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## A CONCLUDING NOTE ON THE STABILITY OF PUNISHMENT: REPLY TO BLUMSTEIN, COHEN, MOITRA, AND NAGIN\*

DAVID RAUMA

I would first like to thank Blumstein, Cohen, Moitra, and Nagin for the kind remarks opening their reply to my article. I concur that the process of testing, interpretation, retesting, and reinterpretation is necessary for science in general and for social science in particular. Although Blumstein and his colleagues ("Blumstein") express disappointment that I do not extend or challenge their theory, I feel that my article is an important step in the scientific process (*i.e.*, re-testing). Indeed, one such effort to extend and challenge Blumstein's position has just been published.<sup>1</sup> There, an effort is made to reformulate the stability hypothesis, derive more compelling tests, and, *when no support for the hypothesis surfaces*, explore alternative explanations.

There are a number of difficulties in Blumstein's reply to which I must respond before addressing a more general and related concern. I shall try to do so without a long, technical discussion that would largely repeat what is stated in my article.

First of all, Blumstein asserts that, by describing stable levels of crime in society, I have misstated his theoretical viewpoint. In his reply, Blumstein states that he is concerned with the level of punished crimes in a society, *not* the totality of unlawful acts as, according to him, I suggest. This is borne out in his earlier discussions,<sup>2</sup> but, with regard to stable levels of punishment, these discussions are fully consistent with my rendering of his position. I merely restated the Durkheimian view that crime is only defined when it is punished in some manner, and that, by this definition, stable levels of crime and punishment are one and the same. Blumstein and Cohen have already described this process:

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\* I benefited greatly from informal discussions with Alfred Blumstein.

<sup>1</sup> Berk, Rauma, Messinger, & Cooley, *A Test of the Stability of Punishment Hypothesis: The Case of California, 1851-1970*, 46 AMER. SOC. REV. (forthcoming).

<sup>2</sup> Blumstein & Cohen, *A Theory of the Stability of Punishment*, 64 J. CRIM. L. & C. 198 (1973).

It is our contention that in their discussions of criminality and deviance both Durkheim and Erikson were not referring to the class of all acts which would be considered criminal or deviant if discovered. Rather, they were concerned with only those acts which are publicly recognized as criminal (deviant) and punished in some way.

. . . Furthermore, for both men a crime is known by the characteristic reaction to it, namely its punishment. . . . The level of crime they speak of, then, includes only those acts which are publicly recognized as crimes and punished accordingly. Hence, it is not the level of actual criminal behavior which is stable, but rather the level of punished criminal acts.<sup>3</sup>

Blumstein and Cohen perhaps misstate Durkheim's position, since Durkheim only defined crime through the reaction to it: "actual criminal behavior" therefore cannot exist outside of its punishment. There is a large sociological literature that attempts to deal with this issue of undetected and/or unpunished crime. And, according to some perspectives, Durkheim's included, undetected crime cannot exist. Blumstein might well address such issues in his future work.<sup>4</sup> In any case, the issues concerning what is crime are complex and, between differing viewpoints, unresolved.

Second, Blumstein makes the point that nonstationarity due to nonhomogeneity (variance nonstationarity) is consistent with his theory. I state that in my article. I deal primarily with nonstationarity due to a nonstable mean (mean nonstationarity) because the mean is at issue in Blumstein's analyses.

Third, Blumstein makes much of trendlessness as evidence for the stability hypothesis. Again, as I state in my article, trendlessness is only one way in which a series can be mean nonstationary. If a time series shows stochastic changes in level, then, *by definition*, it has no mean level. An arithmetic average can be calculated for any observed time series, but that might not be equivalent to the mean or expected value of the underlying process generating the series (*e.g.*, the stability of punishment process). In other words, the average value for a sample is not necessarily the same as the expected value of an underlying theoretical process. Similarly, Blumstein is mistaken when he contends that, if I extended the California data through 1979, it might be stationary. If an underlying process is stationary, any of its observed realizations will, within sampling error, be stationary. Adding nine years to the California data set will not change the underlying process from nonstationary to stationary, but could, through increased statistical power, provide more efficient parameter estimates. Since statistical power increases by the

<sup>3</sup> *Id.* at 199.

<sup>4</sup> See, *e.g.*, H. BECKER, *OUTSIDERS: STUDIES IN THE SOCIOLOGY OF DEVIANCE* (1963); LIAZOS, *The Poverty of the Sociology of Deviance: Nuts, Sluts, and "Perverts"*, 20 *SOC. PROB.* 103 (1972); Thio, *Class Bias in the Sociology of Deviance*, 8 *AM. SOC.* 1 (1973).

square root of the sample size, nine more observations would not dramatically improve the power of the estimated model, nor would they likely alter its fundamental nature.

Fourth, Blumstein contends that his and my models for the United States imprisonment rate series are “analytically equivalent,” and that the univariate models I estimate for the California data are “analytically equivalent” to stationary models consistent with the stability hypothesis. It is true that the parameter values can be shown to be algebraically equivalent, but the underlying, theoretical processes they depict are fundamentally different. Again, a stationary model has a stable mean; a mean nonstationary model has none. No amount of algebraic manipulation can equate these distinct processes. In fact, Blumstein’s use of deviations from the mean ( $\bar{Z}_t$ ) to depict both his and my models prejudices the issue of whether or not a mean exists. My model represents a mean nonstationary process, and therefore deviations from the mean cannot be constructed. I show through my analyses that the two models for the United States imprisonment rate series are *empirically* equivalent, but that is not the same as algebraic or “analytical equivalence.”

Finally, there is some ambiguity, in Blumstein’s reply and in his earlier work, concerning the definition of stability. For example, Blumstein and Cohen border on circularity when they describe the United States imprisonment rate series as “reasonably constant” except for the years 1938 to 1945.<sup>5</sup> They explain how those years might be anomalous due to the impact of the Great Depression and World War II. However, why choose those years, except for the reason that imprisonment rates were anomalous? In his reply, Blumstein discusses recent increases in the United States imprisonment rate, and poses several explanations for them. One account is that United States society is “becoming inherently more punitive, and is moving to a new, higher level of ‘stable punishment.’”<sup>6</sup> If United States society is moving to a new level of punishment, it must be changing; yet, Blumstein’s only evidence for change is the use of punishment. Without an anchor for determining when and how a society is changing, aside from changes in its use of punishment, the stability of punishment hypothesis becomes, in the extreme, nonfalsifiable. Any aberration in the use of punishment can be explained as resulting from changes in society that, in turn, are identified by changes in the use of punishment. Until a theory of social change is incorporated within the stability of punishment framework,

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<sup>5</sup> Blumstein & Cohen, *supra* note 2, at 201-02.

<sup>6</sup> Blumstein, Cohen, Moitra & Nagin, *On Testing the Stability of Punishment Hypothesis: A Reply*, 72 J. CRIM. L. & C. 1799 (1981).

any contrary evidence can be too easily dismissed. Blumstein's contention of a change in United States society during the 1920s is an example of one such post hoc explanation, which I tested in California and for which I found no evidence.

More generally, a theme running throughout Blumstein's reply is that his approach, because it is theoretically based and his models are theoretically derived, is superior to and more desirable than my "data fitting" approach to testing the stability hypothesis. I agree fully that one can only proceed from a theoretical standpoint, and that, in the absence of disconfirming evidence, theoretically derived results are preferable to equally plausible but inductively derived results. In this sense, I have not refuted the stability hypothesis. Rather, I have provided alternative explanations for Blumstein's evidence. By estimating an *equally plausible* alternative model for the United States imprisonment rate series, and nonstationary models for several other series, I have shown that Blumstein's tests are not compelling. And my discussions of time series modeling procedures point out the problems with Blumstein's analysis of the U.S. series. When testing any theoretically derived hypothesis, one aim is to rule out alternative theoretical *and* atheoretical explanations for the observed data. I have shown that Blumstein has ignored the latter.