

## Does Three Strikes Deter? A Non-Parametric Estimation

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*Abstract:* We take advantage of the fortuitous randomization of trials to provide a novel strategy to identify the deterrent effect exclusive of incapacitation. The identification strategy lets us estimate the deterrent effect non-parametrically using data solely from the three-strikes era. We find that California's three strike legislation significantly reduces felony arrests rates among the class of criminals with 1 strike by 48 percent and among the class of criminals with 2 strikes by 12.5 percent.

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## Introduction

California's "Three strikes and you're out" legislation has been called "the largest penal experiment in American history" (Zimring, Hawkins and Kamin 2001).

Formulating effective penal policy for the twenty-first century requires a careful evaluation of the results of this experiment. We focus on one aspect of this evaluation, the deterrent effect. The structure of California's three strikes law allows for a novel and powerful test of criminal deterrence.

California's three strikes law took effect in March of 1994. Despite the name, the law actually contains significant penalty enhancements for both second and third strikes. Table 1 summarizes. A "strike" is a conviction for a serious or violent felony as these are laid out in the California Penal Code (examples are given in the table). A criminal with one strike who is convicted of *any* subsequent felony (not necessarily a qualifying felony) faces an automatic doubling of the sentence length on that conviction and cannot be released prior to serving at least 80% of the sentence length. A criminal with two strikes who is convicted of any subsequent felony faces a prison sentence of 25 years to life and cannot be released prior to serving at least 80% of the 25-year term. Although other states have passed similar sounding laws, the California law is by the far the most severe and only California enforces the law with regularity (Zimring, Hawkins and Kamin 2001, Shepherd 2002).

An initial study of the three strikes law from the California Attorney General's (CAG) Office claimed that it resulted in 5000 fewer murders and a million fewer criminal victimizations (Lungren 1998). The CAG study, however, merely compared the drop in crime in CA between 1994 and 1996 to the drop in crime in other states and concluded

that the difference represented the effect of the law. Even a simple improvement in methods such as an examination of before and after trends finds much smaller and generally insignificant effects (Stolzenberg and D'Alessio 1997, Zimring, Hawkins and Kamin 2001, Greenwood and Hawken 2002.) The most credible analysis is by Shepherd (2002) who uses a structural model estimated with county level data and county fixed effects. Shepherd finds that in its first 2 years the three strike law resulted in just 8 fewer murders but several thousand fewer aggravated assaults and nearly four hundred thousand fewer burglaries.

Our approach is quite different from any previous effort. Rather than examining aggregate crime rates and trying to correlate these with a measure of three strikes law we follow a large subset of criminals who were released from prison in CA in 1994. We estimate the deterrent effect by comparing the subsequent arrest profiles of criminals who were released with 1 and 2 strikes to criminals released with 0 and 1 strike respectively. We condition on the number of trials thereby taking advantage of a fortuitous source of randomization.<sup>1</sup>

Our approach has several virtues. First, we do not need to engage in any pre and post-law comparisons in order to identify a causal effect. All of the previous papers ultimately rely on some such comparison - Shepherd (2002) does employ a number of procedures to control for confounding factors but we know from other areas in labor economics that such procedures are not always successful or compelling (Angrist and Krueger 1999).<sup>2</sup>

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<sup>1</sup> Nagin (1998) and Cameron (1988) review the literature on deterrence.

<sup>2</sup> The key treatment variable in Shepherd (2002) measures the amount of three strikes enforcement across counties and across time – thus it combines a pre and post law comparison and an across county comparison but no indication is given whether one source of variation is of greater importance in driving the results.

Second, because we focus on criminal behavior rather than crimes we measure deterrence exclusively. Most papers on criminal deterrence cannot distinguish deterrence from incapacitation (for a discussion see Kessler and Levitt 1999). The distinction is vital for policy because the same reduction in crime is very much cheaper if produced by deterrence than if produced by incapacitation. (In addition, the existence and size of the deterrent effect is considerably more controversial than that of incapacitation.) If there were only an incapacitation effect, then as Kessler and Levitt (1999) point out, sentence enhancements like three strikes law would not change crime rates until the addition to the sentence began to be imposed. Thus they argue that crime reductions contemporaneously associated with the passage of sentence enhancements must be due to deterrence. The argument is a good one to the extent that sentence enhancements do not ratchet up punishment to such a degree that substantial prison time occurs only because of the sentence enhancement. However, given that the sentence enhancements under CA's three strikes law occur for *any* felony conviction this will not always be true. The case of Leandro Andrade, who was sentenced to 50 years to life for two petty theft shoplifting incidents involving a total of \$153.54 of videotapes, is illustrative of the possibility (see LOCKYER V. ANDRADE 270 F.3d 743, reversed).

Our approach does have a significant limitation. As noted above, we estimate the deterrent effect by comparing the arrest profiles of criminals with 1 and 2 strikes to criminals with 0 and 1 strike respectively (conditioning on the number of trials). We think that this provides a more compelling estimate of the causal effect of three-strikes law on deterrence than other approaches. Reduction in bias usually comes at a price, however, and in this case the price is that we are not able to identify the deterrent effect

of the law on those with 0 strikes. As Shepherd (2002) notes, “fear of the first strike” can deter potential criminals from ever launching a criminal career thus our procedure estimates marginal deterrence rather than total deterrence.

## **Data and Econometrics**

The data for this study are derived from the United States Department of Justice Bureau of Justice Statistics’ Recidivism of Prisoners Released in 1994 (BJS 2002).<sup>3</sup> The data covers 38,624 individuals who were released from prison in 1994. The data contains extensive information on the released prisoner’s entire criminal history prior to release.<sup>4</sup> We have data, for example, on one releasee whose first arrest was in 1944. The criminal history data lets us precisely identify the number of potential strikes a criminal has upon release in 1994.<sup>5</sup> The three-strikes law become operative on March 4, 1994 which is when we define the onset of risk.<sup>6</sup> The data also contains equally extensive information on the criminal history of each releasee in the three years following releasee.

The data covers releasees from 15 states, our specific dataset is derived by eliminating all non California releases. We also eliminate a small number of individuals who were arrested on the same day as their release as these arrests appear to have been arrests on previous charges.

We estimate the survival function, the probability of not being arrested as of time  $t$ , using the non-parametric Kaplan-Meier model. The KM model produces an estimate of the survivor function  $S(t)$ , as

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<sup>3</sup> ICPSR number 3355.

<sup>4</sup> The history is derived from state and FBI RAP sheets “Records of Arrests and Prosecutions.” Juvenile records and records regarding petty offenses such as traffic violations are not generally included.

<sup>5</sup> Prosecutors are required by law to allege and prove all strike convictions of which they are aware although some prosecutors are more lax than others in researching criminal histories.

<sup>6</sup> The three strikes law is likely to deter some crimes even before being implemented because a serious or violent crime committed before the law is passed is counted as a strike when computing penalties for future crimes. We thus also ran our models including January and February of 1994 as at-risk months. Results were virtually identical.

$$\hat{S}(t) = \prod_{j|t_j < t} \left( \frac{n_j - d_j}{n_j} \right)$$

where  $n_j$  is the number of released prisoners at risk at time  $t_j$  and  $d_j$  is the number of arrests at time  $t_j$ .

As the above equation makes clear there are two critical values in the computation. The first is the number of arrests (“failures”). The second is the number of released prisoners still at risk,  $n_j$ . The construction of the latter is complicated by the fact that individuals can exit and reenter the risk set. Consider individual number 2150. He is released on October 27, 1994. Two days later he is arrested on drug trafficking charges that are later dropped. He is arrested again on March 8, 1995, tried on May 9, 1995 and sentenced to prison. He is released from prison on January 7, 1996 and is not rearrested during the remainder of the three year tracking period which ends on October 29, 1997. Two issues arise. Clearly, 2150 is not at risk between May 9, 1995 and January 7, 1996 when he is in prison. More problematic is whether 2150 is at risk between March 8, 1995 when he was arrested and his trial on May 9, 1995. Most defendants are released before trial but a substantial minority are not and the data does not contain any information on pretrial release status. In general, it is better to assume that the individual is not under observation during the questionable period because missing observations per se will not bias the results so long as we systematically exclude such individuals from both the risk set and the failure set. Thus, in the results below we have assumed that individuals are not under observation in the spell between arrest and trial dates. To check for robustness, however, we have also estimated the model by

assuming that all such individuals remain under risk. The results, available upon request, are virtually identical.

A graph of 2150's duration is found in Figure 1.

## **Identification Procedure**

Ideally, to identify the causal effect of a treatment we need to observe two outcomes: an outcome if the individual is treated,  $Y_T$ , and an outcome if the same individual is not treated  $Y_{NT}$ . The treatment effect is then  $Y_T - Y_{NT}$ . But a direct computation of  $Y_T - Y_{NT}$  is impossible because we cannot observe an individual in the counter-factual state of the world where treatment did not occur (Rubin 1974). All methods of evaluation, therefore, must replace counter-factuals with “comparables.” An untreated group is a good comparable for a treated group if on average the treated and untreated group do not differ. Good comparables are statistical dopplengangers. The best case for comparability occurs when treatment is randomly assigned. In the absence of random assignment we must seek out a fortuitous source of random variation in treatment – a so called “natural experiment.”

Criminals with 1 or 2 previous strikes face enhanced penalties if they commit an additional felony. Thus, the “treatment” in our application is an individual's stock of strikes. The causal effect on deterrence of having 2 previous strikes, for example, is the difference between the actual number of crimes committed and the number of crimes that would have been committed if the same individuals had say 1 previous strike. Consider the naïve procedure of comparing the criminal histories of criminals with 2 strikes with

the criminal histories of those with 1 strike. Even if we were to control for other variables, the latter group is unlikely to be a good comparable for the former. Criminals with 2 strikes have more strikes than those with 1 strike for a reason - most probably because they engage in more criminal activity. Thus, we need a source of randomization that creates two similar individuals yet with different strike records.

Our strategy to identify the causal effect of two and three strikes laws on criminal activity is to make use of the fortuitous randomization of trial. Our identifying assumption is that individuals with the same number of *trials* are comparable even when their strike histories (the outcomes of the trials) are different.<sup>7</sup> The random trial outcome serves as a random treatment assignment. Thus, to ascertain the deterrent effect of three-strikes we compare individuals with 2 strikes and trials with individuals with 1 strike and 2 trials. Note that the outcome of a trial is to a considerable degree stochastic. How strong is the evidence? How good is the defense lawyer relative to the prosecutor? How lenient or strict is the judge or jury? How eager is the prosecutor? Consider a criminal with 1 previous strike who is on trial for a second strike. We can easily imagine this criminal thinking himself *lucky*, in the sense of being the recipient of fortuitous random events, when the evidence is found insufficient to justify a strike conviction.

One would hope that an individual found not guilty of a strikeable offense is more likely to be truly innocent of that offense than one found guilty. And one would also hope that the truly innocent have subsequent arrest histories different from the truly guilty. To the extent that this is true our results are biased (but importantly note that they are biased against our finding a positive deterrent effect of two and three strike laws).

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<sup>7</sup> We use the term “trial” to cover any form of adjudication, which is either a jury trial, a bench trial or judicial hearing where the individual may have pled guilty. “Trials” do not include cases where the individual is arrested but the prosecutor drops the charges. To go to “trial,” therefore, implies a stronger case for guilt than does an arrest.



Note, however, that all of the individuals in our sample were in prison prior to being released in 1994. Furthermore, we do not compare individuals with the same number of arrests but rather individuals with the same number of arrests with evidence that the prosecutor thought serious enough to warrant some form of adjudication. Thus, our sample is highly selected against the truly innocent. Also note that not being convicted of a strikeable offense most often means being convicted of a lesser non-strikeable offense – it does not necessarily mean being found to be innocent of all charges. Later we provide some tests of whether our identifying assumption is plausible.

Even if our identification procedure produces fortuitous randomization across individuals it may not do so across crimes if crime conviction rates and subsequent arrest hazards differ. We discuss and control for this issue further below.

## **Results**

Panel A of Figure 2 presents the *unconditional* survival functions of released prisoners with 1 and 2 strikes (where survival means not rearrested). Releasees with 2 strikes, who are subject to a third strike for any new felony conviction, are *more* likely to be rearrested than those with 1 strike. The difference indicates a considerable selection effect and verifies the difficulty with the naïve estimator discussed above. Panel B focuses attention on the subsequent arrest history of releasees with 1 or 2 strikes *conditional* on every releasees having faced two trials. In distinction to the results of Panel A, the survival functions now reverse – criminals with 2 strikes are *less* likely to be subsequently arrested. The difference in the survival functions is statistically significant

at the greater than 5% level ( $p=.02$ ) using a Peto-Peto test (Peto and Peto 1972).<sup>8</sup> The difference in arrests rates is also large enough to be of policy significance. We estimate that the threat of a third strike reduces arrest rates by 5.7 percentage points or 12.5 percent or (an arrest rate of .456 per year with 1 strike and .399 with two strikes).

The third strike of California's "three strikes" law has generated the most attention because of the potential severity of the punishment relative to the seriousness of the third strike. Yet, the "second strike" provisions of the law are also serious and apply to a much larger population. A second strike - any felony conviction subsequent to a "violent" or "serious" felony - requires a mandatory prison sentence of twice the term of the qualifying felony and no release before serving 80% of that term (CA Penal Code S. 667, Zimring, Hawkins, Kamin 2001).

Figure 3 presents the survival functions of those released from prison with 0 or 1 strikes. As before, Panel A contains the unconditional survival functions while Panel B shows the survival functions conditional on having had 1 trial for a strikeable offense. The survival functions in Panel A are very similar which could incorrectly suggest that second strike provisions do not deter. Once we condition on having had a trial for a strikeable offense, however, the arrest histories are very different with criminals with 1 strike having a substantially lower arrest rate (greater survival rate) than those with 0 strikes. The per-year arrest rate for those with 0 strikes is .455 while the arrest rate for those with 1 strike is .237, a dramatic reduction of 21.8 percentage points or 48 percent. The difference is statistically significant at the greater 1% rate ( $p=0.000$ ).

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<sup>8</sup> The Peto-Peto test allows for non-proportional hazards and is not affected by possible differences in censoring patterns across the groups. Results are very similar with other standard tests on survival functions such as the log-rank or Wilcoxon test.

## Testing the Identification Strategy

It is clear that the unconditional survival functions for those criminals with 1 or 2 strikes confound a variety of selection effects with the causal effect of three strikes deterrence. Our identification argument, however, asserts that conditioning on having had two trials creates two comparable groups such that the difference in arrest behavior of the 1 and 2 strike groups is due solely to the deterrent effect of the three strike law. If this argument is correct we ought to find A) if we condition on two trials for groups *not* subject to the three strikes law there ought to be no difference in the subsequent arrest histories also if B) we do controlling for other factors this not significantly change the results.

If conditioning on two trials creates two comparable groups such that the *only* reason the survival functions of the two groups differ is the deterrent effect then conditioning on having had two trials for groups not subject to deterrence should produce identical survival functions.

In Figure 4 we examine the time to rearrest of criminals with non-strikeable convictions. Panel A shows the unconditional survival functions. As before criminals with 2 (non-strikeable) convictions are *more* likely to be rearrested. Conditional on having had two trials, however, the survival functions of criminals with 1 and 2 non-strikeable convictions are statistically identical ( $p=.5$ ). Thus, in the tradition of Koch's postulates, our identification procedure finds a deterrence effect when deterrence is expected to operate and no effect is found when deterrence is not expected to operate.

Although our identification procedure will randomize within crimes, differing conviction rates across crimes will lead to a different mix of crimes in the two categories.

If the hazard rates for further felony arrests differ significantly across crime histories this could be a source of bias. (Note, however, that previous strikes result in penalty enhancements for *any* new felony conviction so the issue is not the recidivism rate for the same crime but the recidivism rate for any new felony and the latter is likely to differ by less than the former by crime history). To control for this possibility and the possibility that other demographic factors may differ even after conditioning by trial we ran a semi-parametric Cox proportional hazards model.

The Cox model assumes that the hazard rate for releasee  $j$  is

$$h(t | x_j) = h_0(t) \exp(x_j \beta),$$

where  $x_j$  are the independent variables and  $\beta$  are the coefficients to be estimated. The principle advantage of the Cox model is that it does not require any assumptions about the shape of the baseline hazard rate (which is not even estimated). Instead, the Cox model estimates the proportional change in the hazard rate with respect to the independent variables. Note that the coefficients reported in Table 2 are exponentiated and so can be interpreted directly as the proportional shift in the hazard rate (probability of arrest).

Included as regressors,  $x_j$ , in the proportional hazards model are a series of demographic variables including age and age at first arrest as well as crime variables that indicate the mean number of arrests for each type of crime that the individual received prior to release in 1994. Thus the crime variables control for a criminal's history. We focus on the main strikeable crimes. As before we continue to control for the number of trials so the model is run on data from criminals with 1 or 2 trials respectively.

The basic results on the control variables can be quickly summarized. The probability of rearrest declines in age and age at first arrest and is higher for blacks than non-blacks. Rearrest is also higher among crimes of income such as robbery, assault and burglary and is higher for those criminals who carry a weapon.

The results of most interest are that criminals with 1 or 2 strikes have a reduced probability of arrest compared to those with 0 or 1 strike respectively (recall that this is controlling for the same number of trials and now also controlling for demographics and crime histories). From column 1 we note that at any point in time the probability of rearrest for those criminals with 2 strikes is .87 times the rate for criminals with 1 strike. Alternatively stated the probability of rearrest for those with 2 strikes is 13 percent lower than for those with 1 strike – this is almost identical to the 12.5 percent reduction we found using the non-parametric estimation. In column 2 we note that the probability of rearrest for criminals with 1 strike is .71 times the rate for those with 0 strikes or 29 percent lower – this is large but lower than the non-parametric estimate of 48 percent.

### **Total Crime Deterrence**

We find that California's three strike legislation reduces felony arrests rates among the class of criminals with 1 strike by 48 percent (using the non-parametric estimate) and among the class of criminals with 2 strikes by 12.5 percent. These are significant figures but note that most crime is committed by criminals who do not have any strikes. Using samples from 3 large California cities, Zimring, Hawkins and Kamin (2001) estimate that 9.6% of felony arrestees have 1 strike and just 4.3% have 2 or more strikes. Using these figures we can do some back-of-the-envelope calculations concerning reductions in total crimes. Assuming that crimes are deterred in the same

ratio as arrests and using the 1996 figure on FBI Index crimes for California of 1,675,435 (the FBI index is approximately a felony arrest index) we find that the two strike provision of the law deter 77,204 crimes and the 3-strike provision 9005 crimes.<sup>9</sup> Recall that these figures are a lower bound on total deterrence.<sup>10</sup>

## **Conclusion**

We take advantage of the fortuitous randomization of trials to provide a novel strategy to identify the deterrent effect exclusive of incapacitation. The identification strategy allows us to estimate the deterrent effect non-parametrically using data solely from the three-strikes era. We find that California's three strike legislation significantly reduces felony arrests rates among the class of criminals with 1 strike by 29 to 48 percent, depending on the precise model, and among the class of criminals with 2 strikes by 12.5 percent.

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<sup>9</sup> Crime and arrest figures for the state of California are available from the website of California's Department of Justice, <http://caag.state.ca.us/cjsc/datatabs.htm>.

<sup>10</sup> Shepherd estimates approximately 400,000 total crimes deterred. Since the number of crimes committed by criminals with 0 strikes is large, it is not difficult to resolve the differences between our figure and that of Shepherd. If deterrence of those with 0 strikes is say 22 percent (less than half the rate reduction of those with 1 strike) then our total deterrence figures are similar.

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Table 1: Summary of California's Three Strikes and You're Out Law			
Examples of Qualifying Convictions: Murder, rape, robbery, attempted murder, assault with intent to rape or rob, any felony resulting in bodily harm, arson, kidnapping, mayhem, burglary of occupied dwelling, grand theft with firearm, drug sales to minors, any felony with deadly weapon.			
	Required Prior Record	Current Offense	Enhancement
Second-strike	1 Qualifying Conviction	Any felony	Twice the current offense term with no release before serving 80% of that term.
Third-strike	2 Qualifying Convictions	Any felony	25 years-to-life with no release before serving 80% of the 25-year term.
Source: California Penal Code S. 667 Zimring, Hawkins, Kamin (2001).			



Table 2: Proportional Hazards Model, Dependent Variable: Arrest		
	(1)	(2)
	Hazard Ratio	Hazard Ratio
Age at Release	0.965 (0.005)**	0.974 (0.004)**
Age at first arrest	0.983 (0.007)*	0.977 (0.004)**
Releasee is black	1.319 (0.089)**	1.374 (0.085)**
Releasee is Hispanic	0.925 (0.062)	0.955 (0.056)
Releasee is male	0.860 (0.117)	0.902 (0.096)
Number of murder arrests	1.061 (0.154)	1.124 (0.163)
Number of rape arrests	0.986 (0.058)	1.010 (0.059)
Number of kidnapping arrests	0.868 (0.117)	1.070 (0.127)
Number of robbery arrests	1.385 (0.065)**	1.436 (0.087)**
Number of assault arrests	1.177 (0.028)**	1.244 (0.037)**
Number of other violent crime arrests	1.068 (0.070)	1.136 (0.100)
Number of burglary arrests	1.472 (0.052)**	1.698 (0.069)**
Number of arson arrests	1.197 (0.248)	0.990 (0.229)
Number of arrests for weapons violations	1.273 (0.052)**	1.398 (0.059)**
Two strikeable convictions	0.880 (0.050)*	
One strikeable conviction		0.719 (0.052)**
Observations	2239	3716
Standard errors in parentheses		
* significant at 5%; ** significant at 1%		

Figure 1

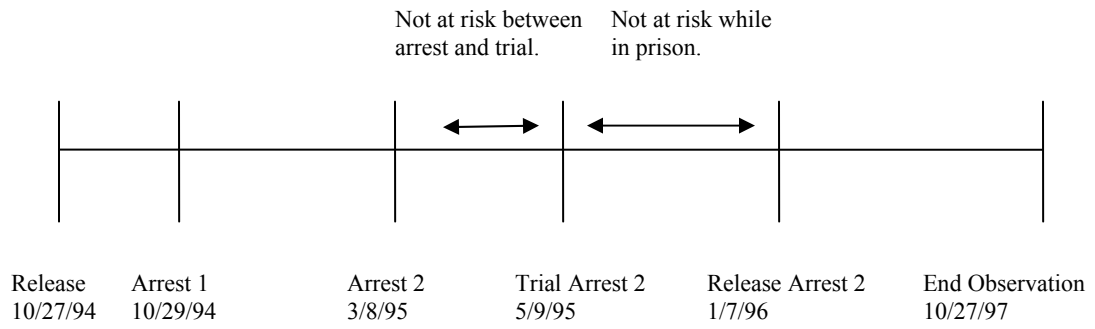


Figure 2

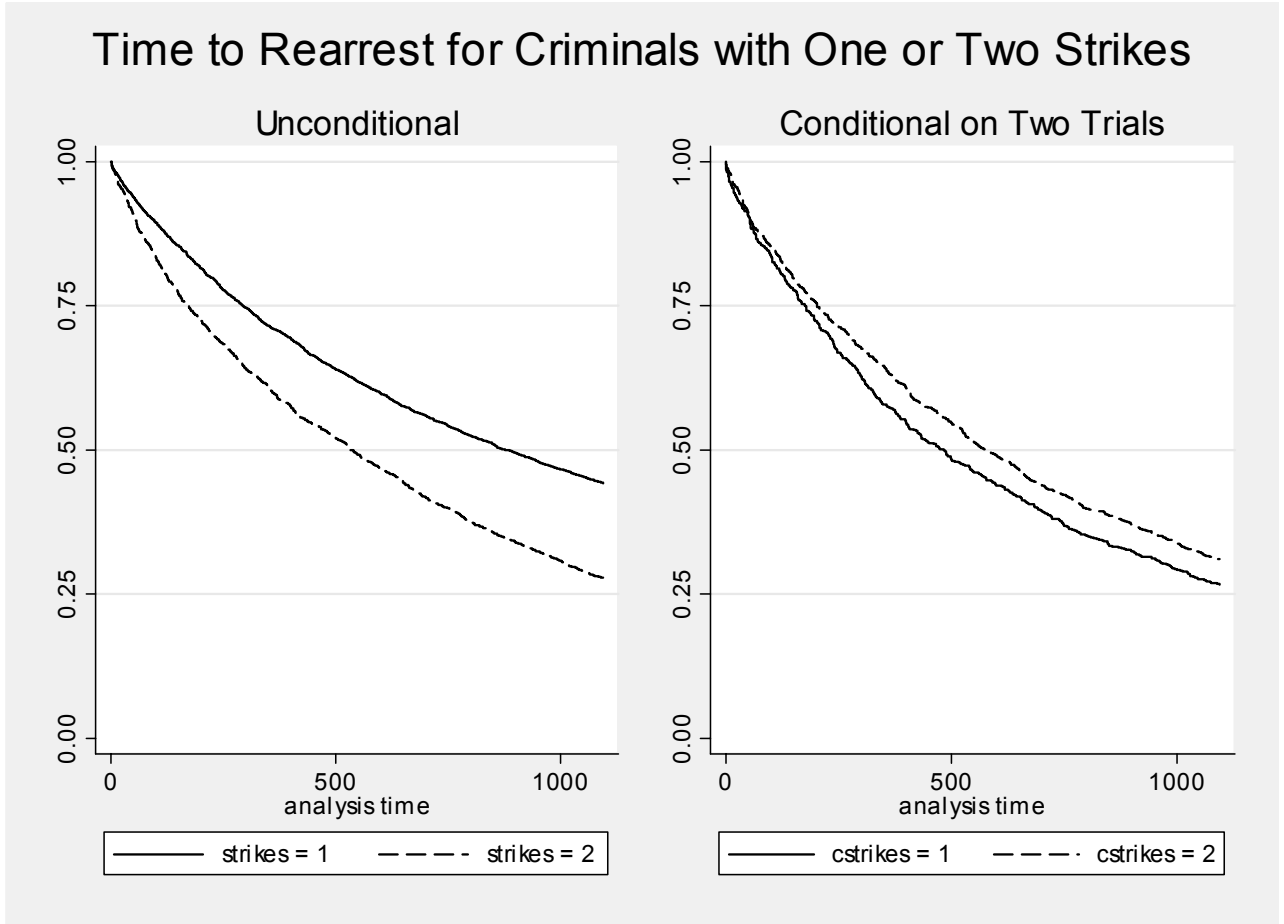


Figure 3

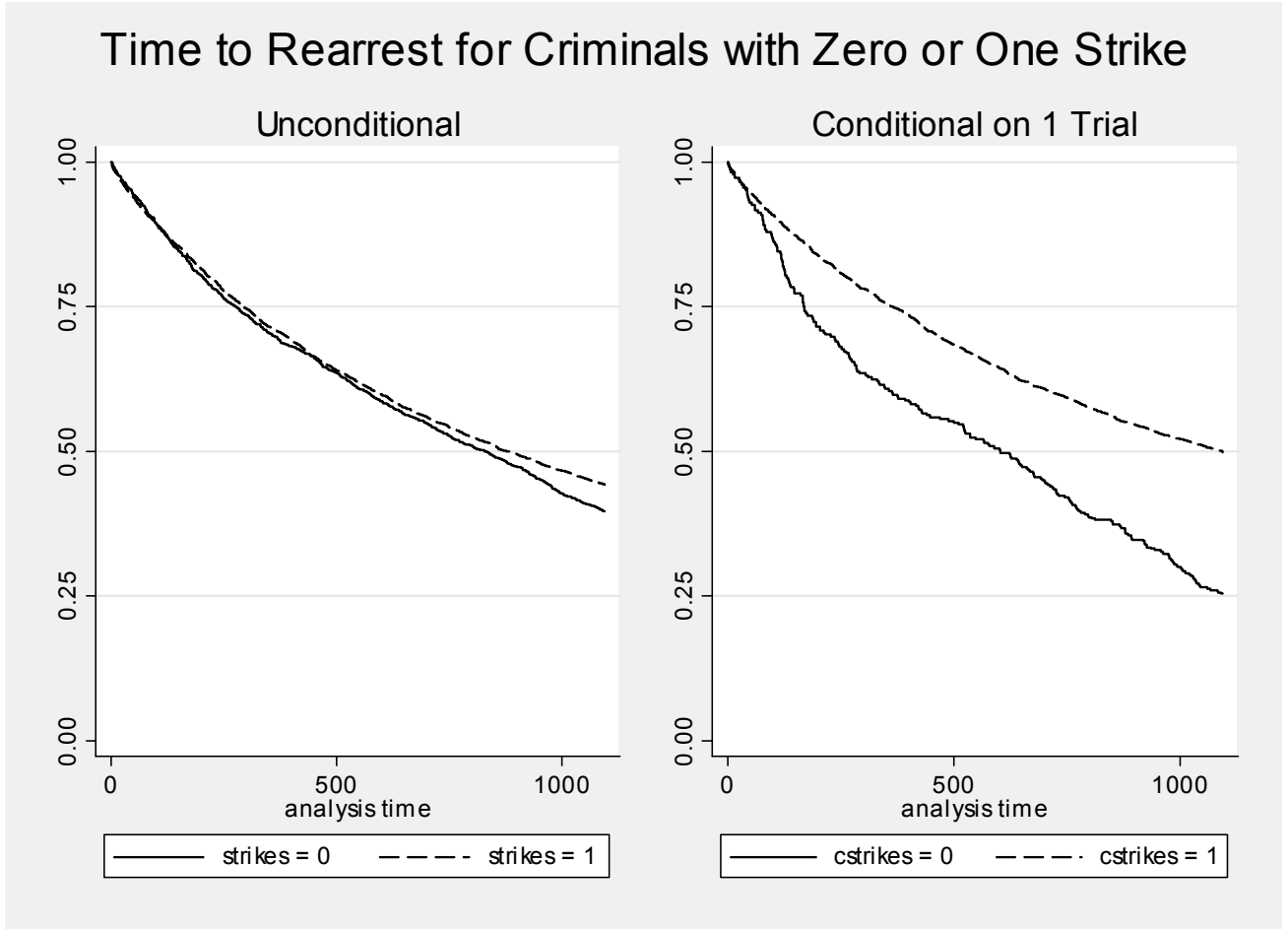


Figure 4

