


Spring 1992

The Measurement of Recidivism in Cases of Spouse Assault

Franklyn W. Dunford

Follow this and additional works at: <https://scholarlycommons.law.northwestern.edu/jclc>

 Part of the [Criminal Law Commons](#), [Criminology Commons](#), and the [Criminology and Criminal Justice Commons](#)

Recommended Citation

Franklyn W. Dunford, The Measurement of Recidivism in Cases of Spouse Assault, 83 J. Crim. L. & Criminology 120 (1992-1993)

This Symposium is brought to you for free and open access by Northwestern University School of Law Scholarly Commons. It has been accepted for inclusion in Journal of Criminal Law and Criminology by an authorized editor of Northwestern University School of Law Scholarly Commons.

THE MEASUREMENT OF RECIDIVISM IN CASES OF SPOUSE ASSAULT

FRANKLYN W. DUNFORD*

I. INTRODUCTION

Implicit in much of the research on spouse abuse¹ is the assumption that if a perpetrator of spouse assault is going to recidivate (*i.e.*, abuse a victim again), he or she will do so within six months of the presenting offense. A six-month follow-up period has thus become the conventional time period used to assess recidivism in cases of spouse abuse.

In 1984 Sherman and Berk conducted a field experiment to assess the effects of different police responses to individuals apprehended for spouse assault. They found, using a six-month outcome period, that arrest was the most effective of three standard methods used by the police (mediation, separation and arrest) to reduce domestic violence. The findings from their research (the Minneapolis Experiment) were so well received by law enforcement and those interested in the welfare of the victims of spouse abuse² that the National Institute of Justice (NIJ) funded research in six additional communities (Dade County, Florida; Atlanta, Georgia; Charlotte, North Carolina; Milwaukee, Wisconsin; Colorado Springs, Colorado; and Omaha, Nebraska) to replicate the Minneapolis Experiment. The primary purpose of these six replications was to assess

This paper was supported by Grant 1R01MH45082-01 from the National Institute of Mental Health and Grant 85-IJ-CX-K435 from the National Institute of Justice. Points of View or opinions expressed herein are those of the author and do not necessarily represent those of the Department of Health and Human Services or the U.S. Department of Justice.

* Institute of Behavioral Science, University of Colorado

¹ See Lawrence W. Sherman & Richard A. Berk, *The Specific Deterrent Effects of Arrest for Domestic Assault*, 49 AM. SOC. REV. 261 (1984); PETER H. NEIDIG & DALE H. FRIEDMAN, *SPOUSE ABUSE: A TREATMENT PROGRAM FOR COUPLES* (1984); DONALD G. DUTTON, *THE DOMESTIC ASSAULT OF WOMEN: PSYCHOLOGICAL AND CRIMINAL JUSTICE PERSPECTIVES* (1988); J. L. Edelson & M. A. Syers, *The Relative Effectiveness of Group Treatments for Men Who Batter*, 26 SOC. WORK RES. & ABSTRACTS 13 (1990).

² Lawrence W. Sherman & Ellen G. Cohn, *The Impact of Research on Legal Policy: The Minneapolis Domestic Violence Experiment*, 23 LAW & SOC'Y. REV. 117, 141 (1989).

experimentally the effects of law enforcement and justice interventions on the reduction of recidivism among those apprehended for spouse assault.³ Four of the six replications were funded on the basis of six-month follow-up periods. Atlanta used an eight-month follow-up, and Omaha⁴ used a twelve-month follow-up.⁵ The purposes of the following paper are (1) to present findings associated with six additional months of outcome (for a total of twelve months) used to assess failure in the Omaha replication of the Minneapolis Experiment and (2) to use the full Omaha data set to understand better the amount of time required to assess recidivism accurately in cases of spouse assault known to the police.

II. TWELVE- VS. SIX-MONTH FOLLOW-UP IN OMAHA

A brief overview of the Omaha Experiment will set the subsequent discussion in perspective. Beginning in March, 1986, all eligible cases of spouse assault coming to the attention of the police throughout the city of Omaha between the hours of eight o'clock in the evening and midnight were randomly assigned to one of three treatments: mediation, separation, or arrest. Mediation amounted to little more than the restoration of order. Separation involved sending one of the parties to the assault away. Arrest meant that the perpetrator was arrested and transported to jail, where he or she remained until bond was posted or they were released by the court. A case was eligible for the experiment if (1) probable cause for an arrest for misdemeanor assault existed, (2) the case involved both a clearly identifiable victim and suspect, (3) both parties to the assault were of age (18 or older), (4) both parties had lived together sometime during the year preceding the assault, and (5) neither party to the offense had an arrest warrant on file. Serious (*i.e.*, felony) cases were excluded from the experiment.

³ NATIONAL INSTITUTE OF JUSTICE, REPLICATING AN EXPERIMENT IN SPECIFIC DETERRENCE: ALTERNATIVE POLICE RESPONSE TO SPOUSE ASSAULT (1986).

⁴ FRANKLYN W. DUNFORD, ET. AL., THE OMAHA DOMESTIC POLICE EXPERIMENT: FINAL REPORT TO THE INSTITUTE OF JUSTICE (1989) [hereinafter DUNFORD ET AL., THE OMAHA DOMESTIC POLICE EXPERIMENT]. Two experiments were conducted in Omaha. One was a replication of the Minneapolis Experiment, which involved cases in which both perpetrators and victims were present when the police arrived in response to calls for assistance. The other was an Offender Absent Experiment in which cases were randomly assigned to receive or not to receive a warrant for the arrest of perpetrators when offenders were not present when the police arrived. The two experiments ran concurrently. For the results of the offender absent experiment, See Franklyn W. Dunford et al., *The Role of Arrest in Domestic Assault: The Omaha Police Experiment*, 28 CRIMINOLOGY 183 (1990) [hereinafter Dunford et al., *The Omaha Police Experiment*].

⁵ Some of these projects did collect twelve or more months of arrest/court data for follow-up purposes.

After responding officers established eligibility, they contacted the Information Unit of the Omaha Police Division and gave a civilian operator the date and time of the call, names and birth dates of victims and suspects, and their own police identification number(s). A computerized randomization program operated by the Information Unit then randomly assigned the case to three experimental treatments. There were a total of 330 eligible cases in the experiment (mediation = 115, separation = 106, arrest = 109).

The Omaha Project used two types of outcome measures at six and twelve months after the presenting offense. The first was official recidivism measured by new arrests and complaints for any crimes committed by perpetrators against victims as found in official police records. The second was victim reports of three forms of repeated violence involving the presenting victims and suspects: (1) victim fear of injury, (2) pushing-hitting, and (3) physical injury. Victims were interviewed three times over a twelve-month period. The initial interview was conducted following the first week after the presenting offense. The second and third interviews were conducted six and twelve months after the presenting offense. Eighteen percent of the sampled victims did not complete initial interviews. The overall interview completion rate (including initial losses) was seventy-six percent at six months and seventy-two percent at twelve months.⁶

III. FINDINGS

The analyses conducted to assess the effects that randomized treatments had on recidivism in Omaha, using a *six-month* follow-up period, indicated that arrests were no more effective in reducing recidivism over the first six months than were the mediation and separation treatments.⁷ Arrest in Omaha, by itself, did not appear to deter subsequent domestic conflict after six months any more than separation or mediation. Also, arrest did not appear to *increase* continued domestic conflict between parties to an arrest for assault. These findings from the six-months analysis of outcome for the Omaha Experiment clearly did not replicate those of the Minneapolis Experiment, in which arrest was reported to deter continued violence.

Since the Omaha replication used both six- and twelve-month

⁶ See the Omaha Domestic Violence Police Experiment for the details of the experimental design and its implementation. DUNFORD ET AL., *THE OMAHA DOMESTIC VIOLENCE POLICE EXPERIMENT*, *supra* note 4.

⁷ Dunford et al., *The Omaha Police Experiment*, *supra* note 4.

follow-up periods to assess outcome, but limited analyses to the first six months of risk in the analyses referred to above, it provides an opportunity to assess the impact that an additional six months of follow-up had on at least one intervention thought to deter continued spousal violence.

Since the replication was designed to test the hypothesis that an arrest treatment deters continued domestic abuse, comparing experimental cases that involved arrest in Omaha with those that did not represents the purest test of that hypothesis.⁸ Following this logic, the mediation and separation treatments were collapsed into an informal treatment category to be compared with arrests (the formal treatment). The data of Tables 1 through 5 present findings resulting from these comparisons at six and twelve months after the presenting offense on each of the five outcome measures.

When an additional six months of recidivism was added and analyzed, the conclusions based upon the six-month period were not altered. That is, findings based on follow-up measures at twelve months did not effect the findings found for the six month comparisons. Interestingly, statistical⁹ differences were found for arrest recidivism at twelve months which were not evidenced at six months (Table 1). However, the differences were counter to those found for the Minneapolis Experiment. Cases randomly assigned to informal treatment (mediation and separation) were found to have both a lower prevalence and a lower frequency of arrest recidivism than were those assigned to the formal treatment (arrest). This difference was primarily due to fewer rearrests among the mediated cases. Setting aside statistical tests for a moment, Tables 1 through 5 were reviewed again to determine if any trends emerged favoring the arrest treatment as deterring continuing domestic conflict after twelve months. No such trends were found. It was clear that the arrest treatment was no more effective at twelve months in reducing subsequent domestic violence relative to the other treatments than it was at six months.

⁸ Arnold Binder & James W. Meeker, *Experiments and Reforms*, 16 J. CRIM. JUST. 347, 355 (1988). It should be noted that the same conclusions that are presented below are reached with analyses based upon all three experimental treatment groups.

⁹ Statistical significance was set at $p > .10$ to increase the probability of replicating the Minneapolis findings.

The finding that twelve months of follow-up in Omaha did not alter conclusions derived from findings based upon six months of follow-up should not divert attention away from the fact that enough additional recidivism occurred during the last half of the total twelve-month follow-up that the results of the Omaha experiment could have been affected. Nor should these findings divert attention away from the possibility that experimental comparisons after two or three years of follow-up may provide statistical and substantive differences between treatments that are not found in shorter follow-up periods.

The differences between victim and official reports of recidivism deserve comment. It is noteworthy that the use of official rearrest data suggests that, at twelve months after a police intervention, those who were arrested at the presenting offense were more likely to have new arrests for spouse abuse than were those handled informally at the presenting offense. A similar result was not found, however, in comparisons using official complaint data. Nor was this finding replicated using any of the victim-reported measures of repeated abuse or violence. One obvious, although not tested, explanation for this difference is that when the police returned to cases that had a prior arrest, they were more likely to arrest again. Thus, the finding that an arrest may actually increase the likelihood of an arrest may be a simple artifact of the way police do business—which may be only marginally related to the behavior of suspects.

IV. SIX MONTHS VS. LONGER OUTCOME PERIODS

In an attempt to understand better the amount of time required to accurately assess recidivism, the cases of *both* of the Omaha domestic violence police experiments were combined and reviewed to determine the distribution of five types of failure over six- and twelve-month follow-up periods.¹⁰ Two questions were of particular interest. First, how many new cases of repeated violence occurred in the second six months of a twelve-month follow-up period compared to the first (prevalence)? and second, how did the frequency of offending vary for those who repeated (Lambda) for the two halves of total twelve-month follow-up period?

A. PREVALENCE OF REOFFENDING

The first question was addressed by cross-tabulating the proportion of cases failing at each of the measurement periods for each

¹⁰ The 247 cases of Offender Absent Experiment were added to the 330 cases of the replication. See *supra* note 4.

TABLE 3
PREVALENCE AND MEAN FREQUENCY OF VICTIM REPORTS OF
BEING ENDANGERED BY SIX- AND TWELVE-MONTH FOLLOW-UP
PERIODS

Informal vs. Formal

Outcome	First 6 Months			Total 12 Months		
	Informal	Formal	Total	Informal	Formal	Total
Yes	80	44	124	94	46	140
	36.2	40.4		42.5	42.2	
No	85	33	118	65	25	90
	38.5	30.3		29.4	22.9	
Missing	56	32	88	62	38	100
	25.3	29.4		28.1	34.9	
Total Frequency*	221	109	330	221	109	330
	2.236	2.416		4.616	4.732	

$X^2=2.148$ DF=2 $p=.342$

*F=1.027 $p=.869$

$X^2=2.241$ DF=2 $p=.326$

*F=.004 $p=.948$

TABLE 4
PREVALENCE AND MEAN FREQUENCY OF VICTIM REPORTS OF
BEING PUSHED OR HIT BY SIX- AND TWELVE-MONTH FOLLOW-UP
PERIODS

Informal vs. Formal

Outcome	First 6 Months			Total 12 Months		
	Informal	Formal	Total	Informal	Formal	Total
Yes	69	29	98	81	39	120
	31.2	26.6		36.7	34.0	
No	96	48	144	78	33	111
	43.4	44.0		35.3	30.3	
Missing	56	32	88	62	38	100
	25.3	29.4		28.1	34.9	
Total Frequency*	221	109	330	221	109	330
	1.612	2.104		3.283	4.014	

$X^2=.972$ DF=2 $p=.615$

*F=.520 $p=.472$

$X^2=1.728$ DF=2 $p=.422$

*F=.210 $p=.648$

TABLE 5
PREVALENCE AND MEAN FREQUENCY OF VICTIM REPORTED INJURY
BY SIX- AND TWELVE-MONTH FOLLOW-UP PERIODS

Informal vs. Formal

Outcome	First 6 Months			Total 12 Months		
	Informal	Formal	Total	Informal	Formal	Total
Yes	45 20.4	16 14.7	61	56 25.3	25 22.9	81
No	120 54.3	61 56.0	181	103 46.6	46 42.2	149
Missing	56 25.3	32 29.4	88	62 28.1	38 34.9	100
Total	221	109	330	221	109	330
Frequency*	.715	.558		1.579	1.606	

$X^2=1.754$ DF=2 $p=.416$ $X^2=1.602$ DF=2 $p=.449$
 *F=.262 $p=.609$ *F=.001 $p=.975$

of the five outcome measures as shown in Table 6. Several important points emerge from a review of these data. As is usually the case, the official measures (arrests and complaints) reflect less recidivism than do the victim reports (fear of injury, hitting and pushing, physical injury), although the gap between the two types of measures narrows when complaint recidivism is compared with victim reports of injury.¹¹ Using *arrest* as outcome, ten percent of the sample recidivated by the end of the first six months compared to seven percent during the second six months. Official recidivism decreased from seventeen percent to thirteen percent within these two periods when failure was broadened to include cases in which the police were called and filed an official *complaint* report. As a result of interviews with victims, recidivism decreased from twenty-six percent for the first six months to twenty percent for the second six months for new incidents of *injury*, from forty-one percent to thirty-three percent for repeated episodes involving *hitting and/or pushing*, and from fifty-two percent to forty-one percent for instances in which victims reported being *fearful of injury*.¹²

B. ADDITIONAL FINDINGS

Three additional findings from Table 6 are of interest. First, comparatively few repeat cases involved recidivism in both time periods and, as often as not, as much or more recidivism was found to have occurred in either one or the other of the time periods than in both. The more official the response or the more serious the offense, the less likely that recidivism was reported in both time periods. Among recidivists, to illustrate this point, only five percent of the arrests, twenty percent of the complaints, and thirty-two percent of victim reports of injury were reported in both time periods. Second, more perpetrators recidivated in the first six months of follow-up than in the second, irrespective of the measure used. This supports the assumption that failure is more likely to occur early than late, given a twelve-month follow-up period. Third, the *differences*, however, in the prevalence of recidivism between the first and second six-month follow-up periods, as a rule, were not very large. Although violence may have abated during the second six months of follow-up, the size of the abatement was relatively small. For example, police arrested ten percent of the sample in the first six months

¹¹ This is probably due to the increased likelihood that the police will be called and an official complaint report will be filed.

¹² The prevalence of recidivism may, in part, be a function of the measuring device used to assess it. As the measures of recidivism were more narrowly defined (in scope and in source) the prevalence of recidivism clearly decreased.

and seven percent in the second six months. The second six-month measures of recidivism produced prevalence rates from seventy to eighty percent of those found in the first six months. Consequently, the reduction in recidivism from the first to the second six-month time periods was between twenty and thirty percent.

Since the arrest and complaint outcome measures reported above included all new conflicts between the couples in the experi-

TABLE 6
PREVALENCE OF REOFFENDING SIX AND TWELVE MONTHS AFTER
THE PRESENTING OFFENSE

		Recidivism During Second Six Months			Total Cases
		No	Yes		
Recidivism	Arrested	No	483 84%	36 6%	577
	Arrested	Yes	53 9%	5 1%	
During	Complaint	No	430 74%	46 8%	577
	Complaint	Yes	71 12%	30 5%	
Six	Fearful	No	154 37%	43 10%	416*
	Fearful	Yes	89 21%	130 31%	
Months	Hit/Push	No	201 48%	45 11%	416
	Hit/Push	Yes	79 19%	91 22%	
	Injured	No	267 64%	38 9%	416
	Injured	Yes	64 15%	47 11%	

* Cases were omitted when victim interviews were not obtained at all interview periods.

ment, irrespective of the content of the conflict, additional analyses were conducted limiting repeated arrests and complaints to incidents involving assault charges. The findings associated with all recidivism were not significantly altered when limited in this way.

V. FREQUENCY OF REOFFENDING

Differences in the *frequency* of offending between the two six-month time periods were addressed by determining the number of perpetrators who repeated a given outcome (arrest, complaint, fear of injury, etc.) in a given time period and the number of new offenses per time period, and then calculating the mean number of repeated offenses per repeat offender. This procedure produced a rate of offending known as Lambda, the mean number of offenses per offender (see Table 7). These data confirm the earlier finding that there were fewer recidivating persons in the second time period than in the first, and they document a similar pattern for offenses. The differences, however, were relatively moderate. Of most interest, and perhaps significance, is Lambda. The rate of offending by those who did recidivate changed very little from one time period to another. Even though the prevalence of offending decreased from the first to the second six-month follow-up period, the frequency of offending among offenders did not decrease; and this finding held for both official and victim-reported measures of recidivism. For example, the mean number of arrests for recidivists was 1.17 in the first six months and 1.02 in the second six months, and the mean number of victim reported injuries was 4.23 and 4.08 respectively. Among those who repeated in their violence, the frequency of violence was relatively constant in both halves of a one-year follow-up.

VI. TIME TO FAILURE

An alternative method used to assess failure over time involved calculating the time from the presenting offense to the first new arrest, the first new complaint, or the first new victim report of a new injury¹³ per recidivist for those living together during the one year follow-up period. The purpose of these analyses was to determine how first failures were distributed over a one-year risk period. A time-to-failure procedure was used to accomplish this task. The number of suspects at risk of failing each month was divided by the number of suspects who failed each month to determine the propor-

¹³ Respondents were asked the date of the first new injury when a new injury was reported in victim interviews. This was not done for reports of pushing and hitting or fear of injury.

TABLE 7
RECIDIVISM AMONG REPEAT OFFENDERS (LAMBDA) FOR FIRST AND
SECOND SIX MONTH FOLLOWUP PERIODS FOR FIVE OUTCOME
MEASURES

Outcome Measure ^a	Number of People Failing	Number of Failures	Lambda
Arrest			
First 6 Months	58	68	1.17
Second 6 Months	41	42	1.02
Complaint			
First 6 Months	101	134	1.33
Second 6 Months	76	95	1.25
Victim Fear of Injury			
First 6 Months	230	1,453	6.32
Second 6 Months	173	925	5.35
Victim Injured			
First 6 Months	120	508	4.23
Second 6 Months	85	347	4.08

tion of new failures each month over a one-year period. The analyses were limited to couples who were living together at least part of the time in both the first and second six-month time periods as a way of insuring that parties to the initial assault were really at risk of reoffending. The results of these analyses are presented in Figure 1. Even though there were sharp and sometimes sizable changes in the proportion of new failures occurring across the thirteen measurement periods assessed, the most impressive finding for the three outcome measures graphed in Figure 1 was the relatively constant rate of new failures across time. That is, while the overall trend was a general reduction in the number of new failures over time for all of the outcome measures, large numbers of suspects did fail for the first time after the midpoint of the year, and in some months thereafter the numbers of at-risk cases failing for the first time actually rose. These data clearly demonstrate that first-time failure continues throughout a one-year followup period among those living together.

VII. LONG-TERM OFFICIAL OUTCOME

Inasmuch as the follow-up period in these analyses were limited to twelve months, the assumption that most first-time recidivism occurs in the first six or twelve months after the presenting offense was only partially addressed. That is, both official and victim-reported

data were not originally available to assess recidivism in the second and third years after the presenting offense. Given the significance of this issue, a new search of police and court records was initiated forty-five months after the first case was taken into the study in Omaha. Because it took nineteen months to generate the sample for the Omaha experiments, the period for which perpetrators were at risk to recidivate fluctuated between two years and two months and three years and nine months. Those persons found eligible for the experiments during the first months of sampling obviously had more time to recidivate than did those who entered towards the end of the nineteen-month sampling period.

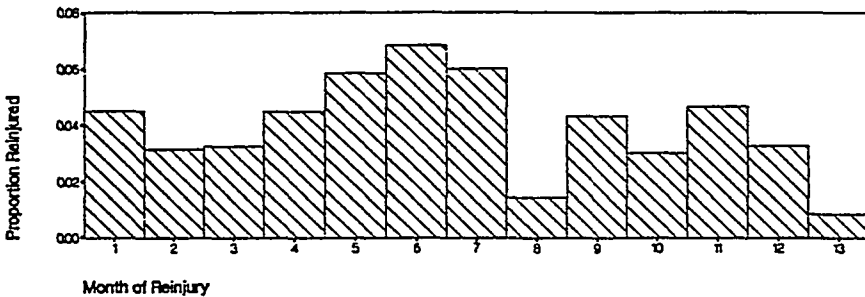
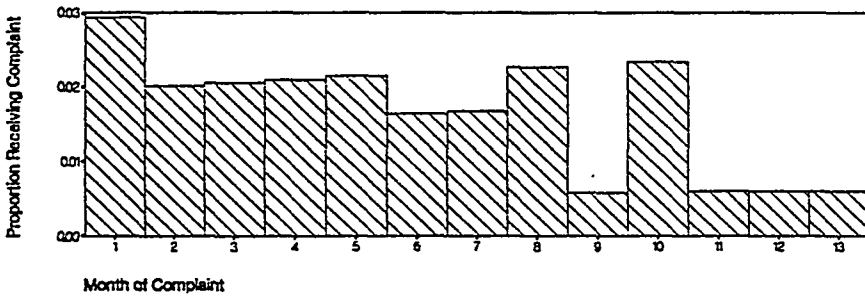
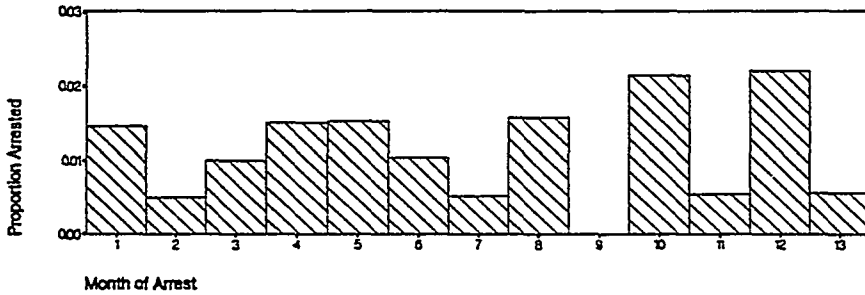
The data in Figure 2 illustrate the number of perpetrators, by month, who were arrested or who had an official complaint filed against them for the first time after the presenting offense. Care must be taken, however, in the interpretation of these data. First, official arrest and complaint records do not reflect the repeat abusive *behavior* of perpetrators. Rather, they represent police actions taken at the end of a chain of events initiated after a repeated incident of spouse abuse (behavior) has been made known to the police. Not all new incidents of abuse are reported to the police. In addition, the police neither respond to all calls nor do they file a complaint or make an arrest in all cases in which they do respond. Further, the police do not consistently identify all spouse assault cases as such; rather, they are often reported as trespassing, disorderly conduct, harassment, destruction of property, etc.(cases) As a result, reliance on official data to determine the incidence of continued spouse abuse and assault among cohabitating couples is risky.¹⁴ Second, the search of police records for new arrests and complaints was limited to the record bureau of the Omaha Police Division and to cases in which the original suspect had committed a new offense against the original victim. Assaults coming to the attention of authorities in other jurisdictions or involving other victims were, in this manner, excluded from the data.

Notwithstanding these limitations, the data of Figure 2 are of interest. First, the majority of official failures occurred within the first year after the presenting offense. Roughly eighty percent of the first official failures recorded in the first two years of follow-up (during which time a full twenty-four months had elapsed since the

¹⁴ If the police are called in less than fifteen percent of all cases of spouse assault (DUTTON, *supra* note 1 at 6-8, 136) and the probability of an arrest, once the police respond and establish probable cause, is close to twenty percent (*id.* at 138-39, 153), it is evident that nearly all spouse assault and violence remains hidden and that police records reflect only a speck of the violent behavior of spouse assaulters.

Proportion of Cases at Risk that Failed Each Month* over a One Year Period

Victims and Suspects Living Together



*A month is defined as a four week period.

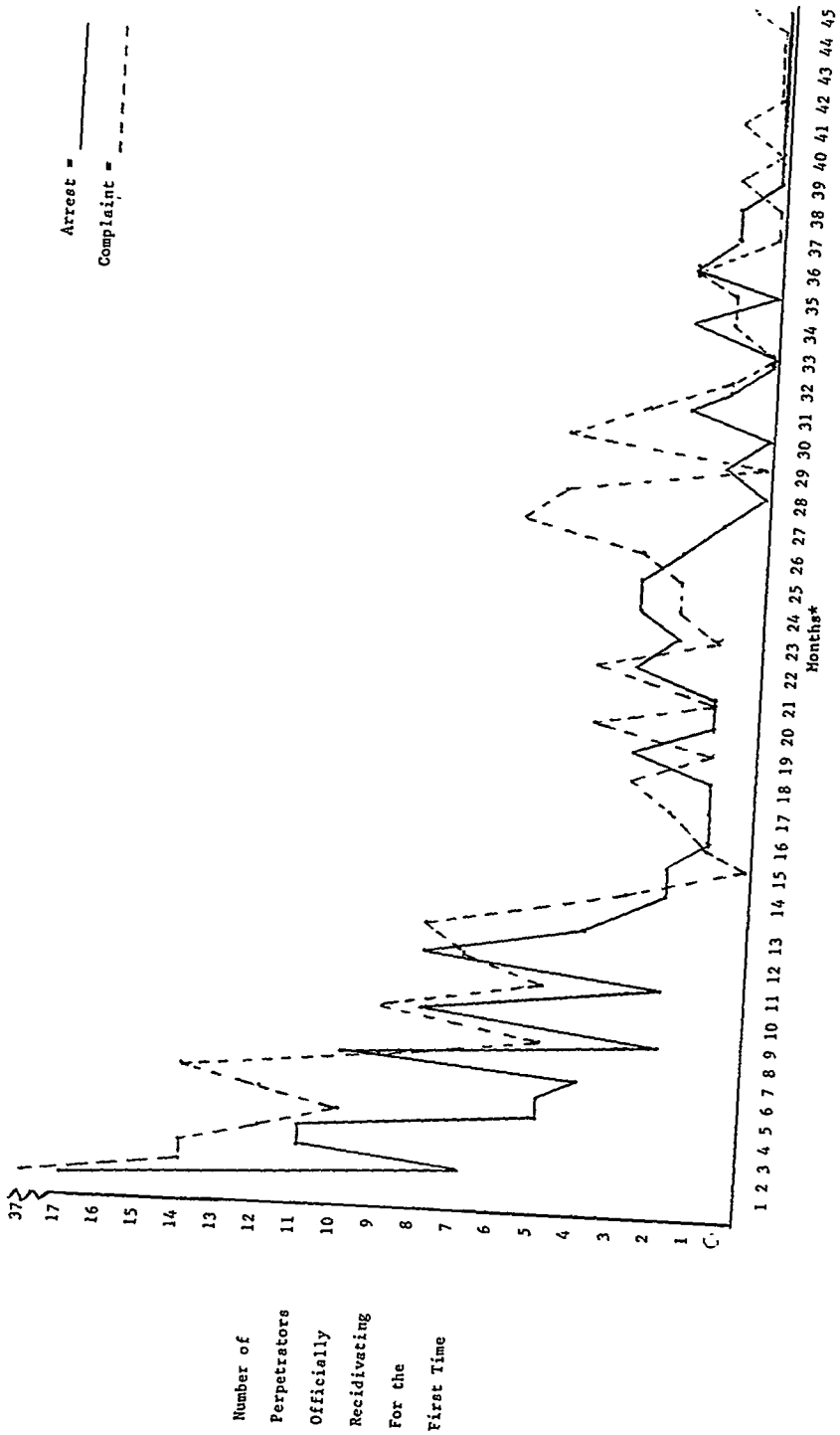


Figure 2: Number of Perpetrators at Risk of First Repeat Arrest or Complaint Who Were Arrested
 * A month is defined as 4 weeks

presenting offense for all 577 suspects) were recorded in the first year following the presenting offense. Similar results were obtained when total numbers of failures (i.e., more than first failures) were compared across the two-year period. These data indicate that victims are at highest risk of official recidivism immediately after the presenting offense and that the risk is inversely related to time. Second, although the numbers of first time recidivists are not large after the first year, somewhere between twenty and thirty percent of the perpetrators continued to be identified officially as abusing their cohabitant partners for the first time long after the first year had concluded.¹⁵ It is clear that not all first instances of official failure take place within one, six or even twelve months after the presenting offense. Although the majority of official failure is recorded early, one can only speculate as to the amount of violence that would surface if victim or self-reports of continued violence were available after the first year. If our assumptions about the limitations of official data are even partially correct, the frequency of *continuing* abusive behavior directed at spouses or cohabitants could be sizable. That is, the absence of police records for repeated spouse assault does not necessarily mean that assaults are not continuing to occur.

VIII. CONCLUSIONS

The analysis of the Omaha replication of the Minneapolis Experiment using twelve months of outcome data did not alter findings based on a six-month follow-up period. The amount of recidivism occurring after the first six months of follow-up, however, certainly does not foster confidence in the assumption that if perpetrators of domestic abuse are going to repeat they will do so within six months of the presenting offense. Nor does it provide support for the assumption that a six-month follow-up period is sufficient time to measure failure accurately.

A number of relevant and significant findings pertaining to the prevailing assumptions about the time required to measure failure in cases of domestic abuse emerged from the analysis of all 577 cases in the Omaha experiment. First, repeat offending in the second six months was more likely to involve new repeat offenders than it was to involve the same perpetrators repeating in both time peri-

¹⁵ The period for which perpetrators were at risk of reoffending was shorter for those coming into the study toward the end of the sampling period. Month 27 in Figure 2 was the point at which cases were no longer at equal risk of reoffending. One of the consequences of a shorter period of risk for some portion of the sample is that the figures presented for late first time reoffending are conservative estimates of official recidivism occurring after the first twenty-six months.

ods. Proportionately, the people failing in the second six months of follow-up tended to be different people from those failing in the first six months. Second, even though more people failed in the first six months than in the second, the prevalence of offending in the second period was from seventy to eighty percent of that of the first period. Third, the frequency of repeat offending among offenders (Λ) was relatively stable across the two time periods. There was very little difference in the rate of offending among offenders for the two six-month time periods. Fourth, when the proportion of those at risk of reoffending for the first time is calculated for both halves of a one-year follow-up period, the rate of recidivism as measured by arrests was almost identical for the two halves. It was about two to one for complaints and approximately four to three as measured by victim-reported injuries. Relatively large numbers of perpetrators repeated for the first time between the 7th and 12th months of follow-up.

These findings may have significant and practical implications. If they can be approximated elsewhere, the practice of accepting six months as an adequate time period in which to assess the effects of interventions involving spouse abusers will have been directly challenged. It may well be that accurate assessments of the effects of spouse abuse programming and interventions will require multiple years of follow-up. This has clearly been found to be the case for accurately assessing the effects of court interventions for persons convicted of driving under the influence of alcohol.¹⁶ It is quite possible that our history of truncating the period in which recidivism for spouse assault is assessed has resulted in underestimates of the extent to which such abuse continues and has fostered spurious or misleading policy recommendations. The time required to measure outcomes accurately in cases of spouse assault may be affected by a variety of variables left unaddressed here, but it is obvious that significant amounts of new recidivism occurred well beyond six months after the presenting offenses of the Omaha Experiments.

¹⁶ SEE MILTON ARGERIOU & DENNIS McCARTY, *DRIVING UNDER THE INFLUENCE OF LIQUOR IN MASSACHUSETTS: A STUDY OF RECIDIVISM* (1982); Dennis McCarty & Milton Argeriou, *Rearrest Following Residential Treatment for Repeat Offender Drunk Drivers*, 49 J. STUD. ALCOHOL 1 (1988); Stephen A. Maisto, et. al., *DRIVING RECORDS OF PERSONS CONVICTED OF DRIVING UNDER THE INFLUENCE OF ALCOHOL*, 40 J. STUD. ALCOHOL 70 (1979); NATIONAL HIGHWAY TRAFFIC SAFETY ADMINISTRATION, *ALCOHOL SAFETY PROGRAM, VOLUME I: NATURE AND SCOPE OF THE NATIONAL PROGRAM* (1979).