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ON TESTING THE STABILITY OF PUNISHMENT HYPOTHESIS: A REPLY

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We commend David Rauma[1] for his effort to challenge and to test empirically our theoretical work[2] on the "stability of punishment," and we thank the *Journal of Criminal Law and Criminology* for this opportunity to respond to his re-analysis and re-interpretation of our work. Much too often in criminology, and in social science generally, theories tend to be pursued and tested only by their proposers, whose credibility is necessarily suspect. Useful theory can only develop as a result of test, challenge, and modification by others. It is in this spirit that we are pleased to have this opportunity to address Rauma's work.

First, we would like to clarify some aspects of the theory that were misstated by Rauma. While our "stability of punishment" hypothesis was certainly stimulated by Durkheim's notions about a constant level of crime, we do not adopt his suggestion that the level of *crime*—defined as the totality of unlawful acts—is stable in society; in fact, there is ample evidence to the contrary. Indeed, in modern societies, even the volume of reported crime is almost always much greater than the capacity to respond to all of it. It is the recognition of the inability of society to

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[1] Rauma, *Crime and Punishment Reconsidered: Some Comments on Blumstein's Stability of Punishment Hypothesis*, 72 J. CRIM. L. & C. 1772 (1981).

[2] This work is addressed in three papers in this *Journal*: Blumstein & Cohen, *A Theory of the Stability of Punishment*, 64 J. CRIM. L. & C. 198 (1973); Blumstein, Cohen & Nagin, *The Dynamics of a Homeostatic Punishment Process*, 67 J. CRIM. L. & C. 317 (1977); Blumstein & Moitra, *An Analysis of the Time Series of the Imprisonment Rates in the States of the United States*, 70 J. CRIM. L. & C. 376 (1979) [hereinafter referred to as *U.S. States*]; additional comments are contained in Blumstein & Moitra, *Growing or Stable Incarceration Rates: A Comment on Cahalan's "Trends in Incarceration in the United States Since 1880"*; 26 CRIME & DELINQUENCY 91 (1980) [hereinafter cited as *Comment*].

punish all crime that stimulated the notion of "stability of punishment," where it is the *punished* crime, or the total amount of *punishment* delivered that we hypothesized to be stable. Thus, we hypothesize that when the "punishment capacity" of a society becomes excessively pressed, that society will find ways to accommodate by diminishing or eliminating punishment for marginally criminal activities.

In his article, Rauma seems to have ignored this underlying conceptual base, and his discussion is dominated by the question of stationarity of time series. In doing so, he misses the conceptual issues in our theory. We were thus disappointed that he did not use the opportunity offered by his re-analysis to extend or challenge the theory. Instead, his dominant attention was focused on the choice of one or another Auto regressive Integrated Moving Average (ARIMA) time-series model.[3] In general, as we show below his analyses in no sense invalidated our choices. Much of his argument, for example, involves the formulation and estimation of time series models that invoke differencing, from which he infers non-stationarity of the process models. We show below, first, that stationarity is not necessary for our theory to hold (*i.e.*, a finding of non-stationarity is not necessarily a contradiction), and, second, that his models using differencing of the time series are not necessarily

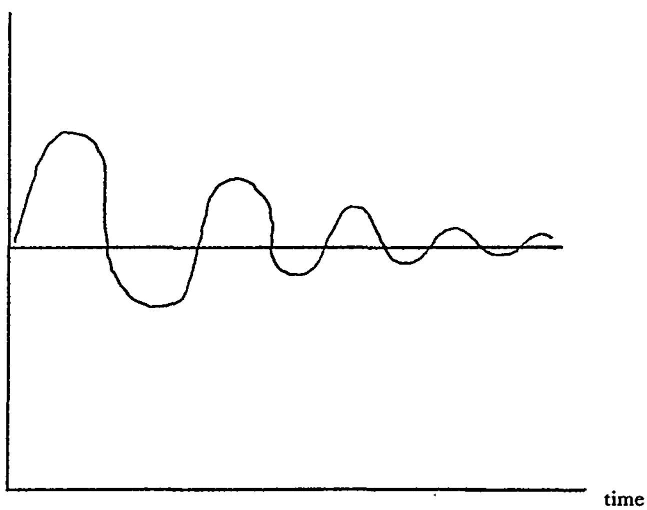

FIGURE 1

NON-STATIONARY TIME SERIES WITH STABLE MEAN AND DECREASING VARIANCE

[3] For a background on these models, see G. BOX & G. JENKINS, TIME SERIES ANALYSIS (1976); R. MCCLEARY & R. HAY, JR., APPLIED TIME SERIES ANALYSIS FOR THE SOCIAL SCIENCES (1980).

more adequate representations of the available data than our alternative choices of stationary models.

If a series were found to be stationary, then it would certainly be consistent with our theory. There are, however, some non-stationary time series that are totally consistent with our theory. It is possible, for example, for the imprisonment-rate time series to oscillate around a constant mean with decreasing variance, as depicted in Figure 1. Such a time series is clearly non-stationary (since its variance is changing with time), but its behavior is consistent with our underlying theoretical construct. On the other hand, a time series that oscillates only slightly around a significantly increasing or decreasing trend in the imprisonment rate, as depicted in Figure 2, is a non-stationary series that does contradict our theoretical construct.

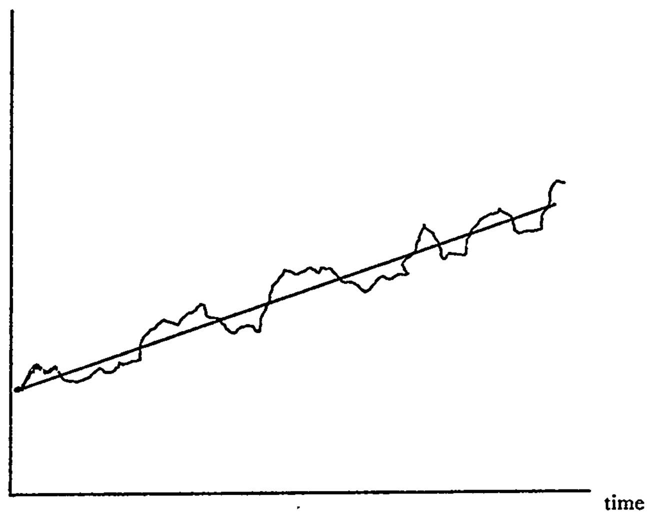

FIGURE 2

NON-STATIONARY TIME SERIES WITH TREND

Our theory emphasizes the *trendlessness* of the mean imprisonment rate and the relatively small magnitude of the fluctuations around that mean. Thus, while a finding of stationarity might be a sufficient test of our hypothesis, it is not a necessary test. Indeed, in all of our work we have focused on testing for the trendlessness of the mean (one very salient criterion), and for a low coefficient of variation (*i.e.*, the ratio of the standard deviation to the stationary mean). We are much less concerned about the stationarity of the standard deviation, as long as it is low compared to the mean.

The choice is not between stationarity and non-stationarity, but rather between a trendless mean with relatively mild fluctuations and a strongly trending mean. The level of the time series may change from time to time, but it fluctuates around a stable mean. The presence of such fluctuations may require differencing to achieve stationarity when developing ARIMA or Box-Jenkins type models. However, the presence of such stochastic non-stationarity does not necessarily imply instability in our terms. Such differencing was in fact required in modeling Louisiana, even though the imprisonment rate there was found to be trendless.[4]

Even if the more restricive requirement of stationarity were a necessary condition for the theory, Rauma has failed to provide satisfactory evidence that the U.S. series is in fact non-stationary. Both our original estimates of a stationary second order autoregressive model (AR2)[5] and Rauma's own estimates of the same model with slightly fewer observations pass the statistical tests of stationarity. The parameter estimates are within the constraints for stationarity,[6] and the test of the residuals indicates that they are approximately "white noise" (based on the Q statistic with $p > .05$). Even though we do not feel it necessary to insist on stationarity, his results clearly support our original choice of a stationary model for this data. Thus, even though the test is excessively severe, it still confirms our hypothesis.

Furthermore, Rauma makes much of the fact that he has found a model of a non-stationary process containing a first difference that appears rather different from ours, and he argues that his model is "consistent with the data and inconsistent with the stability hypothesis," even though it [his model] has a "residual mean square error . . . slightly higher."[7] While we consider a model derived from theoretical underpinning to be inherently far more attractive than one based on "data fitting," his would be of more interest if it represented a truly different formulation. It turns out, however, that this "competing" model is analytically equivalent to ours. In our analysis, we estimated a second order autoregressive (AR2) model of the following form:

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4 Blumstein & Moitra, *U.S. States*, *supra* note 2, at 385.

5 The following notation is used in this paper to denote the various time-series types discussed:

IMA(1,1)	is a first-difference, first-order moving average process
ARI(1,1)	is a first-difference, first-order autoregressive process
ARMA(1,1)	is a first-order autoregressive, first-order moving-average process
AR2	is a second-order autoregressive process

6 The bounds of stationarity of an AR2 model are:

1) $-1 < \phi_2 < 1$, 2) $\phi_1 + \phi_2 < 1$, $\phi_2 - \phi_1 < 1$. In our model, $\phi_2 = -0.7$, $\phi_1 + \phi_2 = 0.84$, and $\phi_2 - \phi_1 = -2.2$, all of which satisfy the constraints.

7 Rauma, *supra* note 1, at 1783.

$$\bar{Z}_t - \phi_1 \bar{Z}_{t-1} - \phi_2 \bar{Z}_{t-2} = a_t \quad (1)$$

Using Rauma's parameter estimates for this model from his Table 1,[8] Equation (1) becomes:

$$\bar{Z}_t - 1.5\, \bar{Z}_{t-1} + .7\, \bar{Z}_{t-2} = a_t \quad (2)$$

Rauma proposes in his Table 2[9] a first difference (ARI(1,1)) model of the form:

$$(1-B)\ Z_t = \frac{a_t}{(1-\phi'_1 B)} \quad (3)$$

Letting $\bar{Z} = Z - \mu$, Equation (3) can be re-written as:

$$(1-\phi'_1 B)(1-B)\bar{Z}_t = a_t \quad (4)$$

or

$$\bar{Z}_t - (1 + \phi'_1)\, \bar{Z}_{t-1} + \phi'_1\, \bar{Z}_{t-2} = a_t \quad (5)$$

which is analytically equivalent to Equation (1). Indeed, (5) is identical to (1) if $\phi_1 = 1 + \phi'_1$, and $\phi_2 = -\phi'_1$. In fact, using Rauma's parameter estimate from Table 2 of $\phi'_1 = .57$, we find his first difference model to be:

$$\bar{Z}_t - 1.57\, \bar{Z}_{t-1} + .57\, \bar{Z}_{t-2} = a_t \quad (6)$$

which is virtually identical to his representation of our second-order autoregressive model in Equation (2).

Many stationary second order autoregressive processes (AR2) can be modeled equaly well by a first-difference first-order autoregressive process (ARI1). In such cases, in addition to satisfying the constraints for stationarity of an (AR2) model, the parameters must also satisfy the relation $\phi_1 = (1 - \phi_2)$ with $\phi_2 < 0$. Both our own and Rauma's estimates for the United States imprisonment rate series do satisfy the stationarity constraints and come very close to meeting this last requirement. It is therefore not difficult to see how this data could be fit reasonably well by both AR2 and ARI(1,1) models. As demonstrated by Rauma's estimates of both models, however, neither model clearly dominates the other. The choice between the two conceptually different models is a very close call. As a sufficient condition test of the stability theory under consideration, the stationary AR2 model is immediately consistent with the stability-of-punishment hypothesis. Rauma's test of the ARI(1,1) model with a trend parameter further indicates that the series is trendless and thus consistent with the theory. Thus, Rauma has built the essence of his argument on a distinction in form with no real difference in substance.

Because it does attempt to provide some theoretical insight, we

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8 *Id.* at 1781.

9 *Id.* at 1782.

found Rauma's section on "Admission Rates: An Alternative Measure of Stability" more appealing.[10] But even here, his arguments are not convincing. He seems to feel that Durkheim's characterization that the "passionate reaction of graduated intensity that society exercises through the medium of a body acting upon those of its members who have violated certain rules of conduct" provides a sufficient basis for concluding that the act of sentencing to prison must be the appropriate measure of "punishment" in a stability-of-punishment context, and so the prison admissions rate is preferable to the imprisonment rate. This question of the appropriate measure of punishment is an important theoretical issue with which we have wrestled in our own considerations. It is entirely conceivable that in a society where the act of sentencing by itself represents a profound stigma,[11] commitment to prison could well represent the relevant "stable" measure of punishment. It is entirely conceivable, furthermore, that in some societies the act of conviction itself could represent an extremely stigmatizing event; in these societies, the conviction rate might be stable. Perhaps appropriate assessments through surveys in the different cultural settings would provide an indication of the most salient aspect of the punishment delivery process in each society. We expect that this most salient dimension will also be the dimension maintained at a stable rate. It is for this reason that we have urged a variety of cross-cultural studies of these issues to explore the differences in these homeostatic processes.

For the United States, we judged the ratio of average daily population of prisoners per capita, reflecting not only commitments but also time served, to be the most appropriate measure of punishment. The saliency of time served is manifested in public expressions of concern over sentence length and mandatory minimum sentences. There is also considerable political debate over prison budgets, prison capacity, and prison construction. These indicators of punishment policy concerns in American society are certainly associated more with the question of prison population than with commitments alone.

Despite Rauma's arguments for using commitments to prison as the more appropriate measure of punishment, he discussed the United States admissions data in considerably less detail than the incarceration rate. The principal finding that emerges, however, is a time series very similar to that of the incarceration rate, and both series seem quite adequately to satisfy our less restrictive stability criteria.[12] Furthermore,

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10 *Id.* at 1784-87.

11 See Blumstein & Nagin, *The Deterrent Effect of Legal Sanctions on Draft Evasion*, 29 STAN. L. REV. 241 (1977).

12 Without the precise parameter estimates for Rauma's estimated first difference autoregressive process of order one, we cannot assess the appropriateness of a second-order

the similarity between admissions and prison populations indicates a fairly constant average time served for people sentenced to prison. Once again, Rauma's arguments rest on a distinction in form, but not in substance.

The analysis of California over the time period 1835-1970 is interesting, both because of the length of the series and because of the attention it is given in Rauma's paper. While we are somewhat disappointed that Rauma failed to confirm our hypothesis, we would like the opportunity to review his data to see if there are minor modifications to his formulation (as was the case for the United States time series) that would be confirmatory. How much of his results, for example, are dictated by the clearly turbulent, Gold Rush period of the 1850s. Also, visual examination of the series suggests that Rauma should pay some attention to the issue of periodicity in the time series, an issue which he fails to address in any of his analyses of the time series. This is important because a stable, periodic time series may appear non-stationary if only particular segments are observed. Over the 1926-74 time period examined in *United States*,[13] we found California to have a stationary *but periodic* time series for its imprisonment rates, with a period in excess of 25 years. Rauma finds non-stationarity over the time period he examines. Although his time period is longer, it may well be that if he extends his data to 1979, the time series will appear stationary after all, with extremely long cycles.

We estimated the California imprisonment rate series (1926-74) as a stationary ARMA(1,1) process with parameter estimates $\phi_1 = .86(t=10.84)$ and $\theta_1 = -.57(t= -4.37)$. These parameter estimates are also within the stationarity and invertibility constraints. The general structure of the equations for an ARMA(1,1) model and for the IMA(1,1) model estimated by Rauma are very similar since:

ARMA(1,1): $(1-\phi_1 B)y_t = (1-\theta_1 B)a_t$ and
IMA(1,1): $(1-B)y_t = (1-\theta_1 B)a_t$.

An ARMA(1,1) model becomes an IMA(1,1) model when $\phi_1 = 1$, which is just outside the stationarity constraints of an ARMA model. As our estimated value of ϕ_1 of .86 is reasonably close to but still less than 1, it is easy to see how this data could also be fit reasonably well by an IMA model. Indeed, when we re-estimated our 1926-74 time series as an IMA model, the resulting parameter estimate was $\theta_1 = -0.537$. Once again, however, the results for the IMA(1,1) model do not clearly domi-

autoregressive process on the admissions data. If those parameter estimates are of the same order of magnitude as found for the incarceration rate, however, then despite Rauma's claims to the contrary, the admissions data are also likely to be satisfactorily modeled as a stationary second order autoregressive process.

13 Blumstein & Moitra, *U.S. States*, *supra* note 2.

nate those for the stationary ARMA(1,1) model, and the choice between the alternative models is a close call. The stationary ARMA(1,1) model is immediately consistent with the stability theory. Rauma's inclusion of a trend parameter in the IMA(1,1) model he estimates indicates that the California series is trendless and thus also consistent with the theory. Thus, in terms of the stability of punishment hypothesis under test, the choice between the models is not crucial, as both are consistent with the theory.

In his discussion of the California series, Rauma makes much of our *Comment*.[14] Cahalan[15] suggested a linear trend model based on nine data points of incarceration rates from 1880 to 1970. While we argued that "it is difficult to test definitively the model of so complex a process as incarceration from data at only nine data points spaced over 90 years,"[16] we did show that even those limited data, introduced to contradict our theory, were explained better by a model consistent with our theory—two constant incarceration rates with a shift from the lower to the higher rate in about 1925 accompanying rapid changes in United States society and in penological thinking. Rauma tries very hard to find such a shift in the California data, and it is not terribly surprising that he fails to find it. The nine data points merely suggest the possibility of such a shift, rather than definitively prove it—much more careful analysis on a richer national data set is required to adequately address that issue. Even if the shift did take place in the United States, however, it is entirely conceivable that the major social changes that took place in California over the past century would fail to mirror an aggregate United States phenomenon, which was more likely to reflect the situation in the Northeast and the Midwest, where the bulk of the United States population was then concentrated.

Furthermore, California is only one of the 47 states we analyzed. While the stability hypothesis did not apply universally,[17] the theory did seem to be broadly applicable. Rauma takes no notice of the fact that the time series for Canada and Norway, both of which cover over 100 years, also support our theory. Certainly the weight of the evidence in these multiple replications in a variety of settings is on the side of "stability of punishment."

This discussion certainly raises a number of important issues regarding the formulation, testing, and validation of criminologic or so-

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14 Blumstein & Moitra, *Comment*, *supra* note 2.

15 Cahalan, *Trends in Incarceration in the United States Since 1880*, 25 CRIME & DELINQUENCY 9 (1979).

16 Blumstein & Moitra, *Comment*, *supra* note 2, at 91.

17 A number of states—notably Nevada—were found to have trends, but the trends were all small (all but Nevada trended less than 2 percent per year).

cial-scientific theories. In the complex task of understanding the behavior of individuals and of social organizations, it is extremely difficult to find regular patterns of behavior. When such patterns emerge in particular settings, it becomes extremely important to find evidence of the degree to which that behavior applies more generally. It is in that spirit that we have formulated and successively tested the stability-of-punishment concept. We fully acknowledge that the variables we have identified need not be universally appropriate, and we expect that examples of jurisdictions can be found which behave in ways contrary to our theoretical construct. While we almost certainly could find rationalizations for those deviant jurisdictions, we feel that the process of developing insight is hindered rather than helped by using such contradictions as a basis for rejecting, rather than modifying the basic initial formulation.

All social theory is much too fragile to withstand rigid, mechanical tests of universality. Failings of universality inevitably will occur, and such observations should stimulate modification and revisions, or generalization of the original theoretical construct. Much of our own research has been stimulated by such a search for variation in formulation and conceptualization. We were hoping that this sort of theory building was Rauma's agenda also. Unfortunately, his agenda was much narrower. Rather than introduce an alternative theoretical construct, he has applied a data fitting methodology, and simply concludes that he can fit the data better. We believe that we have shown even that conclusion to be mistaken. But more importantly, we want to emphasize that even if he were right, the failure to suggest an alternative formulation—or to attempt to resolve the differences in the empirical evidence—contributes little to the process of theory development.

We believe the "stability of punishment" theory can be a useful and potentially important contribution because it does appear to explain a considerable amount of the adaptive behavior within the criminal justice system and society generally. We believe, however, that it should be subjected to many tests which will lead to modification and revision. In this spirit, we were disappointed that Rauma's work failed to seriously challenge the theory (despite the author's conclusions to the contrary), or to propose useful alternatives or extensions. We hope future efforts are more constructive.

We are surprised, for example, that no attention was paid to the significant growth of 40 percent in the United States imprisonment rate from 1971 to 1978.[18] It is entirely possible that American society *is* be-

[18] CRIMINAL JUSTICE RESEARCH CENTER, SOURCEBOOK OF CRIMINAL JUSTICE STATISTICS-1980 493 (1981).

coming inherently more punitive, and is moving to a new, higher level of "stable punishment." Alternatively, the increase in the 1970s might reflect the demographic consequences of aging of the post-war baby boom into the high-imprisonment ages. This is an issue we have addressed elsewhere.[19] This short-term trend could be a natural consequence of a cyclical pattern in imprisonment rates that reached its low point in 1969 and is now moving upward to maintain the long-term stable mean. Another downward shift will identify the rise in the 1970s as just the upward part of a cyclical process. Based on an analysis of demographic shifts in Pennsylvania, we estimated this turn-around would occur in about 1990.[20]

Another contributing factor to the growth in prison populations could also be the emptying over the last decade of mental hospitals under the impetus of psychopharmaceutical therapy and a policy of invoking the "least restrictive alternative" for the mentally ill. "Punishment" could well include the involuntary confinement within mental hospitals,[21] as well as in prison. Since many former mental patients get involved with the criminal justice system after their release, accounting for the transfer of inmates from mental hospitals to prison would diminish the magnitude of net increase in the level of punishment inferred from considering imprisonment rates alone.

We hope some of Rauma's future work in this area pursues some of these or related issues. We also intend to do so ourselves.

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19 See Blumstein, Cohen & Miller, *Demographically Disaggregated Projections of Prison Populations*, 8 J. CRIM. JUST. 1 (1980) [hereinafter cited as *Prison Populations*]; Blumstein, Cohen & Miller, *Crime, Punishment & Demographics*, AM. DEMOGRAPHICS, Oct., 1980, at 32.

20 See Blumstein, Cohen & Miller, *Prison Populations*, *supra* note 19, at 19.

21 See Blumstein & Cohen, *supra* note 2, at 201.