

After the Fall: Assessing the Impact of the Great Prison Experiment on Future Crime Control Policy

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IN THE FOLLOWING article, I summarize the available research on the impact of the increased use of incarceration on crime rates and identify the effects of incarceration on *individual* offenders (specific deterrence, rehabilitation effects) and on *communities*, cities, states, regions, and nations (incapacitation, general deterrence effects). The crime reduction effects of incarceration-focused strategies are then compared to other criminal justice-focused strategies (more policing or more correctional treatment) and to a range of non-criminal justice-focused strategies, such as community-level improvements in poverty, education, treatment services, and health care. Based on this review of the available research evidence, I then examine policy implications regarding more effective uses of both criminal justice-focused and non-criminal justice-focused strategies and consider the prospects for future crime control policies that result in improved individual and community-level outcomes.

The Great Prison Experiment

The decision to send an individual offender to prison represents a critical policy choice with consequences for both offenders and communities that are important to understand. We sanction for a number of different reasons, including punishment, deterrence, rehabilitation, and incapacitation. It is assumed that an *effective* sentencing strategy will achieve these aims and, in the process, improve community safety and foster individual desistance; but if

this is true, then we must also consider the possibility that an *ineffective* sentencing policy will have the opposite effect, resulting in communities that are less safe and offenders who are less likely to desist from crime.

Beginning with the 1964 United States Presidential campaign, the advocacy of “get tough” prison-focused crime control policies as a way to solve the crime problem has been a dominant—and generally successful—political strategy at every level of government (Loo & Grimes, 2004; Finckenauer, 1978). But success as an election strategy may not translate into success as an effective criminal justice policy.¹ Consider the following brief summary of our four-decade experiment in mass incarceration (Stemen, 2007, *Executive Summary*):

In the 1970s the United States embarked on one of the largest policy experiments of the 20th century—the expanded use of incarceration to achieve greater public safety. Between 1970 and 2005, state and federal authorities increased prison populations

by 628 percent. By 2005, more than 1.5 million persons were incarcerated in U.S. prisons on any given day, and an additional 750,000 were incarcerated in local jails. By the turn of the 21st century, more than 5.6 million living Americans had spent time in a state or federal prison—nearly 3 percent of the U.S. population. Having so many people imprisoned over the course of 30 years raises an obvious question: **has this experiment worked?**

The short but definitive answer to this question is that the great prison experiment has failed. First, a sizable amount of research strongly suggests that sentencing an individual to prison—and to longer sentences in particular—does *not* work as a specific deterrent (Nagin, 2010; Nagin, 2013). Second, there is little evidence to support the notion that prisons foster individual offender rehabilitation (Toch, 2005; Tonry, 2013; Byrne & Miofsky, 2009); in fact, recent research strongly suggests that prisons are criminogenic (Bales & Piquero, 2012), an assessment that is reinforced by examining the post-release failure rates of prisoners (Cullen, 2013; Byrne, 2008). Third, prison has been found to have at best only a modest (2–4 percent) general deterrent/incapacitation effect (see, e.g., Spelman, 2005; Levitt, 1996; Durlauf & Nagin, 2011). Even the research identifying modest general deterrent/incapacitation effects has been criticized on methodological grounds (Durlauf & Nagin, 2011). The sole remaining justifications for prison are incapacitation and retribution, but

¹ For a detailed discussion of how crime has been used as a political campaign issue over the last five decades, see Clear & Frost (2013). One of the interesting findings included their review was the realization that the war on crime was not just a response to record-high crime rates; it was at least in part a war on 1960s civil unrest targeting young black males not in the labor force. They point out that in the last three national elections, discussion of crime as a major political campaign issue has been muted at best. This is likely a response to public opinion polls that consistently rank a range of other issues as higher public priorities.

these seem to be an insufficient rationale for current mass incarceration policies, especially when the research on incapacitation effects is critically reviewed (Durlauf & Nagin, 2011), the problem of false positives is considered (Nagin, 2013), and the crime mix of convicted federal, state, and local prisoners is examined (Blumstein, 2011).

It has been argued that the use of *prison-based* sanctions would make communities safer places. This has not proven to be the case, particularly in the small number of communities where crime is the most likely to occur, and where offenders reside before and after their time in prison (Byrne, 2009). As Sampson and Loeffler have documented, "Like the geographically concentrated nature of criminal offending by individuals, a small number of communities bear the disproportionate brunt of U.S. crime policy's experiment with mass incarceration" (2010, p. 20). Putting large numbers of individuals living in poverty-pocket, high-minority-concentration neighborhoods in prison has done little to alleviate the crime problem in these areas; in fact, there is considerable evidence that this strategy increased the level of crime in these communities (Clear & Frost, 2013).

Given the failure of the great prison/mass incarceration experiment, the question becomes: Where do we go from here? Faced with the rising cost of incarceration and a body of empirical research that challenges the continuation of this policy of mass incarceration, there appears to be both broad public and bipartisan political support in many parts of the United States to downsize prisons (Jacobson, 2005) and to spend at least some of the money now allocated to prisons on a new set of crime control policies that will have a larger impact on crime in our communities (Austin et al., 2013; Cullen, 2013), while supporting long-term desistance from crime among individuals (Maruna, 2012).

This strategy has been described broadly as justice reinvestment (Tucker & Cadore, 2003), but there is currently a debate on the nature and extent of this reinvestment strategy, focusing primarily on how best to reallocate resources in order to make communities safer (Austin et al., 2013; Sherman, 2011). Some have advocated for the reallocation of funds within the corrections resource pie, with a greater proportion of funds allocated for individual offender treatment in both institutional and community settings (Taxman, Pattavina, & Caudy, in press), while others argue for increased funding for a broad range of crime

prevention strategies in targeted high-risk/high-crime communities (Austin et al., 2013), including both *criminal justice-focused* strategies based on increasing the number of police in targeted, high-crime areas, and *non-criminal-justice-focused* strategies designed to address the root causes of crime (poverty, education level, inequality, economic opportunity). Before I offer my assessment of these variations on the justice reinvestment theme, it makes sense to examine carefully the research on both the specific and general deterrent effect of incarceration, and then compare the impact of prison to the projected impact of investments in the other strategies—both criminal justice-focused and non-criminal justice focused—being proposed.

The Specific Deterrent Effect of Prison

Recent evaluation research on the impact of incarceration on individual offenders' post-release behavior is summarized in Table 1, which includes a group of studies first identified by Patrice Villettaz and colleagues (2006) in their systematic evidence-based review of the available research on the impact of custodial vs. non-custodial sanctions on offender recidivism. Studies conducted between 1960 and 2002 that met the authors' inclusion criteria were included in their review. A subsequent review of all research on this topic between 2002 and 2013 identified several additional studies and research reviews. The findings from this two-stage review are unequivocal. In terms of specific deterrence effects on *individual* offenders, there is no methodologically rigorous evidence that the experience of incarceration reduces an offender's risk of re-offending upon return to the community. In fact, it appears that when compared to similar groups of offenders placed in one of a range of alternative, non-custodial intermediate sanctions, prisoners actually re-offend at a higher rate. Unfortunately, any definitive statements on the comparative effects of incarceration versus non-incarcerative sanctions await the completion of more and higher-quality research, preferably using experimental designs.

A review of the research from Villettaz and colleagues' (2006) systematic evidence-based review of prison versus community-based sanctions illustrates the current problems facing the evaluation of existing research evidence using "gold"-level scientific review standards, which focus exclusively on the results from RCTs (randomized control trials).

Villettaz et al. (2006) were able to locate only five controlled or natural experiments conducted on custodial versus non-custodial sanctions over a 50-year review period. The researchers concluded that "Although a vast majority of the selected studies show non-custodial sanctions to be more beneficial in terms of re-offending than custodial sanctions, no significant difference is found in the meta-analysis based on four controlled and one natural experiments" (Villettaz et al., 2006, p. 3). Of these five experiments, only three targeted adult offenders. One study that compared prison to probation (Bergman, 1976) showed that probationers fared significantly better. However, a second study comparing prison to community service had mixed results (Killias, Aebi, & Ribeaud, 2000). A third natural experiment comparing the effects of a 14-day prison term to a suspended sentence reported mixed results as well (Van der Werff, 1979).

The conclusions reached in the Villettaz et al. (2006) systematic review focused exclusively on the five experimental studies examined in their meta-analysis, but did not include the other 18 studies they identified as meeting the study's minimum review criteria. (Note that #24, 25, and 26 in Table 1 are more recent studies not included in Villettaz et al.'s 2006 meta-analysis.) Examination of Table 1 reveals that 11 of these 18 studies showed positive effects for a range of non-custodial sanctions, including probation, home confinement, community service, and mandatory alcohol treatment in drunk-driving cases. Only 2 studies showed positive effects for a prison sanction, 1 where prison fared better than electronic monitoring for low-risk offenders (Bonta et al., 2000) and the other where shock incarceration fared better than probation (MacKenzie & Shaw, 1993). The remaining 5 studies (see Table 1) identified no significant differences between experimental (3 prison, 2 shock incarceration) and control (home confinement, probation, community service, and no prison) groups.

A subsequent review of available research findings and reviews conducted in recent years does not suggest that there is new evidence to support the notion that the prison experience has a specific deterrent effect (Cid, 2009; Bales & Piquero, 2012; Spohn, 2007; Durlauf & Nagin, 2011; Nagin, 2010; Nagin, 2013). This research challenges the underlying assumptions of classical and more recent deterrence-based theories of crime used to justify the use of imprisonment for a wide

range of offenders. There is of course a possible caveat. It could be argued that the higher recidivism rates generally reported for prisoners (compared to non-prisoners) do provide evidence that the prison typologies do, in fact, select a target group of convicted offenders who pose a *greater* risk of re-offending than those sentenced to some form of community-based sanction.

It certainly appears that our current corrections system can be described in the following manner: We are better at identifying *risk level* than we are at developing strategies that result in *risk reduction*. However, it is in fact not clear that current sentencing schemes are accurately described as risk-focused, in that many offenders we send to prison are there for punishment purposes, not because they have been identified as high risks to the community. Regardless of an offender's predicted risk level, punishment by use of a prison sanction is imposed in whole or in part as a specific deterrent. Is it possible that the use of this sanction has the opposite effect?

It has been argued that the *prison experience* increases the risk posed by prisoners upon release to the community; indeed, this is the finding reported in two recent studies (Bales & Piquero, 2012; Cid, 2009). The study by Cid (2009) compared two sanctions, prison and suspended sentences, and found that the use of prison increased recidivism risk. A similar finding was reported by Bales and Piquero's comparison of offenders sanctioned to either prison or to Florida's Community Control program. Even after controlling for differences between the two groups (age, sex, race, current offense, prior record) as recommended by Nagin et al. (2009), Bales and Piquero identified a significant criminogenic effect of prison on subsequent offender behavior upon release. However, it is important to note the limitations of the body of research identifying the criminogenic effects of the prison experience. As Bales and Piquero observed, "We did not unpack what it is about imprisonment that produced more crime and alternately what it is about community control that led to less crime after release" (2012, p. 98). While it is clear that we need more high-quality research in this area, there is sufficient evidence supporting the contention that prisons—as currently organized—make offenders worse.

A review of the available research on the impact of the prison experience reveals that classical, deterrence-driven strategies do not have a sound empirical foundation. Prisons not only don't deter, they also appear to make

offenders worse. However, it should also be noted that evidence of positive individual offender change—using a combination of control and treatment—can be found in *both* institutional and community settings (Byrne & Taxman, 2006; Byrne & Pattavina, 1992). Although the reported effect sizes for prison treatment programs are modest (a 10 percent absolute reduction in recidivism), there is reason to anticipate improvements in these effects in prison systems designed to focus more on offender change rather than on short-term offender control (Taxman & Pattavina, 2013; Cullen, 2013; Welsh & Farrington, 2006; MacKenzie, 2006; Byrne & Pattavina, 2007). In other words, *comprehensive assessment-oriented and intensive treatment-focused prisons* may be the appropriate classification for some convicted offenders, but not because there is evidence that the prison experience will deter these individuals from future involvement in crime. Rather, prison may represent the appropriate location (and control level) for the provision of the types of treatment and services targeted to the offender typology being used (e.g., sex offender, drug offender, mentally ill offender, batterer, violent offender, etc.). This is precisely the point being argued by those in favor of downsizing prisons (Jacobson, 2005) and by advocates of prison reform (or rather prison transformation), who argue that we need to replace "bad" control-oriented prisons with "good" change-oriented prisons (Maruna & Toch, 2006; Deitch, 2004; Gibbons & Katzenbach, 2006). These research findings suggest that we need to rethink our prison (in/out) typology focusing on individual offender control concerns rather than on the false promise of specific deterrence. As Durlauf and Nagin (2011, p. 44) recently observed, "The fact that incapacitation might be appropriate for some criminals does not mean that imprisonment needs to be nearly so widespread as it is." Given the research on specific deterrence, it would be hard to disagree with this understated assessment.

One final observation on specific deterrent effects is the fact that "there have been comparatively few studies of the deterrent effects of sentencing enhancements, judged relative to their importance in contemporary crime control policy" (Nagin, 2013, p. 226). Table 2 summarizes the findings from the six studies (five post-Stemen's study review period) identified by Nagin as offering "particularly convincing evidence on the deterrent effect of incarceration" (2013, p. 227). It is unclear, however, why these studies would

be described as constituting convincing evidence. Overall, these studies offer, at best, a mixed bag of findings quite similar—as Nagin himself points out—to a two-decade earlier review by McDowall, Loftin, and Weirsmann (1992) on the impact of mandatory sentence enhancements for gun crimes, which found no deterrent effect.

Of the six studies identified by Nagin (2013), only two reported significant specific deterrent effects associated with the increased certainty of punishment: Weisburd, Einat, and Kowalski (2008) focused on the problem of delinquent fines, while the Kleiman study (2009) examined the problem of drug-test failures. Both of these studies examined the impact of increasing punishment *certainty* on the level of compliance among probationers, and both studies identified significant effects linked directly to the certainty of punishment. Both these studies need to be considered carefully.

Weisburd and colleagues (2008) conducted a randomized field experiment that examined the threatened use of incarceration as a method to induce probationers to pay outstanding fines. Researchers limited their target population to a small subgroup of the probation population with the following characteristics: low risk to recidivate, no substantial drug or alcohol problems, not currently participating in a residential treatment program for substance abuse, no prior arrests for violent or sex crimes, some prior work history, no physical or psychological disabilities that would make employment difficult, and a stable residence. In other words, only the subgroup of probationers who could work to pay off fines, but refused, were targeted for the study. Among this group, there were additional exclusion criteria: Only probationers with at least 12 months of remaining supervision time who were NOT currently involved in a probation program with community service as a component were considered eligible. Even here, they were only placed in the pool of eligible probationers if they "had missed at least 3 months of scheduled payments or were 60% or more in arrears because of missed or partial payments" (Weisburd et al., 2008, p. 15). The initial target population and randomization process involved 228 cases from eight separate New Jersey counties that were placed in one of three groups: a VOP (violation of probation) only group, a VOP plus intensive supervision and community service group (MUSTER: MUST Earn Restitution), and a control group receiving regular probation

TABLE 1.

Characteristics of 23 fully eligible studies identified by Villettaz et al. (2006) and three more recent studies (24, 25, 26)

N	Study Design	Custodial sanction	Non-custodial sanction	Offender type	Standard-limited time served	Specific crime	Follow-up period	Custodial impact
Controlled randomized trials								
1		Placement	Intensive supervision	Juveniles	no	no	24 months	0
2		Prison	Probation	Adults	no	no	12 months	0
3		Prison	Community service	Adults	14 days	no	24 months	0
								0
4		Correction program	Restitution	Juveniles	no	no	22 months	0
Natural experiment								
5		Prison	Suspended sentence	Adults	14 days	no	6 years	0
								0
								0
Matched-pair design studies								
6		Detention	Probation	Juveniles	no	no	5 years	0
7		Prison	Community service	Adults	8 months	no	5 years	0
8		Prison	Probation	Adults	no	no	24 months	0
9		Prison	Home confinement	Adults	no	no	5 years	0
10		Prison	No prison	Adults	no	no	10.5 years	0
Studies with four or more control variables								
11		Probation with institutional treatment	Probation, Conditional prison sentence	Adults	no	no	24-36 months	0
12		Prison	Electronic monitoring and rehabilitation	Adults	no	no	12 months	0
13		Prison	Electronic monitoring and rehabilitation	Adults	no	Low- and high-risk offenders	12 months	0
								1
14		Prison	Non imprisonment	Adults	no	no	36 months	0
15		Prison	Probation	Adults	no	no	Not clearly defined	0
16		Prison	Alcohol treatment and license suspension	Adults	no	Drunk-driving	18 months	0
Studies with four or more control variables								
17		Shock incarceration	Probation	Adults	no	no	24 months	1
18		Shock incarceration	Probation	Adults	no	no	12 months	0
19		Shock incarceration	Probation	Adults	8-State comparison	no	12/24 months	0
20		Prison	Community service	Aboriginal Adults	no	no	3.5 years	0
21		Prison	Probation	Adults	no	no	6-12 months	0
22		Prison	Probation	Adults	no	Drug offenders and others	4 years	0
23		Prison	Alcohol treatment and license suspension	Adults	no	Drunk-driving	24 months	0
24		Prison	Suspended sentence	Adults	No; maximum 3 years	No	8 years	
25		Prison	Community Control	Adults				
26		Prison	Probation	Adults	no	Drug, drug-involved, and nondrug	1-5 years	

Non-custodial impact	Significant effect (p<.05)	Study name
0	n.s.	Barton, W.H., Butts, J.A. (1990) (#10)
1	sig.	Bergman, G.R. (1976) (#91)
0	- n.s., for prevalence and incidence of arrests and convictions,	Killias, M., Aebi, M., Ribeaud, D. (2000) (#25)
1	- sig., improvement before/past arrest rate	
0	n.s.	Schneider, A.L. (1986) (#66)
0	- n.s., for traffic offenders	Van der Werff, C. (1979) (#124)
0	- n.s., for property offenders	
1	- sig., for violent crime offenders	
1	sig.	Kraus, J. (1974) (#76)
1	n.s. (p<.10)	Muiluvuori, M.-L. (2001) (#68)
1	n.s. (p<.10)	Petersilia, J., Turner, S., and Peterson, J. (1986) (#45)
0	n.s.	Smith, L.G., Akers, R.L. (1993) (#74)
0	n.s.	Weisburd, D., Waring, E., Chayet, E. (1995) (#16)
1	sig.	Bondeson, U.V. (1994/2002) (#1002)
1	sig.	Bonta, J., Wallace-Capretta, S., Rooney, J. (2000) (#32)
1	- sig., for high-risk offenders	Bonta, J., Wallace-Capretta, S., Rooney, J. (2000) (#20)
0	- sig., for low-risk offenders	
1	sig.	Börjeson, B. (1996) (#1005)
0	n.s.	Brennan, P.A., Mednick, S.A. (1994) (#23)
1	sig.	DeYoung, D.J. (1997) (#2)
0	sig.	MacKenzie, D.L., Shaw, J.W. (1993) (#31)
1	n.s. (p<.10)	MacKenzie, D.L. (1990) (#56)
0	n.s.	MacKenzie, D.L., Brame, R., McDowall, D., Souryal, C. (1995) (#72)
0	n.s.	Roeger, L.S. (1994) (#64)
1	sig.	Savolainen, J., Nehwadowich, W., Tejaratchi, A., Linen-Reed, B. (2002) (#9)
1	sig.	Spohn, C., Holleran, D. (2002) (#35)
1	n.s. (p<.10)	Tashima, H.N., Marelich, W.D. (1989) (#43)
1		Cid (2009)
1		Bales and Piquero (2011)
1		Spohn (2007)

supervision. Due to an eligibility error, 30 ineligible offenders were originally included in the study, but then subsequently removed, resulting in a final study with 66 VOP cases, 63 MUSTER cases, and 69 regular probation cases. They conducted a six-month follow-up and found that the threat of a violation of probation and the resulting possibility of incarceration had a significant impact on fine payment, with the highest fine repayment levels among probationers that were assigned to the VOP group (39 percent with 100 percent compliance, compared to 34 percent in the MUSTER group, and only 13 percent in control group).

These findings must be considered as preliminary, given the small sample size and the multiple exclusion criteria used to distinguish a subgroup of probationers who were appropriate for this type of intervention. As Weisburd and colleagues point out, "Although our findings strongly support the idea that threats of violation of probation and possible incarceration are a powerful tool for gaining compliance with financial penalties in the courts, they do not mean that such an approach is efficient or cost effective for the criminal justice system" (2008, p. 29). They go on to note that unlike most offenders under probation supervision, "These offenders were people who had the ability to work and often the means to pay financial obligations" (p. 30). Given the low-risk, nonviolent, stable, employment-ready, non-substance-abusing population targeted here, another possible non-incarceration sanction threat may work even better: Threaten to report the failure to pay to the big three credit bureaus. In this study, at least four offenders received some jail time; locking up even a single one of these probationers for failure to pay fines is an incredible waste of resources.

The second study identified by Nagin (2013) was the evaluation of Hawaii's HOPE (Hawaii Opportunity Probation with Enforcement) program, which utilized a combination of drug testing and swift and certain punishments for drug-test failures to increase compliance among substance-abusing offenders on probation. The claims of effectiveness associated with this program are certainly newsworthy, and reminiscent of the claims associated with another swift and certain response strategy, Operation Ceasefire (Kennedy, 2009). Consider the following assessment from one of the two evaluators of the Hawaii HOPE program, Mark Kleiman (2010, p. 120): "In Hawaii, a judicial warning

that the next positive drug test would draw an immediate jail term measured in days succeeded in virtually ending drug use for more than three quarters of a group of chronically defiant felony probationers, most of them methamphetamine users.” As described here, people will stop doing drugs if there are swift and certain responses to drug-test failures; drug treatment is not a necessary feature of this intervention strategy; neither is the threat of a severe sanction (Kleiman, 2010).

When reviewing this study, the first distinction that needs to be made is between the original, large non-experimental study conducted by Hawken and Kleiman (2009), which is available for review on the National Institute of Justice’s (NIJ) website, and the much smaller randomized control trial conducted as a follow-up to the original study, which is included in brief summary form (7 pages) as an Appendix to the NIJ report. It appears that the authors (see, e.g., Kleiman, 2009) and commentators (Nagin, 2013) may misrepresent the findings from the HOPE evaluation, because they jump from discussion of one study to another without emphasizing that due to sample size, target population, and study design differences, these two studies are not interchangeable.

The RCT (randomized control trial) described in the Appendix of the NIJ evaluation includes an overview of the RCT study design and the key findings from this evaluation, which compared compliance rates and selected outcomes for HOPE participants ($n = 330$) and a control group ($n = 163$). One key limitation noted at the outset was that “The RCT used an intent-to-treat design, i.e., all offenders assigned to the HOPE condition were included in the HOPE group, *even if they failed to appear for their warning hearing to formally enter the program (emphasis added)*.” This distinction had important implications for our study, as 30 percent of the offenders who had their probation revoked and were sentenced to an open term under HOPE had never appeared for a warning hearing” (Hawken & Kleiman, 2009, p. 59). Unfortunately, the summary of findings included in the NIJ report does not include any discussion of the implications of this decision and the likely impact on the results presented. The overall findings, including this 30 percent subgroup (who did not receive the “treatment”), were quite positive, and indicated that the HOPE participants had significantly fewer no-shows for probation appointments (9 percent vs. 23 percent), fewer positive urine tests (13 percent

vs. 46 percent), fewer arrests during a one-year follow-up (2 percent vs. 47 percent), fewer revocations (7 percent vs. 15 percent), and less time incarcerated (138 days vs. 267 days). As a result of the original non-experimental study and the subsequent randomized control trial, NIJ is currently funding additional research on the Hawaii HOPE program over a longer follow-up period, but one likely problem with this follow-up research has been identified by Hawken and Kleiman in their non-experimental evaluation, “Due to the high rate of non-compliance in the comparison group, after one year, judges began to transfer comparison probationers to HOPE. By the end of the second year, nearly 40 percent of the probationers were transferred” (2009, p. 49).

In addition to the above-mentioned research on Hawaii’s original HOPE program, NIJ, in conjunction with BJA, is also funding a multi-site replication of the HOPE model—using a randomized control trial—in four separate United States jurisdictions: Clackamas County, Oregon; Essex County, Massachusetts; Saline County, Arkansas; and Tarrant County, Texas. Two of the country’s most respected evaluation researchers, Pam Lattimore from The Research Triangle Institute and Doris MacKenzie from Pennsylvania State University, will conduct the evaluation, which should be completed by 2015. It certainly seems premature to view this research as definitive evidence of combining punishment certainty and celerity to induce probationers to stop using drugs. In fact, the entire focus on formal mechanisms of social control ignores a large body of existing research that supports the contention that informal social control mechanisms are much stronger specific deterrents than formal social control mechanisms (Byrne, 2009).

The General Deterrent and Incapacitation Effect of Prison

The second primary question posed at the outset of this review is whether or not prison has any general deterrent effect. Table 3 was developed by Don Stemen (2007) and included in a review he completed for the VERA Institute of Justice on the impact of incarceration on local, state, and national crime rates. Since this review was completed, a number of other research studies have been completed and critically reviewed, but the substantive findings are unchanged: Prisons have only a modest impact on crime rates (for an overview, see Nagin, 2013).

Stemen’s review of the research on the impact of prison on crime revealed that variation in effect sizes across studies—in particular for the studies looking to demonstrate a general deterrent effect—could be attributed to the following factors:

- how the effectiveness of the prison sentence is to be determined (e.g., impacts on individuals, impacts on neighborhoods, state or national level effects);
- the use of comparison groups and/or comparison policies;
- the criterion measure employed (violent crime, overall crime);
- the statistical procedures, including controls for simultaneity, that were applied; and
- whether cost-effectiveness comparisons were included (money on such alternative crime reduction strategies as improving treatment, the quality of education, early childhood intervention, or employment/anti-poverty initiatives versus money spent on incarcerating an increased number of offenders, in terms of overall crime reduction).

Despite these cross-study differences, Stemen (2007) argued that it is possible to use this body of research to answer the question that policymakers and the general public continually ask: Does prison work as a general deterrent? By focusing on the results of research conducted at different levels of aggregation with, where available, appropriate statistical controls for simultaneity, we begin to see a clearer picture of the general deterrent impact of incarceration (Levitt, 1996; Spelman, 2000; Spelman, 2005). At the *national* level, a 10 percent increase in the rate of incarceration is estimated to result in about a 4 percent decrease in the rate of index crimes, with estimates of the impact on violent crimes between 3.8 and 4.4 percent. Studies (see Table 2) claiming larger reductions in crime (between 9 and 22 percent) using national-level data did not include controls for simultaneity. Based on *state*-level data, a 10 percent increase in the incarceration rate is associated with a decrease in the crime rate between 0.11 and 4 percent. At the *county* level, a 10 percent increase in incarceration is associated with a 4 percent reduction in the crime rate (Stemen, 2007). Thus, as Spelman, Levitt, and others have concluded, America’s incarceration binge has had—at best—only a modest impact on crime rates at the national, state, and local level (note: for a critical review, see Durlauf & Nagin, 2011).

One underlying assumption of general deterrence is that the costs of a particular

prohibited behavior must outweigh the benefits of the action, but only marginally, for an individual to be deterred. There is no assumption that more punishment translates into more compliance with the law. Indeed, too much punishment could have the opposite effect. Two recent studies provide support for this contention, suggesting that there is a “tipping point” for incarceration levels that can be demonstrated at both the state level and the neighborhood level (Liedka, Piehl, & Useem, 2006; Rose & Clear, 1998; Clear, Rose, Waring, & Scully, 2003). Incarceration reduces crime, they argue, but only up to a point. Once the incarceration rate hits a certain level—at the state level this tipping or inflection point appears to be about 325 inmates per 100,000 population—crime rates actually increase (Liedka, Piehl, & Useem, 2006). Although they do not identify a specific neighborhood-level tipping point, Rose and Clear (1998) explain why they believe this also occurs at the local level:

High rates of imprisonment break down the social and family bonds that guide individuals away from crime, remove adults who would otherwise nurture children, deprive communities of income, reduce

future income potential, and engender a deep resentment toward the legal system. As a result, as communities become less capable of maintaining social order through families or social groups, crime rates go up. (Rose & Clear, as summarized by Stemen, 2007, p. 6)

The implication of this research on possible tipping points is not the abandonment of prison as a sanction, but rather greater parsimony in its application. When viewed in this context, it is apparent that definitions of the “in-prison” group were expanded in the 1980s to include “large numbers of nonviolent marginal offenders” (Stemen, 2007, p. 8). Since there is no evidence that this expanded definition had an added effect on crime rates (Zimring & Hawkins, 1997), it makes sense to consider earlier, more restricted definitions of who should be considered for prison, which focused primarily on the identification of serious, violent offenders (Nagin, Cullen, & Jonson (2009).

Other Ways to Reduce Crime and Foster Desistance

It is worth noting that much of the research on general deterrent effects does not include

an examination of various “what if” scenarios. What if we spent the same money used to expand our prison capacity on other strategies designed either as a general deterrent (for example, more police) or as a community-level risk-reduction strategy of investment in education, treatment, employment, housing, health care, or increased wages? According to Stemen (2007), only about 25 percent of the major crime drop that occurred in the United States between 1990 and 2005 appears to be linked directly to our increased use of incarceration. The other 75 percent of the drop can be linked to a variety of other factors, including fewer “at risk” youth in the general population, decrease in crack cocaine markets, lower unemployment rates, higher wages, higher graduation rates, the recent influx of Latino immigrants,² and of course, changes in police strength and arrest tactics (Levitt, 2004; Sampson & Bean, 2006). A review of the research on several of these factors (Stemen, 2007, pp. 9-12) suggests that they are likely to offer more crime reduction benefits than prison expansion does, and at much less cost. Consider the following:

² For a full discussion of what is referred to as the “Latino Paradox,” see Sampson and Bean (2006).

TABLE 2.

Recent Evaluation Studies of Sentencing Enhancements (Nagin, 2013)

Study	Sanction	Method	Outcome
Weisburd, et al., 2008	The use of imprisonment to enforce fine payment	Randomized Control Trial targeting low-risk probationers with ability to pay fines but delinquent	Substantial effects reported with a small subgroup of all probationers, making generalization beyond this group misleading
Hawken, A., and Kleiman, M. (2009); Kleiman, 2009	The use of imprisonment to enforce probation conditions	Randomized Control Trial, targeting drug-involved probationers, but with a significant post-randomization problem noted	Substantial drug use reduction, fewer arrests, and prison use reduction effects reported for HOPE participants; independent multi-site replication study now being conducted
Helland and Tabarrok, 2007	The deterrent effect of California’s Three Strikes Law	Survival Analysis of data from California and 3 states without 3 strikes laws: Illinois, New York, Texas	Modest effect: 17–20% reduction in arrest rates among subgroup of offenders with two strikes, at an estimated cost of \$148,000 per crime avoided
Raphael and Ludwig, 2003	The deterrent effect of sentence enhancements for gun crimes	Estimation of deterrent effects of sentence enhancements for gun crimes	No deterrent effect identified
Hjalmarsson, 2009	Adult jurisdiction and heightened threat of prison	Estimation of deterrent effects based on data from the National Longitudinal Survey of Youth (NLSY97) on perceptions of punishment over time among a cohort of 8984 individuals between 12 and 16 as of Dec. 31, 1996	No deterrent effect: overall, “individuals underestimate the change in punishment severity at the age of criminal majority” (2008, p. 245)
Lee and McCrary, 2009	Adult jurisdiction and heightened threat of prison	Florida sample of youth with at least one felony arrest by age 17	No deterrent effect at age of criminal majority

TABLE 3.

Summary of studies estimating the impact of incarceration rates on crime rates (Source: Stemen, 2007, for all studies 1988–2006; with additional studies 2007–2013 included from a review by Durlauf and Nagin, 2011)

Studies that do not account for simultaneity		
Study	Data	Estimated percentage change in crime rates due to a 10% increase in incarceration rates
Devine, Sheley, and Smith (1988)	National, 1948–1985	-28.4 (violent offenses) -19.9 (property offenses) -22.0 (index offenses)
Marvell and Moody (1997, 1998)	National, 1958–1995	-7.9 (violent offenses) -9.5 (property offenses) -9.3 (index offenses)
Marvell and Moody (1994)	49 states, 1971–1989	-1.6 (index offenses)
Besci (1999)	50 states and D.C., 1971–1993	-0.46 (violent offenses) -0.91 (property offenses) -0.87 (index offenses)
Raphael and Winter-Ebmer (2001)	50 states, 1971–1997	not significant (violent offenses) -1.1 (property offenses)
Donahue and Levitt (2001)	50 states, 1973–1997	not significant (violent offenses) -1.6 (property offenses)
Levitt (2001)	50 states, 1950–1999	-0.76 (property offenses) -1.3 (violent offenses)
DeFina and Arvanites (2002)	50 states and D.C., 1971–1998	not significant (murder, rape, assault, robbery) -1.1 (burglary) -0.56 (larceny) -1.4 (auto theft)
Kovandzic and Sloan (2002)	57 Florida counties, 1980–1998	not significant (index offenses)
Washington State Institute for Public Policy (2003)	39 Washington counties, 1980–2001	-2.4 (index offenses)
Liedka, Piehl, and Useem (2006)	50 states and D.C., 1970–2000	-0.118 (index offenses) (states with incarceration rates <325) +0.05 (index offenses) (states with incarceration rates >325)
Kovandzic and Vieraitis (2006)	58 Florida counties, 1980–2000	not significant (index offenses)
Studies that do account for simultaneity		
Study	Data	Estimated percentage change in crime rates due to a 10% increase in incarceration rates
Levitt (1996)	50 states and D.C., 1971–1993	-3.8 (violent offenses) -2.6 (property offenses)
Spelman (2000)	50 states and D.C., 1971–1997	-4.0 (index offenses)
Spelman (2005)	254 Texas counties, 1990–2000	-4.4 (violent offenses) -3.6 (property offenses)
Additional Studies and Reviews 2006–2013		
Durlauf and Nagin (2011)	Selected Research review	No general deterrent effect identified

(1) **Police:** Levitt (1997) found that a 10 percent increase in the size of a city's *police force* was associated with an 11 percent lower violent crime rate and a 3 percent lower property crime rate (using county-level data); however, other more recent analyses and reviews (Bradford, 2012) suggest that increasing police force size will have no impact on the violent crime rate, and only marginal improvement (1–3%) in property crime rates.

(2) **Employment:** According to several studies (Levitt, 1996; Levitt, 1997; Raphael & Winter-Ebmer, 2001; Gould et al., 2002), a 10 percent decrease in the state's *unemployment* rate corresponded with a 10–16 percent reduction in property crime, but had no effect on violent crime (state and county-level data);

(3) **Income:** a 10 percent increase in *real wages* was associated with a 13 percent lower index crime rate, a 12 percent lower property crime rate, and a 25 percent lower crime rate at the national level (Gould et al., 2002); state-level analyses identified a 16 percent lower violent-crime rate (Raphael and Winter-Ebmer, 2001); and individual-level analyses reveal that a 10 percent increase in real wages is associated with a 10 percent decrease in crime participation (Grogger, 1998);

(4) **Education:** a one-year increase in the average *education* level of citizens resulted in a 1.7 percent lower index crime rate, while a 10 percent increase in graduation rates resulted in a 9.4 percent reduction in the index crime rate and a 5–10 percent reduction in arrest rates, through the increased wages associated with graduation (Lochner & Moretti, 2004, as summarized by Stemen, 2007, pp. 9–12).

While the link between police strength (more police per capita), arrest levels (more arrests, especially for public-order offenses) and subsequent reductions in crime is certainly consistent with deterrence-based strategies, few research studies have compared the crime-reduction effects of criminal justice-focused and noncriminal justice-focused strategies. It seems clear from our brief review that research on the general deterrent effect of incarceration needs to be examined and its effects compared to other possible criminal justice-focused strategies, such as strategies designed to increase certainty and celerity. However, these criminal justice-focused strategies are only one piece of a much larger puzzle, and they need to be considered in the broader context of the wide range of non-deterrence-based social policy changes

that may achieve the greater crime reduction effects at a fraction of the cost.

After the Fall: New Directions in Crime Control Policy

As Robert Sampson and Charles Loeffler pointed out in a recent essay, “Incarceration in the United States is now so prevalent that it has become a normal life event for many disadvantaged young men, with some segments of the population more likely to end up

in prison than attend college” (2010, p. 20). In the aftermath of the dual crisis of confidence in both our economic and mass incarceration policies, there is a search for alternatives among both liberals and conservatives across the United States. One emerging crime control strategy that is currently being embraced across the political aisle comes immediately to mind: justice reinvestment. However, it is becoming increasingly clear that the term justice reinvestment has different meanings, both

within and across countries (Homel, 2014). In the United States, there are essentially three justice reinvestment strategies that have been proposed to date:

1. A *treatment investment* strategy, which would increase the level and quality of treatment provided in both institutional and community corrections systems at the federal, state, and local level (Taxman & Pattavina, 2013; Farrington & Welsh, 2007);
2. A *police investment* strategy, which would increase the certainty of apprehension by increasing the size of the police force in targeted, high-crime communities, and by shifting “the focus of the police from people to places” (Weisburd, 2011, 159); and
3. A *community investment* strategy, which would focus on reallocating corrections resources currently expended on prisons to a variety of crime prevention strategies, including strategies focused on addressing the root causes of crime in targeted high-risk communities (Homel, 2014; Farrington & Welsh, 2007; Welsh & Farrington, 2012; Farrington, 2013).

Each of these strategies of justice reinvestment has empirical support and each strategy should be considered carefully. Side-by-side comparisons of the known crime reduction effects of these strategies need to be conducted, both in terms of individual change/desistance from crime, and community safety. Before the relative merits of these three variations on the justice reinvestment strategy can be assessed, we need to know much more than we do about the potential impact of these policies on both offenders and communities.

Consider for example the notion that we should allocate more resources within corrections for offender treatment, both in prison and in the community. We can identify the effects of this strategy on cohorts of offenders (see, e.g., the recent simulation modeling research by Taxman & Pattavina, 2013, and the recent review by Cullen, 2013), but we know very little, if anything, about the impact of this type of treatment investment strategy on crime rates in the targeted high-risk communities where most offenders reside (Byrne, 2009).

Similarly, the recent proposals by Sherman (2011), Weisburd (2011), and Nagin (2013) to increase the proportion of all criminal justice spending designated for policing need to be informed by research on the impact of this spending shift, not only on community crime rates, but also on community residents’ perceptions of the police. In this regard, Michael

TABLE 4.

Summary of studies 1996-2004 estimating the impact of other criminal justice and non-criminal justice/social factors on crime rates (Source: Stemen, 2007)

<i>Police per capita</i>		
Study	Data	Estimated percentage change in crime rates due to a 10% increase in indicator
Marvell and Moody (1996)	56 U.S. cities, 1971-1992	-3 (index offenses)
Marvell and Moody (1996)	49 states, 1971-1992	not significant (index offenses)
Levitt (1997)	59 U.S. cities, 1970-1992	-11 (violent offenses) -3 (property offenses)
Kovandzic and Sloan (2002)	57 Florida counties, 1980-1998	-1.4 (index offenses)
<i>Unemployment rate</i>		
Study	Data	Estimated percentage change in crime rates due to a 10% increase in indicator
Levitt (1996)	50 states and D.C., 1971-1993	not significant (violent offenses) 10 (property offenses)
Levitt (1997)	59 U.S. cities, 1970-1992	not significant (violent offenses) 10.4 (property offenses)
Raphael and Winter-Ebmer (2001)	50 states, 1971-1997	not significant (violent offenses) 16.3 (property offenses)
Gould et al. (2002)	705 counties, 1979-1997	not significant (violent offenses) 16.6 (property offenses)
<i>Real wages</i>		
Study	Data	Estimated percentage change in crime rates due to a 10% increase in indicator
Gould et al. (2002)	705 counties, 1979-1997	-25.3 (violent offenses) -12.6 (property offenses) -13.5 (index offenses)
Raphael and Winter-Ebmer (2001)	50 states, 1971-1997	-1.6 (violent offenses) not significant (property offenses)
Grogger (1998)	Individual survey data (1980)	-10 (index offenses)
<i>Unemployment rate</i>		
Study	Data	Estimated percentage change in crime rates due to a 10% increase in indicator
Lochner and Moretti (2004)	50 states, 1960, 1970, 1980	-9.4 (index offenses)

Tonry (2010, p. 321) has offered the following cautionary assessment: "It is not entirely obvious to me, however, that increased expenditures to enable more intensive policing would be a good thing. Zero tolerance, public order, and misdemeanor policing have notoriously increased the extent of racial profiling and compromised traditional civil liberties restraints on police interactions with citizens." Elliot Currie (2011) has also weighed in on this issue, suggesting that the question of criminal justice resource allocation/reallocation is too narrowly framed: "A good crime policy... cannot simply weigh how much to put into prisons versus police, but also must consider how much of either merits our investment versus, for example, family support programs, job creation, and much more" (Currie, 2011, p. 112).

Examination of the research on the impact of various non-criminal justice factors on community crime rates can be divided into two categories: 1) research on the implementation and impact of various community crime prevention strategies (see, e.g., Welsh & Farrington, 2012 for a detailed review); and 2) research on the community context of crime that links changes in various community-level factors (such as education level, poverty level, income inequality, size of immigrant population, racial concentration, housing stock, and health care) to increases and declines in the rate of violent and property crime (Kirk & Laub, 2010; Sampson, 2013; Lounsbury & Western, 2010). However, we need to know much more about the impact of both targeted community crime-prevention strategies and broader, general community-change strategies (gentrification, relocation, economic redevelopment, and community activism) on changes in crime rates (Kreager, Lyons, & Hays, 2011; Leventhal & Brooks-Gunn, 2011; Papachristos, Smith, Scherer, & Fugiero, 2011).

Conclusion

We have conducted a nearly four-decade-long experiment with mass incarceration, and the results from this experiment point to the need to move in a different direction. But we need to do so carefully, based on a full assessment of alternative strategies and an objective review of high-quality research (Weisberg & Petersilia, 2010). It certainly makes sense to weigh the relative impact of both criminal justice-focused and noncriminal justice-focused strategies on public safety in targeted, high-risk communities. Before we move further in the development of justice reinvestment strategies, we need to examine the available

research and develop crime-control policies based on a comparative assessment of a full range of individual- and community-change strategies. We cannot focus narrowly on only those strategies within the criminal justice system (e.g., more police or more treatment). As Elliot Currie has observed:

Once the focus is restricted to variations within different strategies of control and punishment, the discourse has shifted away from the social, familial, economic, and communal sources that drive the crime problem to begin with. It is not accidental that the rise of such narrow approaches to cost-benefit analysis tends to coincide temporally with the relative abandonment of social approaches to reducing crime and their displacement by a growing reliance on the criminal justice system. (2011, p. 112)

It seems likely that the great prison experiment is over in most, if not all, regions of the United States. However, it is too soon to tell whether we have learned anything useful from this experiment that can improve community safety and support long-term desistance from crime in targeted high-risk communities.

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