

**RECIDIVISM PATTERNS IN THE  
CANBERRA REINTEGRATIVE SHAMING EXPERIMENTS (RISE)**

by

Lawrence W. Sherman, Jerry Lee Center of Criminology  
University of Pennsylvania, Principal Investigator

Heather Strang, Centre for Restorative Justice  
Australian National University, Project Director

Daniel J. Woods, University of Pennsylvania, Data Analysis



Centre for Restorative Justice  
Research School of Social Sciences  
Australian National University

November 2000

## **Acknowledgments**

This is a project supported by a grant from the Criminology Research Council. The views expressed are the responsibility of the authors and are not necessarily those of the Council.

Overall, the authors would like to express their thanks to the funders whose generosity has made possible not only the research reported upon here, but also the entire Reintegrative Shaming Experiments. Their support over a long time frame has allowed us to undertake a rigorously conducted study that we expect to be a rich resource for restorative justice researchers and practitioners for many years to come.

The Australian National University  
Criminology Research Council  
National Drug-Crime Prevention Fund (Commonwealth Department of Health and the National Police Research Unit)  
Federal Office of Road Safety (Commonwealth Department of Transport and Regional Services)  
National Institute of Justice (USA)  
University of Pennsylvania (USA)

## Executive Summary

This report presents the evidence available to date from RISE (Reintegrative Shaming Experiments) about the effects of diversionary restorative justice conferences on repeat offending. RISE accepted new cases until July 1 of 2000, which means that most (but not all) cases have had at least a one year followup period in which to measure repeat offending. While some experiments have had much longer, the important question this report must address is the effect of diversionary conferences on different offence types. In order to make such comparisons; a standard one year before-after difference in offending rates is presented. As more time passes, future RISE reports will encompass all cases taken into the experiments, with a minimum of two year followup periods. **Thus the findings presented here could well change.** For the moment, however, they are the best evidence we have.

Using the standard reporting format, this report presents evidence that when compared to court, the effect of diversionary conferences is to cause a

- Big drop in offending rates by violent offenders (by 38 crimes per 100 per year)
- Very small increase in offending by drink drivers (by 6 crimes per 100 offenders per year)
- Lack of any difference in repeat offending by juvenile property offenders or shoplifters (though after-only analysis shows a drop in reoffending by shoplifters).

The report presents the data in support of these conclusions and then explores the differences in the four experiments that might account for these differences in repeat offending rates. These differences require substantial additional analysis that will be strengthened when data on all randomized cases are available. This report considers the kinds of offenders allocated to each treatment group in each of the four experiments.

## Introduction

This report describes findings from the Reintegrative Shaming Experiments (RISE) on recidivism behaviour among offenders involved in the almost 1300 cases which were the subject of the experiments. It represents the final report of the research team to the Criminology Research Council.

The aim of the study has been to compare the effects of standard court processing with the effects of a restorative justice intervention known as diversionary conferencing for four kinds of offences:

- Drink driving (over .08 blood alcohol content (BAC) by offenders at any age
- Juvenile property offending with personal victims by offenders aged under 18 years
- Juvenile shoplifting offences by offenders aged under 18 years detected by shop security staff
- Youth violent offences by offenders aged under 30 years

The key criteria for comparing court processing to conferences are these:

- Perceptions of procedural fairness by victims and offenders
- Victim satisfaction with the process
- Costs
- Patterns of repeat offending

Our Progress Reports of July 1998 and July 1999 ([www.aic.gov.au/rjustice/rise/index.html](http://www.aic.gov.au/rjustice/rise/index.html)) revealed that across all four offence categories, both offenders and victims found conferences to be procedurally fairer than court. Heather Strang's report entitled 'Victims and Restorative Justice: the Canberra Reintegrative Shaming Experiments' (forthcoming as a book), found higher levels of victim satisfaction with conferences than with court. The analysis relating to the costing hypothesis is still underway. This report specifically concerns findings relating to repeat offending.

## Hypotheses

A central hypothesis of the experiments is that there will be less repeat offending after a conference than after court. The hypothesis draws on Braithwaite's theory of reintegrative shaming (1989) which argues that formal court justice stigmatises offenders as well as offences and makes it difficult for them to lead lives as responsible members of the community: the shame and mobilisation of a community of care engendered by a restorative intervention like conferencing should provide an opportunity for offenders to confront the consequences of their actions and allow the harm caused by the offence to be repaired. It also draws on Sherman's (1993) research which found that formal justice can result in more repeat offences than diversion to informal processing, especially when the court experience makes offenders generally more defiant of conventional society.

In addition, there is empirical research showing that people who feel they have been treated fairly by the justice system are more likely to comply with the law in future (Tyler 1990, Barnes 1999). RISE findings to date indicate that offenders randomly assigned to a conference expressed much more satisfaction with the procedural fairness of their treatment than offenders randomly assigned to court. We were particularly interested in examining whether previous research findings about this link would be replicated in the RISE experiments.

This hypothesis has taken longer than anticipated to investigate because of the pace with which cases were referred by police into the experiments and because of the necessity of leaving a window of opportunity for offenders to reoffend. We wished to leave two years between the date on which offenders were randomly assigned to court or to a conference and the date by which their reoffending behaviour was measured. This has been achieved in the drink driving experiment. However, new case assignments for the other three experiments ceased only in June 2000. Our earliest cases have five years of reoffending behaviour which can be measured, while the latest have only a few months. In order to compensate for the wide variation in 'time at risk' and to ensure that a fair assessment is made of offenders' post-assignment offending behaviour compared with their pre-assignment offending behaviour, we

have limited the analysis to the minimal time period that we have on the maximum number of cases. Any other approach would lead to misleading comparisons of experimental results across experiments using very different periods of followup measurement of repeat offending.

**Measures of Repeat Offending.** In this report, official criminal history data are used to calculate reoffending behaviour. The Australian Federal Police have made these data available for all offenders in each of the four experiments. We have included self-report criminal history questions in the interview schedule administered to each offender two years after their random assignment, but sufficient data are not yet available to allow their meaningful interpretation. We will report on the results of these interviews in future publications and reports posted on the Australian Institute of Criminology website, where all prior RISE reports may be found at [www.aic.gov.au/rjustice/rise/index.html](http://www.aic.gov.au/rjustice/rise/index.html).

**Explaining Different Effects Across the Four Experiments.** In investigating what factors may contribute to the varying levels of reoffending by court-assigned and conference-assigned offenders across the four experiments, many theories can be tested. These include differences in the kinds of offenders who were selected by police to be included in the RISE tests, as well as theories of the offenders' perceptions of the quality of justice. The latter theories may be examined based on the extensive data collected in the offenders' responses at their interview obtained directly after their final case disposition. We have collected data on their perceptions of procedural justice (how fair they saw each procedure to be), of substantive justice (their perceptions of the substantive sanction they received), of restorative justice (their perceptions about the repair of harm) and their emotional reactions to the treatment they experienced. These findings are based upon interview response rates of 85 percent for drink driving offenders, 75 percent for juvenile personal property offenders, 79 percent for juvenile property (shoplifting apprehended by store security) offenders and 68 percent for youth violent crime offenders. We have also collected observational data from almost all conferences and about 85 percent of the court appearances offenders made when they were assigned to court.

While a full exploration of the reasons that restorative justice conferences produced different effects for different experiments must await the completion of data collection on all initial interviews and observations of justice processes, the current report presents a basic

comparison of a key difference across the experiments: the kinds of people in each experiment. This analysis examines differences in the length and types of prior offence histories they brought to the experiment, as well as their gender, employment status, and other risk factors.

**Analysing Repeat Offending Effects.** There are many ways that repeat offending can be analysed. The prevalence of offenders with any repeat offences, the frequency of offending rate of each offender, the time-to-failure (length of time until next criminal apprehension) and other measures may all be used. In experimental designs, it is also possible to examine only the differences between two groups after they have received different treatments—or to examine the differences in offending rates from before to after the treatment. When experiments consider before-after differences in offending rates, they face the further choice of presenting such differences within treatment groups, or differences between the treatment groups in the magnitude of their before-after differences. Each of these designs has different strengths and limitations, and each is more or less appropriate under the circumstances in which the present report is written: completion of case intake without completion of case treatment or followup interviews or recidivism measurement.

Given the circumstances of data collection at the present stage of RISE, the most appropriate design to use in the present analysis is the analysis of before-after differences in offending rates. This approach has the virtue of controlling for differences between the groups in prior offending rates, which can (and did) occur because of relatively small sample sizes in which such baseline differences can occur by chance. When the full samples become available for analysis, these differences will not be as likely to occur with such magnitude, as randomisation tends to even out such differences (on average) as sample sizes grow larger.

The report also employs offending rates rather than the prevalence of repeat offending. The latter measure would be distorted by the differences between treatment groups in some experiments in the prevalence of prior offending. It is also arguably more important for public policy to stress the effects of policies on offending rates, rather than on the proportion of offenders with any repeat offences. While the latter measure may be easier to comprehend, it may mask differences in the volume of crime in the community—differences that are far more important to victims and potential victims of crimes. Put another way, the use of offending frequency rates as the primary measure of repeat offending draws attention to the number of

criminal events occurring in the community, rather than the number of active offenders residing in the community. The former of the two features the more direct effect on public safety.

The report uses two methods to present the differences in the rates of offending by offenders assigned to court and conference. One method shows the differences in rate of offending one year before to one year after the assignment of the case to court or conference, within each group. The other method examines the difference of differences. Using both approaches results in three tests of statistical significance ('P' values, or probability that the result is due to chance) reported for each graph showing repeat offending rates. One P value refers to the difference between the rates of offending among offenders randomly assigned to conferences. Another refers to such differences among offenders randomly assigned to court. The third refers to the difference between those two before-after differences.

The report uses the standard that if one treatment group shows a significant before-after change in rates, but the other one does not, that this contrast shows a treatment effect. Alternatively, if there is a difference of differences between the two groups that is not likely to be due to chance, then that is also evidence of a treatment effect. Either or both of these kinds of differences provide strong evidence that there is a different effect of court and conference on offending rates.

**What Does Statistical 'Significance' Mean?** In this report the term 'statistical significance' is treated as a concept meaning 'statistically discernible from chance'. We assume, with most statisticians, that the cutoff point for such discernibility is arbitrary and that the probability that a result is due to chance can be interpreted by the reader. Thus the conventional significance level of .05 means that there is a 95 percent chance that a result is not a coincidence, a .15 level means that there is an 85 percent chance. In policy terms, an 85 percent chance of being right may be acceptable. Significance levels therefore are more of a heuristic guide to the interpretation of the results than a bright line between a difference and no difference. The magnitude of effect – or how big a difference is – in contrast, receives more weight in our analysis, especially as measured by the statistic for effect size, known as Cohen's D. Where this is large, even borderline statistical significance in the 80 percent range should give one confidence that this is an important result.

Another statistic, ‘Somer’s D,’ is used to describe whether the magnitude of the differences in offender characteristics across experiments is due to chance. These differences are called ‘categorical’ or nominal data, which simply compare percentages of cases in different categories. This statistic is a measure of how much error can be reduced by predicting that a case will be in a certain category. When the statistic is larger, that suggests that the difference across experiments is substantial enough to warrant further investigation as a possible reason for the different effects of diversionary conferencing.

**What Does ‘Assigned’ Treatment Mean?** The report refers often to the level of difference on these measures between court-assigned and conference-assigned offenders. It should be noted that, in order to preserve the equivalence between the two treatment groups, all analysis is presented on the basis of assigned treatment rather than treatment actually delivered. There was a very low deliberate misassignment rate in RISE (three percent), but there were a number of cases where it proved not possible to give the assigned treatment to offenders.<sup>1</sup> The logic of using the treatment that was randomly assigned, rather than the one that was actually delivered, is that the delivery of treatment may have been partly a function of the offenders’ behavior. If the offender never showed up for a conference, for example, the conference treatment was never delivered. Removing that offender from the ‘assigned’ conference sample, however, would bias the results in favour of conferences. That bias would occur because any ill-behaved or defiant offenders assigned to conference would weed themselves out, leaving the ‘delivered’ conference offenders a group likely to have a lower repeat offending rate than the full, randomly assigned sample. In short, using ‘assigned’ treatments preserves the level playing

---

<sup>1</sup> For Drink Driving, 96 percent assigned to court were treated in court (the remainder were abandoned), 90 percent assigned to conference were treated by conference (five percent went to court, the remainder were abandoned).

For Juvenile Personal Property, 84 percent assigned to court were treated in court (the remainder were cautioned, abandoned or not proceeded with), 69 percent assigned to conference were treated by conference (12 percent went to court, the remainder were cautioned or abandoned).

For Juvenile Property (Shoplifting apprehended by store security), 89 percent assigned to court were treated in court the remainder were cautioned or abandoned), 89 percent assigned to conference were treated by conference (five percent went to court, the remainder were cautioned or abandoned).

For Youth Violence, 90 percent assigned to court were treated in court (the remainder were either cautioned, abandoned or not proceeded with), 81 percent assigned to conference were treated by conference (nine percent went to court, the remainder were cautioned or abandoned).

field between the two treatments, rather than letting other circumstances stack the deck against one or the other of the two approaches.

**When Does the Measured Effect Occur?** All repeat offending rates are calculated from the day the each offender is randomly assigned to a RISE treatment, rather than from the day that treatment is actually completed. This decision has several bases. One is that the offenders are aware of whether they will go to court or conference from the date we employ, and there may thus be effects of that awareness on their offending behavior. Such ‘placebo’ effects have been found in medicine, with anticipation of a treatment having effects even when treatment is not delivered. Secondly, there were sometimes substantial delays between apprehension and final treatment, both in court and in conference.<sup>2</sup> This creates uneven time periods between treatment groups, and makes fair comparisons impossible. The ‘intention to treat’ is actually an indication of a policy of treating people this way, and in that sense a better test of what would happen with such a policy—of trying to implement the treatment—than only examining cases of successfully completed treatments. (The terms ‘pre-RISE’ and ‘post-RISE’ refer to the 365 day periods before and after the RISE random assignment dates).

**What Is the Difference between ‘Cases’ and ‘Offenders’?** RISE was based on the random assignment of cases, rather than offenders, to court or conference treatments. A ‘case’ for these purposes was all of the offenders who were apprehended together for the same criminal offence. The case was the preferred unit of analysis because it is inherent in the theory of restorative justice and diversionary conferencing: that all offenders involved in committing an offence should share responsibility for it. While this was not always the theory of the court—which sometimes treated offenders separately and sometimes together—it was a consistent standard both for eligibility for RISE and for the completion of diversionary conferences. In most cases, conferences were held with all known offenders present in the room and with all known victims of the crime, as well as victim supporters and offender supporters.

---

<sup>2</sup> Average days until final treatment:

Drink driving – court = 54 days, conference = 60 days

Juvenile personal property – court = 74 days, conference = 106 days

Juvenile property (shoplifting apprehended by store security) – court = 37 days, conference = 63 days

Youth violence – court = 120 days, conference = 111 days

Strictly speaking, the analysis of repeat offending is done most precisely when the unit of analysis is the case rather than the offender. The offenders assigned to each treatment vary in number more than the cases assigned to each treatment, which are almost equal. But in this report we employ the offender as the unit of analysis rather than the case. This makes the analysis somewhat easier to follow, as it allows us to avoid using mean offending rates per offender per case as the principal measure of repeat offending. That measure, while technically the best test of repeat offending, is best reserved for the complete analysis of all randomly assigned cases when two-year followup periods are available on all cases.

## Results

### Youth Violence<sup>3</sup>

This experiment found that diversionary conferences reduced offending rates by about 38 crimes per 100 offenders per year, relative to the effect of being sent to court.

The youth violence experiment consists of 100 cases involving 121 offenders. Because the last case came into RISE only in June 2000 and several offenders have not yet been disposed of or interviewed, it has been necessary to conduct most of this analysis on the basis of the first 89 cases, involving 110 offenders, rather than the full experimental sample. We have at least one year of followup and baseline data on all 110 offenders in these 89 cases.

Figure 1 shows that the officially detected rate of offending per offender in the year before the RISE date was .081 per month in the conference group, and .071 in the court group. On an annual basis, this amounts to just under one offence per offender per year, on average. Yet in the year after the RISE date, the officially detected offending rate for the conference cases fell to less than half of one offence per offender—or one offence every two years. The rate of offending by offenders assigned to court barely dropped at all. In terms of significance, the drop in offending rates by offenders sent to court was almost 62 percent likely to have been a chance effect, while the drop in offending rates for conference offenders was only 1 percent likely to have been due to chance. The difference of before-after difference between these two measures was only 16 percent likely to have been due to chance, or 84 percent likely to be a result that would be found repeatedly with a similar sample of cases. The measure Cohen's D shows that the effect size of this difference of differences is .26, which is a relatively substantial effect.

Figure 2 expresses the effects of diversionary conferences on offending rates in percentage terms. While the conference group's rate of offending fell by 49 percent, the court group's fell by only 11 percent. This difference produces a net reduction of 38 percent in the conference group relative to the change in the court group.

---

<sup>3</sup> In the accompanying figures, youth violence offending is referred to as 'JVC' (Juvenile/Youth Violent Crime).

### Drink Driving Experiment<sup>4</sup>

This experiment found that diversionary conferences resulted in a very small increase in all offences (6 crimes detected per 100 offenders per year for all offences; 4 crimes per 100 offenders per year for drink driving offences).

The experiment consisted of 900 cases, each with only one offender. Half were randomly assigned to court processing and half to diversionary conferencing. All cases had been assigned by December 1997 and had been treated by the end of 1998. The data presented in the Figures are based on the entire 900 case sample.

Figure 3 analyses all types of offences by the offenders apprehended for drinking and driving. It shows that there was an increase in offending by both groups in the year after RISE relative to the year before RISE. Both increases were relatively unlikely to be due to chance. Yet the difference between the differences was modest, if only 13 percent likely to be a chance result. The effect size of that difference was a mere .1, which is modest indeed. On the other hand, in relative terms, Figure 4 shows that the net increase in offending rates across all offences was 53 percent more for the conference cases than it was for the court cases. Yet this analysis is based on very low rates of offending—under one-tenth of one offence per year, or one offence every ten years. In comparison to the violent crime experiment, these offenders' mean rate of offending is one tenth the size.

Figure 5 shows that the effect of conferences on detected rates of drinking and driving was about the same as it was on all offences. The difference is that the detected rates of drinking and driving remained unchanged before and after assignment to court, while they doubled after assignment to conferences. But the base rates are so very low that they only amount to a net gain of about 4 offences per 100 offenders per year. While the difference of differences is significant, and while we can be confident that the difference is due to the randomly assigned treatment, the effect size is quite small. The major caveat here is that the low detected rates may reflect much higher rates of undetected drinking and driving. It is by that logic that we designed such a large sample, in order to detect any differences in low rates of detected offending. To the extent that the increase Figure 6 reports in detected drink driving

---

<sup>4</sup> In the accompanying figures, drink driving offending is referred to as 'PCA', an acronym used by the police referring to the term used in the legislation concerning 'proscribed content of alcohol' and adopted by the research staff in the course of the study.

indicates that the undetected rate has risen by the same magnitude, this could mean substantially more offending after conference compared with court.

It is important to note that these differences in reoffending behaviour relate only to the first year post-RISE and decay and vanish over the longer timeframe for which we have data in this experiment. There is also no difference between the court and conference groups in the percentage who had any second-time drink driving offence; that is, the small difference in offence rates is due to higher rates of third and fourth offences in the conference group. One interpretation which would explain such a pattern in the results is as follows. A reason why court cases should have lower reoffending than conference cases is that drivers' licences are suspended in court cases, but conferences do not have the power to do this. For first offences, these court-treated offenders typically had their licences suspended for three to six months. But in spite of the fact that they were put off the road for this length of the follow-up period, they were just as likely to commit a second offence as conferenced offenders. However, once a drink driver has two court convictions within a short space of years, magistrates normally suspend licences for twelve months. This means the incapacitative consequence of a second conviction (twelve months) was normally two-four times that of the first (three-six months). That is enough for the beneficial effects of a conference to be overwhelmed by the effect of the twelve month licence suspension in reducing reoffending. The fact that the difference does not hold up beyond the twelve month follow-up is also consistent with the interpretation that it is licence suspensions which help court to prevent reoffending. If this interpretation is right, drink driving conferences have no chance of preventing drink driving more than court unless conferences are given the same power to recommend licence suspensions. One of our future projects will be to discover whether this interpretation is correct through detailed decomposition of the results.

#### Juvenile Property (Shoplifting Apprehended by Store Security Officers)

This experiment found no differences in offending rates between court and conference groups on the basis of one-year before-after changes (though after-only analyses show significantly lower reoffending for the conference-assigned group than court-assigned group.)

This experiment consists of 117 cases involving 143 offenders. Because the last case came into RISE only in June 2000 and again several offenders have not yet been disposed of or

interviewed, it has been necessary to conduct most of this analysis on the basis of the first 108 cases, involving 135 offenders.

Figure 7 shows changes in the rates of offending in one-year periods pre-RISE and post-RISE. While there may appear to be a difference between the groups, the difference is very small and the odds of chance effects are very large. Neither the within-group nor between group differences are likely to be anything other than chance effects. Thus even while Figure 8 appears to show a reduction in offending rates for offenders assigned to court, the magnitude of change in the rates from the year prior to the year after is so small that the effect is most likely due to chance.

#### Juvenile Property—Personal Victims<sup>5</sup>

This experiment also shows no differences in offending rates according to whether offenders were assigned to court or conference.

This experiment consists of 173 cases involving 249 offenders. Again, because the most recent case came into the experiment in June 2000, a number of offenders have not been treated or interviewed. This analysis was therefore conducted on the basis of the first 162 cases, involving 238 offenders.

Figure 9 shows that none of the before-after differences are statistically discernible. All of them are at least 26 percent or more likely to be due to chance. Despite Figure 10 suggesting a greater decline in offending rates for court cases than for conference cases, this difference reflects such a small change in both groups that it is highly unlikely to be found in repeated tests with the same kinds of samples.

#### **Why Did Results Differ Across Experiments? Baseline Differences and Similarities at Random Assignment**

The primary hypothesis about why results differed across experiments is that restorative justice affects offenders charged with different kinds of offences differently. Put another way, the dynamics of each type of offence may create a different emotional climate and basis for legitimacy of legal intervention using court or conference processes. Yet as a competing explanation for these stark differences across experiments, the following section explores the

---

<sup>5</sup> In the accompanying figures, juvenile personal property offending is referred to as 'JPP'.

thesis that the differences were due to the pre-existing differences in the characteristics of offenders in each experiment.

### Differences Between Experiments

This section describes the findings in Figures 11-13, which indicate differences across experiments that are unlikely to be due to chance.

Figure 11 shows that the violence experiment differed from the others in that a slightly higher proportion of offenders in the conference group had smoked marijuana before RISE than we found in the court group. This reversed the ratio found in the other experiments. It seems hard to construct a theory that drug use would interact with restorative justice, but the difference is nonetheless unlikely to be due to chance.

Figure 12 shows that the violence experiment differed from the others in the ratio of males to females between court and conference groups. The conference group in the violence experiment was much more male than the court group—by a relative difference of almost 50 percent. This difference may have been a bias against showing the effectiveness of diversionary conferences, since males generally have higher rates of offending than females—although it is not clear that females have any lower rates of repeat offending once they have been apprehended for a violent offence. Whatever its effect on the analysis, the difference in reoffending between the violence experiment and the three others that failed to find beneficial effects of conferences is highly unlikely to have been due to chance.

Figure 13 shows that there is a difference in pre-RISE binge drinking by across the four experiments. In this case, however, there is no clear correlation between the violence experiment and the other three experiments. There is no way to explain the different effects of conferences with the distribution shown here.

### Similarities Across Experiments

The remaining Figures display data for all four experiments that fail to show any differences that are not due to chance. While in 22 significance tests, we would expect one difference to be significant at the 5 percent likelihood level, there were three such differences reported in the last section. The other 19 tests are reported here, none of which found any

plausible explanations for the different effects of restorative justice for violence as compared to the other types of offences.

Figures 14, 14a, 15, and 15a show the distribution of offence types for the two experiments taking multiple types of offences; drink driving and shoplifting (security apprehensions) were only able to take one offence type each. Where the offence types were mixed, there was no difference in distributions by type between court and conference.

Figure 16 suggests a reactive measurement, in which people who had been through conference seem less likely to admit prior property crime than those who had gone to court. This consistent pattern (with a question not asked of drinking drivers) cannot explain why violence offenders reacted so well to restorative justice.

Figure 17, in contrast, appears to fit the pattern of different results across experiments: there are higher rates of prior violent crime in the conference cases for the violence experiment than in the other two. But the small size of these differences makes them likely to be due to chance. The higher base rates of prior violence in the property crime experiments than in the violence experiment is interesting, but might also be a reactive measure in relation to the type of offence. That is, persons charged with violence might be less likely to admit to prior violence than persons charged with property crime.

Figures 18-19 show no statistically discernible differences across experiments in percentage of offenders unemployed, or who were Aboriginal or Torres Strait Islander.

### Differences Within Experiments

The remaining figures examine selected baseline characteristics within experiments, finding no differences in the following by experiment by Figure number:

- 20. Prior offences per case by assigned treatment (violence)
- 21. Length of criminal career (violence)
- 22. Time at risk post-RISE (violence)
- 23. Prior offences (Drink driving)
- 24. Prior drink driving offences (Drink driving)
- 25. Length of criminal career (Drink driving)
- 26. Time at risk post-RISE (Drink driving)

27. Prior offences per case (shoplifting-security)
28. Length of criminal career (shoplifting-security)
29. Time at risk post-RISE (shoplifting-security)
30. Prior offences per case (personal property)
31. Length of criminal career (personal property)
32. Time at risk post-RISE (personal property)

## **Conclusion**

Across the four experiments that make up RISE, very different results have emerged for the different offence categories. In the Youth Violence experiment, those offenders who were assigned to conference subsequently offended at substantially lower levels—38 fewer offences per year per 100 offenders – than did the offenders assigned to court. This was not true for any of the other experiments. For Drink Driving offenders, a very small increase in detected reoffending was found for the conferenced offenders, relative to court—about 4 offences per offender per year per 100 offenders.

Our next task in the analysis of the RISE data is to explore the reasons for these differences. We know that consistently across all experiments, conference-assigned offenders reported that their treatment was more procedurally fair than did court-assigned offenders. However, this translated into higher levels of compliance with the law only in one out of four offence categories, at least in the one-year before and after standardised comparison periods. Just why restorative justice worked for this sample of offenders and not for others is a major question to be answered by RISE and other research.

The methodological conclusion of this five-year project is that multiple randomised trials are advisable for testing a new method of justice. The design of RISE anticipated the possibility of detecting different effects for different types of offences. That still remains the most plausible account of the differences reported here--as opposed to differences by type of offender background. Further research should continue to break out different offence types for testing, rather than lumping diverse offence types together.

The substantive conclusion of RISE is that restorative justice can work, and can even reduce crime by violent offenders. But there is no guarantee that it will work for all offence

types. Caution and more research are needed before rapid expansion of any new approach to treating crime. Less caution is needed, however, in testing restorative justice on more serious types of violent offences. The findings in this report provide firm ground for repeating the violence experiment in many other venues and with more refined types of violent offences, including robbery, assault, and grievous bodily harm.

**References**

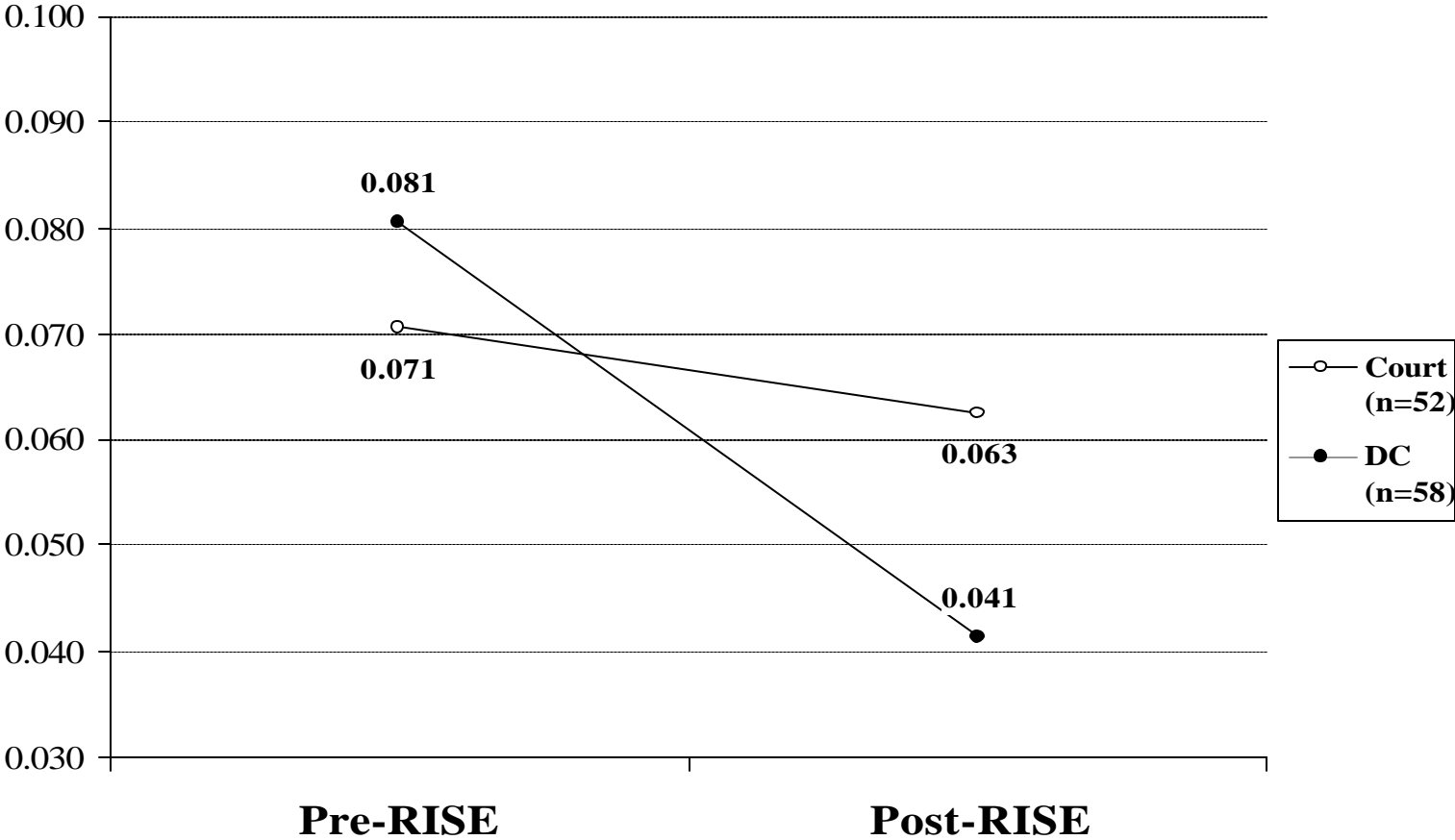
Barnes G 1999, *Procedural Justice in Two Contexts: Testing the Fairness of Diversionary Conferencing*, unpublished PhD thesis, University of Maryland.

Braithwaite J 1989, *Crime Shame and Reintegration*, Cambridge University Press, Cambridge.

Sherman L W 1993, 'Defiance, Deterrence and Irrelevance: A Theory of the Criminal Sanction', *Journal of Research in Crime and Delinquency*, 30 (4): 445-473.

Tyler T 1990, *Why People Obey the Law*, Yale University Press, New Haven.

Figure 1: Change in 1 Year Monthly Offending Rate (Any Offence) by RISE Assigned Treatment (Youth Violence Offenders, N = 110)

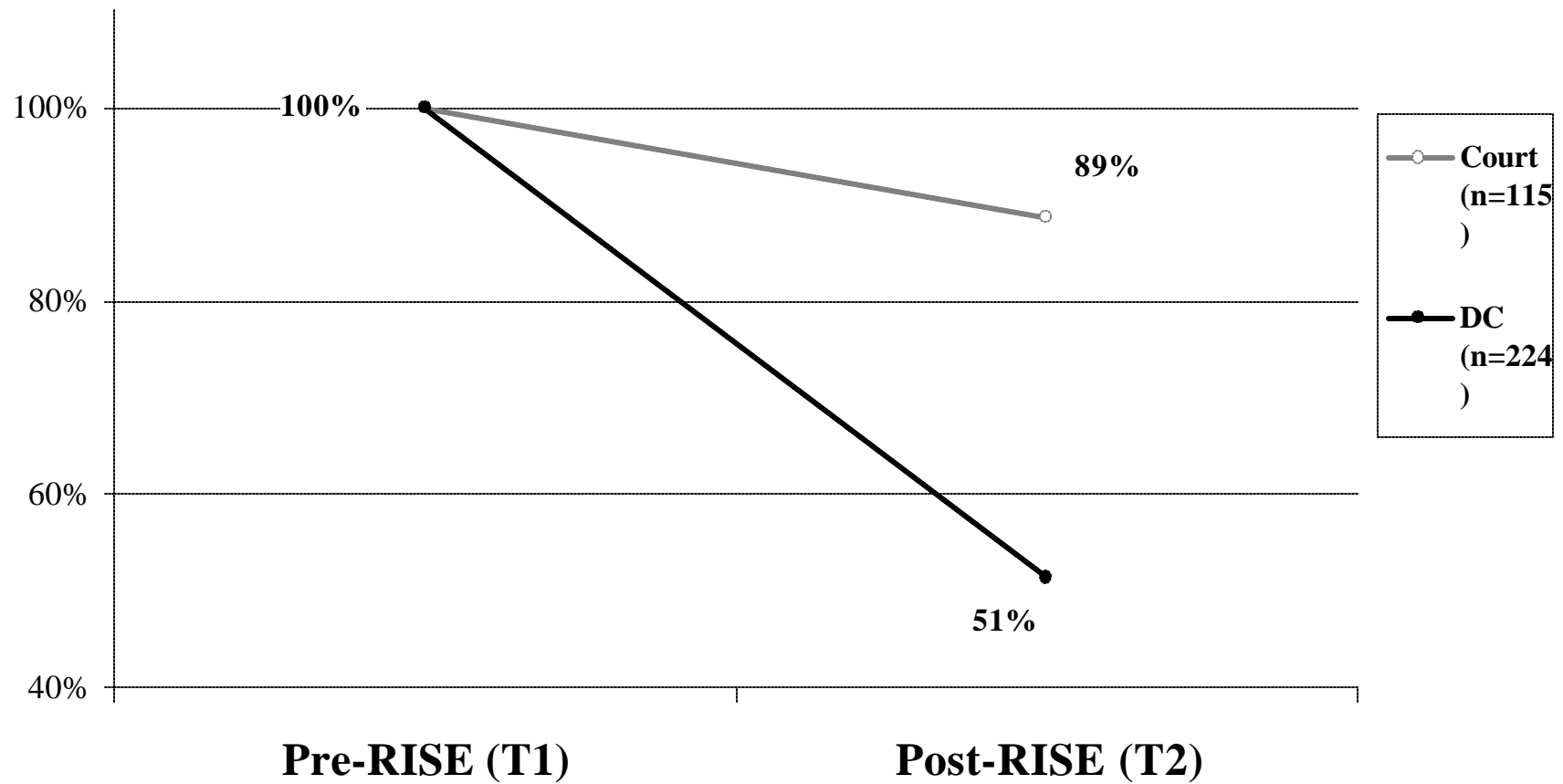


Court T1 to T2,  $t = -.499$ ,  $df = 51$ ,  $p \leq .620$

DC T1 to T2,  $t = -2.542$ ,  $df = 57$ ,  $p \leq .014$

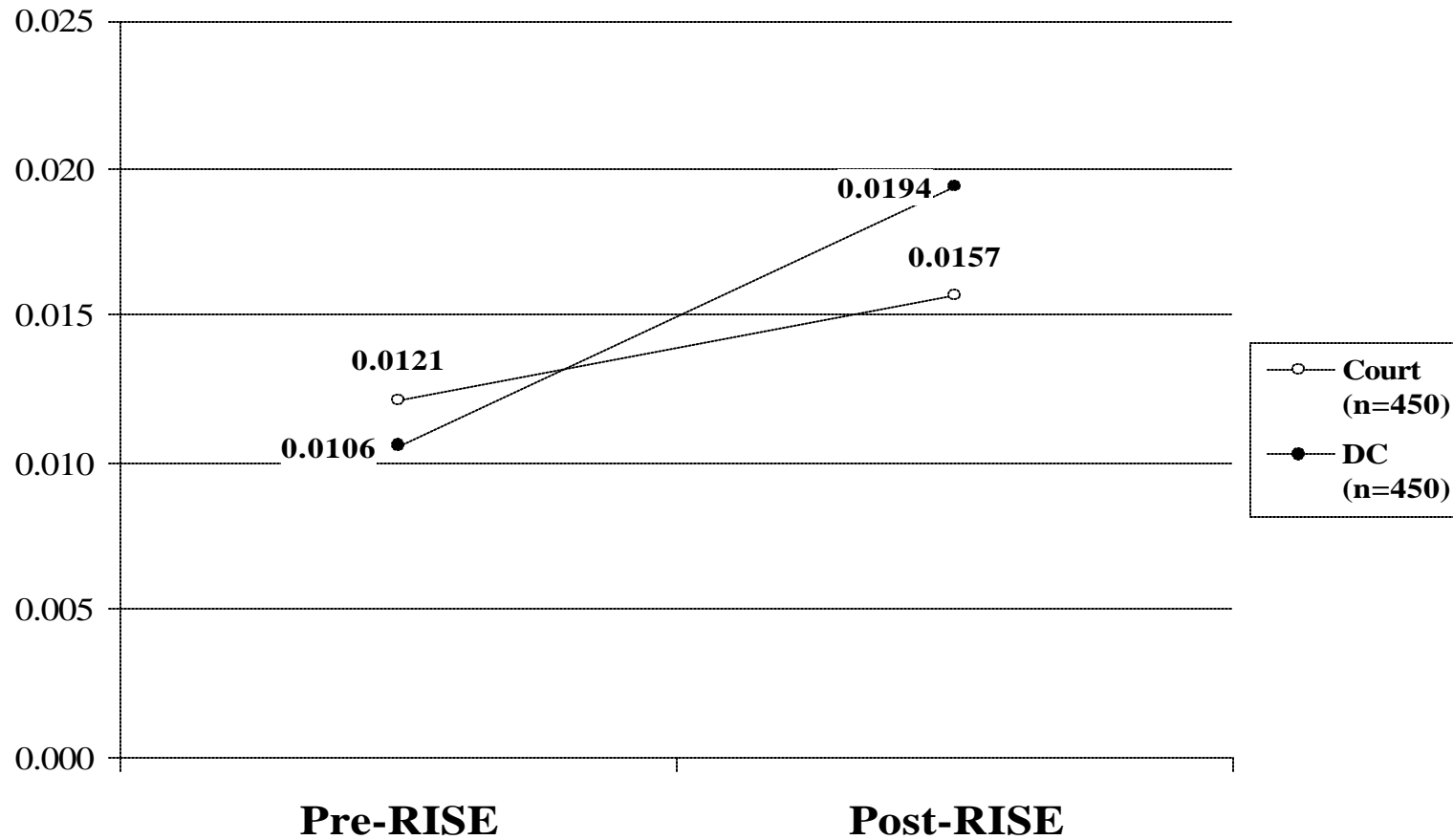
Difference of differences,  $t = 1.398$ ,  $df = 108$ ,  $p \leq .165$ , Cohen's  $d = .259$

Figure 2: Relative Change in Monthly Offending Rates 1 Year Before and After RISE by Assigned Treatment (Youth Violence Offenders, N=110)



\*Rates are for Any Offence

Figure 3: Change in 1 Year Monthly Offending Rate (Any Offence) by RISE Assigned Treatment (Drink Driving, N = 900)

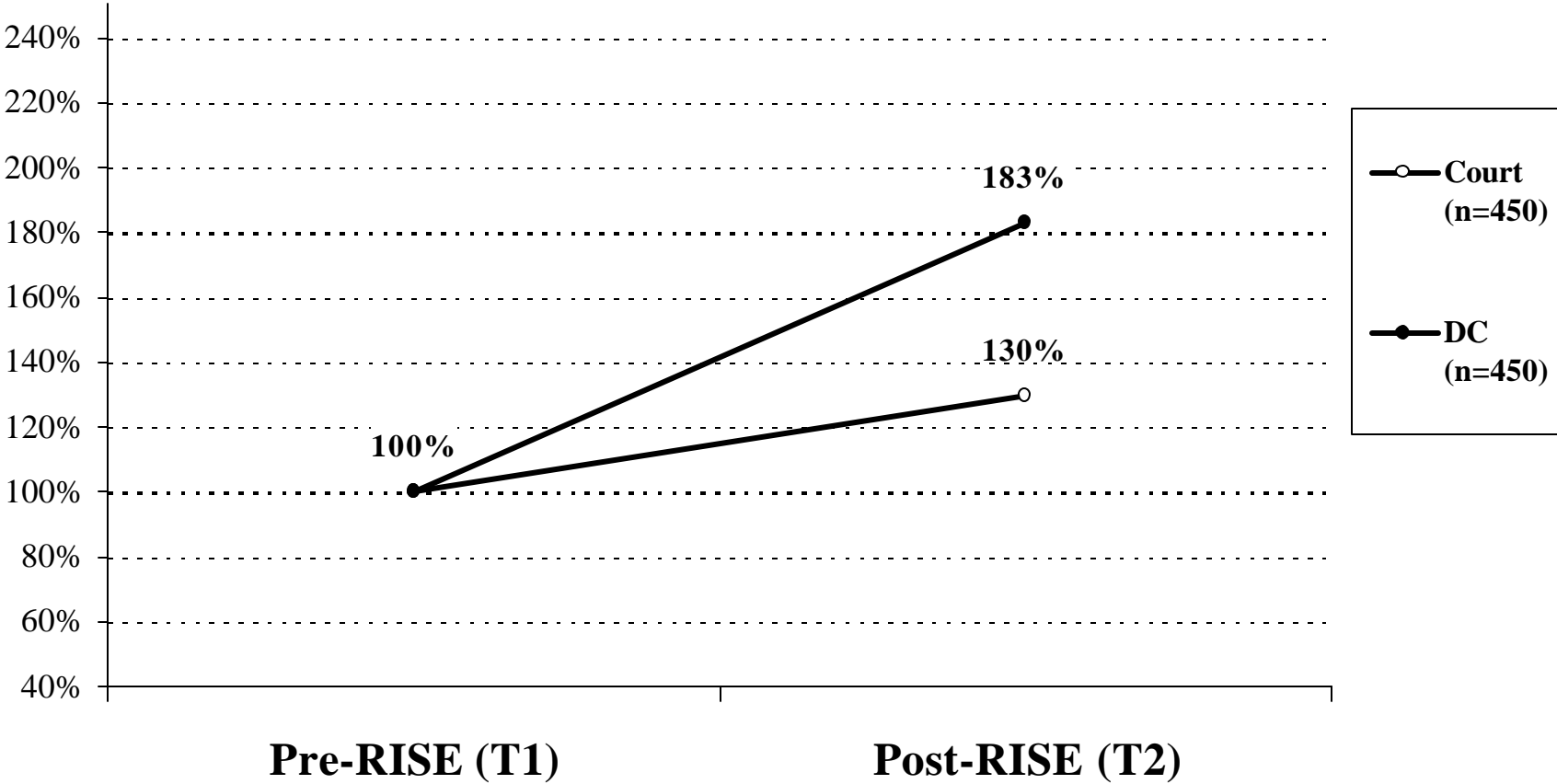


Court T1 to T2,  $t = 1.415$ ,  $df = 449$ ,  $p \leq .158$

DC T1 to T2,  $t = 3.734$ ,  $df = 449$ ,  $p \leq .000$

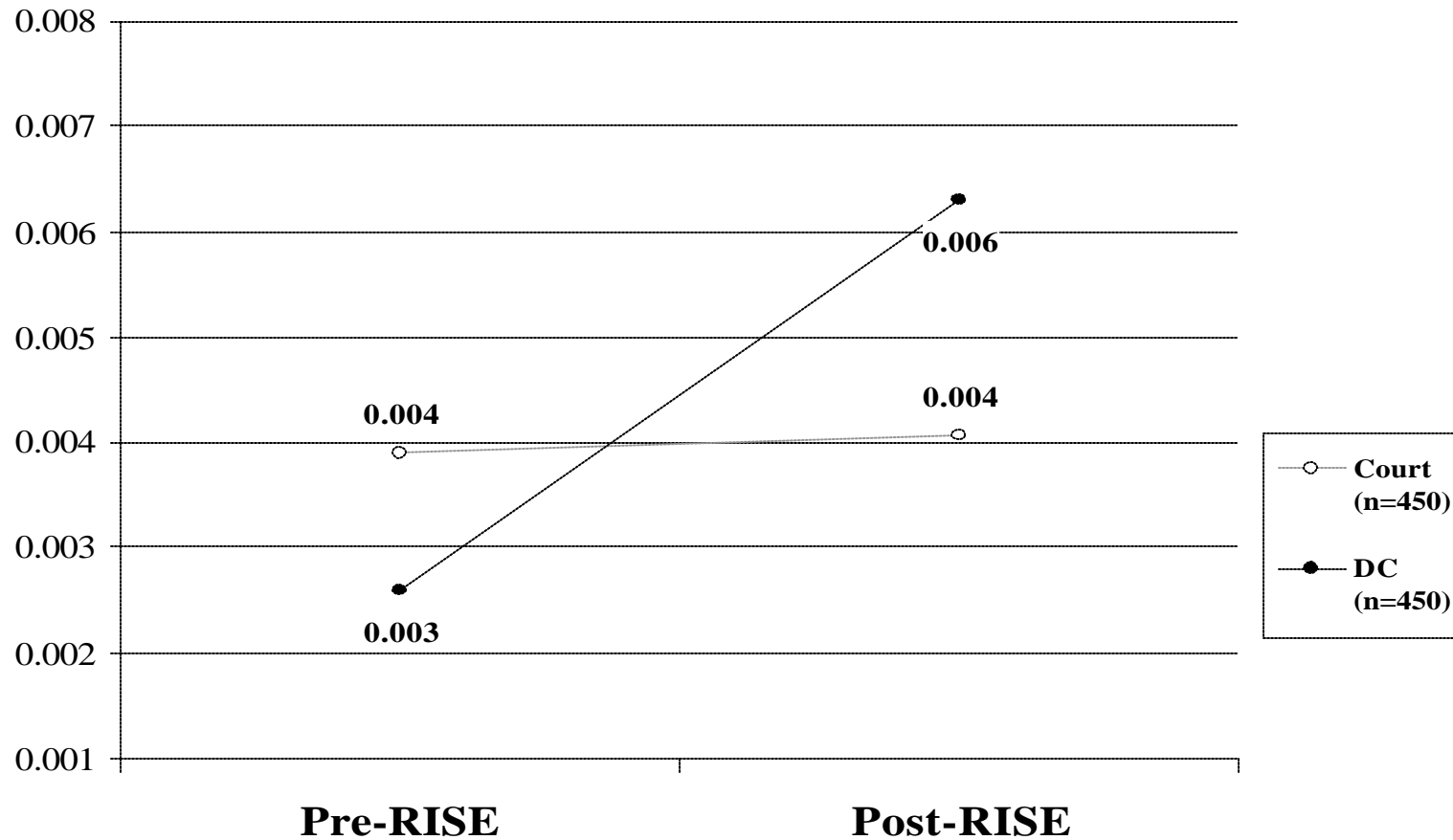
Difference of Differences,  $t = -1.504$ ,  $df = 898$ ,  $p \leq .133$ , Cohen's  $d = .100$

Figure 4: Relative Change in Monthly Offending Rates\* 1 Year Before and After RISE by Assigned Treatment (Drink Driving, N=900)



\*Rates are for Any Offence

Figure 5: Change in 1 Year Monthly Drink Driving Offending Rate by RISE Assigned Treatment (Drink Driving, N = 900)



Court T1 to T2,  $t = -.156$ ,  $df = 449$ ,  $p \leq .876$

DC T1 to T2,  $t = -2.974$ ,  $df = 449$ ,  $p \leq .003$

Difference of Differences,  $t = -2.045$ ,  $df = 895.956$ ,  $p \leq .041$ , Cohen's  $d = .136$

Figure 6: Relative Change in Drink Driving Monthly Offending Rates\* 1 Year Before and After RISE by Assigned Treatment (Drink Driving, N=900)

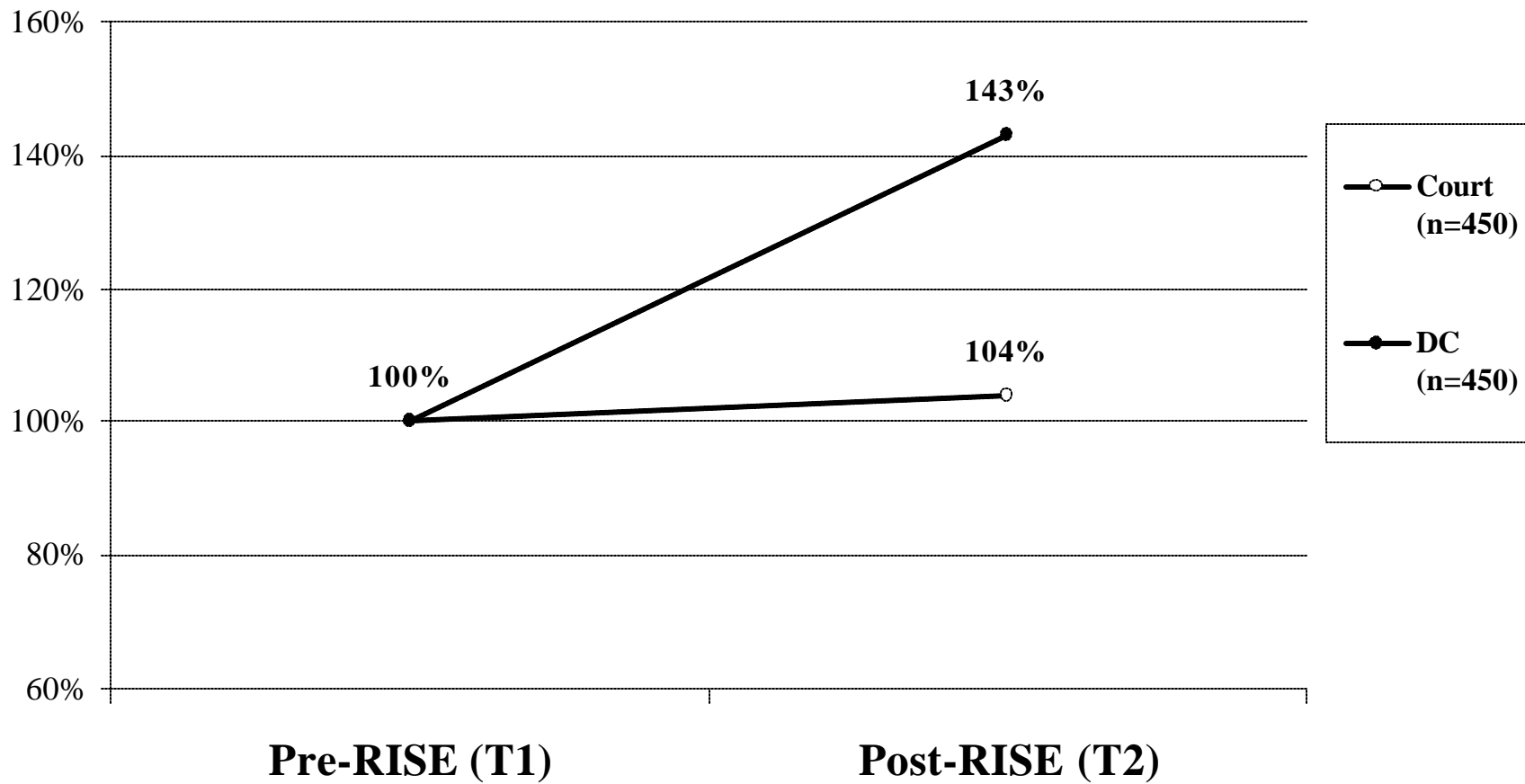
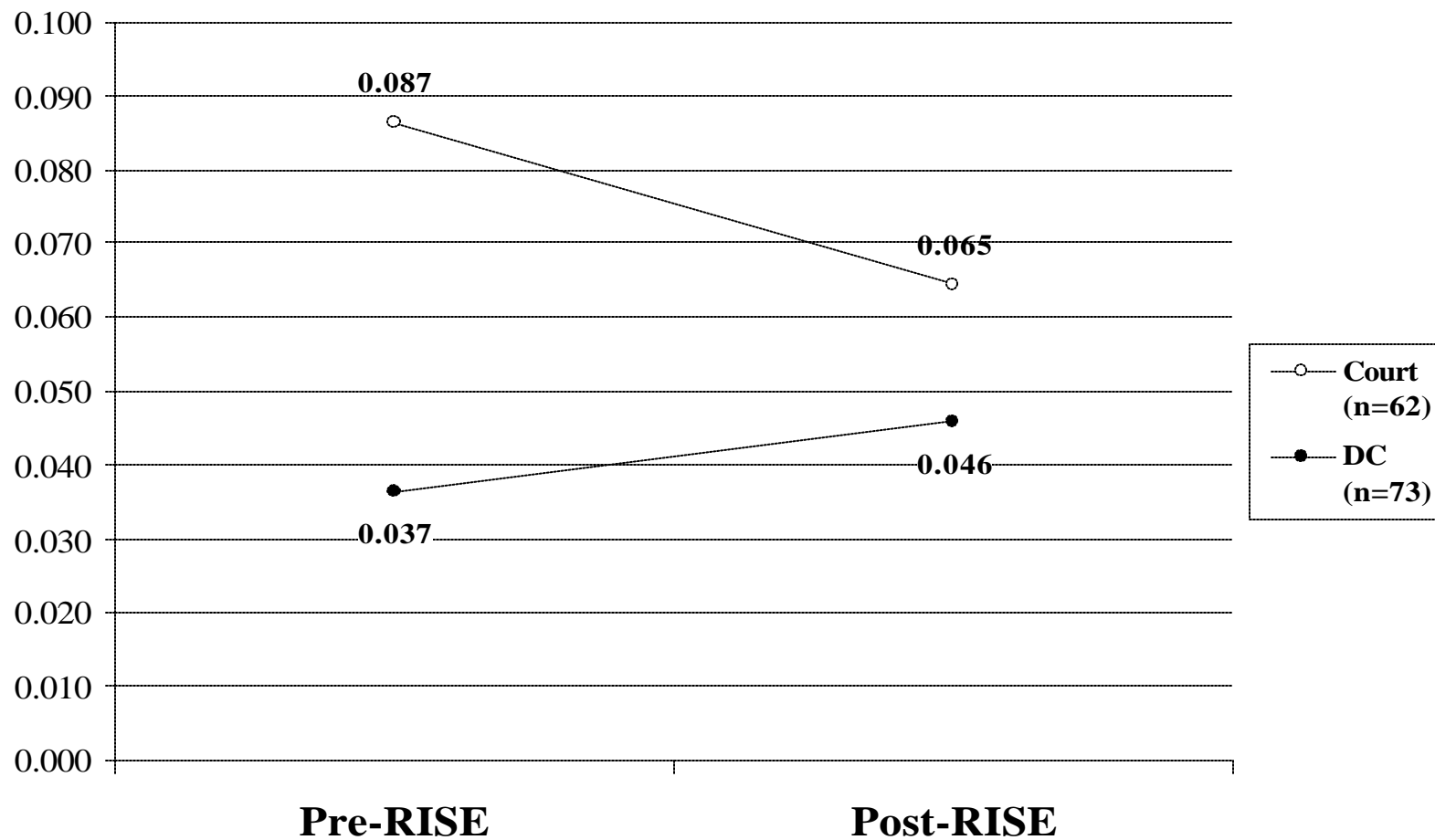


Figure 7: Change in 1 Year Monthly Offending Rate (Any Offence) by RISE Assigned Treatment (Juvenile Shoplifting-Security Offenders, N = 135)

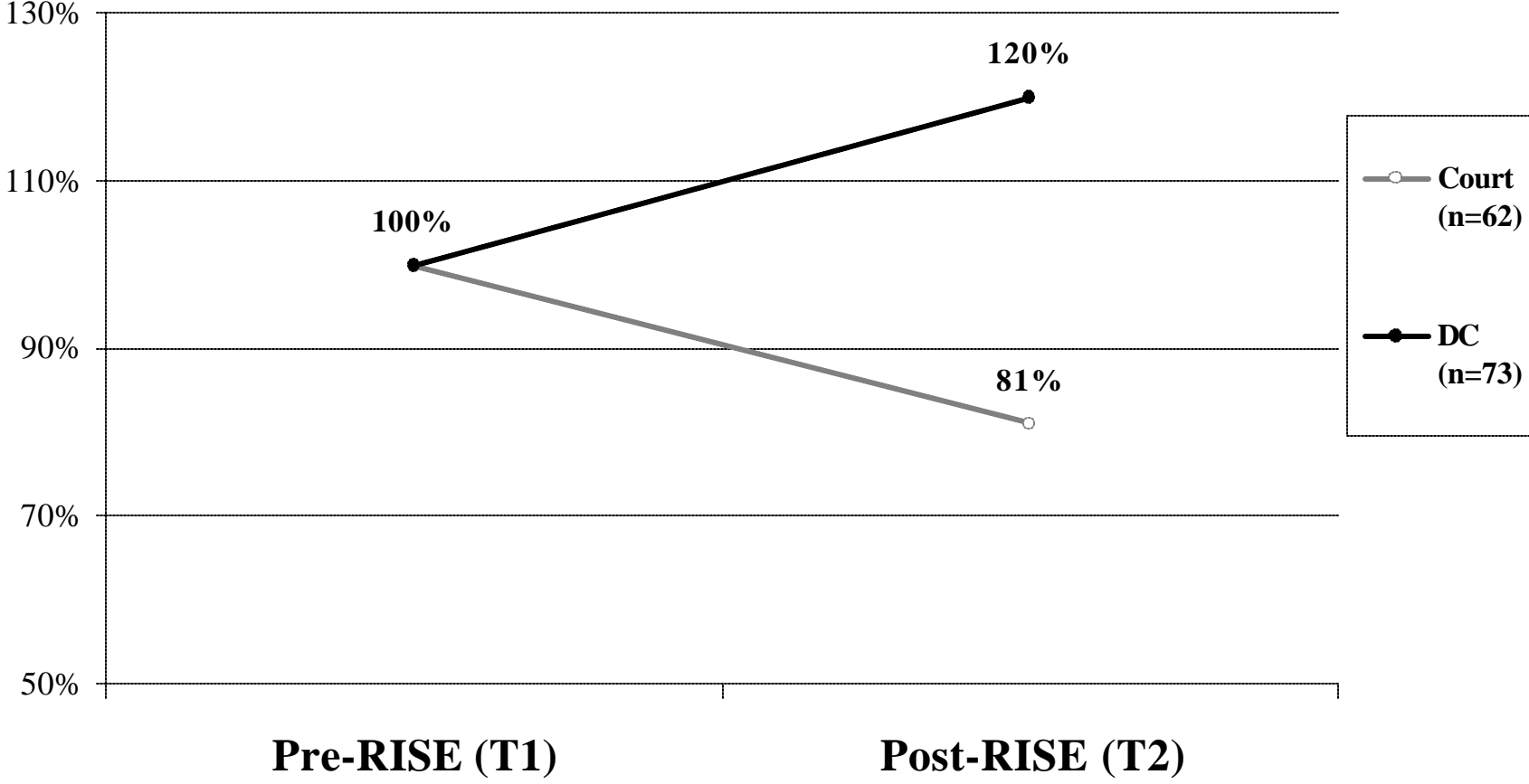


Court T1 to T2,  $t = -.814$ ,  $df = 61$ ,  $p \leq .419$

DC T1 to T2,  $t = .796$ ,  $df = 72$ ,  $p \leq .428$

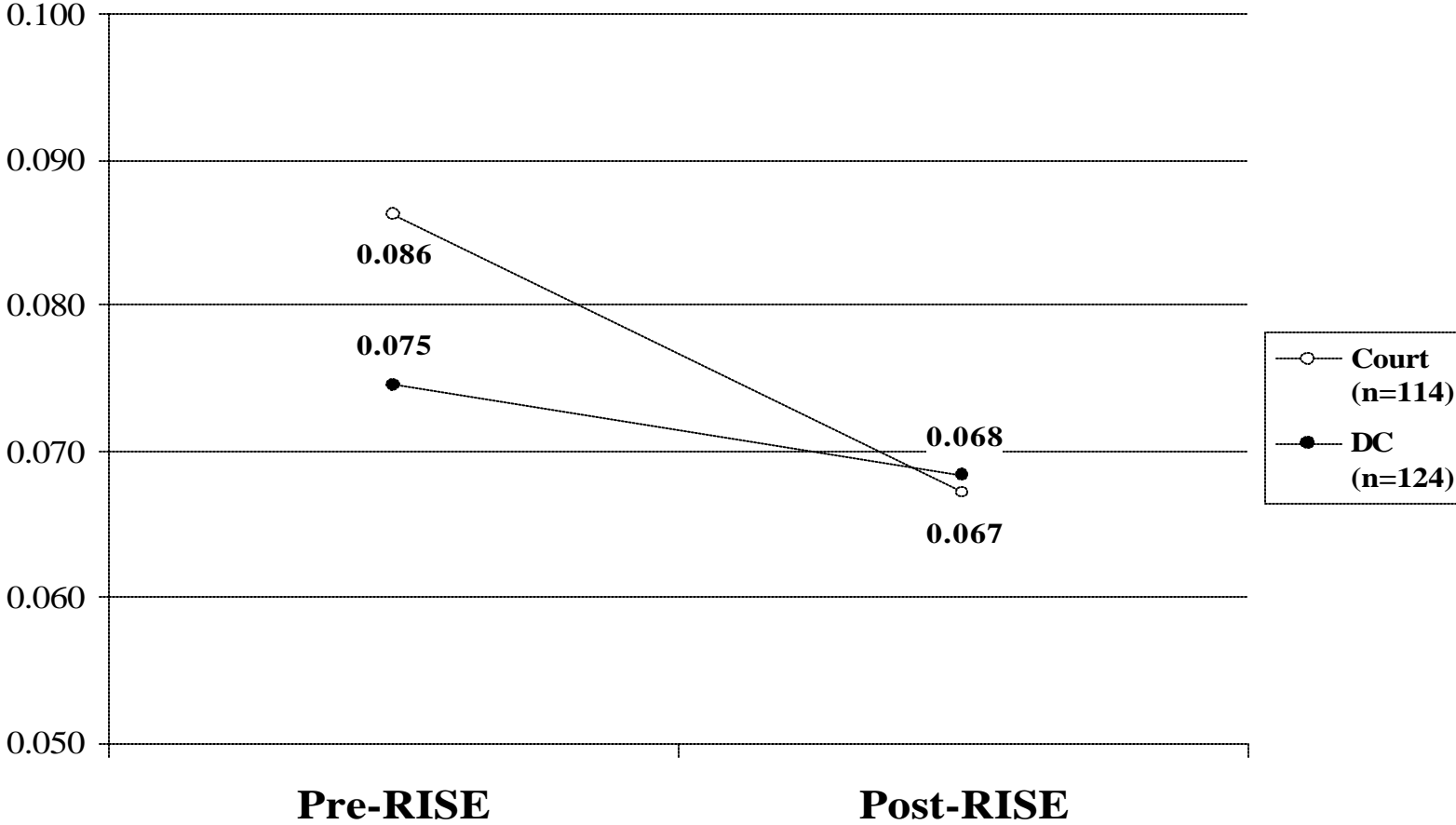
\* Difference of differences,  $t = -1.095$ ,  $df = 133$ ,  $p \leq .256$ , Cohen's  $d = .189$

Figure 8: Relative Change in Monthly Offending Rates\* 1 Year Before and After RISE by Assigned Treatment (Juvenile Shoplifting-Security Offenders, N=135)



\*Rates are for Any Offence

Figure 9: Change in 1 Year Monthly Offending Rate (Any Offence) by RISE Assigned Treatment (Juvenile Personal Property Offenders, N = 238)

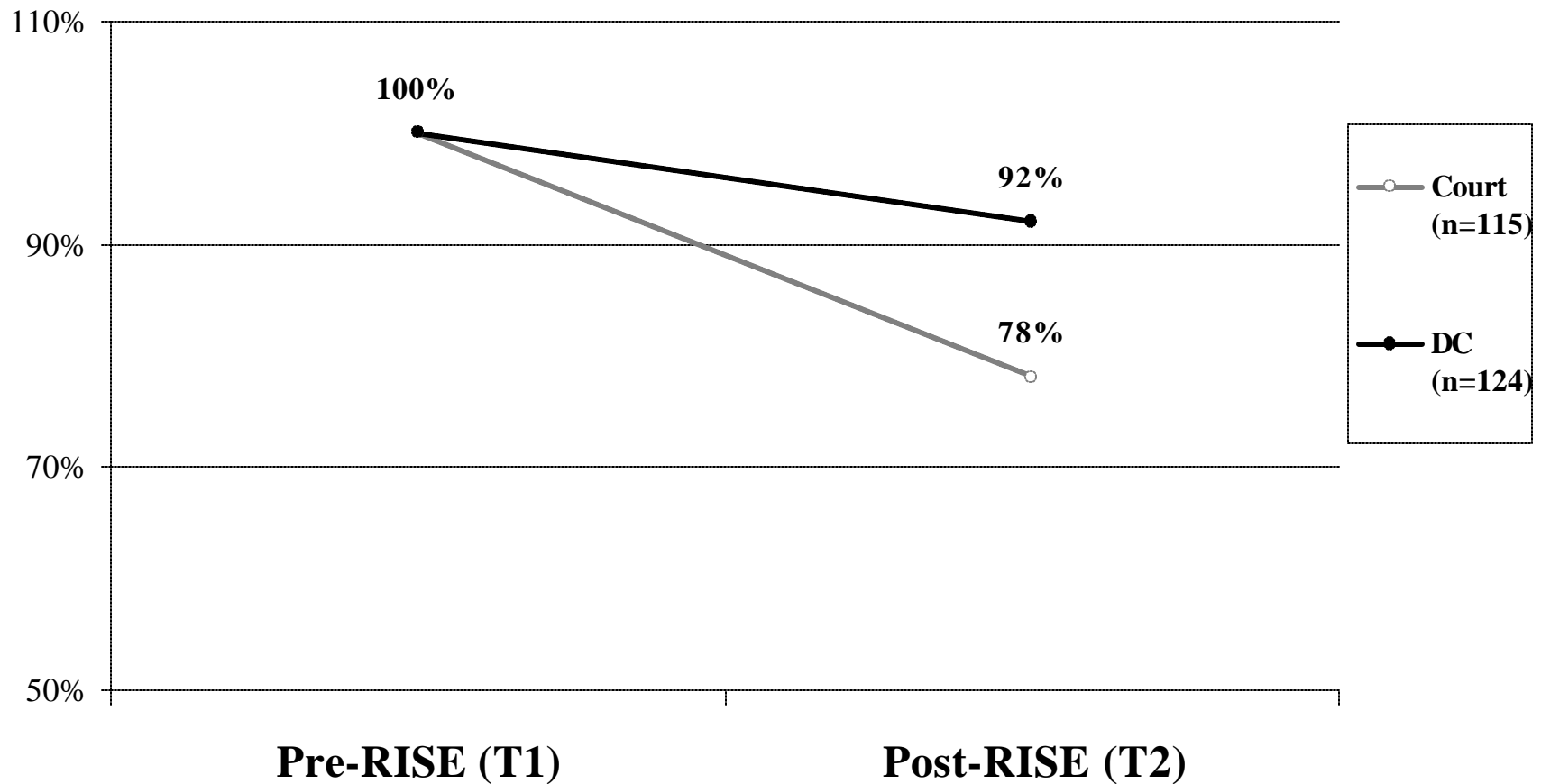


Court T1 to T2,  $t = -1.138$ ,  $df = 113$ ,  $p \leq .257$

DC T1 to T2,  $t = -.429$ ,  $df = 123$ ,  $p \leq .669$

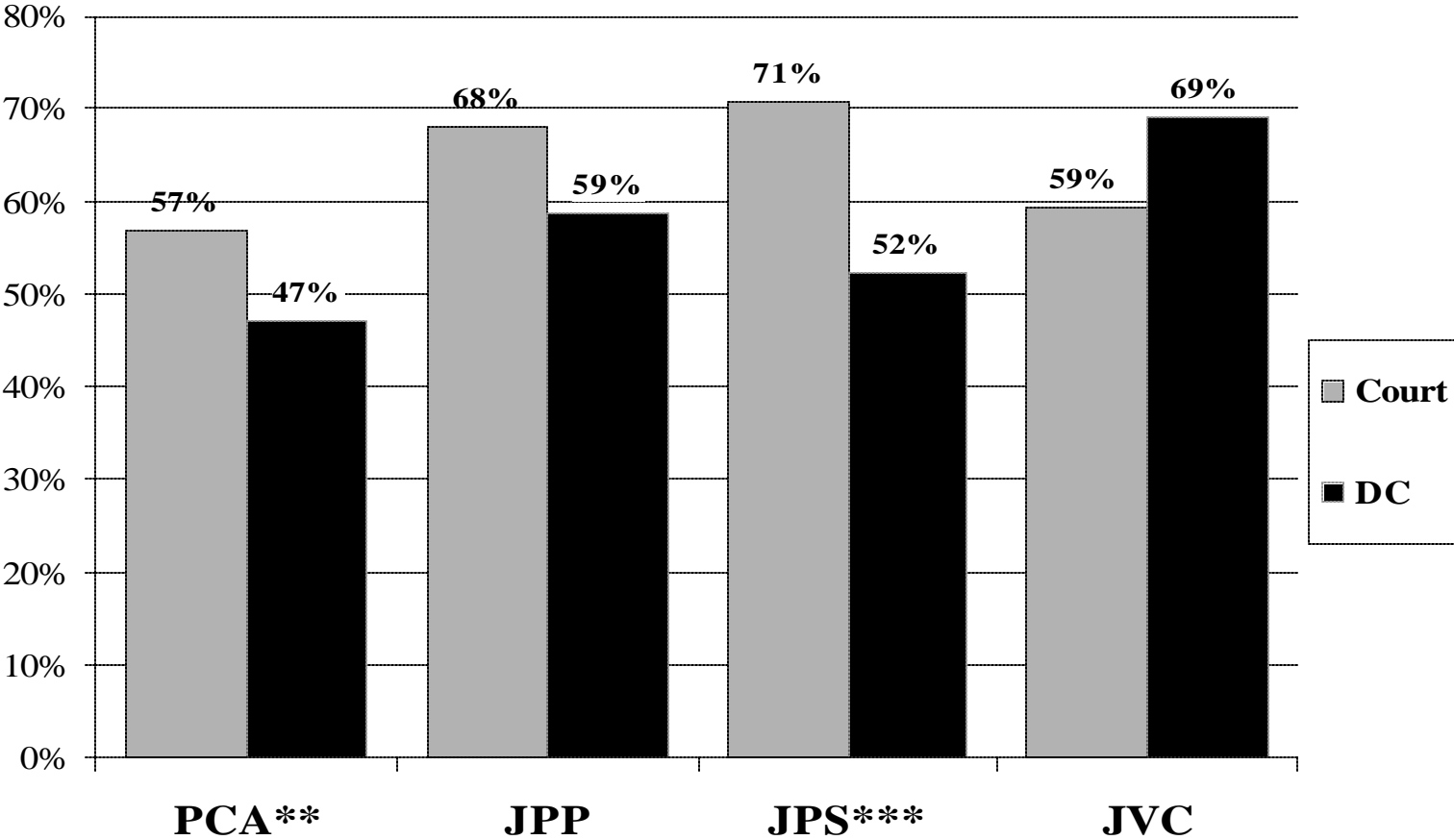
\*Difference of differences,  $t = -.573$ ,  $df = 236$ ,  $p \leq .567$ , Cohen's  $d = .07$

Figure 10: Relative Change in Monthly Offending Rates\*  
1 Year Before and After RISE by Assigned Treatment  
(Juvenile Personal Property Offenders, N=239)



\*Rates are for Any Offence

Figure 11: Percent Self-Reported Marijuana Use by Assigned Treatment and Experiment\*

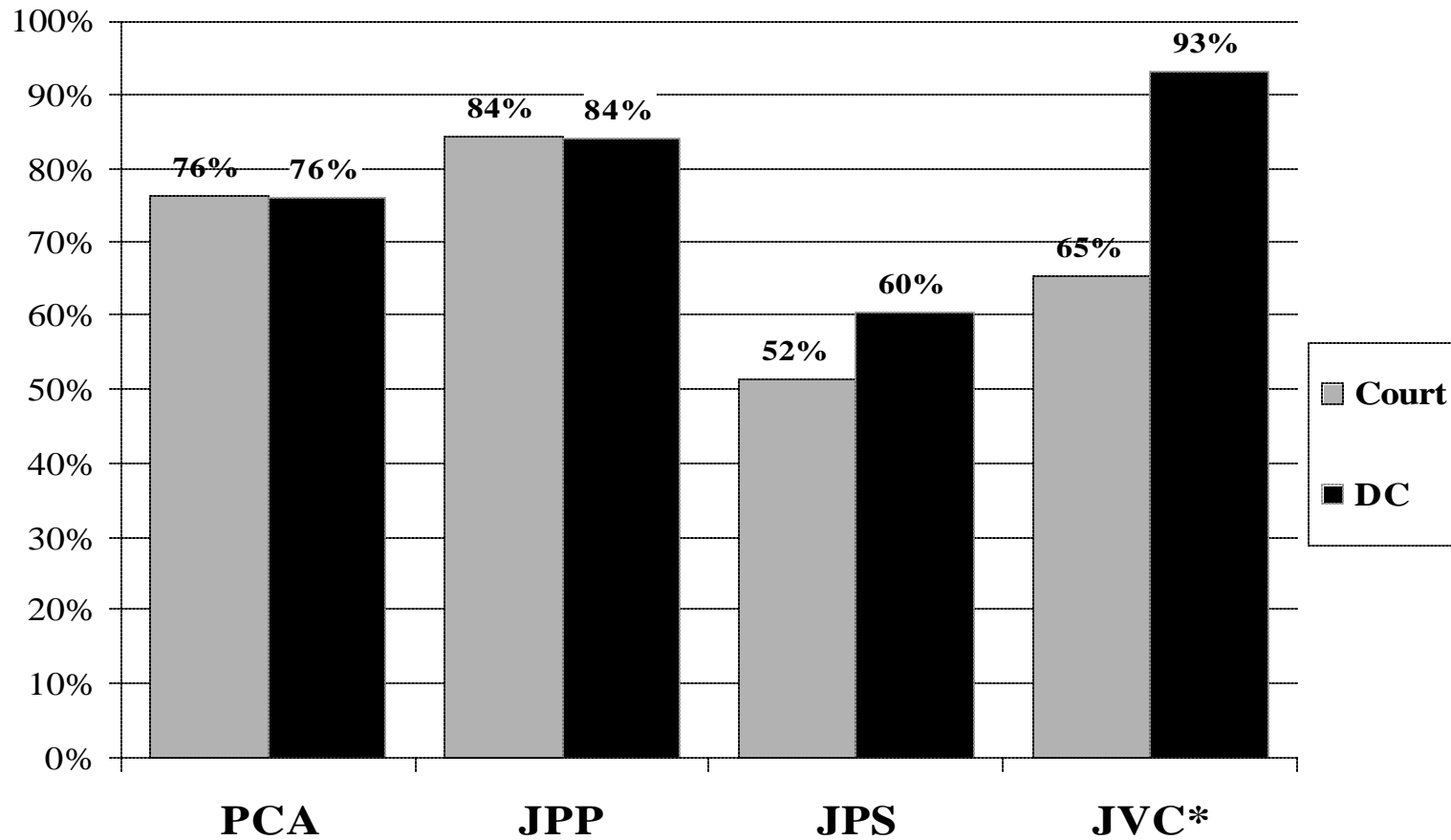


\*Averages are based on any use in previous year to RISE

\*\*PCA difference  $\chi^2 = 6.261$ ,  $df = 1$ ,  $p \leq .012$ , Somer's  $d = -.097$ ,  $p \leq .012$

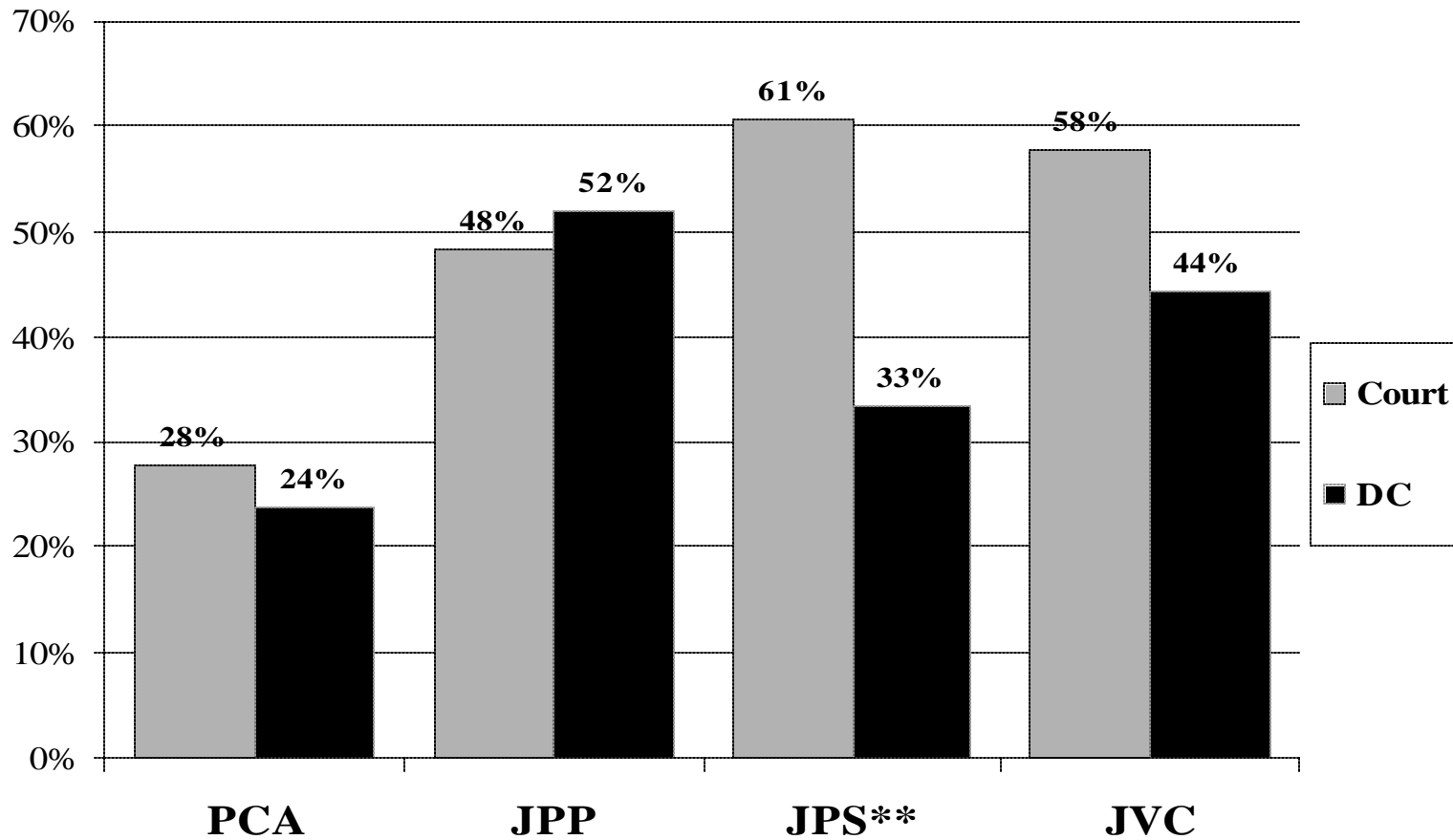
\*\*\*JPS difference Fisher's Exact Test,  $p \leq .064$ , Somer's  $d = -.185$ ,  $p \leq .075$

Figure 12: Percent Male by Assigned Treatment and RISE Experiment



\*JVC Male % is significant, Fisher's Exact Test,  $p \leq 0.0003$ , Somer's  $d = .277$ ,  $p \leq .0001$

Figure 13: Percent Binge Drinking Offenders by Assigned Treatment and Experiment\*



\*Binge drinking is defined as +6 alcoholic drinks per drinking session

\*\*JPS difference is significant using Fisher's Exact Text,  $p \leq .042$ , Somer's  $d = -.273$ ,  $p \leq .029$

Figure 14: Frequency of Presenting Youth Violence Cases to RISE (N = 89)

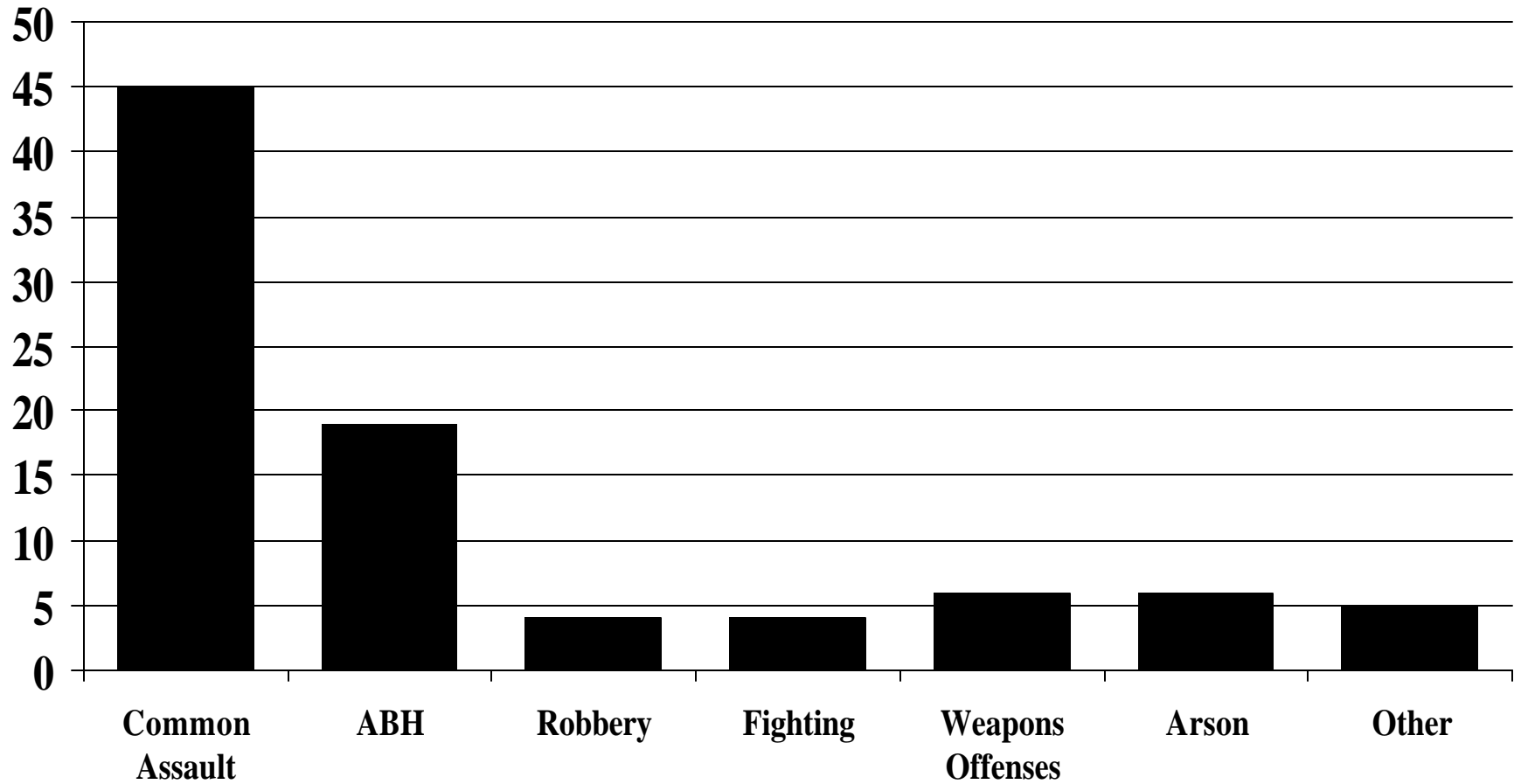


Figure 14a: Frequency of Presenting Youth Violence Cases to RISE by Assigned Treatment (N = 89)

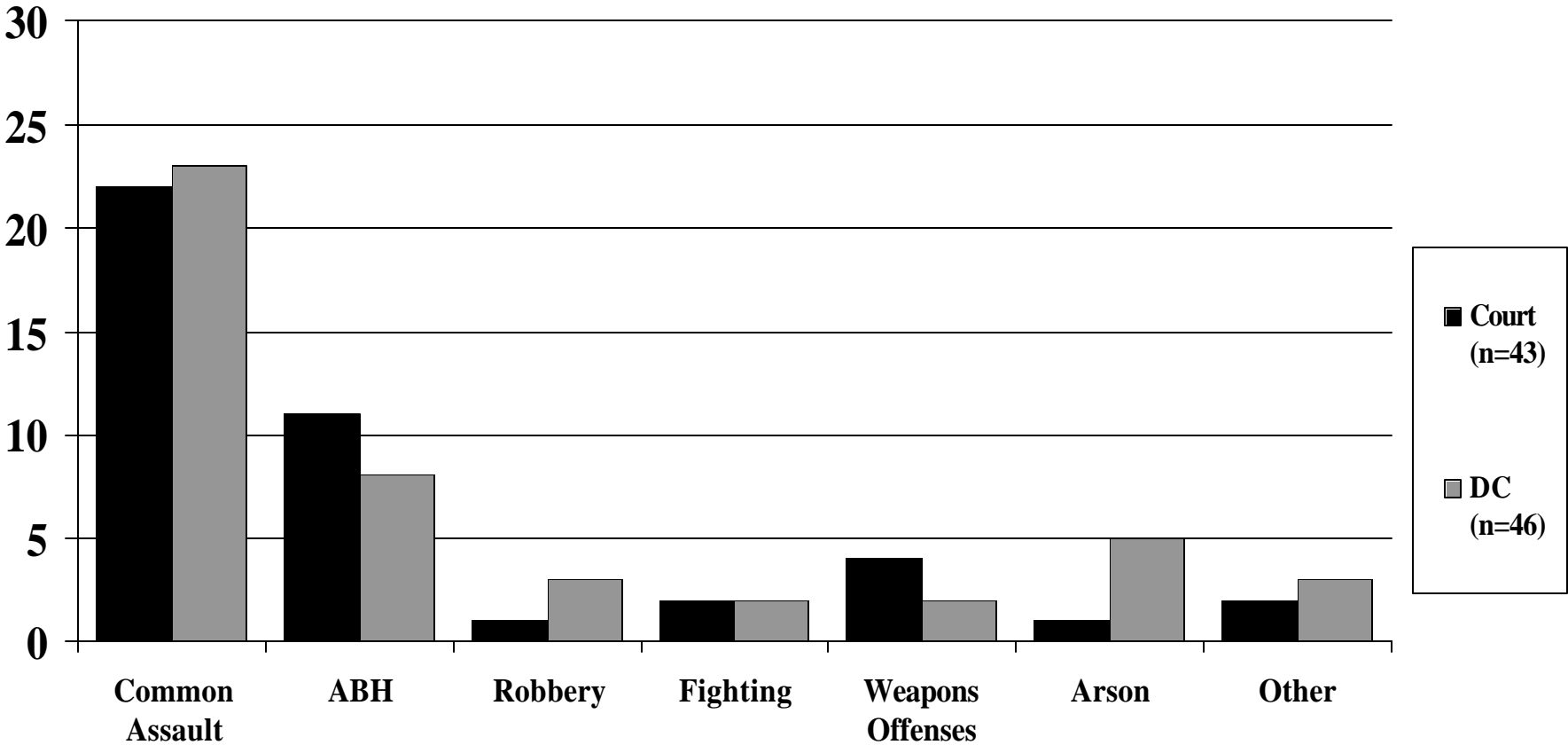


Figure 15: Frequency of Presenting Juvenile Personal Property Cases to RISE (N = 162)

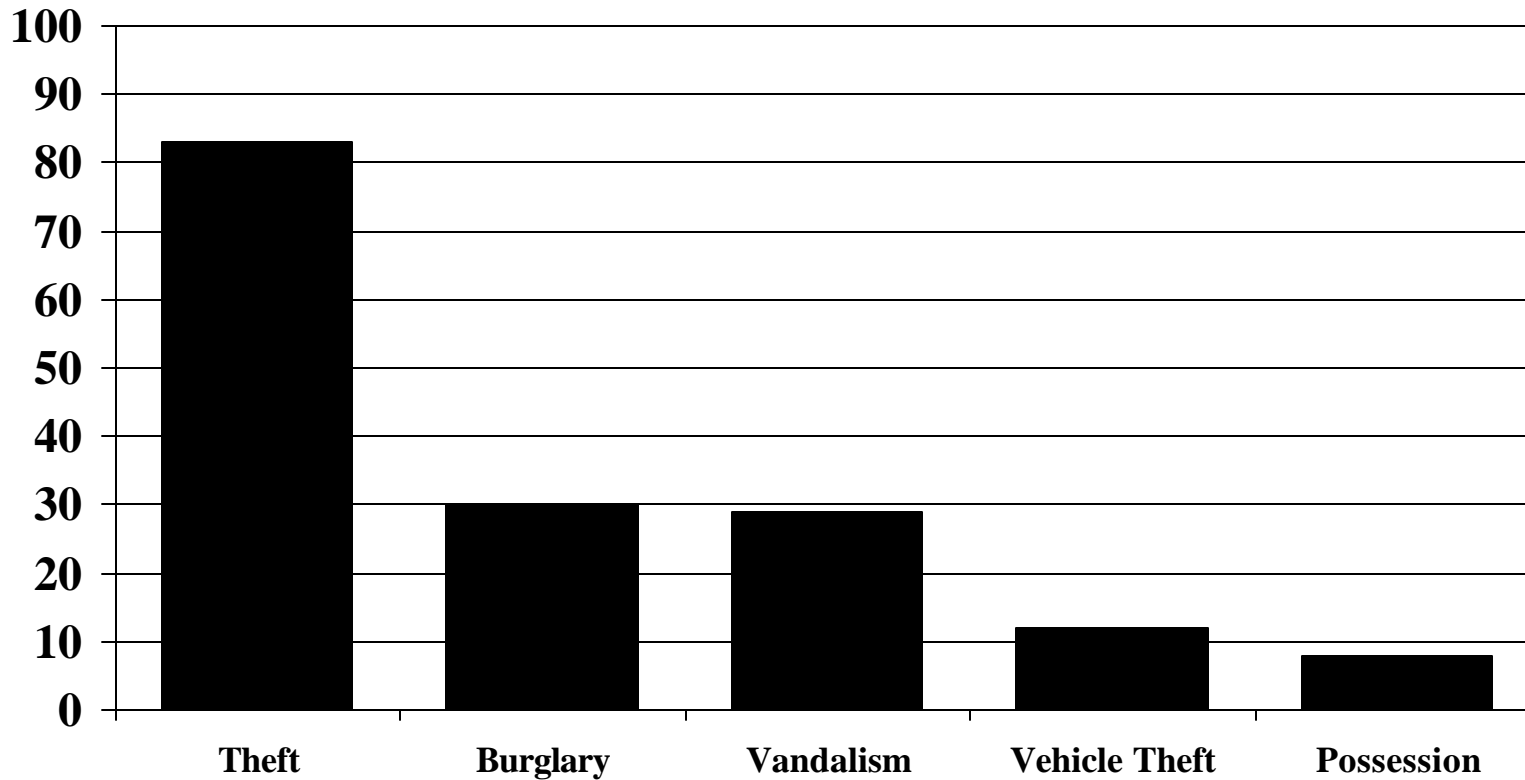


Figure 15a: Frequency of Presenting Personal Property Cases to RISE (N = 162)

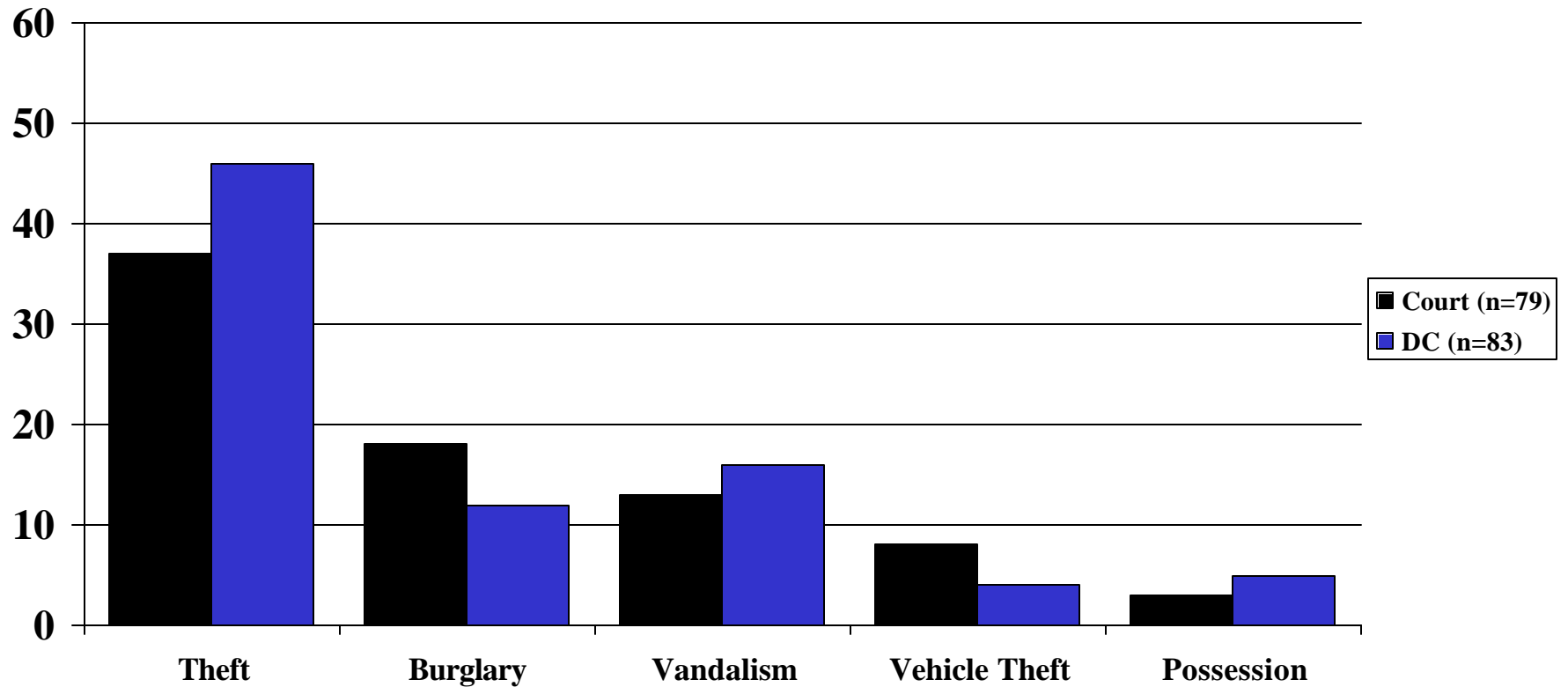
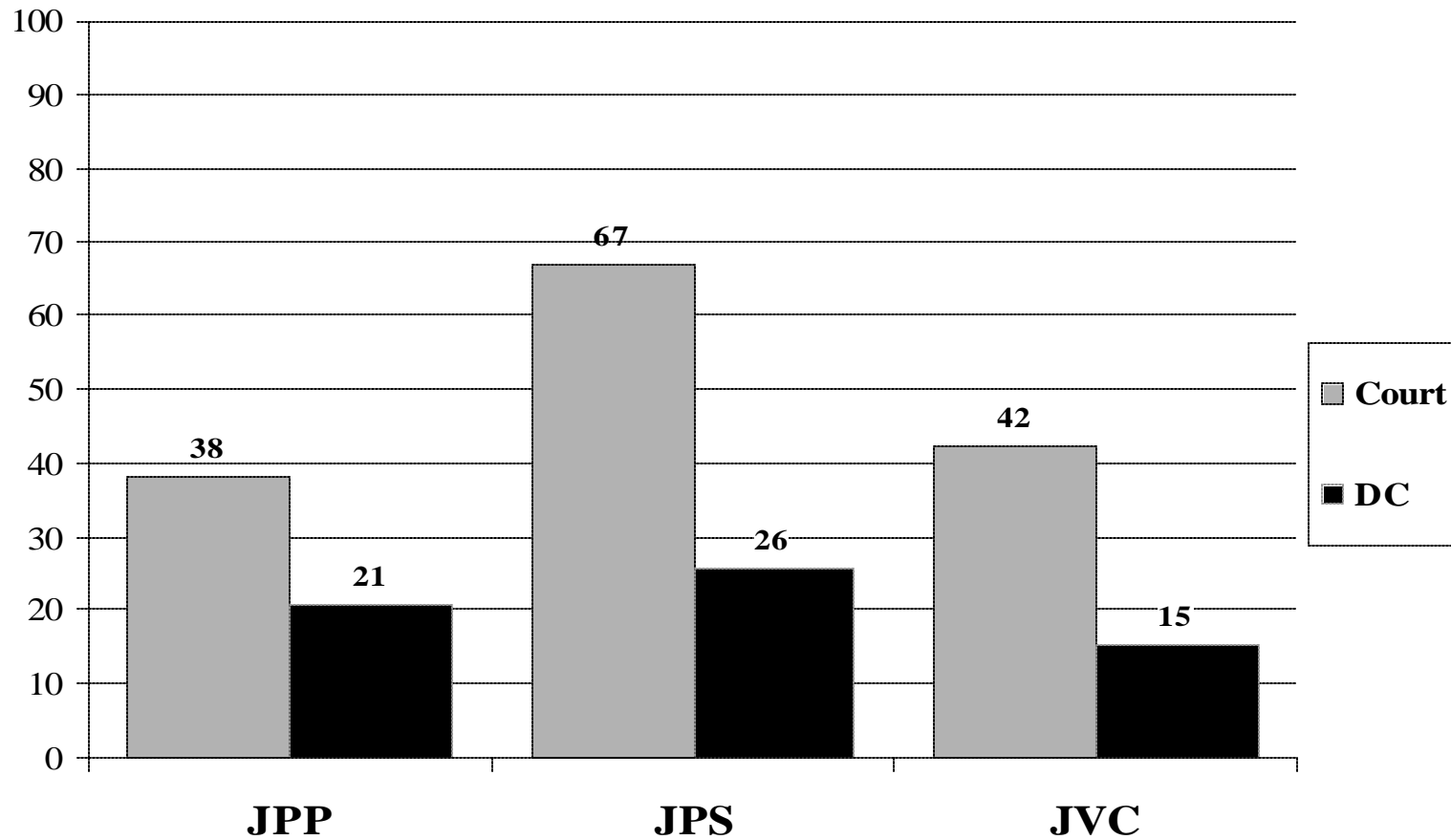


Figure 16: Percent Self-Reported Property Crime by Assigned Treatment and Experiment

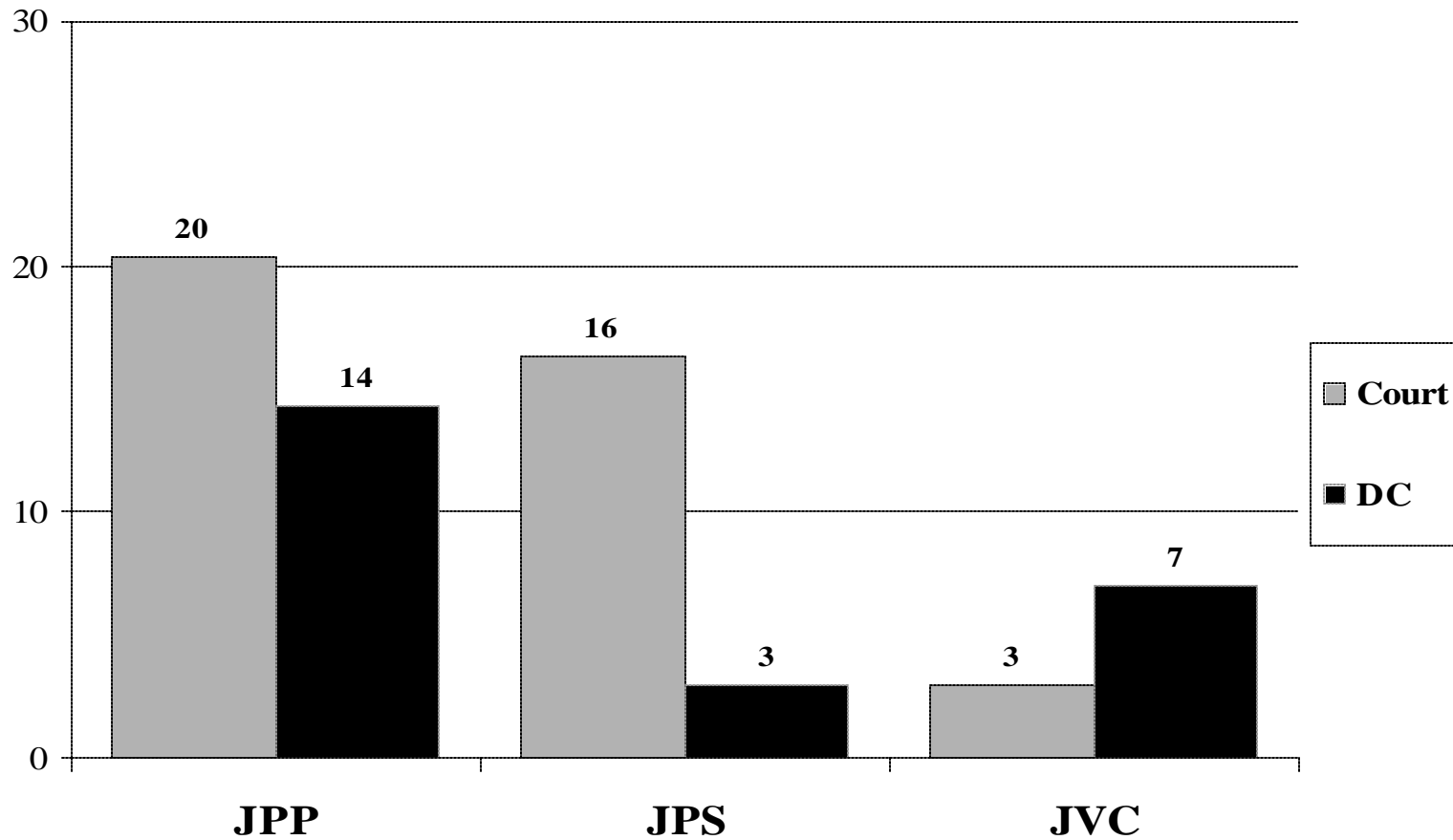


JPP -  $t = 1.318$ ,  $df = 112$ ,  $p \leq .19$ , Cohen's  $d = .233$

JPS -  $t = 1.361$ ,  $df = 47$ ,  $p \leq .18$ , Cohen's  $d = .303$

JVC -  $t = 1.328$ ,  $df = 19$ ,  $p \leq .20$ , Cohen's  $d = .458$

Figure 17: Percent Self-Reported Violent Crime by Assigned Treatment and Experiment

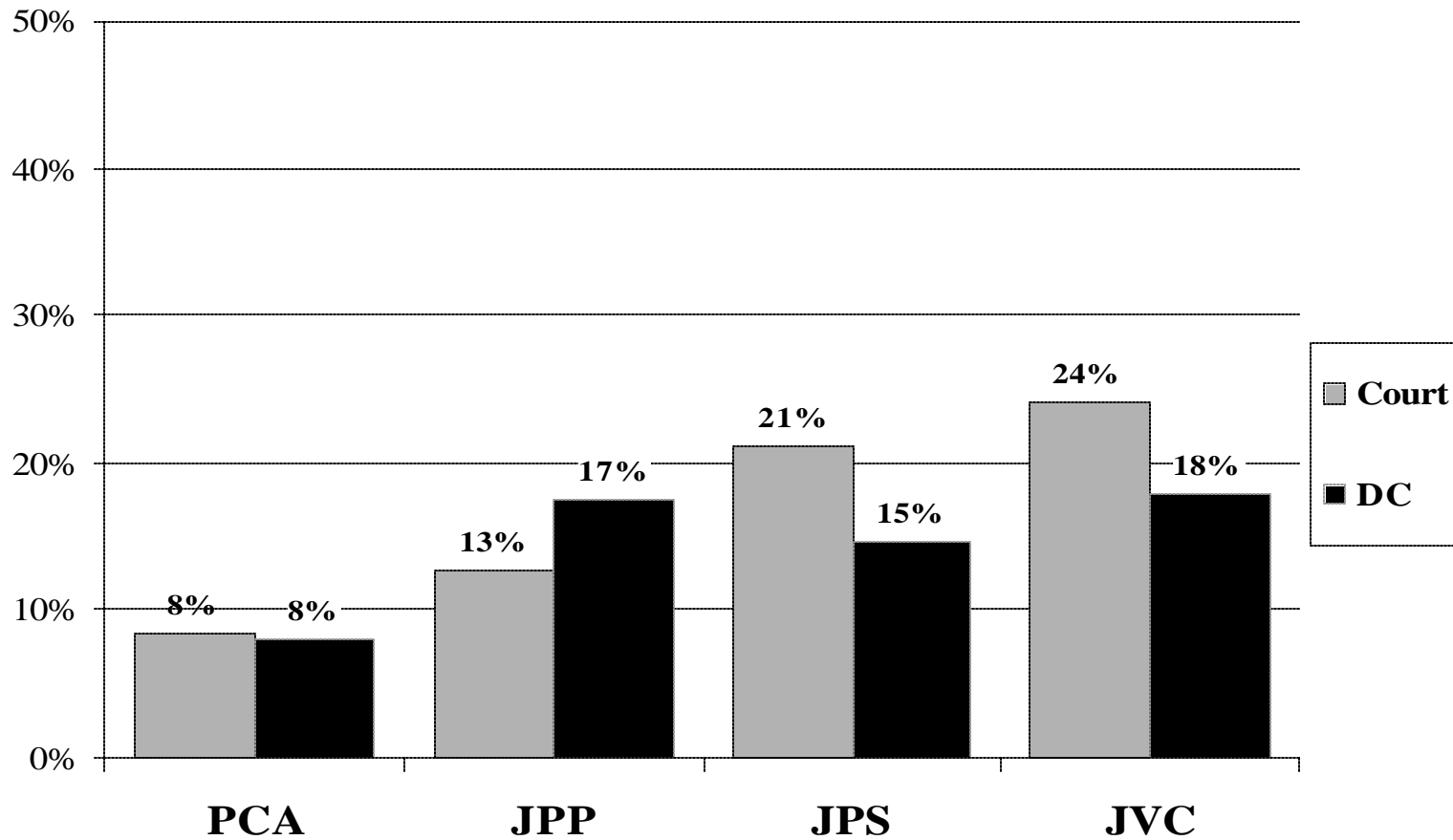


JPP -  $t = .662$ ,  $df = 60$ ,  $p \leq .51$ , Cohen's  $d = .155$

JPS -  $t = 1.528$ ,  $df = 17$ ,  $p \leq .13$ , Cohen's  $d = .509$

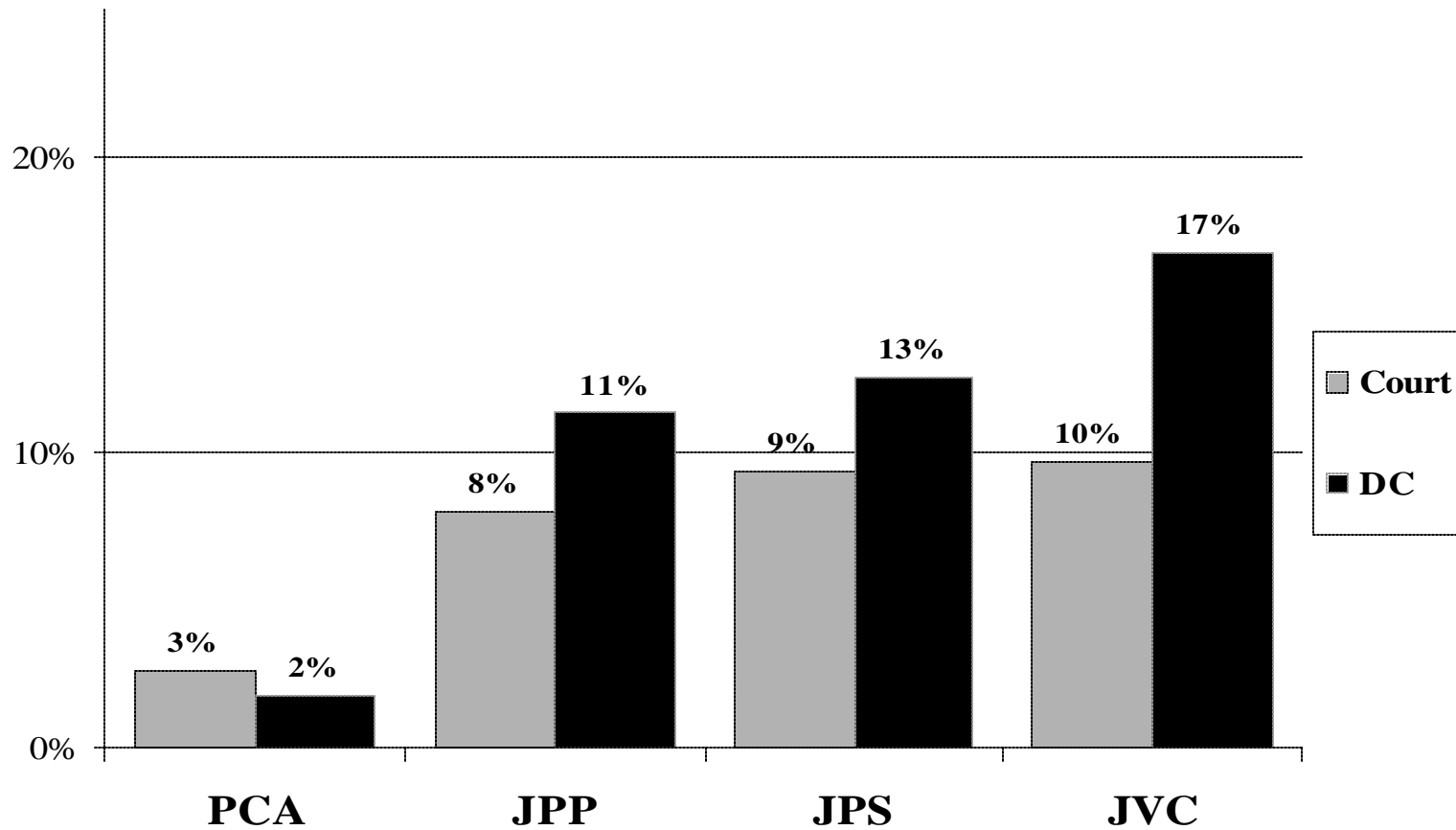
JVC -  $t = -.957$ ,  $df = 34$ ,  $p \leq .35$ , Cohen's  $d = .229$

Figure 18: Percent Unemployed Offender by Assigned Treatment and Experiment\*



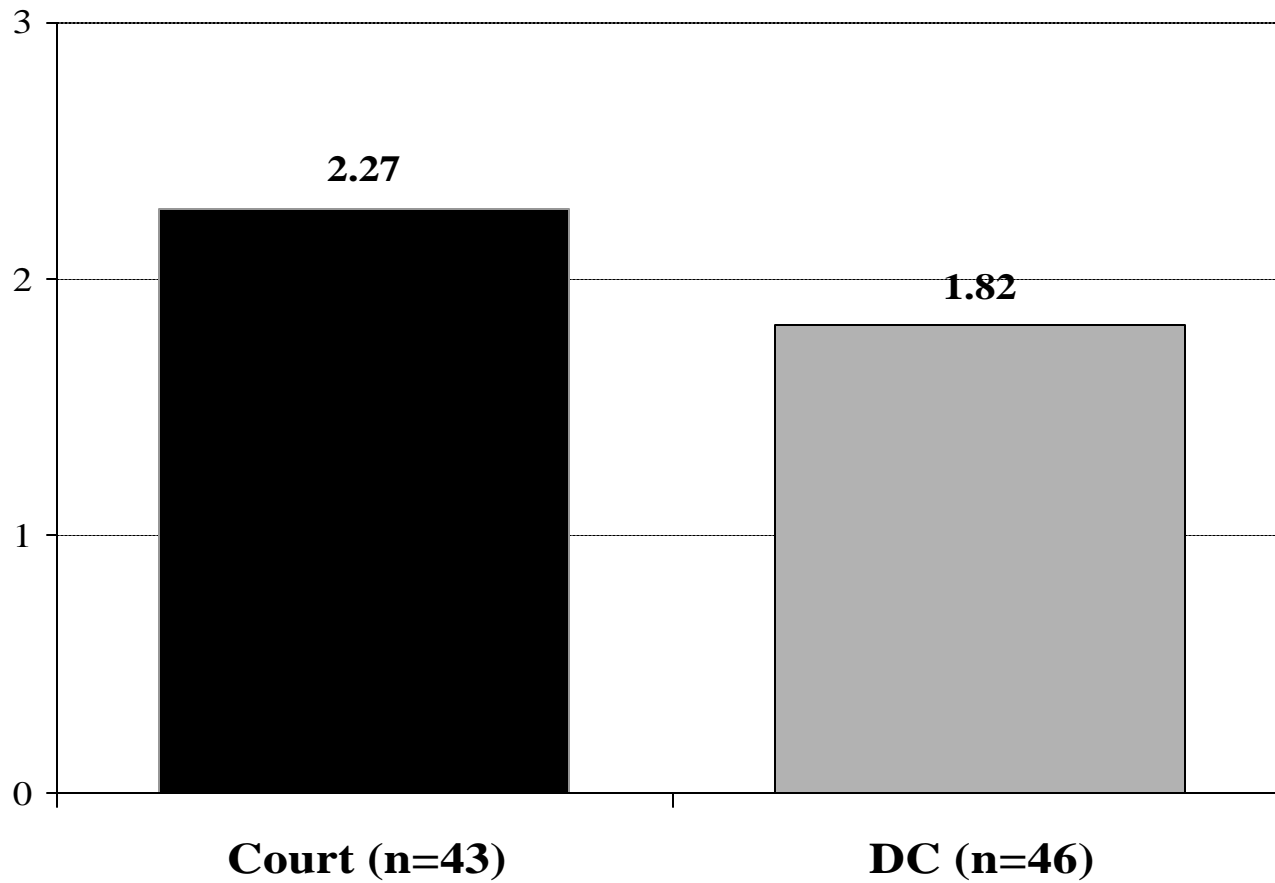
\*Within experiment differences are non-significant using Fisher's Exact Test or Chi-Square where appropriate

Figure 19: Percent Aboriginal or Torres Strait Islander Offender by Assigned Treatment and Experiment\*



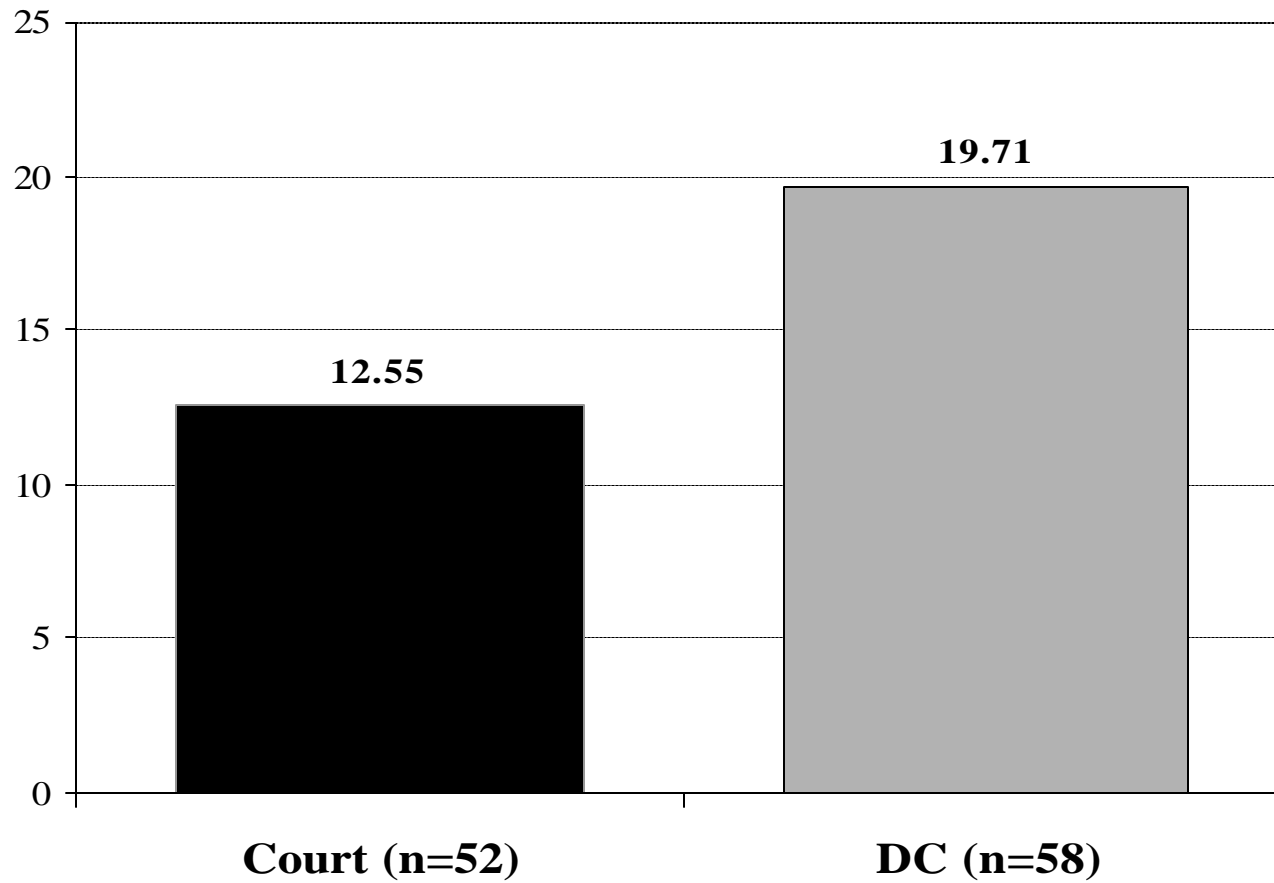
\*Within experiment differences are non-significant using Fisher's Exact Test

Figure 20: Mean Prior Offences per Case by Assigned Treatment (Youth Violence Cases, N = 89)



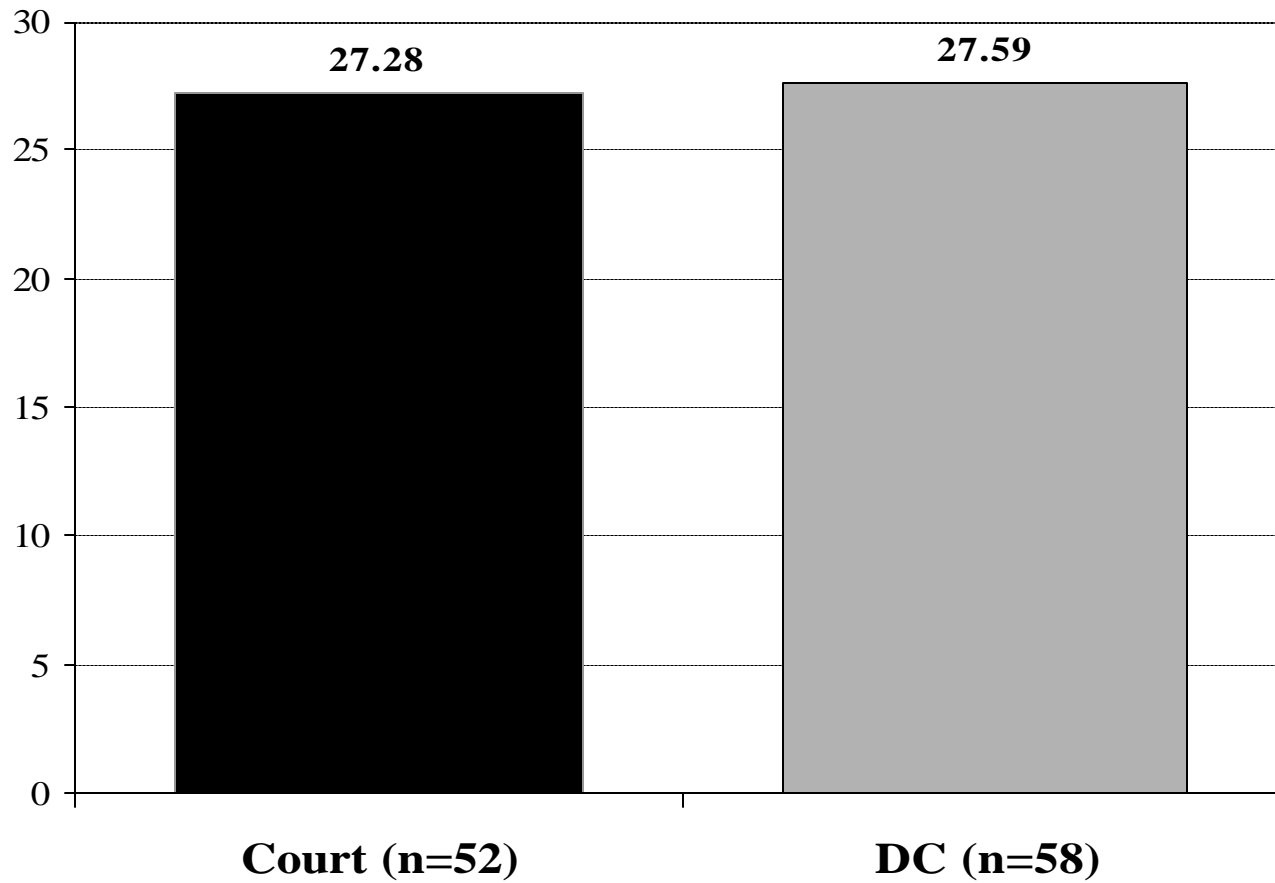
$t = .567, df = 87, p \leq .572, \text{Cohen's } d = .122$

Figure 21: Mean Length of Criminal Career (in Months) Pre-RISE per Offender by Treatment (Youth Violence Offenders, N=110)



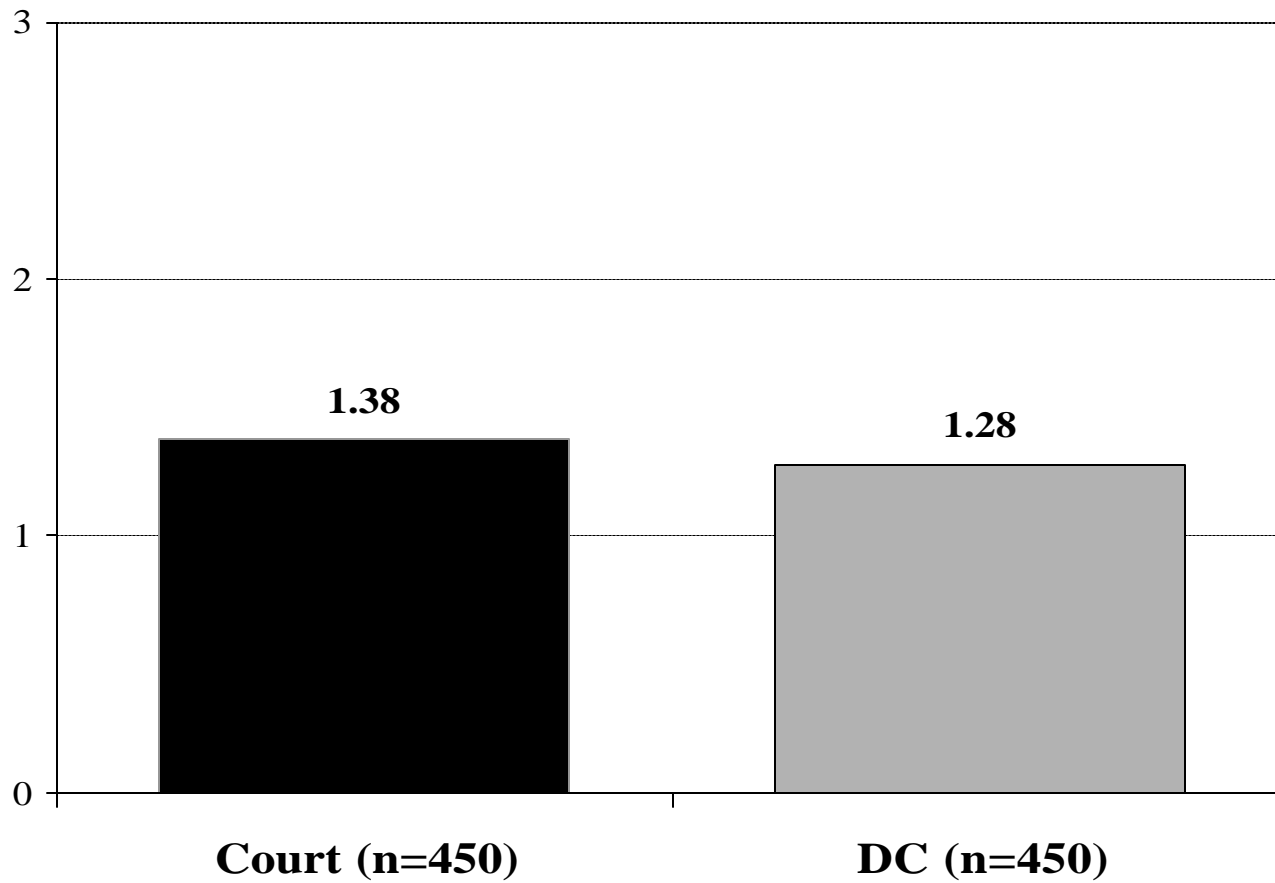
$t = -1.287, df = 108, p = .201, \text{Cohen's } d = .248$

Figure 22: Mean Time at-risk (in Months) Post-RISE by Treatment (Youth Violence Offenders, N=110)



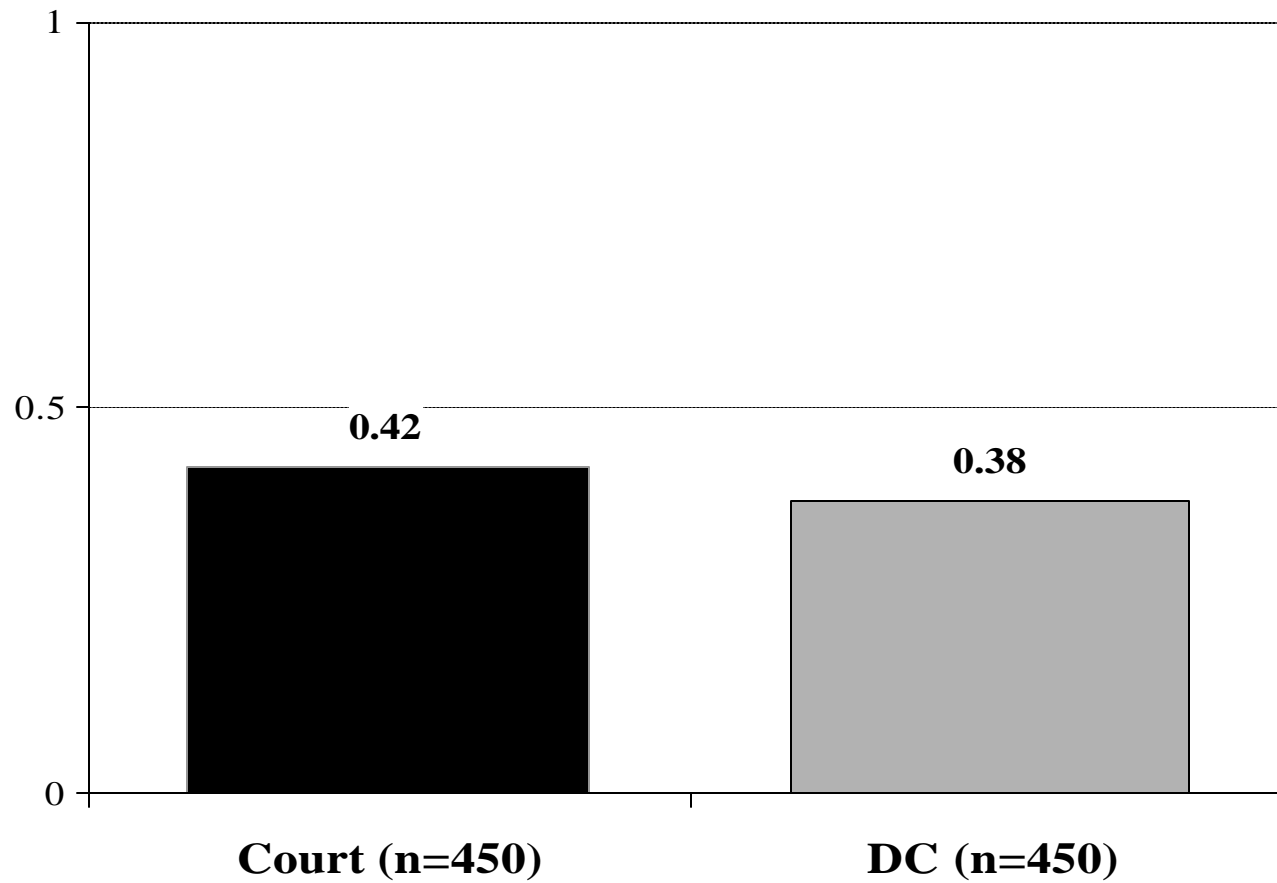
$t = -.108, df = 108, p = .689, \text{Cohen's } d = .027$

Figure 23: Mean Prior Offences (Any Offence) by Assigned Treatment (Drink Driving, N = 900)



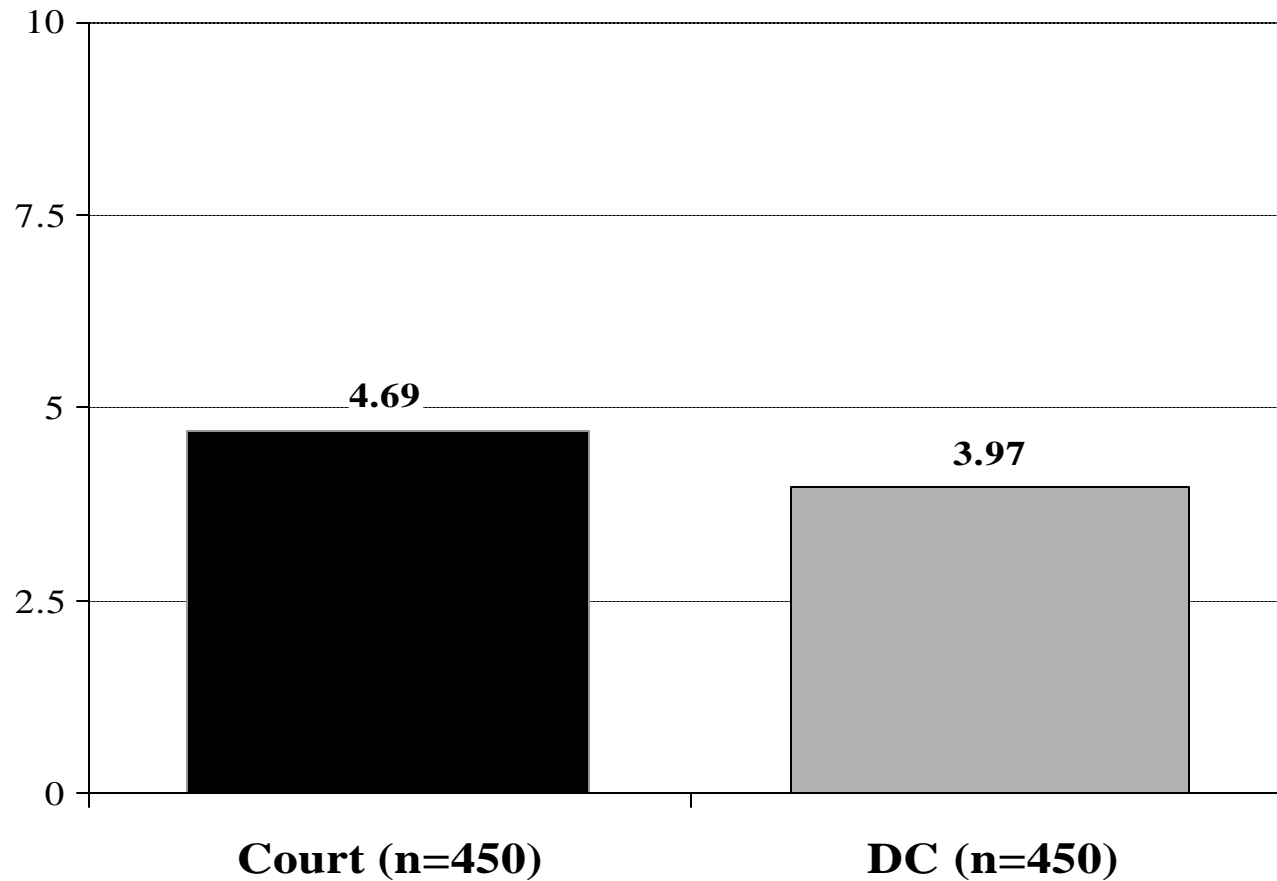
$t = .688, df = 898, p \leq .491, \text{Cohen's } d = .046$

Figure 24: Mean Prior Drink Driving Offences by Assigned Treatment (Drink Driving, N = 900)



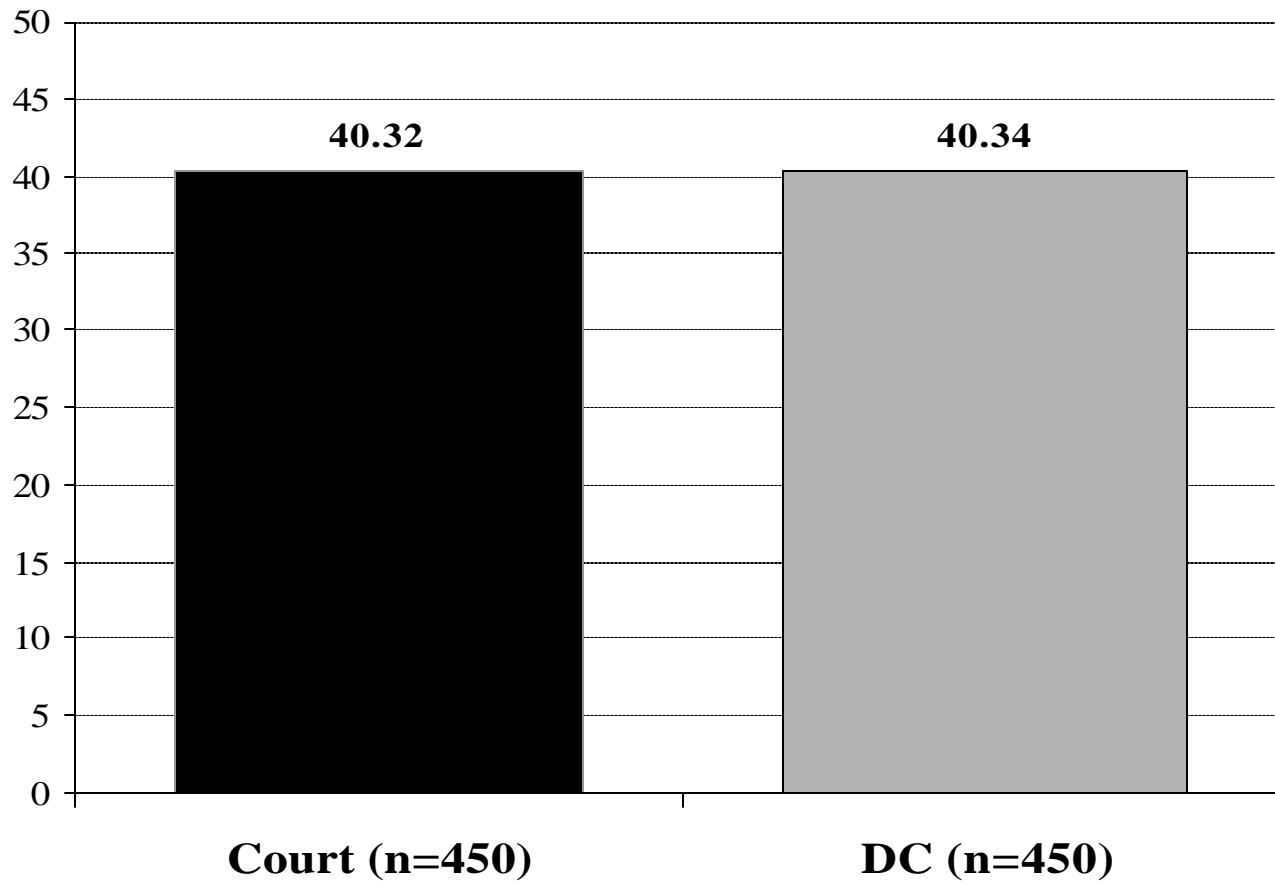
$t = .794, df = 898, p \leq .428, \text{Cohen's } d = .053$

Figure 25: Mean Length of Criminal Career (in Years) Pre-RISE by Treatment (Drink Driving, N=900)



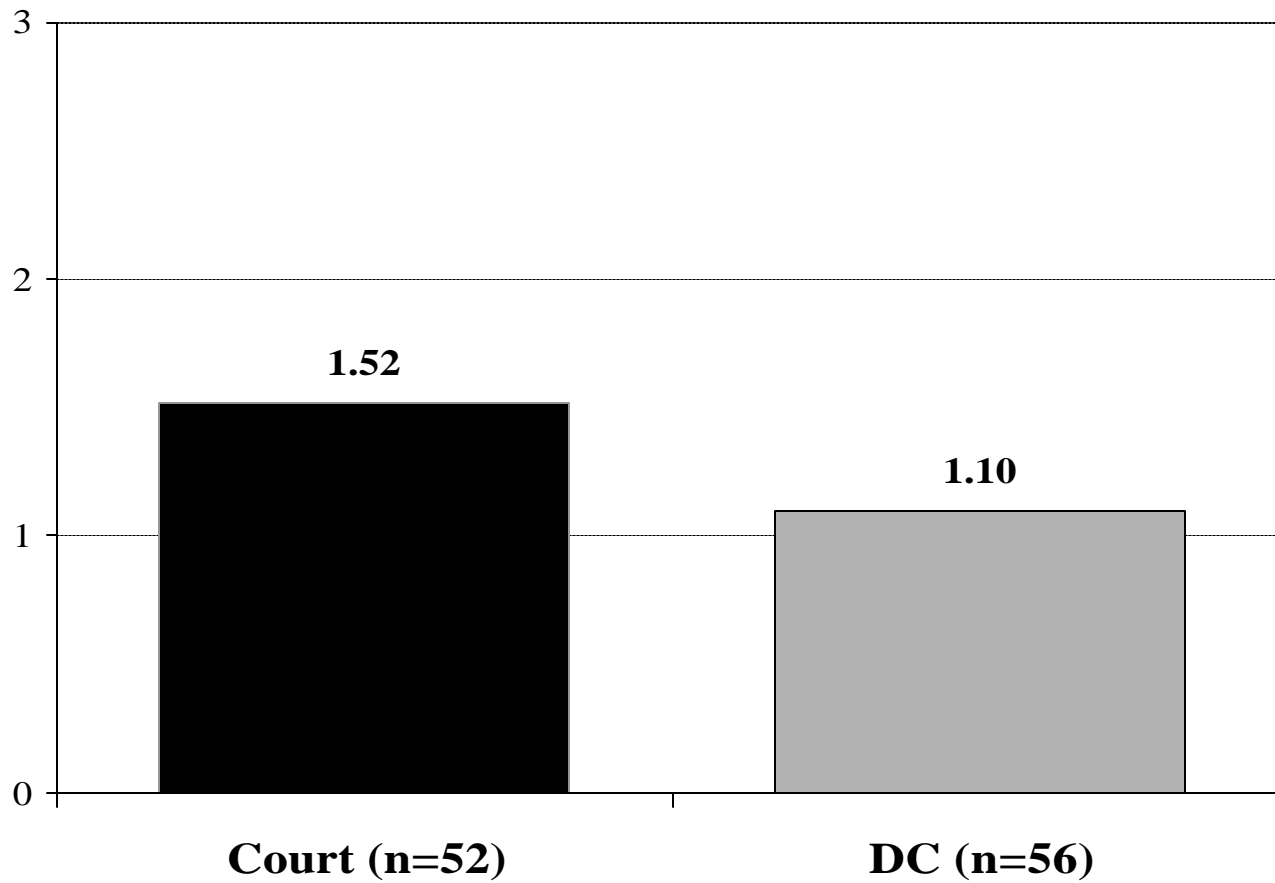
$t = -1.621$ ,  $df = 892.447$ ,  $p \leq .105$ , Cohen's  $d = .108$

Figure 26: Mean Time at-risk (in Months) Post-RISE by Treatment (Drink Driving, N=900)



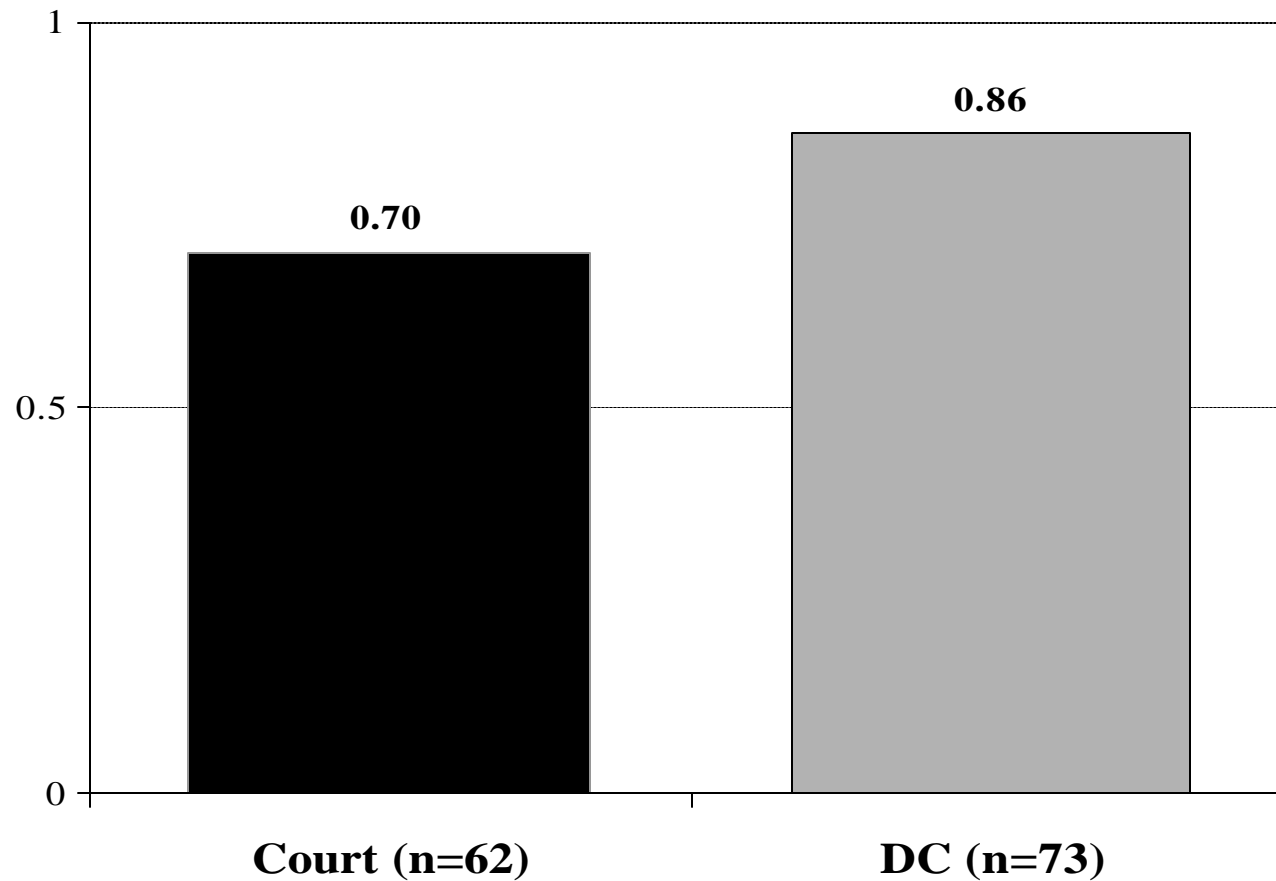
$t = -.037, df = 898, p \leq .971, \text{Cohen's } d = .002$

Figure 27: Mean Prior Offences per Case by Assigned Treatment (Juvenile Property-Security Cases, N = 108)



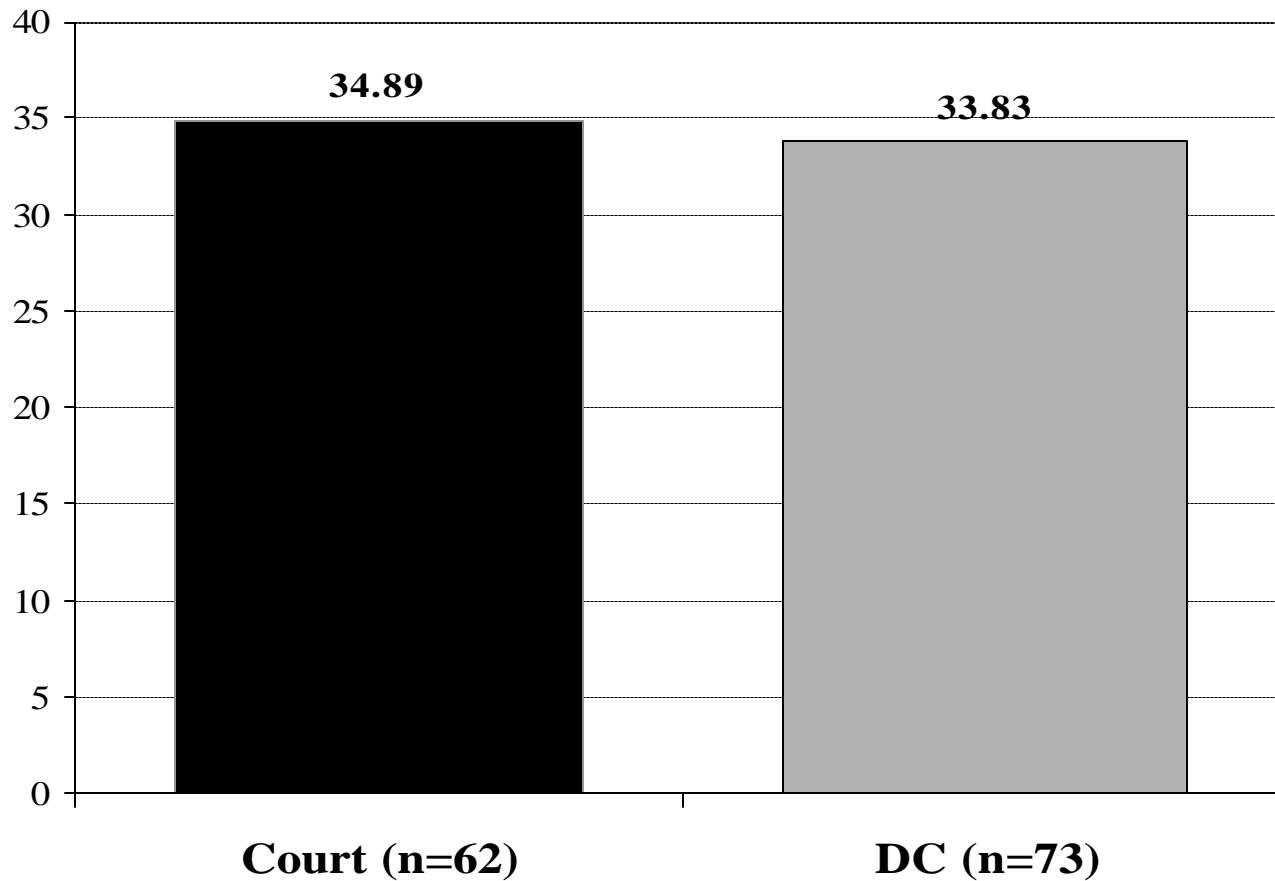
$t = 1.041, df = 106, p \leq .300, \text{ns}, \text{Cohen's } d = .199$

Figure 28: Mean Length of Criminal Career (in Months) Pre-RISE per Offender by Treatment (Juvenile Property-Security Offenders, N=135)



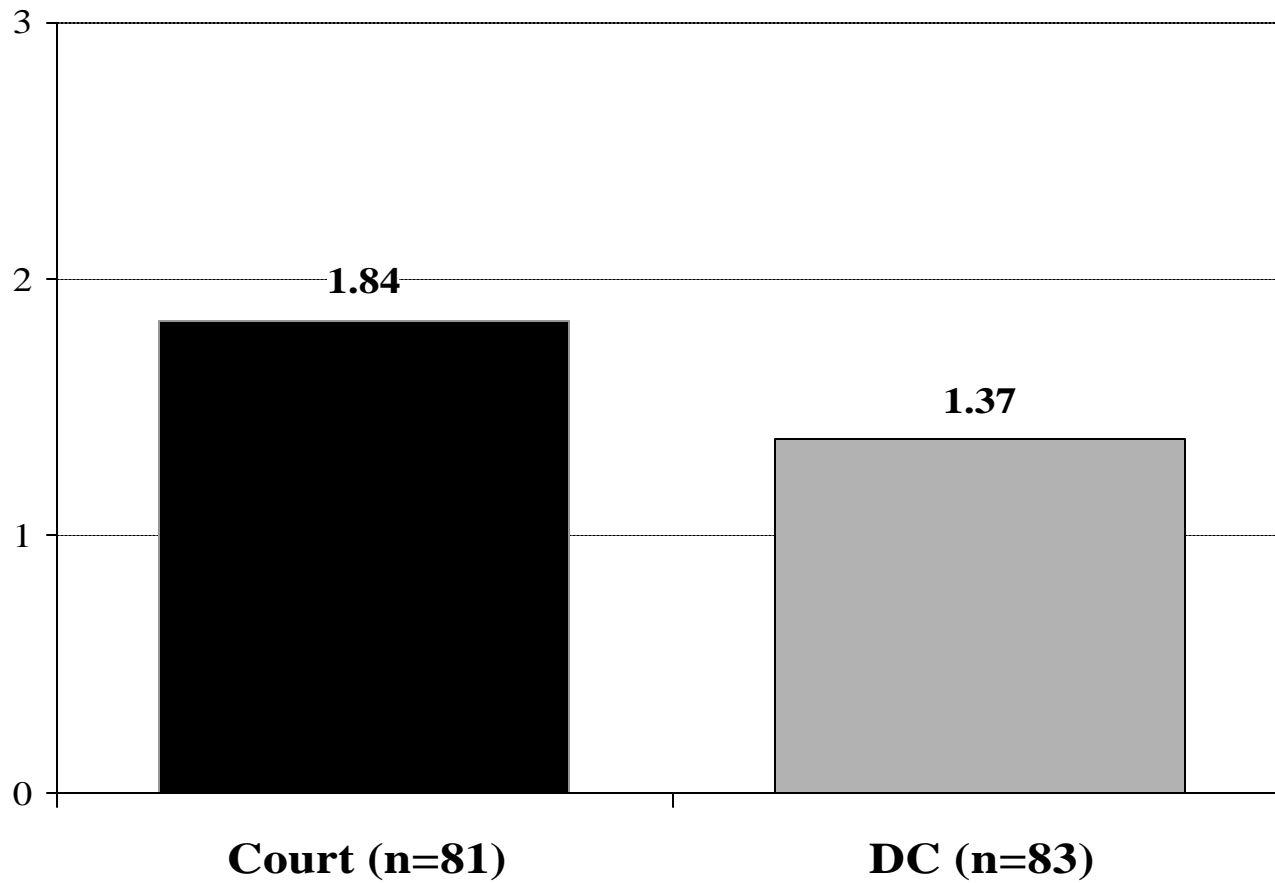
$t = .692, df = 132.298, p = .490, \text{Cohen's } d = .135$

Figure 29: Mean Time at-risk (in Months) Post-RISE by Treatment (Juvenile Property-Security Offenders, N=135)



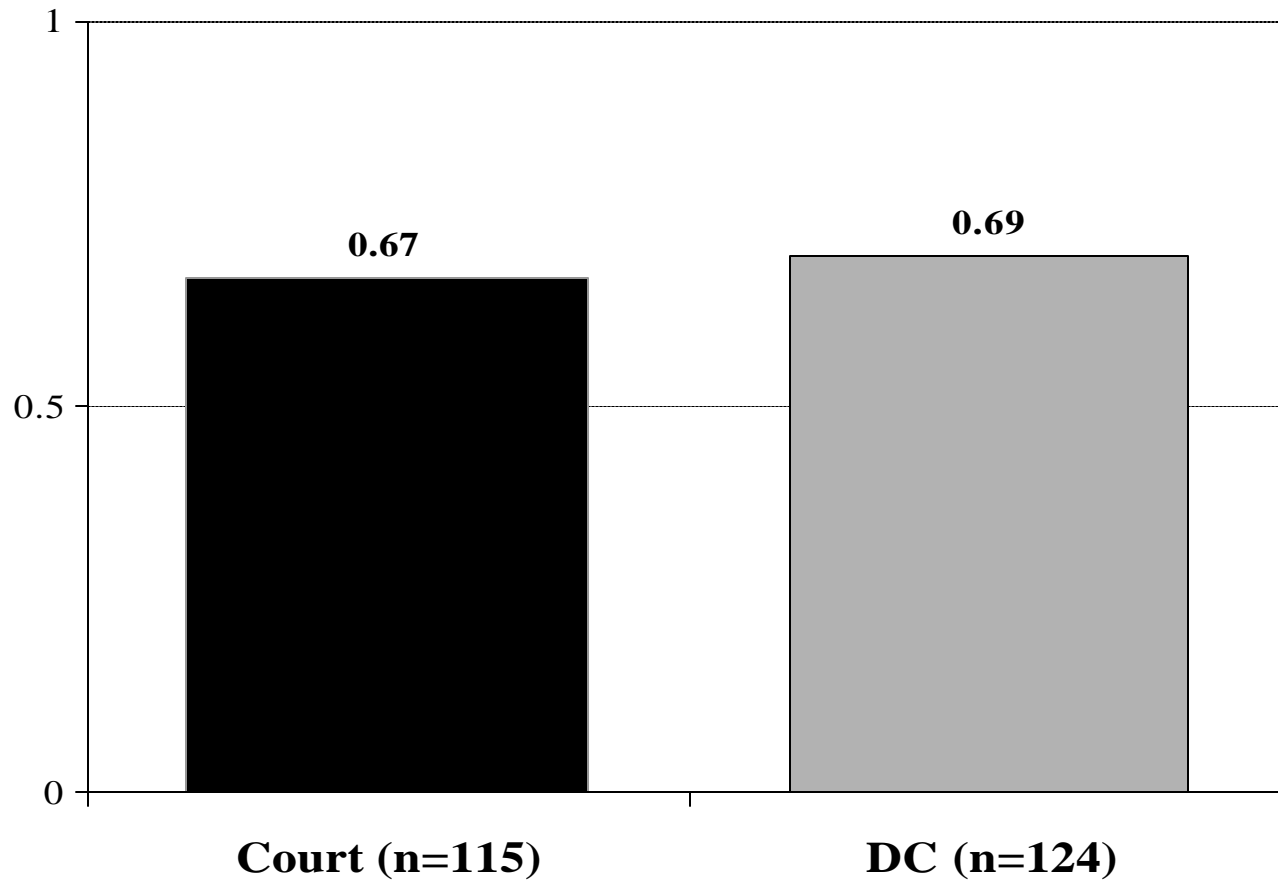
$t = .408, df = 133, p = .684, \text{Cohen's } d = .07$

Figure 30: Mean Prior Offences per Case by Assigned Treatment (Juvenile Personal Property Cases, N = 164)



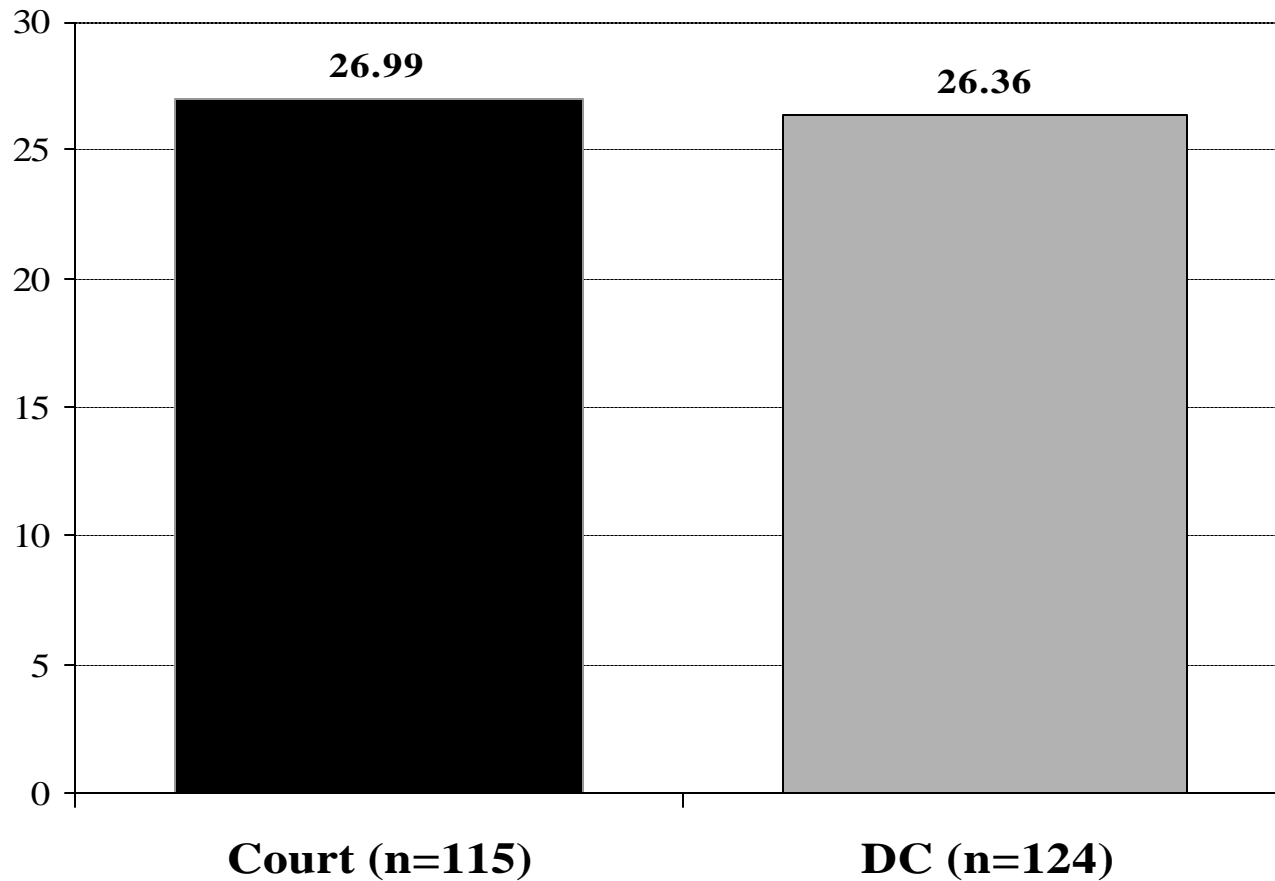
$t = 1.018, df = 162, p \leq .310, \text{Cohen's } d = .159$

Figure 31: Mean Length of Criminal Career (in Months) Pre-RISE per Offender by Treatment (Juvenile Personal Property Offenders, N=239)



$t = .194, df = 237, p = .846, \text{Cohen's } d = .025$

Figure 32: Mean Time at-risk (in Months) Post-RISE by Treatment (Juvenile Personal Property Offenders, N=239)



$t = .295, df = 236.86, p = .768, \text{Cohen's } d = .038$