

# THE TWELVE PEOPLE WHO SAVED REHABILITATION: HOW THE SCIENCE OF CRIMINOLOGY MADE A DIFFERENCE

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*Three decades ago, it was widely believed by criminologists and policymakers that “nothing works” to reform offenders and that “rehabilitation is dead” as a guiding correctional philosophy. By contrast, today there is a vibrant movement to reaffirm rehabilitation and to implement programs based on the principles of effective intervention. How did this happen? I contend that the saving of rehabilitation was a contingent reality that emerged due to the efforts of a small group of loosely coupled research criminologists. These scholars rejected the “nothing works” professional ideology and instead used rigorous science to show that popular punitive interventions were ineffective, that offenders were not beyond redemption, and that treatment programs rooted in criminological knowledge were capable of meaningfully reducing recidivism. Their story is a reminder that, under certain conditions, the science of criminology is capable of making an important difference in the correctional enterprise, if not far beyond.*

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For most of us, there is perhaps the nagging suspicion that what we do as criminologists does not matter. There are, of course, the students we teach and help, the local agencies to whom we give a needed hand, and the intrinsic satisfaction that comes from doing work we care about. But there is a disquieting case to be made, as scholars have done recently, that criminology is largely irrelevant (see, for example, Austin, 2003). One poignant example is that for three decades, criminologists have argued that massive imprisonment is not a prudent response to crime (see, for example, Clear, 1994; Currie, 1985, 1998; Irwin and Austin, 1994). And over that same time period, policymakers have proceeded to increase prison populations seven-fold (Harrison and Karberg, 2004).

I have the burden of being somewhat reflexive—of worrying about whether it makes a difference that what I do for a living is irrelevant. At one moment of much angst, I sought advice from my older and occasionally wiser brother, John, who also is a college professor. When I posed the issue of irrelevance to him—of the possible meaningless of our livelihoods, if not existence—his supportive response was: “well, don’t think about it.”

Despite John’s brotherly wisdom, I am not good at repression and thus have had an uneasy relationship with this issue of disciplinary and personal irrelevance for some time. It is clearly the case that most criminological research, including mine, is ignored. This is due in part to what criminologists say and where we say it, and due in part to having audiences that prefer to consume commonsense and politically palatable messages and to remain deaf to research. There is also the long-known reality that, across virtually all scientific areas of study, most published research simply is little read and infrequently cited (Merton, 1968).

My message today, however, is not one of gloom; it is not an attempt to have my individual angst writ large as a prelude to calling for us to join together in a ceremony of repression. For if much of what you and I write does not matter, there are those times—admittedly more rare than commonplace—when our collective efforts to produce strong science do make a difference in the world, and for the better. The specific junctures at which our ideas and science matter are not generally of our choosing, given that people’s motivation to pay attention to our knowledge is shaped by the sociopolitical context. The ultimate link of knowledge to policy also is typically a loosely coupled, imperfect relationship, which leaves us calling for more research and better implementation. Nonetheless, there are those occasions when the efforts of individual criminologists succeed in redirecting reality and deserve to be celebrated. This address is an effort to relay one such effort: the role that scholars and the research they produced have played in saving correctional rehabilitation.

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This celebration is intertwined with my professional biography. When I first entered college in the late 1960s, the rehabilitative ideal was near-hegemonic in corrections—so much so that in 1968 Karl Menninger could write an acclaimed book entitled *The Crime of Punishment*. When I left graduate school for my first teaching position in 1976, James Q. Wilson's (1975) *Thinking About Crime*, which trumpeted selective incapacitation, was the acclaimed book, and commentators were asking, "is rehabilitation dead?" (Halleck and Witte, 1977; Serrill, 1975; see also, Allen, 1981; Rotman, 1990). When I collaborated with Karen Gilbert (1982) on *Reaffirming Rehabilitation* a short time later, my colleague Larry Travis—who has much in common with my brother John—gleefully encouraged my continuing efforts to defend correctional treatment because, he said, he would enjoy seeing me "piss in the wind."

Now, almost three decades later, the wind has begun to shift—gently in some places but substantially in others. Rehabilitation is making a comeback. It is gratifying to see jurisdictions across the United States begin to realize the limits of punitive-oriented programs and to try to implement interventions based on the "principles of effective treatment" (Latessa, 2004; see also, Palmer, 1992). But it is also scary to realize that this movement to reaffirm rehabilitation was not inevitable but firmly contingent on historical circumstance. Without the efforts of real scholars, the rationale and knowledge underlying this revitalization of offender treatment might never have emerged.

In a special program on the late Stephen Jay Gould, I recall listening to an address he gave in South Africa in which he discussed how human equality was a contingent reality. He noted that had evolutionary events unfolded otherwise, Neanderthals might have persisted and genuine inequality between "collateral cousins" would have existed (Gould, 1989:319). By contrast, this situation did not evolve, and instead we emerged with true equality among homo sapiens—despite efforts by some to socially construct races as unequal.

What I learned from Professor Gould was that the realities that we take for granted today are not foreordained but contingent on a host of events that, with small changes, might have led to a different future (Gould, 1989). Much like the NCAA basketball tournament, where some last-second shots win games and others are missed, if the tournament were replayed, the results could change in a meaningful way. Closer to our scholarly home, futures are also uncertain and created: certain realities may or may not emerge depending on whether people decide to devote their careers to the production of knowledge that impinges on a given domain of criminal justice policy.

In this context, as I reflected on the fate of rehabilitation, it struck me that if only a few scholars had turned their attention to other matters,

offender treatment might never have recovered from its tarnished, “nothing works” reputation. Thus, what might have happened if Ted Palmer had decided he was too busy to reanalyze Martinson’s data? Or if I had remained in urban education—the reason I attended graduate school—rather than wander into criminology? Or if several Canadian scholars had stayed in experimental psychology or had refrained from traveling southward sporting the dubious hope that criminologists in the United States were educable? Or if Mark Lipsey and David Wilson had decided to meta-analyze treatment programs for smokers rather than treatment programs for serious delinquent offenders? Or if Doris MacKenzie and Joan Petersilia had not provided falsifying evaluation results of faddish, punitive-oriented programs that were sweeping the nation? Or if Scott Henggeler had not shared his theory and research on early intervention? Or if my colleagues at the University of Cincinnati, Edward Latessa and Patricia Van Voorhis, had chosen to be office-based criminologists as opposed to criss-crossing the United States conducting evaluations and transferring the technology of effective intervention?

When I completed my ruminations, I was left with the realization that the revival of offender treatment hinged on the life and career decisions of a handful of people. In fact, by my count, *twelve people had saved correctional rehabilitation*. Of course, the number of twelve is somewhat artificial. I like its apostolic quality, but clearly other names could have been added to the roster (for example, James Alexander, Douglas Anglin, Steve Aos, William Davidson, David Dillingham, Barry Glick, Arnold Goldstein, Donald Gordon, Karl Hanson, James Howell, Peter Kinziger, Douglas Lipton, Jerome Miller, Mario Pappozzi, Herbert Quay, Robert Ross, Faye Taxman, Marguerite Warren, and J. Stephen Wormith). Regardless, the general point is clear: a small number of scholars, most often initially working alone or in dyads, had created a loosely coupled network that was responsible for fighting back the ideas that offenders were beyond redemption and that corrections was a uniformly and inherently bankrupt enterprise. It is by no means clear that if they had not existed, some scholarly “replacement effect” would have occurred in which their intellectual shoes would have been ably filled. In all likelihood, the future of rehabilitation would have been different and, I am certain, even more disquieting. As Gould (1989:320–321) reminds us about contingency:

I hope I have convinced you that contingency matters where it counts most. . . . But what would be fundamentally different? Everything, I suggest. The divine tape player holds a million scenarios, each perfectly sensible. Little quirks at the outset, occurring for no particular reason unleash cascades that make a particular future seem inevitable in retrospect. But the slightest

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early nudge contacts a different groove, and history veers into another plausible channel, diverging continually from its original pathway.

My thoughts led to a second conclusion: the chief reason that these scholars—individually and collectively—were able to save rehabilitation was that they were *skilled scientists*, both theoretically and empirically. Using different techniques, they were able to marshal substantial data—rooted in sound criminological understandings of crime—that showed the limits of punishment and the poverty of the view that “nothing works” to change offenders. Today, rehabilitation draws much of its renewed legitimacy from this body of research and from the insights it has produced on “what works” to reduce recidivism. Much like the movie *Jerry Maguire* where the challenge was to “show me the money,” credibility in corrections hinges on the challenge to “show me the data.” The years of science in which these scholars engaged have been instrumental in making this possible.

Below, my intent is to relay a story about how twelve criminologists made a difference in corrections with their commitment to science. This story is necessarily abbreviated by time and space, but I hope to celebrate their special contributions and, more broadly, to illuminate why criminology is an important and, on occasion, an immensely relevant enterprise.

### PALMER: CHALLENGING MARTINSON’S “NOTHING WORKS DOCTRINE”

In the spring of 1974, *The Public Interest* published Robert Martinson’s “What Works? Questions and Answers About Prison Reform.” In this essay, distilled from a 736-page volume that would be published in book form a year later (Lipton, Martinson, and Wilks, 1975), Martinson conveyed the results of a systematic review of 231 correctional evaluation studies undertaken between 1945 and 1967. The findings were not encouraging. “With few and isolated exceptions,” Martinson concluded, “the rehabilitative efforts that have been reported so far have had no appreciable effect on recidivism” (1974a:25).

On the surface, Martinson’s assessment was not unusual. For its time, Martinson and his colleagues’ work was state of the art, offering the most comprehensive and carefully conducted appraisal of the correctional treatment literature. Even so, a host of evaluation reviews had reached the same conclusion (see, for example, Bailey, 1966; Berleman and Steinburn, 1969; Cressey, 1958; Gold, 1974; Kirby, 1954; Robison and Smith, 1971; Wootton, 1959). But something was unusual at this juncture. Whereas these earlier analyses had generated only mild interest among scholars and

other correctional observers, the response to Martinson's article was pervasive and consequential, outstripping anything modern criminology had seen in recent or, for that matter, in distant memory.

Indeed, according to Palmer (1992:28), "rarely if ever did a research article have as powerful and immediate an impact on corrections. . . . Within a year, the view that essentially no approach reduces recidivism was widely accepted." Commenting shortly after the article's publication, Adams (1976:76) similarly noted that this work had "shaken the community of criminal justice to its root," with many now "briskly urging that punishment and incapacitation should be given much higher priority among criminal justice goals." In a more recent appraisal, Blumstein (1997:352) has echoed these themes, observing that Martinson's essay "created a general despair about the potential of significantly affecting recidivism rates of those presented to the criminal justice system" (see also, Petrosino, 2004).

Why did Martinson's essay, which by all rights should have been but an obscure footnote in the history of criminology, have such a profound and enduring effect? One critical factor was *timing*. As has been discussed in much detail elsewhere (Cullen, 2002; Cullen and Gendreau, 1989, 2000; Cullen and Gilbert, 1982), this article appeared in the midst of a broader assault on the legitimacy of criminal justice and corrections. As social and political turmoil spread from the 1960s into the 1970s, leftist commentators criticized the state for the rampart abuse of its powers. In corrections, the rehabilitative ideal came under withering attack because of the unfettered discretionary powers it gave to state officials who were supposed to work paternalistically to ensure an offender's reform. Instead, critics now unmasked how judges were class biased and racist, how institutional settings were not therapeutic communities but bastions of inhumanity, how wardens were less interested in curing offenders and more interested in coercing order, and how parole boards made decisions that were at best formulaic and at worst politicized. In response, those on the left argued for an abandonment of "enforced therapy" and for the embrace of a "struggle for justice" that would endow offenders with an array of legal rights limiting the state's discretionary powers over them (see, for example, American Friends Service Committee Working Committee, 1971; Fogel, 1979; Kittrie, 1971; von Hirsch, 1976).

Conservatives, I might add, were more than pleased to join in this effort to remove discretionary powers from the corrections end of the criminal justice system and to give legislators the prerogative to set punishments through determinate sentencing. They viewed rehabilitation as allowing corrections officials to be overly lenient—as justifying offenders' coddling and early release from prison when stringent penalties were needed. Further, for conservatives, rehabilitation was infected with the worst

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aspects of the social welfare state: the willingness to give human services to a population that was undeserving and would only learn from this generosity that waywardness is rewarded (more broadly, see Garland, 1990, 2001).

Within criminology and its home discipline of sociology, a corresponding paradigm shift was well underway (Akers, 1968; Cole, 1975; Gouldner, 1970, 1973). Labeling theory and critical criminology were in ascendancy, and scholars' attention was shifting from the behavior of offenders to the behavior of labelers and "state agents of social control." Particularly important, this "new criminology" trumpeted a range of scholarship—some by criminologists, some by those outside the field—that illuminated how the "benevolent" therapeutic ideology of the rehabilitative ideal in reality served class interests and enabled the state to expand its power over the minds and bodies of socially disruptive, surplus, and/or vulnerable populations (see, for example, Foucault, 1977; Ignatieff, 1978; Platt, 1969; Rothman, 1980).

In this context, Martinson's conclusion that rehabilitation programs were ineffective found a receptive audience. His message confirmed what critics "already knew" and gave them a weapon—scientific data—to back up their attack on correctional treatment. But a second factor was also critical to the appeal of Martinson's study: *the broader interpretation he gave to his findings*.

In his essay, Martinson might have stopped with the rather technical conclusion that correctional treatment programs had "no appreciable effect" on recidivism, perhaps as a prelude to calling for more research and for the better implementation of interventions. But he did not. Instead, he raised the possibility that offender treatment programs were inherently flawed. Most noteworthy, he asked the question, "does nothing work?" (1974a:48). His answer was varied, but his discussion led to the observation that there may be "a more radical flaw in our present strategies—that education at its best, or that psychotherapy at its best, cannot overcome, or even appreciably reduce, the powerful tendency for offenders to continue in criminal behavior" (p. 49).

Later in the year, Martinson (1974b:4) was even less reticent. He noted, without any sign of regret, that rehabilitation is merely an "unexamined assumption" and "is about to lose its privileged status as the unthinking axiom of public policy." He also admitted that, although reluctant to call offender treatment a "myth" previously, it was "a conclusion I have come to... based on the evidence made available" (p. 4). Martinson's most public proclamation of the ineffectiveness of rehabilitation, however, came when, in August of 1975, he appeared on *60 Minutes* in a segment entitled, "It Doesn't Work." Mike Wallace, as the interview proceeds, announces that Martinson's "findings are sending shockwaves through the

correctional establishment” (CBS Television Network, 1975:3). When probed by Wallace about his research, Martinson offers that various treatment approaches have “no fundamental effect on recidivism” (p. 3). He depicts parole as “almost a Machiavellian attempt” by offenders “to get out” (p. 5). Psychological counseling may be a “good way to pass the time,” he admits, but otherwise it has “no effect” (p. 4). In the end, Wallace is left to ask, “Is it conceivable that nothing works?” (p. 7). The answer is obvious.

In this context, Martinson’s research was soon reified—with no dispute on his part—into the “Nothing Works Doctrine” (Adams, 1976:75; see also, Palmer, 1978). For criminologists, being against rehabilitation—rejecting it as a case of good intentions corrupted for sinister purposes—became part of the discipline’s professional ideology, an established, unassailable truth that required no further verification (Cullen and Gendreau, 2001). Scholars spent little time studying how to make interventions more effective and, in fact, were cheered for showing that treatment programs did not work or, even better, “widened the net of state social control” (Binder and Geis, 1984). The efforts to discredit rehabilitation were so extreme—yet unrecognized as so by criminologists—that Michael Gottfredson (1979) could not resist authoring his humorous and revealing account of the “treatment destruction techniques” used by scholars to dispute any sign of program effectiveness.<sup>1</sup>

What would have happened if this “nothing works doctrine” had been fully unchallenged? Or if the task of disputing this now-hegemonic doctrine had been left to those who had little credibility in the research and correctional communities? Silence or ineffective rebuttal might have allowed anti-treatment rhetoric to become so entrenched that the empirical poverty of rehabilitation would have remained beyond discussion. After all, if a correctional strategy has been “proven” not to work, is it not prudent to move onto more pressing matters?

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1. Within five years of his essay’s publication, Martinson (1979) had publicly rejected the “nothing works doctrine” based on data from a follow-up study that, due to his death, was never published. He noted that “any conclusion in scientific inquiry is held provisionally, subject to further evidence” and that “new evidence from our current study leads me to reject my original conclusion and suggest an alternative more adequate to the facts at hand (1979:252). In a balanced appraisal, he asserted that whereas some programs can be harmful, “contrary to my previous position, some treatment programs *do* have an appreciative effect on recidivism” (p. 244, emphasis in the original). It is noteworthy that few scholars paid much attention to Martinson’s cautionary statements and instead continued to reject rehabilitation (Cullen and Gilbert, 1982). The professional ideology of criminologists had become so anti-treatment that research in favor of rehabilitation was either ignored or simply dismissed (Binder and Geis, 1984; Gottfredson, 1979).



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Fortunately, however, a researcher of much credibility—Ted Palmer (1975, see also, 1978)—had the courage not to remain silent but to develop a systematic rebuttal to Martinson’s “nothing works” conclusion. It is instructive to pause for a moment to ask why Ted Palmer was the scholar to step forward. Although speculative on my part, I believe that two interrelated considerations were involved. First, Palmer was not a sociologist-criminologist. As a psychologist, he did not share in criminologists’ emergent professional ideology that viewed virtually all criminal justice interventions as ineffective (Cullen and Gendreau, 2001). It is instructive that by my count, eight of the twelve scholars who I have identified as “saving rehabilitation” are psychologists; I am the only sociologist.<sup>2</sup> Second, his personal and professional experiences suggested a “what works” doctrine. As a researcher for the California Youth Authority, he had conducted studies showing that interventions can be effective and, in due course, had seen concrete examples of youths being reformed (see Palmer, 1978, 2002; Palmer and Petrosino, 2003). The reality portrayed by Martinson did not strike him as accurate.

In taking on the “nothing works” idea, Palmer did not offer mere criticism but rather returned to Martinson’s article and reanalyzed the eighty-two studies cited in its pages. In his now-classic 1975 essay, he drew three important conclusions. First, in his most famous finding—at least in terms of how often it has been repeated—Palmer questioned Martinson’s conclusion that there were only “few and isolated” instances of treatment effectiveness. In fact, his count showed that among the studies reported in Martinson’s article, “39 studies—48% of the total” yielded “positive or partly positive results.” As Palmer (1978:xxi) noted not long thereafter, “a cup half empty is also half full. That is, one should not overlook the fact that many programs *have* reduced recidivism and have provided personal assistance to a sizable portion of the offender population” [emphasis in the original].

Second, although not often understood, Palmer also revealed how Martinson could transform this finding of recidivism reductions among nearly half the programs reviewed into the devastating conclusion that “nothing works” (see also, Cullen and Gendreau, 2000:127). As an analytical strategy, Martinson and his collaborators (Lipton et al., 1975:9)

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2. In addition to Palmer, the eight scholars with doctorates in psychology are Andrews, Bonta, Gendreau, Henggeler, Lipsey, MacKenzie, and Wilson. The other fields of study include criminology/criminal justice (Petersilia and Van Voorhis), public administration (Latessa), and sociology of education (Cullen). Joan Petersilia might be “counted” as a sociologist in that she became a nationally recognized scholar as a researcher at the RAND Corporation after receiving a master’s degree in sociology and before earning her doctorate from the University of California at Irvine in Criminology, Law, and Society.

created eleven “treatment methods” into which they grouped studies (for example, probation, skill development, individual psychotherapy, milieu therapy). They discovered that in virtually every category, the results varied; some studies showed reductions in recidivism, some did not. In Martinson’s (1974a:49) view, this failure to find a specific type or modality of treatment that always worked meant that the extant research provided “little reason to hope that we have in fact found a sure way of reducing recidivism through rehabilitation.” In more practical terms, if asked what program might be used to reliably reduce recidivism, Martinson believed that he could provide no clear advice. In this sense, “nothing works”—at least not in a “sure way.”

Third, Palmer (1975) asserted that Martinson’s analytical framework was needlessly constraining; the data could be viewed profitably from another angle. In Palmer’s (1975, 1978, 1992) view, it might be possible to study the research not only by modality but also by variables such as offender characteristics, treatment setting, and worker or service-provider talents. With this insight, it becomes possible to explore not “what works for offenders as a whole” but rather “which methods work best for *which* types of offenders, and under *what* conditions or in what types of setting” (1975:150, emphasis in the original). Subsequent scholars studying the principles of effective intervention would take his advice (Andrews and Bonta, 2003).<sup>3</sup>

Again, it is difficult to overestimate the importance of Ted Palmer’s rebuttal to the “nothing works doctrine”—a challenge he has sustained for three decades (see, for example, Palmer, 1975, 1978, 1983, 1992, 1996, 2002). Amidst all the complexity of the debate, his most salient contribution was the simple, understandable point that 48 percent of the studies in Martinson’s essay showed reductions in recidivism. To be sure, most criminologists ignored Palmer’s work, since their minds were already made up. But this insight created a fissure in the “nothing works doctrine” that could not be fully dismissed and that, under the weight of additional data, would ultimately crack wide open.<sup>4</sup>

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3. Palmer had identified what later scholars would call “responsivity” factors—that is, conditions specifying when treatment interventions are likely to be most effective.
  4. Although Martinson’s essay and subsequent commentaries had a disquieting impact on the legitimacy of correctional treatment, his work may have contained an ironic, silver lining: By *framing* the issue of rehabilitation in terms of *effectiveness*, he created the possibility of a somewhat narrow, ongoing debate over the empirical issue of “what works.” Thus, to the extent that advocates could supply data challenging the “nothing works” view, they could claim that the abandonment of rehabilitation was both based on faulty evidence and a policy choice that was scientifically invalid. To be sure, the empirical data compiled by advocates were typically ignored or undermined by “treatment destruction techniques”

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## CULLEN: REAFFIRMING REHABILITATION

As an advocate of labeling theory (see, for example, Cullen and Cullen, 1978) and as a child of my times, I had come—like other criminologists—to reject rehabilitation and to embrace the justice model. I was relieved that, after three years on the faculty of Western Illinois University, I had finally finished my dissertation. Optimism abounded. The summer of 1979 approached, and I was now on my way to the University of Virginia to spend six weeks in a National Endowment of the Humanities faculty seminar led by Gresham Sykes. My intent on this paid academic holiday was to play a lot of tennis and do as little work as possible. Little did I know that my career soon would change.

I did not realize that Professor Sykes would actually assign papers to write. Faced with the daunting prospect of having to turn in something of value to an eminent criminologist, I tried to imagine what I might say. After a rather prolonged moment of panic, it struck me—why I have no lasting remembrance—that I could fashion a short piece on rehabilitation, but with a different angle. Two underdeveloped thoughts came to mind. First, although attracted to notions of restraining state power, I was troubled that the law and lawyers only had the obligation to provide people with rights and not to provide them with substantive help (de-institutionalized mental patients abandoned to the street came to mind). Second, I wondered what might happen if the well-intentioned models now being proposed as alternatives to rehabilitation would themselves be corrupted by class, organizational, conservative, and state interests. Together, these two thoughts prompted me to author an essay on why abandoning rehabilitation might not be such a great idea. I recall that its brilliance left Professor Sykes dazzled (or was it disoriented?).

I had no intention of writing a corrections book—only of completing a paper that would allow me to escape embarrassment and return to the tennis court. But this exercise of taking the other side—the pro-treatment side—proved troubling. As I gradually escaped the grip of the prevailing professional ideology, I became increasingly worried that the rejection of correctional treatment was a risky, if not horrible, idea. With Karen Gilbert as a collaborator, this line of inquiry eventually produced the volume *Reaffirming Rehabilitation* (1982).

It is immodest to include myself as one of the twelve who saved rehabilitation, but others have defined me as a corrections scholar (Wright

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(Gottfredson, 1979). However, as the evidence in favor of rehabilitation mounted over the years, it became increasingly “irrational” to dismiss offender treatment as a legitimate goal of the correctional enterprise.

and Miller, 1999). In any event, I believe that my writings have made four contributions that I will note here briefly.

First, I suspect that my stroke of genius was in titling my book with Karen “reaffirming rehabilitation.” This simple title conveyed the serious message that reason existed to resist the movement to consign treatment to the criminological dustbin. It was a slogan—a rallying cry—but one that had meaning beyond my book.<sup>5</sup>

Second, I have attempted to show that the attack on rehabilitation was largely a response to the prevailing social context—one in which events combined to create a distrust in state discretionary power. Although this wariness of discretion was warranted, the rejection of rehabilitation was excessive and not fully thought out. Although the critique of corrections and of state power had merit, critics placed unfounded blame on rehabilitation and failed to appreciate the humanizing influence of treatment ideology.

Third, I warned that the alternative to rehabilitation—the embrace of punishment as the goal of corrections—was dangerous. It is ironic that progressives who could criticize the good intentions of previous reformers did not consider that *their good intentions* might also be corrupted; and they were. In particular, the rejection of the rehabilitative ideal led decision-making powers to be unwisely transferred (under determinate sentencing) from corrections officials to politicized, if not downright conservative, legislators and prosecutors. As treatment ideas were discredited and often stripped from the system, there was also no coherent rationale remaining that could combat the inclination to increase the “cost of crime” through longer sentences and more painful prison conditions. I could continue, but I will stop with the observation that the dire predictions Karen Gilbert and I made in 1982 have largely come true.

Fourth, for two decades, I have conducted a series of studies on public opinion about rehabilitation (Cullen, Fisher, and Applegate, 2000). Given the anti-treatment context of the early 1980s, I initially thought that I would discover that citizens had, in fact, abandoned rehabilitation. But, for over two decades, my research—conducted with a number of coauthors—has reached the same conclusion: although the public is punitive and offender treatment has been excoriated repeatedly, Americans still

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5. The choice of titles can be consequential. I recall my mentor, Richard Cloward, telling me how his publisher wished to change the title of his 1960 book with Lloyd Ohlin, *Delinquency and Opportunity*. Their insistence to retain the concept of “opportunity” in the title proved fortunate, given the attention the book received in the equal opportunity movement of the 1960s. In a similar vein, the title of *Reaffirming Rehabilitation* gave my work with Karen Gilbert a clear identity and earned it attention—both from those who favored rehabilitation and from those who did not.

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strongly support the view that efforts should be made to rehabilitate offenders (see Applegate, Cullen, and Fisher, 1997; Cullen, Cullen, and Wozniak, 1988; Cullen, Golden, and Cullen, 1983; Cullen, Pealer, Fisher, Applegate, and Santana, 2002; Cullen, Skovron, Scott, and Burton, 1990; Moon, Sundt, Cullen, and Wright, 2000; Sundt, Cullen, Applegate, and Turner, 1998). This finding is significant for two reasons. First, it suggests that notions of reforming the wayward are deeply ingrained in our culture. Second, it suggests that the concept of rehabilitation is an important resource to be used in efforts to humanize the correctional system (see also, Cressey, 1982).

## GENDREAU: BIBLIOTHERAPY FOR CYNICS

The publication of *Reaffirming Rehabilitation* had another serendipitous outcome: It prompted Paul Gendreau to write to me. Recognizing that we were among the few scholars in North America admitting our continued embrace of correctional treatment—Larry Travis put the number at three<sup>6</sup>—Paul wrote to give me positive reinforcement for my odd conduct (his behaviorist roots showing through). After several exchanges and meetings, we became friends and collaborators.<sup>7</sup> My main service to American criminology was convincing Paul to attend ASC and ACJS meetings, to publish in our journals, and to expand his correctional networks to the United States. A secondary outcome was that Paul and I formed a partnership that resulted in a number of publications that, in one way or another, proposed to “reaffirm rehabilitation” (Andrews et al., 1990; Cullen, Blevins, Trager, and Gendreau, 2004; Cullen and Gendreau, 1989, 2000, 2001; Cullen, Wright, Gendreau, and Andrews, 2003; Gendreau, Cullen, and Bonta, 1994; Gendreau, Goggin, Cullen, and Pappozzi, 2002; Latessa et al., 2002). I will leave it to others to judge the impact of these works, but taken as a whole, they represent one of the few persistent efforts to respond, on a variety of levels, to critics of offender treatment.

Beyond our partnership, Gendreau has had a long-standing collaboration with Don Andrews and James Bonta—an association that I will discuss in the next section. Before doing so, however, I want to

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6. Along with Paul and myself, Larry counted our colleague Pat Van Voorhis. There were, of course, several others. Even so, Larry’s point was well taken: the number of scholars advocating correctional treatment would have comprised a small club.
  7. Paul Gendreau and I also discovered our mutual love of two Boston sports teams: the Bruins and the Red Sox. Baseball fans will understand his capacity for cruelty when I relate that, until recently, he took every opportunity to remind me of “The Curse” by giving me books like *One Pitch Away*—an account of the 1986 World Series made infamous by Bill Buckner’s lack of fielding prowess (Sowell, 1995).

highlight his special contribution in taking from Ted Palmer the mantle of defending rehabilitation *empirically*. Indeed, when Gendreau learned of the “nothing works” conclusion, he was appalled. He had seen and evaluated effective correctional interventions and, more generally, knew of a vast literature on behavioral change that showed the capacity of all varieties of humans—including offenders—to alter their conduct. With Robert Ross (1979, 1987), he thus set out to review the extant literature so as to provide “bibliotherapy for cynics.”

Gendreau and Ross (1979, 1987) undertook two important narrative reviews that sought to cover the treatment literature published after the post-Martinson era (recall that Martinson’s study focused on studies appearing between 1945 and 1967). Much like Palmer, they argued that the research revealing effective treatment programs was extensive. Three points emerge from these reviews.

First, Gendreau and Ross (1979:463) argued that “criminal behavior is learned.” This simple insight had important implications, because, they observed, the “‘nothing works’ belief reduced to its most elementary level suggests that criminal offenders are incapable of re-learning or of acquiring new behaviors. Why, we wonder, should this strange learning block be restricted to this population?” (1979:465–466).<sup>8</sup> Second and relatedly, treatment programs that were ineffective were not inherently flawed, as Martinson had suggested, but failed for good reason—most often because they had no credible theoretical base or were implemented so poorly that they lacked “therapeutic integrity.” Third, in light of the data, they urged criminologists and other opponents of rehabilitation to surrender their cynicism and “nothing works” ideology. Later, Gendreau would broaden this criticism to the “common-sense” thinking that led both to foolish treatment programs (for example, boot camps) and, more disquieting, to the idea that recidivism can be suppressed by “getting tough” with offenders (Gendreau et al., 2002; see also, Cullen et al., 2004). The cost of relying on common sense and of ignoring the evidence, he cautioned, is “correctional quackery”—the practice of exposing offenders to scientifically absurd interventions that leave their criminality untouched and that ultimately endanger public safety (Gendreau, 2000; see also, Cullen and Gendreau, 2000; Latessa et al., 2002).

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8. As subsequent life-course or career-criminal research would reveal, offender behavior is marked not only by continuity but also by change (see, for example, Laub and Sampson, 2003; Piquero, Brame, Mazerolle, and Haapanen, 2003). Rehabilitation is based on the assumption that change not only occurs “naturally” as a consequence of real world events and relationships but also through planned intervention within the correctional system.

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The reviews supplied by Gendreau and Ross were impressive in their breadth and depth. They were important because they showed that Palmer's rebuttal of Martinson was not idiosyncratic and, in turn, helped to keep the empirical debate alive. Still, the "nothing works doctrine" was deeply entrenched and not easily abandoned by cynics. First, as advocates of rehabilitation, Gendreau and Ross were vulnerable to the charge that their narrative reviews were biased—that they were prone to see the treatment glass as half full when it was really half empty; as we will see, meta-analysis did much to address this criticism. Second, although they provided hints in their review essays, Gendreau and Ross did not spell out directly a compelling answer to Martinson's (1974a:49) assessment that there was no "sure way" to rehabilitate offenders. That is, a bunch of positive evaluation findings does not constitute a coherent correctional policy. Andrews and Bonta would take up that challenge.

### ANDREWS AND BONTA: PRINCIPLES OF EFFECTIVE CORRECTIONAL INTERVENTION

In 1947, Dr. Louis P. Gendreau was appointed as Canada's first Deputy Commissioner of Penitentiaries for Psychiatric and Medical Care. With an eminent prison psychiatrist as his father, Paul Gendreau pledged that he would never become involved in correctional work. Alas, in the early 1960s, his resolve waned as he took advantage of his father's influence to secure a summer internship at the Kingston Penitentiary in Ontario. A year later, Don Andrews would take up the same internship. Thereafter, Paul and Don would earn their Ph.D.'s from Queen's University a year apart (1968 and 1969, respectively).

From the inception of their careers, they saw themselves as "scientist practitioners" who wished to involve the university in "social activist roles." For them, this meant establishing a clinical practicum/research unit at a prison in which undergraduates might participate. Colleagues who were experimental psychologists saw their work as inappropriately applied (the word "dirty" was used), whereas a sociologist at Carleton University complained that Don Andrews was "using undergraduates as agents of the state." Over time, Gendreau moved more fully into the Ontario Ministry of Correctional Services, where he implemented and assessed treatment programs. One of his more prudent decisions was hiring James Bonta, who has subsequently spent much of his career as a correctional psychologist and research director. Throughout this time, Don, Paul, and Jim maintained ongoing professional and personal relationships.<sup>9</sup>

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9. This biographical account is based on personal correspondence supplied by Don Andrews (October 10, 2004), James Bonta (October 8, 2004), and Paul Gendreau

Beyond its voyeuristic value, this brief biographical account helps to explain why these three Canadians became leading opponents of the idea that “nothing works” and instead would offer a theory of “what works.” Similar to Ted Palmer, their training in psychology provided an intellectual foundation for a belief in the science of behavioral change. In clinical roles and experimental research, they had seen behavioral change—whether by humans or rats. In correctional settings, they had implemented programs that had proven to be effective. Although politically progressive, they viewed social activism not as waxing endlessly about the evils of capitalism or about a class revolution but as finding ways to help at-risk, troubled offenders lead decent lives.

The special contribution of Don Andrews and James Bonta was developing the correctional treatment theory that came to be labeled the “principles of effective intervention.” With Bonta and various other collaborators, Andrews was the driving force behind the evolution of this treatment theory. I use the word “evolution” purposefully, because these principles developed in bits and pieces over time before they eventually coalesced into a coherent theory of correctional treatment. I will refrain from summarizing the theory, in part because space is limited and in part because this model has been conveyed in full detail on numerous occasions (see, for example, Andrews, 1995; Andrews and Bonta, 2003; Gendreau, 1996). But I will quickly note four key insights at the core of this approach.

First, based on theory and research, Andrews and Bonta begin by specifying the known risk factors for crime and by identifying those that are amenable to change, such as antisocial values (these factors are called “criminogenic needs”). Second, the task is then to find treatment modalities, such as cognitive-behavioral programs, that are responsive to—that is, capable of changing—these risk factors. This is the principle of “general responsivity.” Third, the principle of “specific responsivity” mandates that interventions take into account offender individual differences (for example, IQ, level of anxiety) in how the treatment is delivered. Fourth, interventions should target for change the highest risk offenders; this is the “risk principle.”

Beyond its criminological merits, Andrews and Bonta’s focus on *principles* of effective intervention was of immense *strategic* value.<sup>10</sup> In a

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(October 9, 2004). I also had access to their vitas (as well as to the vitas of the ten other scholars who saved rehabilitation). See also Wormith’s (2004) historical account of Rideau Correctional Centre, a setting in which Andrews, Bonta, and Gendreau conducted much research and that nurtured their thinking about effective correctional intervention.

10. In a similar way, Sutherland’s theory of crime took on importance because he set it forth in propositional form and organized it around the “principle of differential



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way, Martinson had a reasonable challenge when he said, in essence, that the mere fact that some programs reduced recidivism was not the same as telling him what was a “sure way” to change offenders. In essence, Andrews and Bonta took up this challenge when they set forth a clear set of principles that, if followed, were said to have meaningful treatment effects. As a result, they offered a clear blueprint of “what worked” in offender rehabilitation.<sup>11</sup>

The statement of a correctional theory had two corresponding positive consequences. First, by developing principles, Andrews and Bonta succeeded in placing knowledge about treatment effectiveness in a form in which it could be transferred to practitioners. Perhaps not surprisingly, advocates of the “what works” movement were besieged with numerous and continuing requests to address conferences, to give workshops, and to conduct evaluations.<sup>12</sup> Second, the statement of principles made their theory testable and thus of potential scientific value. In this regard, Andrews, Bonta, and their colleagues conducted one of the early meta-analyses. The results showed that programs that conformed to the principles of effective intervention were able to reduce recidivism 30 percent (Andrews et al., 1990; see also, Andrews and Bonta, 2003; Dowden and Andrews, 1999a, 1999b). Of course, when a theory is tested by its advocates, favorable results understandably provoke a measure of skepticism (see Logan and Gaes, 1993; for a rebuttal, see Cullen and Applegate, 1993). Soon, however, this skepticism would be hard to sustain.

#### LIPSEY AND WILSON: THE PERSUASIVENESS OF META-ANALYSIS

In the course of his research in the latter part of the 1970s, Mark Lipsey faced a measure of cognitive dissonance. Due to his interest in applied research, he was—“entirely by happenstance”—invited to evaluate juvenile diversion programs in Los Angeles and Orange Counties. His research found that the intervention “produced a positive delinquency reduction effect” (Lipsey, Cordray, and Berger, 1981:283). As he familiarized himself with the correctional field, however, he encountered

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association.” By contrast, consider how little attention is given to the similar observations of Shaw and McKay on “cultural transmission.”

11. Andrews and Bonta also were instrumental in developing technology that practitioners could use to implement their theoretical ideas. In particular, they authored the Level of Supervision Inventory, a classification instrument based on the principles of effective intervention (Andrews and Bonta, 2003; see also, Bonta, 1996, 2002).
12. We will revisit this issue near the end of the manuscript when we discuss the contributions of Edward Latessa and Patricia Van Voorhis.

the “nothing works doctrine.” As Lipsey recalls, “there I was with evidence that the programs we studied seemed to work, but the apparent consensus in the field was that they shouldn’t have. The question, then, was how to resolve this apparent inconsistency” (personal communication from M. Lipsey, October 8, 2004).

Rather than see his own research as incorrect, Lipsey decided that the “nothing works literature was wrong.” He reasoned that the likely problem was an “over-reliance on statistical significance outcomes from underpowered studies” (personal communication from M. Lipsey, October 8, 2004; see also, Lipsey et al., 1981:304). At this point, meta-analysis was emerging as a new and powerful way to quantitatively synthesize results. For Lipsey:

meta-analysis looked like a better way to summarize the research than the traditional practices used in Lipton, Martinson, and Wilks and the other nothing works literature. In particular, working with effect sizes instead of vote counting statistical significance and aggregating across studies seemed like promising ways to deal with the statistical power issues that I was, by then, convinced were a big part of the explanation for why “nothing works” was wrong (personal communication from M. Lipsey, October 8, 2004; more broadly, see Lipsey and Wilson, 2001).

Lipsey subsequently undertook a systematic review of more than 400 delinquency treatment programs, which he published in 1992. Since that time, he has had an ongoing research agenda assessing treatment effectiveness (see, for example, Lipsey, 1999a, 1999b; Lipsey, Chapman, and Landenberger, 2001), with some of the most influential works published with David Wilson (see, for example, Lipsey and Wilson, 1993, 1998; Wilson and Lipsey, 2001).<sup>13</sup> Wilson has also established a collateral research agenda with his own set of coauthors (see, for example, MacKenzie, Wilson, and Kider, 2001; Wilson, Allen, and MacKenzie, in press; Wilson, Gallagher, and MacKenzie, 2000; Wilson, Gottfredson, and Najaka, 2001; Wilson, Mitchell, and MacKenzie, 2002). This research has reached three important conclusions that have helped to shape the debate on rehabilitation.

First, contrary to the “nothing works doctrine,” the overall effect of treatment programs is positive and is likely to be “large enough to have practical significance” (Lipsey, 1992:98). Second, treatment program effects are marked by “heterogeneity”; some interventions work better

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13. James Derzon and Sandra Wilson also have been prominent coauthors of Mark Lipsey. For a list of treatment-related and meta-analytic publications and reprints, see this website: [www.vanderbilt.edu/cerm](http://www.vanderbilt.edu/cerm).

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than others. Third, treatment programs that are consistent with Andrews and Bonta's (2003) principles of intervention tend to produce larger effect sizes (Lipsey, 1992:159). By contrast, programs that depart from these principles—especially those that are deterrence- or punishment-oriented—are largely ineffective, if not criminogenic (Lipsey and Wilson, 1998).<sup>14</sup>

These studies have had an enormous impact. As Lipsey (1999b:611) observes, “one of the arguments against the rehabilitative perspective is that rehabilitation simply does not work.” However, “it is no exaggeration to say that meta-analysis of research on the effectiveness of rehabilitative programming has reversed the conclusion of the prior generation of reviews on this topic” (p. 614).<sup>15</sup> There are three reasons, I suspect, why Lipsey and his colleagues' work has proven so influential. First, despite his pro-treatment leanings, Lipsey uses measured language and is not identified as a rehabilitation activist (as would be the case with Palmer, the Canadians, and myself). Second, unlike narrative reviews where qualitative assessments of the literature create opportunities for biased interpretations, the quantitative nature of meta-analysis makes the results replicable and hard to criticize. Third, quality science matters: the rigor of Lipsey's work is exemplary, making it difficult to dismiss the findings favorable to treatment.<sup>16</sup>

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14. In general, the meta-analyses showed that the overall effect size was about .10. In practical terms, this meant that if a control group had a 50-percent recidivism rate, the treatment group's recidivism rate would be 45 percent (or 10 percent lower calculated on a 5-point reduction on a 50-percent base recidivism figure). This overall effect size, however, covered all interventions, including punishment-oriented programs. It also likely underestimated the true impact of interventions because effect sizes “are attenuated by the unreliability of the study outcome measures on which they are calculated” (Lipsey, 1992:98). Finally, the effect sizes of “effective” programs have been found to range up to and, at times, beyond .30 (Lipsey, 1992, 1999a; see also, Andrews et al., 1990).
  15. Lipsey's assessment is echoed by Farrington (personal communication, November 2, 2004). As Farrington notes, “changing the method of summarizing the results from vote-counting (that is, counting X significant out of Y) to meta-analysis was crucial in changing views about the effectiveness of interventions.” Interpreting a mix of studies that showed both significant and non-significant findings was difficult. By contrast, “the beauty of meta-analysis is that . . . it produces an average effect size which is practically and statistically significant. So the change from focusing on significant findings to focusing on the average effect size was crucial.”
  16. Lipsey did not conduct the first meta-analysis of treatment studies, but clearly his research agenda has been the most comprehensive and influential. Other meta-analyses have reported similar findings (Cullen and Gendreau, 2000; Petrosino, 2004).

### PETERSILIA AND MACKENZIE: SHOWING WHAT DOES NOT WORK

The idea that “nothing works” created ideological space for an alternative idea: Punishment does work (Wilson, 1975). The downside to “penal harm” (Clear, 1994)—especially when it involves mass incarceration—is that it is pricey. But in the mid-1980s, a proposal emerged on how to punish inexpensively and, supposedly, effectively. These were “intermediate sanctions”—correctional strategies “between prison and probation” (Morris and Tonry, 1990) that sought to reduce recidivism by increasing control over offenders either in the community or during short stays in prison. Two of the more prominent proposals were intensive supervision programs (ISPs) for probationers and parolees and boot camps or “shock incarceration” (Cullen, Wright, and Applegate, 1996).<sup>17</sup>

Today, it is generally understood that these control-oriented interventions are ineffective (Cullen et al., 1996; Cullen, Pratt, Miceli, and Moon, 2002; Gendreau, Goggin, Cullen, and Andrews, 2000; Lipsey and Wilson, 1998; MacKenzie et al., 2001; McGuire, 2002; Petersilia, 1998).<sup>18</sup> But in the excitement of the 1980s, they appealed to the commonsense notions that watching offenders more closely would deter them from law-breaking and that exposing offenders to the discipline of a military-style boot camp would “break them down and build them back up” (Cullen et al., 1996; Cullen et al., 2004). I wonder, therefore, what might have happened to ISPs and boot camps if Joan Petersilia and Doris Layton MacKenzie (and their coauthors) had not stepped forward to evaluate whether these programs did, in fact, reduce recidivism. Might these programs have spread even farther than they did, consuming many more millions of dollars and exposing offenders to needless discomfort? Might the legitimacy accorded the commonsense notion that “punishment

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17. Liberals also supported ISPs because they promised to be an alternative to incarceration. In general, however, I am skeptical of proposals that *both* liberals and conservative support, because their goals are different. It is not clear that in the politicized realm of criminal justice, liberals have the muscle to prevent their goals from being corrupted to serve punitive purposes by their supposed partners.

18. See also the findings of a panel commissioned by the National Institutes of Health to study the causes of youth violence. In a report recently released, the panel concluded that “‘Scare tactics’ don’t work. . . . Programs that seek to prevent violence through fear and tough treatment do not work” (*New York Times*, 2004:25). It should also be noted that starting with Finckenaur’s (1982) early evaluation, studies showing the ineffectiveness of “scared straight” programs also contributed to undermining the legitimacy of deterrence-oriented interventions. For a recent meta-analysis of these programs, see Petrosino, Turpin-Petrosino, and Buehler (2003).

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works” have helped to stifle the nascent recovery that correctional rehabilitation was making? In both cases, however, the power of Petersilia’s and MacKenzie’s negative findings stemmed from the high quality of their experimental research designs. Science mattered.

With Susan Turner, Petersilia deflated the ISP balloon through what must now qualify as a classic criminological study (Petersilia and Turner, 1993a, 1993b; see Lane, in press). The respect due this study comes from its attempt to use a random, experimental design to assess the effects of control-oriented ISPs across *fourteen* sites. The conclusion was stark. “At no site,” reported Petersilia and Turner, “did ISP participants experience arrest less often, have a longer time to failure, or experience arrests for less serious offense” (1993a:310–311). They continued that “this is a strong finding, given the wide range of programs, geographical variation, and clientele represented” (p. 311). There was one glimmer of hope. Although selection effects could not be ruled out, program participants who were also involved in *treatment* programs experienced decreases in recidivism (pp. 313–315; see also, Gendreau et al., 1994).

Doris MacKenzie embarked on a series of quasi-experimental studies that assessed boot camps first in Louisiana and then across multiple sites (in seven other states). Prior to conducting these studies, she was worried about boot camps’ governing philosophy that “we want to break them down and then build them back up.” Further, she reasoned that “from a theoretical point of view, I thought the camps would not be effective if they did not include human service type treatment” (personal communication from D. MacKenzie, October 14, 2004; see also, MacKenzie et al., 2001:139). Her concerns proved prescient. The evaluation studies revealed that the “tough love” of boot camps—the “military-style discipline, physical training, and hard labor”—had “negligible” effects on recidivism (MacKenzie and Souryal, 1996:292; see also, MacKenzie, 1993; MacKenzie, Brame, McDowall, and Souryal, 1995; MacKenzie and Shaw, 1993).

In the end, the research by Petersilia and by MacKenzie combined to show that control, deterrence-style programs were likely theoretically flawed and, in practice, ineffective (Cullen et al., 1996; Cullen, Pratt et al., 2002). At a key historical juncture—at a time when these programs, legitimated by commonsense logic, were spreading rapidly—their research raised doubts and called for caution. In the words of Gould (1981:322), for policymakers and fellow criminologists, this was a case of “learning by debunking.”<sup>19</sup>

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19. These conclusions are not meant to imply that control-oriented programs might not be a conduit through which treatment services might be provided (Gendreau, Cullen, and Bonta., 1994; MacKenzie et al., 2001). However, it is less clear why,

This debunking was possible, I should emphasize, because Petersilia and MacKenzie were each committed to using science to inform practice—an orientation that was not commonplace for criminologists of their academic cohorts. Thus, in a biographical account, Lane (in press:3) records Petersilia as reflecting that “she would be most gratified if she had accomplished a rather simple career goal: having practitioners use empirically grounded findings for their policy and program decisions.” Similarly, MacKenzie (2000:469) notes that “correctional decision makers have made little use of science to try to inform their decisions.” This omission is consequential, for “if we are to advance our knowledge about ways to effectively change delinquents and offenders, it is imperative that we move toward evidence-based corrections” (p. 469).<sup>20</sup>

### HENGGELER: CHILD SAVING REVISITED

Research is clear in showing that at-risk children, especially in inner cities, only infrequently receive intervention for their problems (Stouthamer-Loeber, Loeber, and Thomas, 1992; Stouthamer-Loeber, Loeber, van Kammen, and Zhang, 1995). If this is disquieting news, we can take a measure of solace from the growing recognition that early intervention would be prudent social policy. First, surveys reveal that there is extensive support for early intervention among the public (Cullen, Wright, Brown, Moon, Blankenship, and Applegate, 1998; Moon et al., 2000). Second, the findings from life-course criminology have linked early misconduct to later misconduct, especially for more serious offenders (Piquero, Farrington, and Blumstein, 2003). Criminologically, it “makes sense” to intervene earlier than later (Loeber and Farrington, 2000; Tremblay and Craig, 1995). Third, there is increasing evidence that early intervention programs are effective (Currie, 1998; Farrington, 1994; Farrington and Coid, 2003; Farrington and Welsh, 2003).<sup>21</sup>

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beyond expediency, a control-oriented program would be the preferred setting in which to deliver effective treatment interventions.

20. As Lawrence Sherman informed me (personal communication, November 19, 2004), rehabilitation experienced a discrediting attack and then reaffirmation in Great Britain similar to that which occurred in the United States. More recently, there has been a movement—again similar to that in the United States—to implement correctional programs that are “evidence-based” (McGuire, 2002).
21. Another argument in favor of early intervention programs is that they are “cost effective” because they are less expensive than more punitive sanctions and have a number of benefits in youths’ lives (see, for example, Henggeler, 1998:44). More generally, the case for reaffirming rehabilitation can be buttressed by data showing the cost effectiveness of treatment interventions within correctional settings (see, for example, Aos, Phipps, Barnoski and Lieb, 2001; Cohen, 1998; Welsh, 2004; see also, Fass and Pi, 2002).

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Notably, the conclusion that programs “work” to treat children involved in crime and related problem behaviors runs counter to the once-prevalent skepticism about the benevolence and effectiveness of the “child saving” enterprise (for general and at the time poignant critiques, see Platt, 1969; Rothman, 1980; see also, Cullen et al., 1983). The very existence of an increasing number of successful early intervention strategies undermines the “nothing works doctrine.” Indeed, a range of programs richly deserve praise and emulation.<sup>22</sup>

It is not possible to review all of these initiatives, but I can single out for special attention what likely has been the most influential early intervention program: Scott Henggeler’s *multisystemic therapy*, commonly referred to as “MST.” MST programs are in thirty states (including statewide programs in Connecticut, Ohio, South Carolina, and Hawaii) and in eight countries (including nationwide programs in Norway and Denmark). Remarkably, these programs service 10,000 youths and their families (personal communication from S. Henggeler, October 13, 2004; see also, Sheidow, Henggeler, and Shoenwald, 2003). Again, the credibility of the MST program is a powerful testament to the conclusion that treatment modalities exist that are capable of achieving an appreciable reduction in wayward conduct.

The first intervention effort that used the clinical principles that would evolve into MST was undertaken by Henggeler and psychology students from Memphis State University in 1978 when he developed a juvenile diversion program (Sheidow et al., 2003). When confronted with the idea that “nothing works” for delinquent youths, Henggeler surmised that existing programs were ineffective not because these juveniles were intractably pathological but because the programs in use “didn’t make sense for this population.” Embracing a social ecological or systems theory, he believed that delinquency was “contextually driven.” By contrast, the “prevailing treatment model” was inappropriate because it was based on “individual psychotherapy from one theoretical perspective or another.” He also rejected getting tough on delinquents because “it did not take a rocket scientist to figure out that incarceration wasn’t going to have a positive impact on their behavior when they got out” (personal communication from S. Henggeler, October 13, 2004).

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22. Many of the most effective early intervention programs are described in detail in the series of monographs edited by Delbert S. Elliott that were published under the title of *Blueprints for Violence Prevention*. Further, I use the term “early intervention” as a sensitizing concept that covers programs from the pre-natal period through the juvenile years. These programs typically are given in the community and address multiple problems a child or adolescent may be experiencing.

The components of MST have been described previously in considerable detail (see, for example, Cullen and Gendreau, 2000; Henggeler, 1998, 1999; Sheidow et al., 2003), but let me revisit a theme found elsewhere in this address: the success of MST was largely contingent on Henggeler's firm embrace of science and commitment to be "evidence-based" (Sheidow et al., 2003:305). First, MST targets for change the "empirically established determinants of serious antisocial behaviour" (Henggeler, 1999:3). Second, when intervening with youths, there is a reliance on the "integration of evidence-based techniques" (Sheidow et al., 2003:303). Third, there is a continuing commitment to evaluate MST programs to ensure quality control and to understand the factors that shape its effectiveness across types of problem behaviors and settings. In general, MST has enjoyed firm empirical support (Farrington and Welsh, 2002, 2003; Sheidow et al., 2003; cf. Littell, in press).

### LATESSA AND VAN VOORHIS: SPREADING THE GOOD NEWS

My preference for an apostolic metaphor extends beyond the selection of my roster of twelve "saviors" to how I now describe the special contributions to reaffirming rehabilitation of Edward Latessa and Patricia Van Voorhis, my colleagues at the University of Cincinnati: As evangelizers for treatment, they have traveled widely to spread the "good news" that correctional interventions "work." As Van Voorhis (1987) has wisely cautioned, there is a "high cost to ignoring success."

In more secular language, they have been engaged in the daunting task of "technology transfer"—in this case, the dissemination of the scientific knowledge on the principles of effective intervention and on how to implement these principles in agency settings. Similar to many fellow criminologists, they have conveyed knowledge through their books and research articles (see, for example, Latessa and Allen, 2003; Latessa et al., 2002; Lowenkamp and Latessa, in press; Van Voorhis, 1994; Van Voorhis, Braswell, and Lester, 2004; Van Voorhis, Spruance, Ritchie, Listwan, and Seabrook, 2004). But, unlike many of us who remain "office-based" criminologists, they have devoted a substantial part of their careers to sharing knowledge to practitioners "in the field" through program evaluations, workshops, invited addresses, and training.

For Latessa, his air travels are so frequent that he has "gone platinum" and is just 100,000 miles short of the million mile standard. He not only has evaluated over 100 programs in Ohio but also has conducted several hundred workshops and training experiences in over 40 states (personal communication from E. Latessa, October 15, 2004; see also, Latessa, 2004). Van Voorhis works on a regular basis in twenty-two states—



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including Ohio—as well as with the National Institute of Corrections and two Canadian agencies. Her presentations, which she has now stopped counting, exceed 150 (personal communication from P. Van Voorhis, October 17, 2004). These numbers do not simply attest to the important influence my colleagues have had, but also suggest a collateral point: There is a near-insatiable desire on the part of practitioners and agencies to learn “what works” to reform offenders. There are, of course, many impediments to effective technology transfer. But these stubborn realities should not obscure the fact that there is a large, if not unending, audience prepared to learn the science of correctional rehabilitation that has yet to be served.<sup>23</sup>

Finally, my colleagues’ commitment to advancing and disseminating the science of treatment effectiveness extends to our department at the University of Cincinnati. One goal is to produce scholars versed in the principles of effective intervention who will disseminate this knowledge as professors, researchers, and consultants. Toward this end, our Ph.D. program is organized to provide doctoral students with coursework, experience on projects, and skills in training relevant to correctional rehabilitation. Another goal is to facilitate technology transfer through the University of Cincinnati Corrections Institute, whose mission is to “disseminate best practices for changing offender behavior in communities across the nation.” The Institute is a conduit for “training for effective correctional programming” and for “evidence-based technical assistance.”<sup>24</sup> It is hoped that these institutional structures will not only prove successful but also be replicated in other settings so as to increase our collective capacity to transfer the technology of offender treatment.<sup>25</sup>

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23. Of course, many other advocates of the principles of effective treatment, such as Andrews and Gendreau, have also been extensively involved in technology transfer to practitioners and correctional agencies.

24. Quotes from a brochure advertising the “UCCI—University of Cincinnati Corrections Institute.” Note that Patricia Van Voorhis is the Institute’s Director.

25. In Merton’s (1995:44) terms, the faculty in the Division of Criminal Justice at the University of Cincinnati have created a “social and cognitive micro-environment” that fosters the creation and transmission to its members of knowledge on correctional rehabilitation (for example, all students learn about the “nothing works” debate and the principles of effective intervention). Beyond the local scholarly setting, Merton (1995:44) also illuminates how knowledge is in turn diffused to “associates-at-a-distance” in the “socio-cognitive macro-environment.” Again, part of the mission of scholars at the University of Cincinnati is to engage in this more general diffusion of theory and data about correctional rehabilitation.

## CONCLUSION: FIVE LESSONS FOR THE SCIENCE OF CRIMINOLOGY

As a pre-modernist, hermeneutically challenged, undeconstructed advocate of rationality, I must confess that I would find it objectionable if . . . we did not attempt to manage and improve society. . . . I would argue . . . that any influence of evaluators on the everyday judgment and knowledge of practitioners that makes it more rational or more “scientific” is desirable. . . . [B]elief in astrology, alien abductions, guardian angels, psychic hotlines, past lives, channeling, and the like . . . are widespread among those exhibiting commonsense in our society. . . . The short history of program evaluation provides ample instances of well-intentioned attempts to help those in need through means based on practical wisdom which, when examined by pesky evaluators with their objective and systematic methods, proved to be useless or downright harmful to those they were supposed to help (Lipsey, 2000:221–222).

My intention today was to tell a story about correctional rehabilitation and the people who saved it. I have suggested that the saving of offender treatment was not inevitable but a contingent reality. It depended on real people making real decisions about their careers and about the knowledge they attempted to produce. If some or all of these twelve scholars had proceeded along different life paths, rehabilitation might well have remained a discredited idea. By contrast, in the space of three decades, these scholars have contributed mightily to transforming the discourse on rehabilitation from the “nothing works doctrine” to inquiries about “what works” and “best practices” (Cullen and Gendreau, 2001). Their combined efforts constitute a turning point in the field of criminology and in correctional policy and practice (more generally, see Laub, 2004).<sup>26</sup>

In ending this address, it seems useful to explore the broader implications that might be drawn from this story. I have five lessons to share.

*Lesson #1: We need to develop a theory of “criminological relevance”—that is, a theory of when criminology makes a difference in policy and/or practice. As a first step, this might well involve developing a clearer*

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26. As Petersilia (1991:5–6) notes, scholarship on knowledge utilization suggests that research can influence policy or practice in two ways: an instrumental effect, in which research tells officials what precisely to do; and a conceptual effect, in which research influences how officials think about problems and their solutions. In the case of rehabilitation, the efforts of these twelve researchers—and others like them—have had both instrumental effects (“this is what works”) and conceptual effects (“a rehabilitative approach is preferable to a punitive approach”).

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understanding of why so much of what we do seems to be irrelevant (Austin, 2003). But it might also include examining those instances—such as with correctional rehabilitation—in which the knowledge we have produced has shaped policies and practices (see, for example, Sherman and Cohn, 1989).<sup>27</sup> It is possible that criminology matters only in unique historical circumstances that individual scholars cannot choose. Alternatively, it might be possible to develop systematic strategies that will enable criminology to impact policy and practice in more effective ways. This is certainly a problem that merits our attention.

*Lesson #2: Engaging in rigorous science increases the chances that criminology will make a difference.* Why should anyone listen to criminologists? The direct answer is because as scientists, we have a form of knowledge—scientific knowledge—that has a special legitimacy. As Austin (2003:558) cautions, however, we are faced with the reality that “criminologists have very little good ‘science’ to offer policy makers.” The saving of rehabilitation shows the opposite possibility. Because the science was so strong—experimental studies, massive meta-analyses, theory-informed tests of programs—it became difficult for many corrections officials to continue to embrace failed punishment-oriented interventions and to ignore the mounting evidence that certain treatment modalities achieve meaningful reductions in recidivism.

*Lesson #3: Knowledge construction—“having answers about what works”—will increase the chances that criminology will make a difference.* Criminology traditionally has been a field that has been far better in showing what does not work than in showing what does work (Cullen and Gendreau, 2001). Debunking, or falsification, is an essential function of science and evaluation research. However, unless we engage in knowledge construction—the task of amassing information on what is effective—we will forever be professional naysayers: we will tell officials what not to do, but then will leave them bereft of guidance as to what to do. By contrast, a key factor underlying rehabilitation’s revival was the packaging of the research findings into the principles of effective intervention—a coherent set of prescriptions for practitioners to follow.

*Lesson #4: To make a difference, criminologists will have to disseminate knowledge by engaging in technology transfer.* The renewed standing of rehabilitation in the field of corrections was due not simply to scientific knowledge construction but also to the willingness of scholars to write in forums read by practitioners, to lecture widely, to evaluate and consult, to hold workshops, and to undertake training. On a broader level, however, successful technology transfer will depend on more than the good

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27. See also, Petersilia (1991:5) for a partial list of instances in which research has affected criminal justice policy and/or practice.

intentions of individual scholars to “spread the good news.” Instead, it will be necessary to create institutional structures—perhaps in universities or perhaps sponsored by ASC—that are devoted to the dissemination of research knowledge in an accessible form. Technology transfer might also involve working with practitioner groups to reinforce a professional orientation that is receptive to evidence-based knowledge about best practices (Cullen and Sundt, 2003; Latessa et al., 2002; MacKenzie, 2000; Sherman, 1998).

*Lesson #5: Criminologists can—and should—make a difference.* What we do is important. In the area of corrections, it is painful to see how many lives are wasted and how much public safety is endangered when offenders are subjected to interventions that border on sheer quackery (Latessa et al., 2002). In my academic niche of rehabilitation, I have seen twelve scholars work diligently to combat correctional failure. Their work, as we have seen, has made an immense difference.

I cannot promise that your—or my—individual research efforts will matter in the future. But if the science of criminology is seen as a collective enterprise, I am optimistic that we are not consigned to irrelevance. As today’s story reveals—and as I look upon a sea of fine scholars and people in this audience—I am persuaded that, together, we have both the scientific expertise and kindness of heart to make the world a better place.

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