing or disagreeing with the auditor's conclusions. However, we

do disagree with some of the auditor's comments regarding the Batterers Intervention Program (BIP). On page 22 of the report, the auditor states:

> When sentencing, judges indicate on the sentencing document whether the offender is required to attend BIP. This represents the first obstacle in the enforcement of BIP referrals. The court system does not consistently record these domestic violence BIP referrals in the court system database. Consequently, there is no reliable electronic means of efficiently identifying defendants court-ordered to a BIP. The only means of identifying BIP referrals is through obtaining copies of the judgments from the court system indicating that an offender is required to attend BIP.

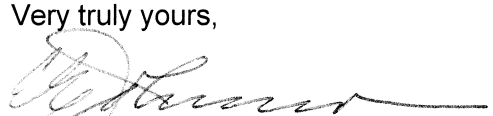
We take issue with this paragraph because it implies that the court system has a duty to collect information about BIP referrals that we are not fulfilling. Primary responsibility for enforcement of BIP referrals rests with the prosecuting authority. We send the prosecuting authority a paper copy of every referral. We do not require clerical staff to record referrals in our case management system because we do not need this information to perform our core business functions, we are not mandated to keep the information electronically, and in most court locations we are not in a position to perform non-essential data entry.

We also do not believe that the report should single out the court system for not maintaining an electronic record of referrals when there are other agencies that could also be entering the information into a database. To correct these problems, we suggest that the language be changed along the lines shown below

> When sentencing, judges indicate on the sentencing document whether the offender is required to attend BIP. This represents the first obstacle in the enforcement of BIP referrals. The court system <u>sends a paper copy of the sentencing document to the prosecuting authority, but</u> does not consistently enter these domestic violence BIP referrals in the court system database. <u>Prosecuting authorities also do not maintain an electronic record of referrals.</u> Consequently, there is no reliable electronic means of efficiently identifying defendants court-ordered to a BIP. The only means of identifying BIP referrals is through obtaining copies of the judgments from the court system indicating that an offender is required to attend BIP.

We have no comments on the other two audit reports, which are directed at other agencies.

Very truly yours,



Christine Johnson
Deputy Administrative Director

Cc: Stephanie Cole, Administrative Director

alaska judicial council

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Chief Justice
Supreme Court

April 9, 2007

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APR 09 2007
LEGISLATIVE AUDIT

Pat Davidson
Legislative Auditor
Division of Legislative Audit
P.O. Box 113300
Juneau, AK 99811-3300

Dear Ms. Davidson:

Thank you for inviting our comments on the three Legislative Audit reports on recidivism. We have appreciated the opportunity to work with you and your agency throughout this project. The reports are important additions to Alaska's criminal justice system knowledge. They will be valuable references for research in many different fields.

Our staff reviewed the reports during their preparation with Kristin Dzinich on your staff, and reviewed the most recent drafts with Anne McLean. Ms. Dzinich made our suggested changes in the reports. In the most recent version of the reports, the only changes were half a dozen very minor changes that we reviewed by phone with Ms. McLean.

The reports give legislators and researchers guidance about how to use recidivism to measure the performance of programs that intend to improve the criminal justice system. They set new standards for the rigorousness of evaluations and data collection in Alaska. You and your staff are to be complimented on the quality of these reports, both substantively, and in their presentation. We look forward to working with you again.

Sincerely,

Larry Cohn
Executive Director

cc: Chief Justice Dana Fabe

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