Law & Society REVIEW

Symbol and Substance: Effects of California's Three Strikes Law on Felony Sentencing

John R. Sutton

California's "three strikes and you're out" law is the most notorious example of the wave of mandatory sentencing policies that many states enacted beginning in the late 1970s. While advocates and critics predicted the law would have profound effects on aggregate punishment trends and individual case outcomes, Feeley and Kamin's analysis of previous sentencing reforms suggested the law's impact would be mainly symbolic because local officials would ignore, subvert, or nullify its major provisions. While aggregate analyses have tended to confirm this argument, so far there has been no systematic test of the law's effect on individual cases. This analysis uses multilevel models applied to case-level data from 12 urban California counties to test hypotheses about shifts in average punitiveness, the relative influence of legal and extralegal factors on sentencing, and the uncertainty of sentencing outcomes. Results mostly support Feeley and Kamin's symbolic interpretation, but also reveal important substantive impacts: since Three Strikes, sentences have become harsher, particularly in politically conservative counties, and black felons receive longer prison sentences.

California's "three strikes and you're out" law, enacted in 1994, is the most notorious example of the wave of mandatory sentencing reforms that swept the United States from the late 1970s through the '90 s. The law's notoriety is not due to its originality. Washington state passed the first Three Strikes law the year before, and nine other states passed their own laws the same year as California (Austin et al. 1999). Moreover California's law was not qualitatively different from the state's existing mandatory sentencing policy, nor from the habitual offender statutes that had long existed in almost all states. The new law's notoriety arose from two sources. One was its significance as an exemplar of penal populism. The measure originated as a ballot initiative, and the Three Strikes movement exploited two recent and particularly cruel murders, both committed by repeat felons, to stampede the media, voters, and state

I am grateful to Ryan King, Traci Schlesinger, and *LSR* reviewers and editors for their careful reading of earlier drafts and thoughtful suggestions for their improvement. Remaining limitations, errors, and verbal infelicities are wholly my responsibility. Please direct all correspondence to John Sutton, Department of Sociology, University of California, Santa Barbara CA 93106-9430; e-mail: sutton@soc.ucsb.edu.

Law & Society Review, Volume 47, Number 1 (2013)

https://doi.org/10.1111/lasr.12001 Published online by Cambridge University Press

politicians toward adoption, in the process turning local tragedies into national icons (Domanick 2004). The second source was the exceptional breadth and harshness of the new policy. Similar to reforms in other states, California's law required that defendants convicted for the second time of any of a certain class of felonies receive twice the normal sentence, and that those convicted for a third time receive a sentence of life imprisonment with no parole after less than 25 years. But the California law defined "strikeable" offenses expansively, including with the usual list of violent felonies a set of "serious" but nonviolent felonies such as selling drugs to minors, burglary, and weapons possession. Moreover, California is unique in that *any* felony can be called a third strike at the discretion of the prosecutor, not just those on the "serious or violent" list (Austin et al. 1999; California Legislative Analyst's Office 2005:5–9).

Following the law's enactment, legal officials, correctional administrators, politicians, and academics tried to predict its likely impact. Three schools of thought emerged. Advocates of the law, including the governor and attorney general, argued that the new policy would reduce crime and improve public safety in a cost-effective way by incapacitating some bad actors, deterring others, and encouraging still others to leave the state for more lenient climes; and further that by reducing judges' discretion it would reduce racial disparities in sentencing (Greenwood et al. 1994; King & Mauer 2001; Lungren 1996). A second group of critics doubted whether the habitual offenders targeted by the new law actually existed as an identifiable class of defendants (Auerhahn 1999), and worried that it would lead to gross inequities of justice, a paralyzing rise in demands for jury trials, overcrowding in jails and prisons, and ultimately to severe fiscal strain on state and local governments (Cushman 1996). Still others predicted that California's Three Strikes law was mainly a symbolic accomplishment that would prove to be neither panacea nor catastrophe. Feeley and Kamin (1996:135) stated this position most forcefully when they diagnosed Three Strikes laws as the products of "moral panics or symbolic crusades with only marginal instrumental value in terms of improving the effectiveness and efficiency of crime control." Reasoning from prior instances of panic-induced sentencing reform, Feeley and Kamin predicted that criminal justice officials would adapt to the new policy in a series of stages: they would initially swim with the political tide, proclaiming their enthusiasm for a policy they may not in fact welcome; then they would experience a period of uncertainty and confusion about how the law should be applied in particular cases; and eventually, once the political hubbub had died down, officials would use their discretion to undermine the provisions of the new policy they find most

https://doi.org/10.1111/lasr.12001 Published online by Cambridge University Press

troublesome, nullifying its most draconian effects and smoothing the flow of cases through the system.

Reduced to their basic elements, these forecasts suggest three kinds of potential effects of Three Strikes on the criminal court process.[1] The first concerns its impact on overall levels of punitiveness. Virtually all commentators, including Feeley and Kamin, predicted that prison sentences under Three Strikes would be longer and more frequent. Available aggregate data shed little light on this prediction: convicted Three Strikes defendants receive harsher sentences than others, but the law's contribution to the total inmate population has been only about a third of what was projected (California Legislative Analyst's Office 2005:15–16). The real question is whether similar defendants with similar case characteristics are sentenced differently under Three Strikes than before—a question that can only be answered using individual-level data. The second issue is the impact of the law on racial-ethnic sentencing disparities. Elimination of racial bias is an often-stated motive for sentencing reforms, including Three Strikes, and has been the central focus of research on specific policies (e.g., Albonetti 1997; Kramer & Ulmer 2002; Spohn 2000; Ulmer & Kramer 1996; Wooldredge, Griffin, & Rauschenberg 2005; Zatz 1984). The evidence on Three Strikes so far—again from aggregate data—is mixed: the proportions of blacks, Latinos, and Anglos in the second- and third-strike populations are about the same as their proportions in the state prison system, but black offenders are overrepresented among third strikers by 15 percent (California Legislative Analyst's Office 2005:21–22). Whether this is because of differential offending rates, racial differences in criminal histories, or disparities in the application of Three Strikes has so far not been tested.

The third issue is the predictability of sentencing. Three Strikes proponents argued that sentencing was not only excessively lenient and potentially biased, but arbitrary and uncertain in general; and they predicted that a tighter mandatory sentence policy would increase predictability by forcing judges to adhere to a strict sentencing formula. Feeley and Kamin (1996) challenge this prediction, noting that while Three Strikes may have brought clarity to the judge's role, it raised ambiguities for prosecutors about how potential Three Strikes defendants should be charged. They observed that these ambiguities initially led to unsettled relations within courtroom workgroups and raised uncertainty about case outcomes—a point echoed by Harris and Jesilow (2000). Feeley and

[1] I am not concerned with the effects of Three Strikes on crime rates. Careful studies have already been done, and show no long-term impact (e.g., Stolzenberg & D'Alessio 1997; Turner et al. 1999; Worrall 2004).

https://doi.org/10.1111/lasr.12001 Published online by Cambridge University Press

Kamin saw intra-jurisdictional uncertainty as a short-term problem that would abate as prosecutors worked out routines for applying the law; over the long term, they predicted, the major source of uncertainty would be variance *between* jurisdictions in how aggressively Three Strikes is applied. Recent data confirm that counties vary widely and rather consistently in their rates of Three Strikes sentencing (California Legislative Analyst's Office 2005:24–26), but these aggregate differences do not by themselves prove that the law increased inter-jurisdictional sentencing variability. Counties must differ in the mix of crimes that flow into the courts, and this would obviously lead to variability in sentence outcomes. Furthermore, counties no doubt varied in punitiveness before the enactment of Three Strikes; if, as Feeley and Kamin argue, court communities sought to adapt the law to established local customs, there may have been no net long-term change in sentencing variability. Again, this issue can only be addressed with data that represent variability in individual cases across jurisdictions and over time.

In this analysis I test predictions about Three Strikes effects on sentence severity, racial disparities, and individual- and jurisdiction-level variability in sentence outcomes using data on felony cases from the 12 most populous counties in California. The following section frames the analysis by drawing on the literature on sentencing reform and prosecutorial discretion to motivate hypotheses about both individual-level effects and the characteristics of counties in which those cases are decided. Subsequent sections describe the data and an analytical strategy based on a simple before-and-after model of reform effects; present the results; and draw lessons from the findings. Conclusions can be forecast in one sentence: California's Three Strikes law was a symbolic reform with important substantive effects.

Framework for the Analysis

How might Three Strikes have affected decision making in the criminal courts? The large literature on sentencing reform in the United States concludes that the major structural impact of mandatory minimum sentence laws (of which California's Three Strikes law is an exemplar) is a shift in the locus of discretion, from the imposition of sentences by judges to charging decisions made by prosecutors. Savelsberg (1992) frames this tendency in Weberian terms by characterizing guidelines and mandatory minimums as "neoclassical" reforms, in the sense that they aim to rescue criminal law from the grip of alien substantive norms and restore its formal rigor. But, as Savelsberg shows further, neoclassical ideals tend to be undermined in the hurley-burley of implementation because court

https://doi.org/10.1111/lasr.12001 Published online by Cambridge University Press

officials are more inclined toward bargaining and negotiation than deductive application of formal rules, and will tend to seize whatever strategic advantages the law offers to serve their occupational and organizational ends. Thus while mandatory sentencing reforms constrain judges' control over sentencing, and may—as in the case of Three Strikes—forbid prosecutors to bargain over sentences, they are likely to increase the incidence of charge bargaining, a form of (perhaps implicit) negotiation that works "backwards from the sentence to the offense" (Tonry 1988:303). Savelsberg therefore predicts that, as a result of reform, "The power of the prosecutor will increase. Disparities in charging decisions will be invisible" (1992:1376).

Miethe (1987) referred to this tendency as the "hydraulic displacement of discretion." His analysis of Minnesota sentencing guidelines finds little evidence of displacement with regard to charge severity, dismissals, charge reductions, or rates of guilty pleas, and Wooldredge and Griffin's (2005) similar analysis of sentencing guidelines in Ohio yielded mostly negative results as well. Miethe (1987:174–75) accounts for his findings by arguing that cultural norms operating within courtroom workgroups tend to inhibit self-interested manipulation of the law by prosecutors. We might expect different results in this case, however, because California's Three Strikes law was infused with extraordinary political heat (Domanick 2004), and, as several observers have noted (e.g., Austin et al. 1999; Zimring, Hawkins, & Kamin 2001), it offers exceptional incentives and opportunities for prosecutorial manipulation. Certainly California prosecutors welcomed the added leverage it gave them. At first the California District Attorneys Association opposed the Three Strikes law because it forbade plea bargaining over strikeable offenses, but the association switched its position when it became clear that the law enhanced prosecutors' power to induce guilty pleas in a wide range of cases (Domanick 2004:130). Prosecutors gained leverage because, in many cases, eligibility for second- or third-strike sentence enhancements is not straightforward, but rather depends on how prosecutors choose to interpret present and prior offenses. One type of choice involves "wobblers," crimes that can be charged either as felonies or as misdemeanors, such as assault and auto theft (California District Attorneys' Association 2004:10; California Legislative Analyst's Office 2005:14). Prosecutors can discount prior strikeable offenses "in the furtherance of justice," or simply ignore them, since "there is no one to complain" if they do (Feeley & Kamin 1996:150). If a defendant has been convicted of a prior strikeable offense, any felony—including those that fall below the "serious or violent" threshold—can be counted as a second strike; if a defendant has been convicted of two serious or violent felonies, any felony can be

https://doi.org/10.1111/lasr.12001 Published online by Cambridge University Press

counted as a third strike (California Legislative Analyst's Office 2005:5–6). Thus a defendant with two strikes can be sentenced to prison for 25 years to life for a third offense that might have been charged as a misdemeanor.

Appellate courts have consistently supported prosecutors' most aggressive interpretations of the law. In *Ewing v. California* (2003), for example, the U.S. Supreme Court declared that a third strike 25 years-to-life sentence for stealing a set of golf clubs is not cruel and unusual punishment, and the California Supreme Court in *People v. Fuhrman* (1997) allowed prosecutors to find two strikeable offenses arising from a single prior incident. One possible check on prosecutorial zeal is given in the *Romero* decision (1996), in which the state Supreme Court protected the authority of judges to dismiss prior strikes; but subsequent analysis has shown that judges use this authority "sparingly" (Austin et al. 1999:145). Thus on the whole the law "conveys a great deal of authority to the prosecutor to determine the ultimate sentence that the offender will receive if convicted" (Austin et al. 1999:143).

An important question is whether prosecutors will use their enhanced discretion to mitigate or aggravate the most punitive aspects of the Three Strikes law, and for what sorts of defendants. Prior research on determinate sentencing laws leads to no clear conclusion. Some work on federal sentencing guidelines (U.S. Sentencing Commission 1991) and state-level sentencing reforms (Tonry 1996:147) has found evidence of lowball charging to evade mandatory minimum penalties, and Feeley and Kamin (1996) report similar responses to California's Three Strikes law among prosecutors in the relatively liberal northern counties where they conducted their interviews. Contrary to these arguments, Kessler and Piehl's (1998) analysis of a determinate sentencing law enacted in California in 1982 shows no evidence of mitigation, and instead indicates a dominant strategy of "prosecutorial maximization"—a tendency to push to the limit of the law for harsher penalties. There is no need to choose between these competing scenarios, since available sources show that California counties vary widely in their inclination to seek Three Strikes sentence enhancements (Austin et al. 1999:145–49; California Legislative Analyst's Office 2005:24–26; Feeley & Kamin 1996; Zimring, Hawkins, & Kamin 2001): as a matter of policy, some opt for mitigation and others for maximization of penalties. The modeling strategy described below explicitly allows for variation of this sort.

The Individual-level Model

The starting point for the analysis is a model that has become fairly standard in the literature on criminal sentencing. As imple-

https://doi.org/10.1111/lasr.12001 Published online by Cambridge University Press

mented here, that model emphasizes three types of effects. The first is legally relevant factors, such as the seriousness of the offense for which the defendant is convicted and the defendant's prior record. The second comprises nonlegal ascriptive factors that may introduce bias into the sentencing process, especially race and ethnicity, but also gender, and in some studies social class, employment status, and family background. Race is the central issue in the sentencing literature: given the gross overrepresentation of African Americans and Latinos in U.S. prisons, and aggregate-level analyses showing considerable randomness and often outright bias in criminal sentencing (Blumstein 1982, 1993; Hagan 1989), the question of interest is whether bias is apparent at the level of individual cases. Despite the aggregate evidence, it is remarkable that in the dozens of case-level studies that have been published, evidence of direct racial bias in sentencing is mixed (for recent reviews, see Sampson & Lauritsen 1997; Spohn 2000; Spohn & DeLone 2000; Zatz 2000). The present analysis focuses on race and ethnicity, with the expectation that black and Latino defendants are sentenced more harshly than Anglos. Unfortunately the data set used here contains no information about defendants' class, employment status, or family background, so these factors will not be explored.

The third set of factors comprises effects that are endogenous to the criminal justice process itself. The basic idea here is that the criminal justice system involves a sequence of decisions, from arrest to charging, bail, detention, and possible plea bargaining, in addition to adjudication and sentencing, all of which are fraught with uncertainty for officials and defendants alike, and any of which may be tainted by bias. It is well understood that officials manage uncertainty by attributing moral culpability to defendants based on stereotypes that associate typical offenders with their typical crimes (Albonetti 1991, 1997; Bridges & Steen 1998; Emerson 1983; Engen, Steen, & Bridges 2002; Steen et al. 2005; Sudnow 1964). These stereotypes are likely to involve ascriptive status with regard to race, gender, and class, and at the sentencing stage they may also involve stigma arising from prior official decisions. In short, criminal justice is a path-dependent process; adverse decisions at early stages can create cumulative disadvantages that affect sentencing (Chen 2008; Schlesinger 2007). I focus on two most conspicuous decision points. The first is pretrial detention: defendants who are held in jail pending trial—whether because bail is denied or set at an unaffordable level—are likely to be seen by court officials as especially dangerous, and should be inclined to plead guilty in return for relatively small concessions (Feeley 1979). In any event, research tends to show that they receive harsher sentences. The second is mode of conviction: guilty pleas seem to reduce moral

https://doi.org/10.1111/lasr.12001 Published online by Cambridge University Press

blameworthiness even as they confirm formal-legal culpability. Research tends to show, as one would expect, that defendants who go to trial receive harsher sentences than those who plead guilty (Albonetti 1990; Brereton & Casper 1982; Britt 2000; King, Johnson, & McGeever 2010; LaFree 1985). I attend to three other factors whose theoretical status is more ambiguous. Defendants who are represented by a private attorney may fare better than those who do not—or alternatively, following Sudnow's (1964) insights, they may fare worse if private attorneys are less familiar than public defenders with the folkways of the court. Defendants who are already "active" in the criminal justice system at the time of arrest—those who are on probation or parole, or already incarcerated—and those who are charged with a second, less serious, felony are likely to appear more culpable than those who are not, and will likely receive harsher sentences.

Note, finally, that all of these processual factors—detention, pleading, criminal justice status, multiple charges, and legal representation—are likely to be skewed by race and ethnicity. Indeed, the research suggests that decisions at pre-adjudication phases of the process are especially prone to bias because they are mostly informal. For example, Demuth (2003) found that black and Latino defendants are more likely to be detained that Anglos; some research suggests that blacks are relatively unlikely to plead guilty, but the limited evidence on Latinos is mixed (Albonetti 1990; Frenzel & Ball 2007; Kellough & Wortley 2001; LaFree 1985). Moreover we would expect that minorities are more likely to be active in the system (Goffman 2009), and perhaps less able to afford private legal representation. Thus, while these endogenous effects are of intrinsic interest, they are also important to include as controls, to help isolate race-ethnicity effects operating at the point of sentencing.

Jurisdiction-level Effects

The individual- or case-level model is supplemented with a model of effects that emanate from the environments of courts—in this case, county jurisdictions—and that are likely to influence decisions about particular cases. This macro-level analysis is motivated by a long line of (mostly qualitative) studies that challenge the conventional view of criminal courts as clearly bounded bureaucratic organizations (Dixon 1995; Eisenstein & Jacob 1991; Hagan 1989; Kautt 2002). In this literature, courts appear instead as loosely-coupled collaborative networks of agencies pursuing disparate and often conflicting goals. Order arises not from bureaucratic hierarchy, but from interorganizational negotiations in the context of local political cultures in which agencies are embedded.

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I focus on two hypotheses that are particularly relevant to California's Three Strikes law, and that carry implications about the impacts of mandatory sentencing reforms in general. The first factor is the political climate. Macro-level studies tend to show that more politically conservative environments—whether local jurisdictions, U.S. states, or whole societies—tend to be more punitive (Jacobs & Carmichael 2001; Jacobs & Helms 1996; Stucky, Heimer, & Lang 2005; Sutton 1987, 2004; Weidner & Frase 2003). The few studies that have used appropriate multilevel models to test effects of political conservatism on individual sentences have found no effects (Fearn 2005; Ulmer & Johnson 2004; Weidner et al. 2005). But Feeley and Kamin's (1996) interview results and available data on Three Strikes implementation (California Legislative Analyst's Office 2005) give reason to suspect that criminal justice in California is more politicized than elsewhere.

The second contextual hypothesis is drawn from Stuntz's (1997, 2001) analysis of the politics of prosecution. The nub of Stuntz's argument is that criminal court outcomes are influenced by the relationship between "litigation opportunities" (the flow of cases into the prosecutor's office) and "litigation resources" (the prosecutor's personnel budget). Litigation is expensive, so resource limitations place a ceiling on the number of cases that can be litigated; the remainder—in fact, the vast majority—must be pled out. One implication of this is that the higher the crime rate, net of resources, the more choosy prosecutors can be about which cases to litigate, and the higher the proportion of cases they will resolve through guilty pleas (Stuntz 1997:23–25). A further implication is that prosecutors can raise the rate of guilty pleas only by offering concessions they would not otherwise offer. These concessions will be targeted, of course, on what we might call the marginal defendant, neither one who would plead guilty in any event, nor one whom the prosecutor views as a dead-bang conviction at trial, but the net result is a reduction in the severity of the average sentence. This chain of reasoning leads to a baseline prediction: the greater the inflow of criminal cases relative to prosecutorial resources—what Stuntz (1997:24) calls the "crime-to-time ratio"—the lower the average level of sentence severity.

Micro-level Effects of Three Strikes on Felony Cases

How, if at all, did the Three Strikes law change the institutional logic of felony sentencing in California? The perspectives introduced earlier—representing proponents who expected it to enhance the capacity for social control, critics who expected it to exacerbate systemic inequalities, and those, like Feeley and Kamin

https://doi.org/10.1111/lasr.12001 Published online by Cambridge University Press

(1996), who saw Three Strikes as primarily a symbolic reform with little long-term substantive impact—suggest different causal scenarios.

The most important and general predictions have to do with the impact of Three Strikes on the overall punitiveness of criminal sentencing. The law explicitly mandates longer prison sentences for some classes of crimes, but proponents and critics saw this occurring by somewhat different mechanisms. To proponents, the law would rein in the discretion of judges, leading to more predictable and evenhanded sentences. Critics saw the law mainly increasing the incentives of prosecutors to overcharge, and strengthening their bargaining power in plea negotiations. Thus the baseline hypothesis is that after passage of Three Strikes in 1994,

> H_1: The average probability of a prison sentence and the average length of prison sentences both increased.

Two other predictions arise from the more optimistic expectations of the law's effects. Three Strikes supporters predicted that stronger formal constraints on sentencing would enhance the salience of formal law and reduce opportunities for bias. Empirically, this implies patterned shifts in effects coefficients:

> H_2: Under Three Strikes, the influence of legally legitimate factors on sentence outcomes increased and the influence of extralegal factors declined.

In contrast to this hypothesis, critics would suggest that the ambiguity of the law creates opportunities and incentives for bias insofar as prosecutors seek to harvest the low-hanging fruit—that is, to invoke Three Strikes aggressively in cases involving disadvantaged defendants who are accused of relatively minor offenses. Thus there are theoretical grounds to predict the *opposite* of H_2.

Stuntz's (1997) analysis suggests a third hypothesis about shifts in the endogenous effects of the court process—specifically, the impacts of pretrial detention and guilty pleas. In general, he argues, "expanded criminal liability makes it easier for the government to induce guilty pleas, as do high mandatory sentences that serve as useful threats against recalcitrant defendants" (1997:4). If detained defendants receive harsher sentences than those who are freed pending trial, it is in part because they are more often willing to accept disadvantageous plea deals, and the threat of Three Strikes sentencing enhancements should increase their disadvantage. Conversely, guilty pleas should become cheaper—that is, fewer and smaller sentence concessions should be required to extract a given quotient of pleas. Put more formally:

> H_3: Under Three Strikes the penalty for pretrial detention increased, and the benefit for pleading guilty declined.

https://doi.org/10.1111/lasr.12001 Published online by Cambridge University Press

Supporters expected that, by reducing the discretion of judges, Three Strikes would lead to greater predictability in sentencing. The issue of predictability is different, and more general, than the issue of bias, and deserves specific attention. As detailed below, I evaluate this argument at both the jurisdictional level and at the level of individual cases. The hypothesis at both levels is the same:

> H_4: Under Three Strikes, the average uncertainty of sentence outcomes declined.

Feeley and Kamin's (1996) analysis of Three Strikes cuts across the grain of most of these hypotheses. They concur with others who expect the law to "ratchet up" average sentence severity (1996:136). Otherwise they offer little in the way of concrete predictions about the law's effects in the long term beyond the general prediction that, after a period of disequilibrium, the law's intended effects will be blunted by the adaptive behavior of prosecutors and judges. Feeley and Kamin are not legal nihilists: they do not argue that sentencing reform in general, and Three Strikes in particular, has no impact, but they foresee no changes to the institutional structure of county court systems. Thus, contrary to H_2, they would probably expect no long-term changes in the relative weights given to legal and extralegal factors in determining sentence outcomes. Contrary to H_4, their analysis suggests no long-term increase or decrease in sentencing uncertainty.

Macro-level Effects of Three Strikes

Finally, I offer hypotheses about how Three Strikes may have altered the jurisdiction-level effects of political conservatism and crime-to-time ratios. Several observers have suggested that the implementation of Three Strikes has been skewed by longstanding differences in the political climate across counties (Austin et al. 1999; Feeley & Kamin 1996; Zimring, Hawkins, & Kamin 2001). In particular, Feeley and Kamin (1996:148) observed that while southern California prosecutors were inclined to maximize Three Strikes prosecutions, those in the relatively liberal northern counties more often used strategies of avoidance. Recent evidence suggests that wide variation in the rate at which Three Strikes is invoked conforms roughly to this geographic pattern (Austin et al. 1999:145–49; California Legislative Analyst's Office 2005:24–26). The baseline model outlined above predicts that, even before the law went into effect, politically conservative counties were more punitive than politically liberal counties; the present argument suggests that after the law goes into effect the gap would widen:

> H_5: The expected positive effect of political conservatism on sentence severity is stronger under Three Strikes than before.

https://doi.org/10.1111/lasr.12001 Published online by Cambridge University Press

If, as Stuntz (1997) argues, mandatory sentencing reform increases the leverage of prosecutors, we should expect changes not only in individual-level effects, but also in the aggregate effects of case flows. Sentencing reforms like Three Strikes do not directly effect either litigation opportunities (the inflow of cases) nor litigation resources (the number of prosecutors), but they increase the level of risk for defendants who go to trial. From the prosecutor's point of view, therefore, they lower the cost of guilty pleas, leading in the aggregate to smaller average sentence concessions. This leads straightforwardly to a macro-level prediction: if Three Strikes strengthened the hand of prosecutors,

> H_6: The expected negative effect of the crime-to-time ratio on sentence severity is weaker after 1994 than before.

Methodological Concerns

Data and Measures

Data used for this study are from the State Court Processing Statistics (SCPS) files (U.S. Bureau of Justice Statistics 2007). SCPS files include case-processing data on samples of felony defendants in large urban counties in the United States between 1990 and 2004. I use a subset of the data that includes all male defendants in the twelve California counties included in the survey. I focus on males—83 percent of the total—because prior research shows that both the severity of sanctions and the role of race and other characteristics in the decision-making process differ by gender (Daly 1989; Kruttschnitt & McCarthy 1985; Spohn & Holleran 2000; Spohn & Spears 1997; Steffensmeier & Demuth 2000; Steffensmeier, Kramer, & Streifel 1993). Otherwise I include all felony cases, rather than attempt to isolate cases eligible for two- or three-strikes sentence enhancements, for several reasons. In practical terms, SCPS data do not permit clear identification of the "serious or violent" felonies that unquestionably qualify defendants for Three Strikes conviction. More importantly, as I have already described, the law gives prosecutors discretion to sweep virtually any felony into the Three Strikes net if the defendant has a prior serious or violent felony conviction. A narrow focus on patently eligible cases would therefore lead to serious underestimates of the law's impact. Zimring, Hawkins, and Kamin (2001) support the approach taken here in their analysis of felony arrests and sentences in California during 1994–1995. They find that, while prosecutors are selective in how they apply formal Three Strikes provisions, the explicit or implicit threat of second- and third-strike enhancements is likely to affect outcomes in all sorts of cases. Two

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Table 1. Distribution of Cases by County and Year

	Year								
County	1990	1992	1994	1996	1998	2000	2002	2004	Total
Alameda	0	0	155	121	107	138	155	206	882
Contra Costa	0	0	0	0	0	165	137	119	421
Los Angeles	812	876	744	746	723	531	628	778	5,838
Orange	160	0	0	262	230	292	318	354	1,616
Riverside	0	0	0	0	0	181	337	331	849
Sacramento	194	73	200	168	172	0	0	0	807
San Bernardino	85	42	173	164	268	257	339	373	1,701
San Diego	164	157	0	0	0	157	204	236	918
San Francisco	0	145	155	102	109	0	0	0	511
San Mateo	0	0	0	0	0	82	96	101	279
Santa Clara	89	109	251	191	251	198	193	204	1,486
Ventura	0	0	107	98	111	0	0	0	316
Total	1,504	1,402	1,785	1,852	1,971	2,001	2,407	2,702	15,624

related studies reach similar conclusions. Kessler and Piehl (1998) offer compelling evidence of "spillover" effects of the determinate sentencing law passed in California in 1982, from cases that qualified for sentencing enhancements to "factually similar" cases that did not. Bjerk (2005:596) argues—with supportive data—that even where prosecutors are inclined toward sentence mitigation, three strikes laws lead to harsher sentences even in cases where prosecutors seek to evade mandatory sentence requirements.[2]

The analysis of prison sentences is based on the 15,624 cases in the sample in which male defendants were convicted of felonies, and the analysis of sentence lengths is based on the 6,102 of those defendants who were sentenced to prison. The distribution of cases in the larger sample across counties and years is shown in Table 1. It should be mentioned that the empty cells in the table are the result of sampling strategy, not nonresponse—they do not, in other words, signify missing data. SCPS samples some counties with very large volumes of cases at every wave, but most counties are sampled on a rotating basis. This raises no problems for this analysis.

Two outcome variables are of interest. The indicator of prison sentences is a binary variable coded 1 if the defendant is sentenced to state prison and 0 for any lesser sentence (jail, probation, or fine). Measuring sentence length is a bit more complicated because SCPS data report only maximum sentences, in months, and life sentences are coded as an arbitrarily large number. I recoded life sentences to five years longer than the next longest sentence in the data set

[2] Bjerk's (2005:596) argument is as follows: Suppose that of a group of defendants arrested for crime A, a strikeable felony, the prosecutor declines to convict some proportion who have less serious criminal records, and allows them instead to plead guilty to lesser crime B that does not put them at risk of sentence enhancements. As a result, the groups convicted of A and B both now comprise more serious offenders than before, raising the severity of the average sentence within each group.

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(already over 100 years), and re-expressed the resulting distribution of sentences in (log) months. This measure is probably not as sensitive as it should be. Three Strikes and many similar reforms aim to raise mandatory *minimum* sentences for repeat offenders, so a measure of maximum sentences may understate the impact of the law. Interpretation of results will bear this in mind.

Most independent variables are binary (0,1) measures. Race and ethnicity are captured by two variables, one for (nonhispanic) black defendants and another for Latinos (black or white Hispanics). The remaining defendants are almost entirely nonhispanic whites, with about three percent Asians and Native Americans. The race and ethnicity data contain a nontrivial amount of missing values. Missingness appears to be random, at least with respect to other variables used in the analysis, and rather than discard observations with missing data I impute values as part of the estimation process. The pretrial detention variable is coded 1 if the defendant is detained and 0 if he is released, regardless of the mode of release. The plea variable is coded 1 for guilty pleas, whether explicitly negotiated or not; the reference category includes cases that are decided by bench or jury trial. I control for defendants' legal status with a variable coded 1 if the defendant was in custody, on probation or parole, in a diversion program, or a fugitive at the time of arrest, and 0 otherwise. It is important to include a measure of defendants' prior convictions, since this is the trigger for sentence enhancements under the Three Strikes law. The best approach in principle might be to create a variable that identifies defendants with one or more prior strikeable (serious or violent) felony convictions. Available data do not allow such fine-grained distinctions, and in any event such a measure would understate the reach of the Three Strikes law. As Zimring et al. point out (2001:68), the law gives prosecutors broad discretion to declare current and prior offenses as strikeable, so it is reasonable to assume that defendants with *any* prior felony convictions may be at risk of two- or three-strikes enhancements. I use a binary variable coded 1 for at-risk defendants. An additional dummy variable controls for any prior prison sentence.

Offense severity is likely to be the most important predictor of sentence outcomes, and the usual strategy is to derive a measure of severity from the conviction offense. In the case of mandatory sentencing reform, however, the most important information is given by the offense for which the defendant is initially charged. This is true regardless of expectations about how the law has been implemented. If Three Strikes were applied in the strict terms intended by reformers, the charge should determine the sentence straightforwardly in most cases because downstream discretion has been eliminated, so the anticipated positive association between charge severity and sentence severity should grow stronger after

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the law is adopted. If instead we expect that discretion has been displaced to the prosecutor, the initial charge sets the baseline for negotiations. Then, as Kessler and Piehl (1998:260) argue, the "conviction offense is endogenous; indeed, in a determinate sentencing system, choice of conviction offense is likely to be the mechanism through which discretion operates" (see also Engen & Gainey 2000). Under this scenario, however, the association between charge and sentence severity should be *weaker* after the adoption of Three Strikes: in general, the more severe the charge, the greater the latitude for charge reduction; the added threat of sentence enhancements after 1994 increases both the prosecutor's leverage and defendant's incentive to plead guilty. Both scenarios call for a measure of hypothetical sentence severity if the defendant were to be convicted of the offense for which he or she was initially charged—what Engen and Gainey (2000) call the "presumptive sentence." I construct such a measure in two steps. The first step is to regress the sentence outcome variable on the conviction charge, represented by a set of dummy variables representing 16 of the 17 felony offenses recorded in the SCPS data, and retrieve the coefficients. The second step is to calculate hypothetical sentence severity as the linear product of the charge offense, again represented by a set of dummy variables, and the corresponding coefficients from the regression. I calculate separate measures for the analyses of prison sentences and sentence length, and for simplicity I refer to both measures as charge severity.

Estimation

California counties differ in their eagerness to invoke Three Strikes provisions, and perhaps also in the impact of race-ethnicity, gender, prior record, and other factors on sentencing outcomes. This suggests a multilevel modeling approach that allows coefficient estimates to vary over space and time. Consider first the model for prison sentences. Define the dependent variable as

$$y_i = \begin{cases} 0 & \text{noncustodial sanction or jail} \\ 1 & \text{prison} \end{cases}$$

and the probability of a prison sentence as $\Pr(y_i = 1) = p_i$. The appropriate individual-level binomial logistic model for individual i in county j and year t is

$$\text{logit}(p_i) = \alpha^0_{jt} + \mathbf{x}_i'\boldsymbol{\alpha}_{jt} + (\beta^0_{jt} + \mathbf{x}_i'\boldsymbol{\beta}_{jt})I_t$$

In this equation, coefficients vary randomly across counties and years, and estimates also vary between time periods: α^0_{jt} is the

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intercept for county-year *jt* in the pre-Three Strikes period 1990–1994, $\mathbf{x}_i$ is a vector of covariate values for defendant i, and $\boldsymbol{\alpha}_{jt}$ is a vector of random effects coefficients. I_t is a dummy variable with values 0 in 1990–94 and 1 in 1996–2004, so that β^0_{jt} is the shift in the intercepts following the implementation of Three Strikes and $\boldsymbol{\beta}_{jt}$ denotes shifts in the random effects. The analysis of sentence length uses a linear regression model in the form

$$y_i \sim \mathrm{N}\left(\hat{y}, \boldsymbol{\sigma}^2_{jt}\right)$$

$$\hat{y} = \alpha^0_{jt} + \mathbf{x}_i'\boldsymbol{\alpha}_{jt} + \left(\beta^0_{jt} + \mathbf{x}_i'\boldsymbol{\beta}_{jt}\right) I_t$$

where y_i is observed prison sentences in (log) months and $\boldsymbol{\sigma}^2_{jt}$ signifies unequal error variances across counties and years.

The macro-model predicts the random intercepts α^0_{jt} and β^0_{jt} in terms of factors that vary across space and time:

$$\alpha^0_{jt} \sim N(\mathbf{z}'_{jt}\boldsymbol{\gamma}_{\alpha^0}, \tau^2_{\alpha^0})$$

$$\beta^0_{jt} \sim N(\mathbf{z}'\boldsymbol{\gamma}_{\beta^0}, \tau^2_{\beta^0})$$

In these models, $\mathbf{z}$ is a vector that includes measures of the Republican gubernatorial vote share and the crime-to-time ratio, and $\boldsymbol{\gamma}_{\alpha^0}$ and $\boldsymbol{\gamma}_{\beta^0}$ are vectors of coefficients, including a constant. Effects coefficients in $\boldsymbol{\alpha}_{jt}$ and $\boldsymbol{\beta}_{jt}$ vary randomly but are not otherwise modeled, for example: $\boldsymbol{\alpha} \sim N(\boldsymbol{\gamma}_\alpha, \boldsymbol{\tau}^2_\alpha)$, $\boldsymbol{\beta} \sim N(\boldsymbol{\gamma}_\beta, \boldsymbol{\tau}^2_\beta)$. Quantities of primary interest are the hyperparameter vectors $\boldsymbol{\gamma}_{\alpha^0}$, $\boldsymbol{\gamma}_{\beta^0}$, $\boldsymbol{\gamma}_\alpha$, and $\boldsymbol{\gamma}_\beta$. In addition, net effects for the post-Three Strikes period can be calculated by summing the baseline estimates and the shift estimates, for example $\boldsymbol{\gamma}_{\alpha^0} + \boldsymbol{\gamma}_{\beta^0}$ for the intercept.

Models are fit using Bayesian estimation techniques in which coefficient estimates are drawn from a more-or-less restrictively defined parameter space, compared to the data, and updated in an iterative fashion. There are sound statistical reasons for preferring a Bayesian approach to classical methods for fitting multilevel models, particularly those with qualitative outcomes (Rodriguez & Goldman 1995, 2001; Western 1999:23). A more practical problem is that, for complex multilevel models such as the one used here, classical regression methods may not yield enough information for accurate estimation of the variance parameters (Gelman & Hill 2007:345). Unlike classical methods, Bayesian estimation assumes that the coefficients rather than the data are random draws from some shared distribution, and yields probabilistic inferences about their true values. This is particularly useful here, since we are interested not just in shifting parameters, but also changes in the uncertainty of the parameter estimates.

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Bayesian estimation requires clear statements of distributional assumptions. A frequently used practice is to specify "skeptical priors" (Weiss et al. 1999) with normal distributions, zero means, and wide variances. My strategy is to use modestly informative priors that are more skeptical about whether Three Strikes had any impact than they are about particular coefficients. This involved, for each outcome, estimating a simple individual-level model with the pooled data from both time periods, and using the estimated intercept from that model as the prior mean for the pre-Three Strikes intercept γ_{α^0}, and using the effects estimates as the prior means for γ_α, the pre-Three Strikes effects parameters. Priors for the pre-Three Strikes effects of the county-level covariates and all of the coefficients for the Three Strikes shift were given prior means of zero. These are conservative priors because they suggest that parameter estimates do not change at all in response to Three Strikes. All priors were given wide variances (sd = 1,000) to allow the data to move the estimates as appropriate. The county-year variances in sentence lengths σ^2_{jt} and the coefficient variances are given flat distributions with a generous range: $\sigma^2_{jt} \sim \mathrm{U}(0, 10)$, $\tau^2 \sim \mathrm{U}(0, 10)$. Data for categorical covariates have been centered at their grand means, and the continuous measures of Republican vote strength, the crime-to-time ratio, and charge seriousness were centered and divided by two standard deviations. Centering speeds convergence by reducing correlations among county-level coefficients and allows convenient interpretations of intercepts as predicted means. The models reported below are based on 10,000 iterations for the linear regression and 20,000 iterations for the logit model, of which the first half were treated as burn-in and discarded. Of the remaining iterations, 1,000 were sampled and saved. Convergence was evaluated using traceplots and the $\hat{R}$ statistic (Gelman et al. 2004:296–97). All reported coefficient estimates are based on samples that converged at the appropriate level of 1.1 or less.

Results

Prison Sentences

Results from the logistic models predicting prison sentences before and after the enactment of Three Strikes appear in Table 2 and Figure 1. The first four columns contain statistics on the posterior distributions for 1990–94 (estimates of γ_{α^0} and γ_α). The first column contains posterior means, which I treat as coefficient estimates. The next two columns show credible intervals—that is, the lower and upper bounds of the central 95 percent of the posterior distributions. The third column shows the proportion of each posterior sample that falls on the same side of zero as the mean—a

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Table 2. Binomial Logistic Model of Prison Sentences

	1990–94				Three Strikes shift				1996–2000			
	Mean	2.5%	97.5%	p|coef| > 0	Mean	2.5%	97.5%	p|coef| > 0	Mean	2.5%	97.5%	p|coef| > 0
Intercept	−0.878	−1.164	−0.629	1.000	0.203	−0.100	0.541	0.898	−0.675	−0.858	−0.498	1.000
Republican vote	0.384	−0.096	0.829	0.961	0.171	−0.389	0.742	0.726	0.555	0.227	0.906	1.000
Crime-time ratio	0.281	−0.083	0.623	0.921	−0.277	−0.877	0.281	0.825	0.004	−0.482	0.467	0.508
Black defendant	−0.144	−0.372	0.125	0.846	0.157	−0.143	0.433	0.831	0.013	−0.156	0.196	0.553
Latino defendant	−0.070	−0.295	0.195	0.729	0.112	−0.196	0.379	0.772	0.042	−0.113	0.197	0.715
Detained before trial	1.121	0.884	1.393	1.000	0.253	−0.071	0.551	0.944	1.375	1.187	1.570	1.000
Guilty plea	−2.280	−3.197	−1.414	1.000	0.280	−0.753	1.263	0.696	−2.000	−2.552	−1.468	1.000
Private attorney	0.241	−0.195	0.634	0.874	0.096	−0.341	0.579	0.645	0.337	0.133	0.517	1.000
Second felony charge	0.284	0.123	0.433	0.999	0.249	0.064	0.452	0.996	0.533	0.397	0.676	1.000
Criminal justice status	0.582	0.326	0.814	1.000	−0.173	−0.480	0.168	0.867	0.409	0.247	0.575	1.000
Prior felony conviction	1.199	1.007	1.384	1.000	−0.098	−0.385	0.217	0.752	1.102	0.853	1.351	1.000
Prior prison sentence	0.458	0.096	0.799	0.992	0.722	0.294	1.167	1.000	1.180	0.938	1.428	1.000
Arrest charge seriousness	0.869	0.666	1.075	1.000	−0.051	−0.305	0.188	0.684	0.817	0.669	0.954	1.000

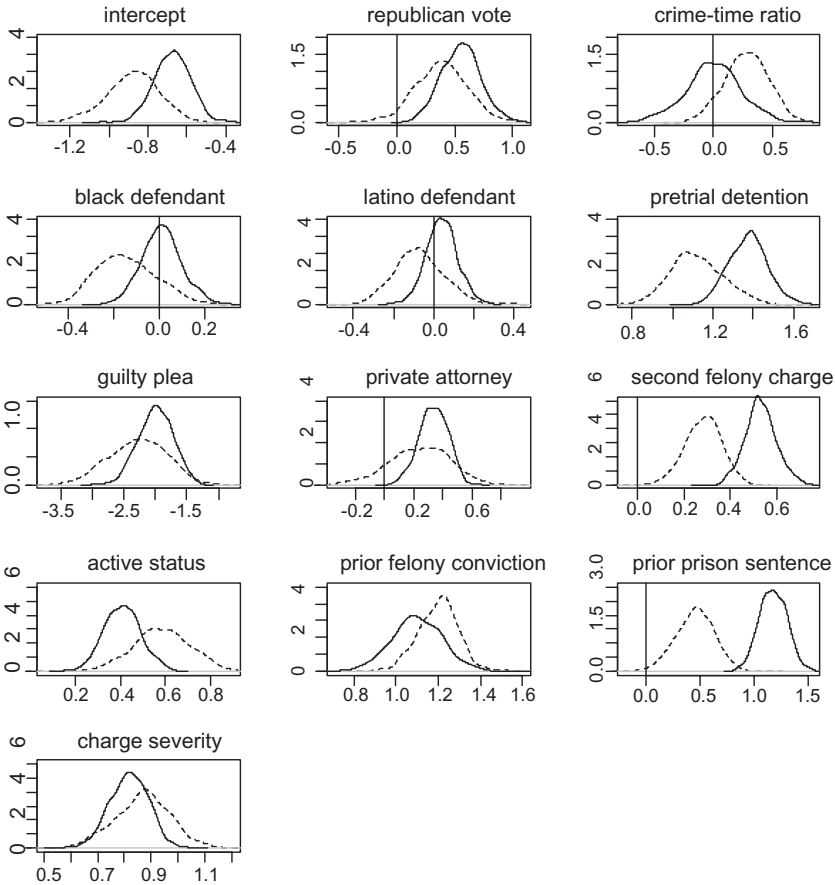

Figure 1. Posterior Densities, Pre- and Post-three Strikes: Prison Sentences. Note. Dashed Lines Show Posterior Distributions for 1990–94; Solid Lines Show Posteriors for 1996–2004.

measure of the probability of a true effect. The middle four columns report the same statistics for the posterior samples of $\boldsymbol{\gamma}_{\beta^0}$ and $\boldsymbol{\gamma}_{\beta}$, the vectors estimating changes in the parameters between periods. The sums of respective distributions yields the posterior distributions for the post-Three Strikes period 1996–2000; these are reported in the last four columns. Figure 1 contains density plots of the posterior distributions of the pre- and post-Three Strikes effects.

Look first at the pre-Three Strikes coefficients. Since all covariates are grand-mean centered, we can interpret the intercept as showing that the odds of the average convicted felon going to prison in this period are about $e^{-0.88} = 0.4$, or nearly 30 percent. As expected, the mean rate is higher in politically conservative

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counties. Where the Republican gubernatorial vote is two standard deviations above the mean, average odds of a prison sentence are $e^{(-0.88+0.38)} = 0.6$, or about 38 percent. But contrary to expectations, the effect of the crime-time ratio appears to be positive (certainty in a true effect is 92 percent). Also surprising are the negative estimates for race and ethnicity. The coefficient estimate for Latinos is substantively small and highly uncertain (support for a true effect is only 73 percent), but the estimate for black defendants is twice as large and nearly 85 percent certain. A further unexpected result is that having a private attorney may increase the likelihood of a prison sentence. Remaining estimates are as expected. Defendants who are detained before trial are much more likely to be sentenced to prison (net odds are $e^{(-0.88+1.12)} = 1.27$, or three times the average odds), and defendants who plead guilty are much less likely than average to receive a prison sentence (net odds are only 0.04). A second felony charge, active criminal justice status, a prior felony conviction, a prior prison sentence, and a more serious arrest charge have unequivocally positive effects.

How, if at all, did this regime change under Three Strikes? Attention to Figure 1 is useful here. The average likelihood of imprisonment clearly increased: the mean shift in the intercept is about 0.2, leading to net odds of $e^{-0.675} = 0.5$ in the post-Three Strikes period. The posterior offers nearly 90 percent support for a real shift (see the first row in the middle section of Table 2), and the plot in Figure 1 suggests that the difference between periods is substantial. This punitive shift may be slightly more pronounced in more politically conservative jurisdictions, but the estimate is highly uncertain (about 73 percent). The unexpected positive impact of the crime-to-time ratio disappears completely after 1994.

Shifts with regard to defendants' race and ethnicity are small, but interesting: estimates for the post-Three Strikes period show no differences between blacks or Latinos and the average defendant. There are clear shifts in the effects of the court process. The stigmatizing effect of pretrial detention grows much stronger: whereas detention tripled the average odds of imprisonment before Three Strikes, afterward the multiplier is $e^{1.375} = 3.96$. There is no apparent change in the bonus for pleading guilty, and while the shift coefficient for private attorneys is insignificant, the net effect in the second period is unequivocally positive. Results concerning the remaining legally relevant factors are mixed. Defendants charged with a second felony and those who have served a prior prison sentence are sentenced more harshly after Three Strikes than before, but there is no apparent change in the impact of a prior felony conviction or offense seriousness, and the effect of criminal justice status may have grown weaker.

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Sentence Length

Coefficient estimates from the models of sentence length are shown in Table 3, and density plots appear in Figure 2. Looking first at the intercepts, we see that the estimated mean sentence length rose about six percent, from $e^{3.39} = 29.6$ months to about 31.5 months. The Republican vote effect is positive in the first period and does not change in the second. Again the crime-time ratio performs contrary to the hypothesis: we see no effect on sentence length in the first period, and the expected strongly negative effect in the second.

Individual-level effects show no clear patterns. Coefficient estimates for the race-ethnicity variables show that black defendants drew about six percent shorter than average sentences in the first period (with 89 percent certainty), but the change associated with Three Strikes is positive and large, so that in the second period their predicted sentences are almost ten percent longer than those given to the average defendant—nearly four months. Latino sentences do not differ from the sample average in either period. The penalty for pretrial detention and the discount for pleading guilty both may have increased. The estimates of the shift coefficients are highly uncertain—they support only 70 and 76 percent confidence, respectively—but the plots in Figure 2 suggest substantively serious changes. The mean detention effect shifted from a six percent increase in sentence lengths to nine percent, and the mean estimate of the guilty plea discount shifted from 70 to 75 percent. Defendants with private attorneys may have received slightly shorter sentences before Three Strikes, but their sentences are on average about 16 percent longer after the law went into effect. Estimates for legally relevant effects are perplexing. Defendants charged with a second felony draw longer prison sentences initially; this effect is cut by half in the Three Strikes period, but is still apparent. Defendants who are active in the system draw *shorter* sentences, and this effect too is weaker under Three Strikes. The effect of a prior prison sentence is initially positive but weak, then becomes quite strong in the second period, increasing average prison sentences nearly 20 percent (seven months). The effect of charge offense seriousness is strongly positive in the first period, but surprisingly does not change at all under Three Strikes. The strangest finding of all concerns the effect of a prior felony conviction: defendants with prior felonies were sentenced more harshly in the first period, as expected; but received *shorter* sentences (by nearly nine percent) in the second.

Sentence Predictability

The remaining important issue concerns the effects of Three Strikes on the predictability of sentence outcomes. The analysis

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Table 3. Regression Model of (log) Sentence Length

	1990–94				Three Strikes shift				1996–2000			
	Mean	2.5%	97.5%	p|coef| > 0	Mean	2.5%	97.5%	p|coef| > 0	Mean	2.5%	97.5%	p|coef| > 0
Intercept	3.389	3.291	3.505	1.000	0.060	−0.063	0.165	0.866	3.449	3.395	3.496	1.000
Republican vote	0.101	−0.049	0.247	0.912	−0.014	−0.193	0.163	0.530	0.087	−0.010	0.192	0.960
Crime-time ratio	0.022	−0.119	0.155	0.650	−0.181	−0.387	−0.016	0.985	−0.159	−0.309	−0.029	0.991
Black defendant	−0.059	−0.144	0.048	0.892	0.162	0.033	0.263	0.997	0.102	0.034	0.170	0.997
Latino defendant	0.008	−0.066	0.088	0.577	0.010	−0.077	0.100	0.589	0.018	−0.038	0.075	0.744
Detained before trial	0.062	−0.005	0.138	0.960	0.027	−0.051	0.118	0.700	0.089	0.026	0.156	0.996
Guilty plea	−1.216	−1.676	−0.774	1.000	−0.201	−0.804	0.422	0.755	−1.418	−1.814	−1.032	1.000
Private attorney	−0.078	−0.225	0.058	0.849	0.238	0.086	0.418	1.000	0.161	0.084	0.235	1.000
Second felony charge	0.119	0.043	0.185	0.999	−0.057	−0.146	0.050	0.916	0.062	0.014	0.113	0.996
Criminal justice status	−0.082	−0.168	−0.002	0.977	0.039	−0.059	0.146	0.799	−0.042	−0.099	0.012	0.922
Prior felony conviction	0.061	−0.035	0.156	0.899	−0.147	−0.265	−0.016	0.986	−0.086	−0.164	−0.003	0.978
Prior prison sentence	0.033	−0.048	0.129	0.784	0.164	0.051	0.252	0.998	0.197	0.135	0.257	1.000
Arrest charge seriousness	0.616	0.498	0.747	1.000	0.035	−0.130	0.156	0.661	0.651	0.559	0.742	1.000

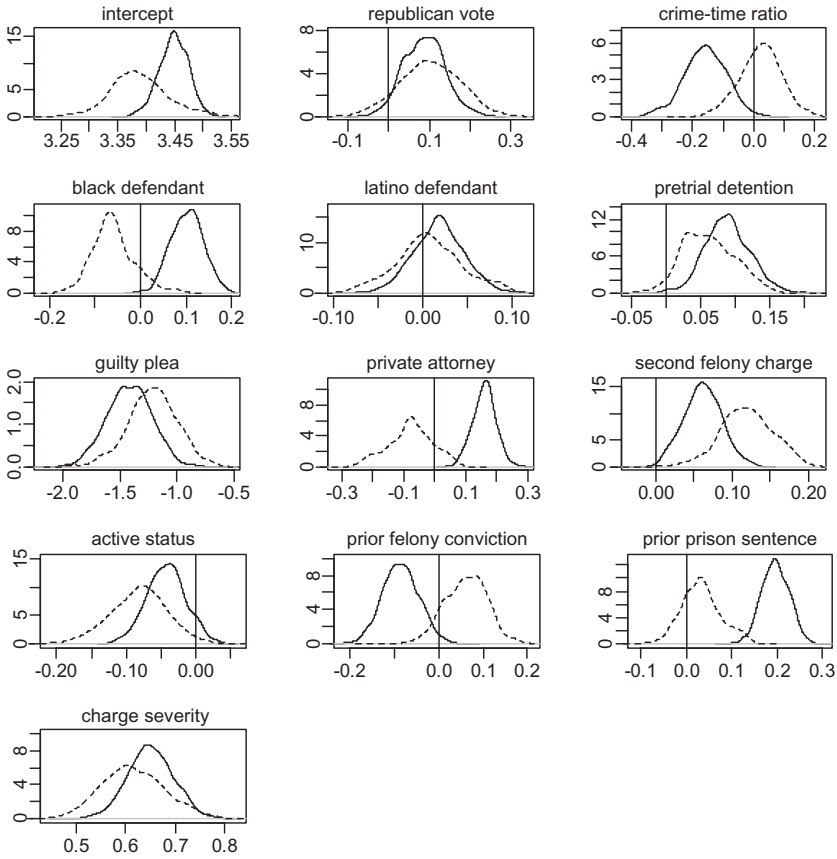

Figure 2. Posterior Densities, Pre- and Post-three Strikes: Sentence Length. Note. Dashed Lines Show Posterior Distributions for 1990–94; Solid Lines Show Posteriors for 1996–2004.

presented here is based on simple assumption that practical uncertainty in sentencing outcomes should register as model uncertainty; in particular, if sentencing became more arbitrary after 1994, predictions from the model should become more random. In a Bayesian framework, the appropriate strategy is to use simulation techniques that combine the observed values of the covariates and the posterior distributions of the coefficient estimates to generate probabilistic predictions of the outcome of interest. Given the multilevel structure of the model, prediction occurs in two steps that are essentially the reverse of the estimation process. The first step is to simulate the random county-year coefficients. Take, for example, the intercept for the pre-Three Strikes period: define α_{0jt}^{sim} as the simulated intercept for county j in year t, and draw values randomly from a distribution defined as $\alpha_{0jt}^{sim} \sim N(\mathbf{z}'_{jt}\boldsymbol{\gamma}_{\alpha 0}, \tau^2_{\alpha 0})$, where

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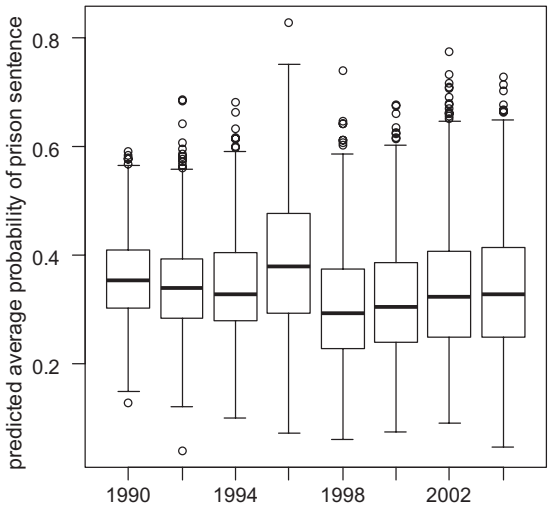

Figure 3. Predicted Average Probability of Prison Sentence by Year (Simulated County Means).

again **z** contains observed values of the Republican vote and the crime-time ratio. The second step is to generate individual-level predictions using the simulated county-year coefficients and the observed values of the individual-level covariates. These are simulations in the sense that each calculation is performed 100 times (an arbitrarily large number), using subsamples of parameter estimates drawn from the posteriors. The result for each model is $N \times 100$ predictions, enough to preserve much of the uncertainty inherent in the parameter estimates.

We are interested in uncertainty occurring at two levels: at the county level, where sentence outcomes may vary due to jurisdictional differences in the implementation of Three Strikes, and at the level of individual cases, reflecting potential intra-jurisdictional ambiguity. County comparisons can be made by calculating predicted mean outcomes across counties and years, using the simulated intercepts: $\hat{\theta}_{jt} = \hat{\alpha}_{0jt}^{sim} + \hat{\beta}_{0jt}^{sim} I_t$, where I_t is the time-varying dummy variable. The resulting distributions are arrayed by year as boxplots in Figures 3 and 4. Figure 3 shows predicted probabilities of a prison sentence for the average defendant (the inverse logit of $\hat{\theta}_{jt}$), and Figure 4 shows the predicted length of the average maximum sentence. In these plots, relative uncertainty is denoted by the height of the boxes (containing the central 50 percent of each distribution) and the length of the whiskers (the ends mark the central 75 percent): the greater the predicted variability among counties, the more the predicted means will sprawl. Figure 3

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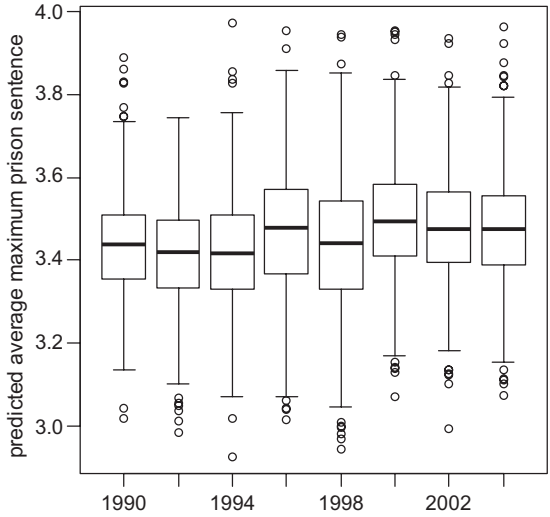

Figure 4. Predicted Average Prison Sentence Length by Year (Simulated County Means).

suggests that Three Strikes may have widened differences among counties in the use of prison sentences in 1996, the first sample year in which the law was fully in effect, but inter-county variances in subsequent years are not clearly different from those in the pre-Three Strikes period. A similar pattern appears in Figure 4. Variation in average sentence length may have increased in 1996 and 1998, but very slightly if at all; distributions for 2000–2004 show no more spread than those for 1990–1994. The conclusion from both sets of results is the same: net of the observed effects of political conservatism and prosecutorial caseloads, if Three Strikes had any effect on jurisdiction-level variation in sentencing practices it was no more than a transient shock, after which counties returned to business as usual.

Assessment of individual-level uncertainty requires a different strategy. Here I use Bayesian marginal model plots, or BMMPs (Cook & Weisberg 1997; Pardoe & Cook 2002). This technique involves smoothing both observed and simulated values of the outcome variable with respect to some interesting criterion variable h. When plotted, the smooths of the predictive simulations form a band and the smoothed observed values form a line; in a well-fitting model the line should lie within the limits of the band at all points. More to the point of this exercise, greater model uncertainty appears as a wider spread in the simulations. Plots shown in Figures 5 and 6 offer two kinds of comparisons. As criterion variable h I use arrest charge seriousness, on grounds that the relationship

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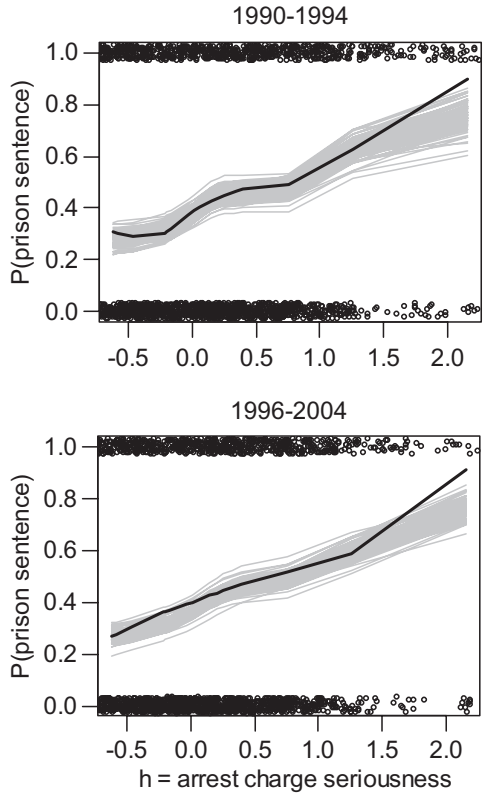

Figure 5. Bayes Marginal Model Plots: Observed and Simulated Probabilities of a Prison Sentence Against Arrest Charge Seriousness, 1990–94 and 1996–2004.

between initial charge and sentence is theoretically fundamental. To assess the impact of Three Strikes on this relationship I plot results separately for cases decided before and after the law went into effect.

Figure 5 includes jittered values of the observed outcomes at the top and bottom of each plot. Predictive simulations merge together to form a gray band, and the smoothed observed outcome is shown as a black line. Both plots suggest some problem with model fit for defendants arrested for the most serious crimes: at extremely high values of *h*—scores above 1.5, or three standard deviations above the mean—the smoothed mean observed values rise well above the range of the predicted values. These are all cases in which defendants are charged with murder; the plots show that murder defendants are sentenced to prison at a higher rate than the model predicts. The fact that this pattern appears in both plots indicates it has nothing to do with Three Strikes. More importantly,

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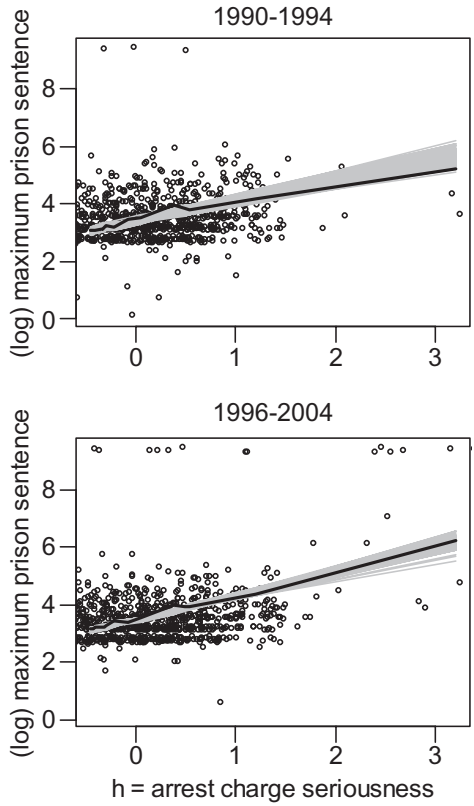

Figure 6. Bayes Marginal Model Plots: Observed and Simulated Maximum Sentence Length in (log) Months Against Arrest Charge Seriousness, 1990–94 and 1996–2004.

the gray bands formed by the simulations are about the same width in both plots, suggesting no change in individual-level sentencing uncertainty. Parallel results for sentence length are shown in Figure 6, with scatterplots of a subsample of the observed data underimposed. These plots indicate no problem with model fit in either period, even for the most serious offenses. Just as important, they show no sign of increased uncertainty after Three Strikes went into effect.

Summary and Discussion

What do these findings say to the hypotheses that informed the analysis? The most fundamental prediction (H_1) is that Three Strikes made the system as a whole more punitive, increasing both

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the probability of imprisonment for the average felon and the length of the average prison sentence. These data support both parts of the hypothesis: for the average defendant, Three Strikes raised the odds of a prison sentence nearly 23 percent; among those sentenced to prison, maximum sentences grew six percent longer. Borrowing a tactic from Huber and Gordon (2004:255–56), we can use these results to suggest the practical magnitude of these shifts. In 1994, the year Three Strikes was enacted, California courts sentenced 41,757 defendants to prison terms. If that were an average year—with an average mix of case and defendant characteristics—and Three Strikes had already been in effect, the model in Table 1 predicts an increase of 6,768 prison sentences. If we assume the same number of prison sentences, the predicted increase of 1.9 months in the length of the average sentence from Table 3 implies an aggregate increase of 6,612 man-years of time served.[3]

Another hypothesis (H_2) concerned the claim by Three Strikes advocates that stricter sentencing rules would increase the weights given to formal-legal factors such as offense seriousness and prior record, and thereby reduce the influence of legally irrelevant factors such as race; an inverse effect is possible if prosecutors sought to take arbitrary advantage of their enhanced discretion. Here the results are complex, but on the whole there is no evidence of systematic change in either direction. Black defendants were sentenced relatively leniently before Three Strikes; after the law went into effect they were sentenced to prison at about the same rate as the average defendant, but they received significantly longer prison sentences. This is the only evidence of an invidious race effect in these results. Latinos do not differ from the sample average on either outcome in either period. At the same time, there is little evidence that legally legitimate factors became more salient. The only expected finding, consistent across both outcomes, is the increasingly punitive sentencing of defendants with prior prison sentences. Legal factors that are supposedly most likely to trigger Three Strikes sentencing enhancements do not behave as expected: there is no discernible change in the effect of offense seriousness in either model; the effect of a prior felony conviction on the

[3] These figures are heuristic only, since they make no allowance for the uncertainty of the estimates, the "average year" assumption is unrealistic, and the number sentenced to prison includes females as well as males. Table 2 estimates the mean probability of imprisonment as 0.29; given the observed number of prison sentences, a backwards prediction of the number of felony convictions is thus 143,990. Raising the probability to 0.337 (again from Table 2) yields a prediction of 48,525 prison sentences. The predicted increase in time served assumes simply that Three Strikes added an average of 1.9 months to the terms of 41,757 sentenced defendants. The count of new prison sentences in 1994 is from the California Criminal Justice Statistics Center, Office of the Attorney General: http://ag.ca.gov/cjsc/datatabs.php.

probability of a prison sentence is unchanged, and grows perversely negative in the model of sentence length. Other legally relevant effects show no patterned responses to the Three Strikes sentencing regime.

The third hypothesis H_3 predicted that, due to the anticipated increase in prosecutorial leverage under Three Strikes, the penalty for pretrial detention would increase and the benefit of pleading guilty would decline. Results showed a large increase in the impact of detention on prison sentences, and weaker evidence of an increased impact on sentence length. Leniency in return for guilty pleas did not change in either model. We thus find no evidence of systematic prosecutorial manipulation. In this vein, note that representation by a private attorney *raises* the likelihood of a prison sentence by about the same amount in both periods; defendants with private counsel may have received shorter prison sentences before Three Strikes, but received longer sentences (by an estimated 16 percent) after 1994. This tends to confirm the notion that criminal justice is an insider's game, perhaps increasingly so under Three Strikes. Hypothesis H_4 adopted the logic of neoclassical reform to predict decreased uncertainty in sentence outcomes after Three Strikes. Tests of this hypothesis were not given in parametric model results, but were rather induced from simulations of predicted county- and individual-level means based on the posterior distributions. Simulations yielded no evidence that sentencing became either more or less predictable as a result of Three Strikes. At most, they suggest that there may have been a transient increase in the variability across counties in the rate at which defendants are sentenced to prison, but no lasting change.

Remaining hypotheses concerned patterned variation in punitiveness across counties. Impacts of political conservatism are very much as expected (H_5). More conservative counties consistently sentence defendants to prison at a higher rate, and this tendency increased after Three Strikes. Prison sentences are longer in conservative counties, but the effect does not vary between periods. In contrast to this, effects of the crime-to-time ratio did not conform to expectations at all. The hypothesis (H_6) predicted an initial negative effect, growing weaker as Three Strikes made guilty pleas easier to coerce. Instead, models show an initial positive effect of the crime-to-time ratio on imprisonment rates, moving to zero after Three Strikes; and no initial effect on sentence length, and a negative impact after Three Strikes. The only consistent feature of these findings is that the effect moved in a negative direction between periods, suggesting that Three Strikes made prosecutors more generous—again, contrary to the hypothesis.

Taken together, these results offer scant support either to the rosy predictions of Three Strikes proponents or to the more

https://doi.org/10.1111/lasr.12001 Published online by Cambridge University Press

dystopian predictions of the law's critics. Reform did not make criminal sentencing more evenhanded or predictable, and it did not in any systematic way increase the weight given to the most legally salient factors in sentence outcomes. Nor, for the most part, do the findings support the worst fears of critics that Three Strikes would encourage the arbitrary and potentially biased exercise of prosecutorial discretion. Since the law went into effect black defendants receive significantly longer prison sentences than Anglos and Latinos. This is an important finding that calls for further investigation—the mechanism that generates this disparity is far from clear. It is the more interesting, and perplexing, because it stands alone: otherwise, Three Strikes seems indifferent to race and ethnicity. Overall, the pattern of results conforms to Feeley and Kamin's (1996) more ironic interpretation of mandatory sentencing reform. As they predicted, sentencing became significantly more punitive under Three Strikes; and, in particular, the rates at which defendants are sentenced to prison rose most in politically conservative counties where voters expect district attorneys to apply the law aggressively. Otherwise, not much happened—or, put more precisely, there is no evidence of the kind of systematic change that would signify a deep institutional transformation in the logic of felony court practice. On the contrary, it appears that for the most part Three Strikes rules have been absorbed fairly smoothly into sentencing routines established before the law was enacted.

Feeley and Kamin accurately characterized Three Strikes as a "symbolic" reform with limited—but important—substantive consequences. I would qualify that conclusion, and in doing so raise questions for further research. It seems incontestable that Three Strikes increased potential prosecutorial control over felony case outcomes, and while the present analysis suggests that prosecutors used their enhanced discretion to make sentencing harsher in a general sense, it tells us little about the mechanics of that transformation. The question remains: If reform displaced discretion from judges to prosecutors, how was that discretion used? One approach to this question, following Miethe (1987) and Wooldredge and Griffin (2005), would be to shift the analytic focus to preadjudication decisions, and analyze how the determinants of pretrial detention, charge adjustments, dismissals, and guilty pleas may have changed under the Three Strikes regime. For example, the present study found no change in the mitigating value of guilty pleas, but that may conceal changes in the kinds of defendants that receive plea deals, and the size of the charge reductions they receive in return. A second approach would be to explore the impacts of defendant and case characteristics, and how these effects may be changed by sentencing reform, in combinatorial rather than

https://doi.org/10.1111/lasr.12001 Published online by Cambridge University Press

linear-additive terms. Both race-ethnicity and legal variables yielded unexpected and not easily interpretable results when considered independently. But the literature on stereotyping and attribution processes in criminal justice suggests that officials do not assess defendants' moral worth in linear terms, but in terms of clusters of attributes (Albonetti 1991; Bridges & Steen 1998; Engen, Steen, & Bridges 2002; Steen, Engen, & R. Gainey 2005). Thus a defendant's minority status, the particular crime with which he is charged, or his prior record considered independently may mean nothing extraordinary, but a defendant who is black, for example, *and* charged with drug dealing *and* has a prior felony conviction may personify a type of offender that is the target of particularly strong moral censure. This approach can be pursued inductively using combinatorial methods of the sort described by Ragin (1987). Third, and most fundamentally, we need more grounded ethnographic studies of the inner workings of felony courts, and particularly the habitus of the prosecutor. Classic studies of court practice that have guided the field for decades (e.g., Feeley 1979; Mather 1974; Sudnow 1964) require updating if we are to come to grips with the new regime of formally restricted sentencing options and enhanced prosecutorial power. Bowen's (2009) recent study of plea bargaining under Washington state sentencing guidelines is an important step along this path. Interestingly, and contrary to Stuntz's broader analyses (1997, 2001, 2011), her observations show no influence of caseload pressures on plea negotiations, and no apparent reluctance by prosecutors to take cases to trial. Additional studies of this sort, in a variety of jurisdictions, are required to assess local adaptations to broad criminal justice trends.

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John R. Sutton *is Professor of Sociology at the University of California-Santa Barbara. He teaches and does research on law, crime and punishment, and organizations. His research tends to be concerned with change over time in legal systems, organizational structures and practices, and institutional fields. He is the author of* Stubborn Children *(University of California Press, 1988),* Law/Society *(Pine Forge 2001), and numerous journal articles.*

https://doi.org/10.1111/lasr.12001 Published online by Cambridge University Press