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Facts, Fallacies, and California’s Three Strikes

Franklin E. Zimring and Sam Kamin*

The Duquesne Law Review recently published an article by Brian Janiskee and Edward Erler1 that contains a number of strong criticisms of a study we conducted on the impact of California’s Three Strikes and You’re Out legislation on crime and punishment in California.2 In particular, four of these criticisms require a reply from us because they combine substantial misunderstanding of our research methodology with insinuations about the honesty and competence of the authors of the study. In addition to these methodological differences, we also object to two demonstrably false statements about the content of the report. Finally, we discuss the aspect of the critique that we find to be the most disturbing — namely, its distaste for what it purports to be, an empirical analysis of criminal justice policy. Each of these topics will be discussed after a brief summary of our study’s findings.

I. THE THREE STRIKES DETERRENCE STUDY

In the Crime & Punishment monograph, we used two methods to assess the impact of the Three Strikes law, passed by the California legislature and signed into law in March 1994, on crime rates. First, we analyzed seasonally adjusted monthly crime levels for nine major California cities from 1990 through 1996 — in other words, from 50 months before Three Strikes became law to 33 months after the law went into effect. We concluded that this monthly crime data did not clearly indicate a deterrent effect attributable to the 1994 law. While crime rates did decline after March of 1994, we observed that the crime rate had actually started to decline 17 months prior to the enactment of the Three Strikes legislation, and the slope of the decline did not change appreciably

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* Gordon Hawkins was unable to participate in this response.
2. ZIMRING ET AL., CRIME AND PUNISHMENT IN CALIFORNIA: THE IMPACT OF THREE STRIKES AND YOU’RE OUT (Institute of Governmental Studies Press 1999). The study discussed in that article had since been expanded into a book: PUNISHMENT AND DEMOCRACY: THREE STRIKES AND YOU’RE OUT IN CALIFORNIA (Oxford University Press 2001).
after March of 1994. In other words, crime was dropping in California before Three Strikes went into effect and continued to drop at the same rate after the law went into effect.

We then analyzed the specific terms of the 1994 legislation to create a more refined test of deterrence. Three Strikes changed the penalties for two defendants with one or two prior strikes; however, penalties for defendants without strikes on their record remained the same as before. Thus, it can be expected that the deterrent effect of the new legislation will be greatest on those defendants with prior strikes on their record. If this were true, we should expect to find fewer repeat offenders in the offending population after the law went into effect than we did before the law went into effect. In order to test this hypothesis, we sampled data from of several thousand arrests occurring before and after the new law in three California cities, and analyzed and coded the criminal records of the defendants.

This study produced three significant findings on deterrence. First, the maximum percentage reduction in crime rate achievable by targeting the “strike groups” was smaller than we thought. That is, based on the data collected, persons eligible for a third strike sentence were responsible for about 3.3% of California urban felony arrests before Three Strikes, and the second strike group was responsible for an additional 7.3%. Therefore, we concluded, if all crime by these offenders ceased, the resultant reduction in crime rate would be just over 10%.

The second finding was that the share of arrests attributable to the second strike group did not change when pre-and post-Three Strikes samples were compared. In other words, those eligible for second strike punishment under the 1994 law made essentially the same contribution to California crime after the law’s passage as they did before.

The third finding was that those eligible for the most serious sentences, the third strike group, declined from only 3.3% of all arrests to 2.7% of arrests.

In generalizing the results of our three-city study to the state as a whole, we found that the Three Strikes law reduced California crime by only six-tenths of one percent. This contradicts the claim by California’s Attorney General that the drop in crime during the “Three Strikes Era” was over 30%. In fact, our final estimate of the deterrent effect in the monograph under review was between zero

3. ZIMRING ET AL., supra note 2, at 86.
II. THE JANISKEE/ERLER CRITICISMS

The article we are responding to made four purportedly methodological criticisms of our study. The first of these was that we should not have been studying deterrence:

The principal conclusion of Zimring is that the Three Strikes legislation fails to provide any measurable deterrent effect on the target groups. In coming to this conclusion, Zimring violates the first principle of policy analysis — that any law or policy must be understood in terms of its intent. It is a palpable fact that the California legislature did not intend the principal purpose of the law to be deterrence, but rather “to ensure longer prison sentences and greater punishment of those who commit a felony and have been previously convicted of serious and/or violent offenses.”

However, this criticism fails to comprehend that our study did in fact analyze short-term incapacitation in addition to deterrence. Furthermore, while our critics are correct in stating that the Three Strikes law was originally justified on the basis of incapacitation, they fail to mention that after the law was passed, claims that declining crime rates were the result of Three Strikes deterrence proliferated. Indeed, even Janiskee and Erler allude to the deterrent power of the Three Strikes legislation in their critique of our study. This attempt to have things both ways, that is, to claim a deterrent effect while at the same time objecting to our focus on claims of deterrence, is internally inconsistent.

The second assault on our methodology alleges that “the study derives from the statistical conflation of arrests and crime.” Our study assumes that the three different groups of offenders (those with zero, one, and two or more strikes) face the same chance of arrest per 100 crimes committed. That is, we deduce from the fact that 7% of those arrested have one strike on their records that 7%
of crime in the general population is committed by members of this group.\textsuperscript{10}

Two problems with this assumption are suggested. First, Janiskee and Erler believe that the arrest rate per 100 crimes committed will be \textit{higher} for second and third strike eligible defendants.\textsuperscript{11} We do not think this is true, but if it were the case, it would \textit{further decrease} the share of crime that these special target groups commit and thus \textit{further decrease} the potential crime saving of a Three Strikes program. If the odds of being caught were twice as high for third strike eligible defendants than for those without any strikes on their record, their 3.3\% share of all arrests in April of 1993 would be evidence that they committed only 1.7\% of California crime during that period. Thus, even if our assumption that arrest rates are a good proxy for commission rates is inaccurate — and we do not believe that it is — this would only strengthen the case against Three Strikes as a major crime prevention tool.

The second objection about what our critics call “conflation” is that some of the criminal offenses for which post-Three Strikes arrests occurred might have been for crimes committed before the new law went into effect. The authors assert that “[i]t is not uncommon for an arrest to occur many months — even years — after the fact,” without any citation to any authority or statistics.\textsuperscript{12}

While some robberies, burglaries, and larcenies that occurred before April 1 may lead to April arrests, the precipitating event that produces an arrest is almost always a recent crime.\textsuperscript{13} The bias that

\begin{itemize}
\item \textsuperscript{10} Zimring \textit{et al.}, supra note 2, at 41-43.
\item \textsuperscript{11} Janiskee and Erler, supra note 1, at 49.
\item \textsuperscript{12} \textit{Id.}
\item \textsuperscript{13} There are three reasons why the overwhelming majority of arrests are the product of criminal acts quite proximate to the date of arrest. First, the overwhelming majority of felony arrests are for offenses at the low end of the seriousness scale. In the United States, there were 1,166,362 arrests for larceny in the United States compared to 13,277 for murder, a ratio of 88.2 to one. The ratio of drug arrests to murder arrests was 119.4 to one for year 2000 (see F&I UCR 2000 at pp. 216). This bottom-heavy pattern is also found for the arrests that make suspects eligible for second and third strike treatment in the 1994 and 1995 samples (see Zimring, Hawkins, and Kamin 2001 at Chapter 4). For crimes of low seriousness, there is no incentive for sustained investigation. Arrest is a same-day phenomenon.

The second evidence of minimum time lag between crime commission and arrest is the fact that most arrests are made not by detectives, who are responsible for investigation, but by patrol officers (see e.g. Indianapolis Police Department Annual Report for 2000). A patrol officer only makes arrests he or she happens across. The time lag in this majority of cases is zero.

The third piece of evidence on the short time between crime and arrest concerns the effort and attention span of detectives. In \textit{The Criminal Investigation Process}, Jan Chaiken, Peter Greenwood, and Joan Petersilia report that for Pittsburgh detectives, the average case file is active for only 3.8 days before investigative effort is suspended (Chaiken, Greenwood,
could result if second and third strike offenders were arrested after April 1 of 1994 solely for offenses committed prior to March 8 (when the Three Strikes law went into effect) would be that the percentage of targeted groups in the arrest population would be higher than the percentage of criminal acts committed by the targeted groups after the new law went into effect. The reason we doubt that this happened is that the percentage of arrests for both the second and third strike groups was the same in the second month of Three Strikes (April 1994) as it was in the 14th month after the law took effect (April 1995). If there were persons arrested for crimes committed before March 8, 1994, we would expect this group to be found in the April 1994 sample, but not the April 1995 sample, resulting in a higher proportion of second and third-strike defendants in the April 1994 sample. However, as we report in the monograph, this was simply not what was observed.

The third major complaint about the study is that we sampled for deterrent impact too early in the career of the Three Strikes law:

One of the most obvious defects in the research design is the inexact placement of the longitudinal samples. The Three Strikes law went into effect in March 1994. Zimring took the first post-Three Strikes sample from April 1994, only one month after the effective date of the legislation and the second sample from April 1995. . . . By taking samples so soon after the law went into effect, Zimring set the bar unreasonably high for judging the success or failure of Three Strikes. The study seems therefore to be designed to show no effect for the new law — otherwise, how can this unorthodox research design be explained?

In making this criticism, the authors cite no research literature on general deterrence. In fact, the literature on deterrence suggests that because publicity and public concern are generally at their maximum around the time of legal change, the closer the observation to the change, the greater the chance that the effect of the legal change will be detected.

One wonderful example of this phenomenon was reported in H. and Petersilia, 1977 at Table 8.7, p. 118). There are, of course, exceptions to the rule in crimes of high seriousness. And detectives will often clear up a host of old offenses when they clear a new crime. But as long as the new crime is a few days old, there is no "conflation" on that account.

14. ZIMRING ET AL, supra note 2, at 78.
15. Id.
16. Janiskee and Erler, supra note 1, at 47.
Lawrence Ross's *Law, Science and Accidents*, in which Ross demonstrated that the maximum deterrent impact of the British Road Safety Act happened just *prior* to its effective date.\(^{17}\) Furthermore, it has been shown that well-publicized counter-deterrent changes like police strikes also have relatively immediate effects, presumably because publicity is greatest at or prior to the onset of such occurrences.\(^{18}\) Thus, the timing of our two large samples was not only orthodox by the standards of deterrence research, but in fact, demanded by convention.\(^{19}\)

The authors also imply that "the results would have been different had the samples been taken in 1997 or 1998."\(^{20}\) Do they mean that third strike eligible defendants are now significantly *less* than 2.7% of total California felony arrests and second strike eligible significantly less than 5.5%? That is the only way a more current sample would provide empirical evidence of delayed impact general deterrence. Given the surprisingly low contribution of the two-and three-strike offenders to California crime *before* the law went into effect, it is only if these groups had severely curtailed their criminal participation by the late 1990's that a greater deterrent effect could possibly have been found. The aggregate crime data Janiskee and Erler want us to consult tell us nothing on this question, only a replication of our design could answer that question.

A fourth innuendo-laden criticism relates not to the database we created for the monograph, but to our use of pre-existing crime and imprisonment data in the early part of the study. The three sentences that discuss these issues defy paraphrasing and are therefore reproduced in their entirety:

> It is a further matter of curiosity that Zimring, in a piece entitled *Crime and Punishment in California*, did not collect a single datum that was statewide in scope. In constructing a figure that represented statewide criminal activity, Zimring used data only from the ten most populous cities and discontinued the analysis in 1996 even though the data was

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19. We expect that we would have been vulnerable to well-informed methodological criticism if we had done anything else.

20. Janiskee and Erler, *supra* note 1, at 47.
available for 1997-98. One is left to wonder whether the data from 1997-98, if included in their time series, might have proved embarrassing to the Zimring thesis, as our figures indicate they might have.21

The reason for our use of big city crime data rather than statewide data was clearly stated in the monograph,22 and the limits of using statewide data are unfortunately illustrated by Janiskee and Erler's analysis in their critique. Statewide data, such as the annual aggregations Janiskee and Erler use to criticize our work,23 lump huge time periods together in the nation's largest state, and is not available in monthly totals. However, the data for large California cities was available for these shorter periods. Our analysis involved 84 monthly totals from the beginning of 1990 until the end of 1996.24 As described above, this month-by-month tracking showed a downturn that started after October of 1991 and continues at about the same slope for 62 intervals including 33 months after the Three Strikes law went into effect. Could 1997 and 1998 have altered the inconclusive nature of the interrupted time series? The authors state that we admit that "properly cautious interrupted time series analysis would require a distinct and sharp downward slope in crime rate proximate to the new penal regime."25 But 1997 and 1998, the years they complain were missing, were far removed from the legal change. While our critics attribute a nefarious purpose to our exclusion of the 1997 and 1998 data from the analysis, it is hard for us to conceive of an effect during those years that could be fairly attributed to the law passed back in early 1994.

The authors' analysis of homicide rates is a good example of the problems with annual aggregations. The authors compare the average year-to-year percentage change for two pre-Three Strikes years and four post-Three Strikes years.26 Having adopted a data set that can tell us nothing about the time frame just before and just after the new law, they then use an averaging technique that counts declines in 1998 just as clear a Three Strikes response as 1995 patterns.27 The formula they use is not a method of discerning time

21. Id. at 54.
22. ZmRiNg ET aL, supra note 2, at 67.
23. Janiskee and Erler, supra note 1, at 52-54.
24. ZmRiNg ET aL, supra note 2, at 67-71.
25. Janiskee and Erler, supra note 1, at 54 (emphasis added).
26. Id. at 52-54. They should of course discard all of 1994 in this analysis because it combines both pre-and post-periods.
27. Id.
trends, but instead a method of rendering them non-visible. By contrast, we were able to carefully investigate trends in the data because we chose to utilize a database of 84 different periods rather than 2.28

III. SOME NON-FACTS

Up to this point, we have focused on the differences in methodology of us and the authors of *Crime, Punishment, and Romero*. In this brief section, we must mention two factual misstatements that we believe cannot fairly be attributed to a difference of opinion.

The most striking misstatement is on page 45 of Janiskee and Erler's critique: "The study also makes the almost incredible discovery that the law had a statistically meaningless impact on criminal sentences."29

The statement is demonstrably false. An *entire chapter* of the report (Chapter 3) details the impact of the law on criminal sentences, both statewide and in the large samples the study constructed.30 What we in fact show in Chapter 3 is the following. Ninety percent of all sentences under the Three Strikes law were meted out to second strike offenders where plea bargaining can reduce the severity of the second-strike crime prior to the required doubling of the nominal sentence.31 We found that there are increases in these second strike punishments — just not anything near the doubling that a literal application of the law would generate.32 Turning to the third strike defendants, all of whom should have received a sentence of 25-years-to-life if convicted of any felony, we found that although they constituted approximately 30% of all defendants in the second and third strike pools, they account for only 10% of the sentences actually meted out under the new law.33 Based on our analysis, we concluded that while the sentences for second and third strike offenders increased in the aggregate, as few as one in ten of those who appear eligible for the full-force of the Three Strikes' penalty actually received it.34

30. ZIMRING *et al.*, *supra* note 2, at 41-63.
31. Under California’s Three Strikes law, the punishment for a second strike is double what it would be for a zero strike offender. The punishment for a third strike is twenty-five years to life regardless of the triggering felony.
32. ZIMRING *et al.*, *supra* note 2, at 42.
33. *Id.*
34. *Id.* at 44-63.
A second misstatement appears in the first paragraph of the article where readers are told, "[t]he thesis of the study is that California's Three Strikes law has failed to deter crime."35 The problem here is quite simply that the failure of general deterrence is neither the thesis of the study nor one of its conclusions. Instead, the study employs sensitive designs for detecting deterrence and, after analyzing the data, comes to very different conclusions for the second strike and third strike groups. For the second strike group, there is no evidence of any marginal deterrent effect attributable to the 1994 Three Strikes penalty increments — the proportion of second-strike offenders in the criminal population after the law goes into effect is not significantly different than the proportion of those offenders in the population before the law goes into effect. For the third strike group, we found statistical evidence of deterrence, even though we conclude that its total impact on California crime is quite small.36 Furthermore, we found a large gap between the official claims for crime reduction and the aggregate possible deterrence. We are perplexed at how an article devoted to critiquing a deterrence study can misstate that study's principal statistical findings.

IV. A Distaste for Analysis

Notwithstanding the problems with the Janiskee and Erler analysis described above, perhaps the most troubling aspect of their critique is its clear distaste for empirical analysis of the criminal justice system. This aversion to research is most clear in the second paragraph of their article: "The underlying assumption of this study — and all similar statistical studies — is that the abstract world of probability is more reliable as a basis for public policy than experience and common sense."37 However, we did what the authors urged: we observed real world events and applied common sense analytical tools to those events. Our analysis is based not on "the abstract world of probability" but on the creation of a database of actual cases, a close reading and coding of the facts of those cases, and a comparison of conditions before and after the law went into effect.

Most disturbing to us, however, is the deep skepticism of empirical research generally that is revealed by the quotation above. Janiskee and Erler seemingly urge us to leave well enough

35. Janiskee and Erler, supra note 1, at 43.
36. ZIMRING ET AL., supra note 2, at 79-81.
37. Janiskee and Erler, supra note 1, at 43.
alone because Three Strikes is working, and imply that only a cynic or someone with an axe to grind could think otherwise. Thus, the fact that they have mischaracterized our methods is hardly surprising: no empirical methodology could satisfy them. They state quite clearly that “all similar statistical studies” are a poor substitute for intuition. Thus, it seems that the problem is not with the way in which we are attempting to resolve the issues we have tackled, but that we have even asked the questions in the first place.\textsuperscript{38}

This anti-empirical viewpoint is particularly disturbing given that it is also the official position of the state of California. In 1999, Gray Davis, the democratic governor of California, vetoed a bill that would have funded an empirical study of Three Strike’s impact. In doing so, the governor essentially argued that state funds should not be spent to investigate a law that was so clearly functioning as it was intended to.\textsuperscript{39}

V. CONCLUSION

The monograph discussed in Crime, Punishment and Romero, and the larger book based on that study, are part of a substantial body of literature about issues of deterrence, incapacitation, and public policy. Unfortunately, the critique to which we are responding both fails to comprehend that literature, and does little to contribute to the scholarship in this area.

\textsuperscript{38} In our later study, we refer to ourselves and others who have dared to question the orthodoxy of Three Strikes in California as heretics:

[D]oubts about Three Strikes are not resented because they may lead to political difficulties; they are in and of themselves a denial of the normative beliefs that supporters hold. It is the heresy itself rather than what further harm it might accomplish that provokes anger of Three Strikes supporters.

\textit{Zimring et al., Punishment And Democracy, supra} note 2, at 222. It seems to us that the response of Janiskee and Erler is the response of those who have had their orthodoxy challenged.

\textsuperscript{39} \textit{Id.} at 222.