FROM NOTHING WORKS TO WHAT WORKS: CHANGING PROFESSIONAL IDEOLOGY IN THE 21ST CENTURY

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The authors explore changes over time in criminologists' "professional ideology"—a core set of underlying beliefs that focuses academic thinking along certain lines but not others. Until the late 1960s, criminologists believed that the scientific study of the causes of crime would form the basis of individualized treatments that would reduce offender recidivism. By the mid-1970s, this view had collapsed and had been replaced by a professional ideology emphasizing that "nothing works" in corrections, that the causes of criminality are structural, and that crime can only be reduced through social justice. Although not without its merits, the authors suggest that this professional ideology has had the unfortunate consequence of legitimating "knowledge destruction" (showing what does not work) as the core intellectual project of criminology and thus of undermining efforts at "knowledge construction" (showing what does work). A "what works" movement within corrections, however, is advancing an alternative professional ideology that, once again, endorses the use of science to solve crime-related problems. The authors believe that, if embraced, this vision will improve criminology as a discipline and contribute more than "nothing works" scholarship to the commonweal of both offenders and the public order.

A central premise of the sociology of science is that academic thinking, like that of the "common" person in society, is shaped by the prevailing social context (see, e.g., Gould, 1981; Gouldner, 1970). At times, this insight is taken too far. Thus, misguided attempts are made to reduce academic knowledge to "mere" social constructions of reality in which all claims of truth are seen as equally relevant. Or the premise is advanced too deterministically. Observers forget that scientific paradigms are not fully permeable but create their own internal logic—that is, ideas can coalesce and, in a sense, have a life of their own that influences their development apart from outside forces. Still, in a more modest form, the central premise of the sociology of science is

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surely correct: As a human and thus social undertaking, scientific thinking is not purely a rational exercise in which conclusions are dictated solely by "what the data say."

C. Wright Mills (1942) recognized this fact when he poignantly illuminated the "professional ideology" of the "social pathologists" who studied society's wayward peoples and social problems. A professional ideology is a core set of beliefs—some explicit, some implicit—that focuses academic attention along certain lines of inquiry and away from others. These beliefs may be empirically valid, but they need not be. They may be logically powerful, but this does not mean they are not conditioned by social experiences. In any case, they cause their adherents to resist and reject competing empirical results that are inconsistent with the underlying set of beliefs. For the ideology to be effective, it must be endorsed by enough members within a discipline so that those disputing it will be delegitimated, if not sanctioned. To be *really* effective, the ideology must not be seen as an ideology but as an agreed-on set of beliefs and "background assumptions" that are "obviously true."

In this context, it is our thesis that the academic study of corrections—and the discipline of criminology, more generally—has been dominated by a professional ideology for the past quarter century that is committed to "knowledge destruction" rather than to "knowledge construction." Phrased alternatively, scholars have been "raised" within academic criminology to show what doesn't work as opposed to showing what does work. This professional ideology has been important in the creation of knowledge that reveals the foolishness of numerous correctional interventions. But it has been dysfunctional to the extent that it has inhibited academic criminology's ability to create an evidence-based agenda for how best to correct offenders.

The hegemony of this professional ideology is, fortunately, weakening. Serious efforts are now being initiated to build knowledge on "what works" in corrections (see, e.g., Loeber & Farrington, 1998; MacKenzie, 2000; Pearson & Lipton, 1999; Taxman, 1999). Still, the future of corrections research—and of criminology as a discipline—is hardly settled. It is unclear whether a new professional ideology will emerge that is more diverse in the kinds of research and thinking it will tolerate.

This article is an attempt to call for just such a transformation of what we think our discipline should be about. We start by focusing on the early years of modern American criminology and its views on correctional rehabilitation to show how the prevailing professional ideology was optimistic and open to knowledge construction. We then show how this optimism dissipated into pessimism, with the end result being that criminology became a discipline largely antagonistic to the creation of knowledge that would help the "state"

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control crime. Finally, we call for the creation of a new sense of what our profession should strive to attain—a professional ideology that takes seriously the opportunity, if not the obligation, it has to build scientific knowledge that contributes to the effectiveness of corrections.

USING SCIENCE TO CHANGE THE LAWBREAKER: THE LOGIC OF CORRECTIONAL TREATMENT

The early years of modern American criminology were informed, if not dominated, by the positivist school of criminology. The biological determinism of positivism—at least that found in Lombroso's work—was eventually forfeited, but the general principles of the positivist criminology paradigm were embraced. These paradigmatic principles structured thinking about crime and, in turn, views on how to control crime. Eventually, these principles became near hegemonic and developed into what we have called a "professional ideology." That is, every "good criminologist" seemed to accept them.

This prevailing professional ideology might be seen as comprising five main principles. First and most important, criminologists believed that crime had definite causes that could be unearthed only through systematic scientific study. The emphasis on science is critical because it meant that the entire framework hinged on the assumption that the study of and fight against crime should be *evidenced-based* and thus constitute a *rational* way of formulating practice and policy. Second, the punishment or infliction of pain on offenders was held to be, at best, of limited effectiveness in reforming offenders and, at worst, counterproductive. Third, because crime had definite causes, interventions should be targeted—as in medicine—on changing these causes. Fourth, using the correctional system in this way to change offenders was challenging but ultimately the only rational approach to crime control. Fifth, because the causes of crime differ for each offender, interventions should be individualized. Notably, this way of thinking had emerged with force by the first part of the 1900s and helped to usher in, during the Progressive Era, a wave of reforms that created probation, parole, indeterminate sentencing, and the juvenile court—innovations based largely on the ideal of individualized treatment (Rothman, 1980).

Arthur Mac Donald's (1893) *Criminology* offers an early illustration of a criminologist expressing this "modern" approach to the study of crime. Mac Donald noted that "whatever the remedy" to crime, "the causes must be studied first" (p. 272). "The method of the scientific study of criminals," he observed, necessitates "a thorough investigation of the criminal himself, both

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psychologically and physically, so that the underlying and constant cause of crime can be traced out" (p. 272). Although his book is dedicated to Lombroso, he rejected the idea that most offenders are atavistic reversions with little hope of betterment. Rather, he contended that "the greater part of crime arises out of social conditions, and hence is amenable to reformation, by changing these conditions" (p. 272a). An important part of the answer to crime thus rested in developing reformatories for youth and "reformatory prisons" for adults. But Mac Donald also recognized—to use current language—that crime developed over the life course. Many children were allowed "to live the first years of their life in surroundings that almost predestine to crime" (p. 272b). These children required early intervention through the provision of "moral and social education of a home or home-like institution" (p. 272b). "This," Mac Donald concluded, "is the foundation of all prevention of crime" (p. 272b).

As the modern era of criminology developed, these kinds of ideas were endorsed by most of the giants in the field. An exemplar of the professional ideology of positivism is found in the 1939 edition of Edwin Sutherland's *Principles of Criminology*, whose Preface began with the telling sentence: "A science of criminology is greatly needed both for satisfactory understanding [of crime] and for adequate control" (p. v).

Sutherland (1939) recognized that "a large proportion of the offenders under the care of any agency are recidivists" (p. 585). This "persistence of criminals" was important because "a large proportion of crimes can be attributed to recidivists" (p. 585). These observations led to the question of what interventions might be undertaken to reduce reoffending and its concomitant problems. One common answer based on "strictly hedonistic theory," according to Sutherland, was to punish or inflict "suffering" on the offender (p. 591). This approach, however, had "been discarded by psychologists" (p. 591). Although "pain undoubtedly has some value in the control of behavior," he stated, "the value is more or less completely balanced by the antagonism, isolation, and [criminal] group loyalties it produces" (p. 591). Similarly, "isolation" through imprisonment would have little impact on recidivism (p. 594).

Why were these correctional strategies unable to reduce reoffending? In large part, their ineffectiveness stemmed from their failure to target for change the actual causes of crime and, in particular, of recidivism: differential association. "The essential reason why persons become criminals is that they have been isolated from the culture of the law-abiding group . . . or else have been in contact with a rival criminal culture" (Sutherland, 1939, p. 595). It makes sense, therefore, that "criminality, which is the product of this isolation from culture, will not be overcome by more isolation" (p. 595). Simi-

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larly, making offenders suffer is ineffective because "the infliction of punishment upon the offender does not change the situation which produced the criminality" (p. 591).

Indeed, for correctional interventions to be effective, they needed to prompt the offender to have more contact with and "assimilation" into conventional culture and relationships. Sutherland (1939) thus favored correctional policies that fostered "more and more provision for contact" (p. 595). These included, for example,

> probation, which permits or assists the offender to come into contact with society instead of isolating him behind prison walls; education and self-government in prison, which are attempts to develop social interaction even while offenders are physically isolated; classification of similar offenders within prisons; parole, which acts in the same way as probation, though it begins after a period of imprisonment; and various other efforts to assist the offender after release from prison to gain or regain contacts with normal groups. (p. 595)

Sutherland (1939) also maintained that a key to the effective delivery of correctional interventions was "individualization." Giving all offenders "fixed sentences" was analogous to the time when medicine did not practice individualization of treatment but rather exposed the sick to "blood-letting . . . varying in amount with the seriousness of the ailment" (p. 596). Sutherland thus embraced the practice of the "intensive study of the individual offender for the purpose of learning the specific conditions, circumstances, processes, and mechanisms involved in the criminality" (p. 597). Once this investigation was completed, an intervention could be devised that used this "knowledge regarding the offender, in connection with knowledge previously secured regarding the methods of dealing with such cases" (p. 597). Notably, this kind of individualization did not mean that every offender would have a unique treatment, "any more than scientific treatment of diseases means an entirely different policy for each patient" (p. 597). But would not such individualization cause "criminal justice to be discredited in the eyes of the public because of the inequalities of penalties" (p. 599)? He rejected this reasoning because he was persuaded that "real confidence must be based on a belief in the scientific efficiency of the courts" (p. 599).

Sutherland (1939), we should note, did not argue that interventions should be limited to the correctional system; instead, he favored attacking the sources of crime across multiple contexts. As with treatment, he endorsed "policies . . . for the prevention of crime" that were "closely related to the theories of crime causation" (p. 617). He cautioned that some interventions, such as boys clubs, could be criminogenic by "directly or indirectly

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promoting delinquency, probably through the association of boys who were inclined to delinquency" (p. 625). Still, he advanced a range of possible interventions—from child guidance clinics and "visiting teachers" for at-risk "predelinquents" to attempts to reduce social disorganization through community organization. After all, "prevention is a logical policy to use in dealing with crime. . . . It is futile to take individual after individual out of the situations which produce criminals and permit the situations to remain as they were" (p. 614).

This type of thinking became standard fare in textbooks conveying the accepted wisdom in the field. Take, for example, Donald Taft's (1960) *Criminology* in which he discusses the "apparent principles of effective correctional programs for adults" (p. 727). Taft noted that the best ways to rehabilitate offenders had not been settled and that criminologists continued to differ on whether programs should place "emphasis on dealing with the individual [or] emphasis on working through groups." Regardless, he asked rhetorically, "But can it not be asserted that the neoclassical basis of current correctional practices has been largely discredited?" It is obviously the case that "criminals are not alike. They came to crime by different roads." And punishment is inherently limited, for "it may deter at the moment, but it never socializes and so never protects society in the future" (all quotes from p. 728).

The prevailing professional ideology did not mean that existing correctional and preventative practices were naively assumed to be effective. But if not, their ineffectiveness was defined largely as a problem of science: These practices just were not based on the correct scientific theory of crime. This perspective is seen in the work of another set of criminological giants, Sheldon and Eleanor Glueck. In *Unraveling Juvenile Delinquency*, Glueck and Glueck (1950) presented their empirical analysis of whether a range of factors differentiated one sample of 500 delinquent boys from a second sample of 500 nondelinquent boys. They reported that the roots of maladjustment and criminality often extend to childhood and that across multiple factors, "delinquent boys differ—often markedly so—from non-delinquents" (p. 286). These distinguishing factors are important because they offer "specific targets for clinical and preventative programs." The difficulty, however, is that because "sufficiently exact knowledge of the causes of youth's maladjustment" is not available, correctional interventions—including the juvenile court—"cannot be too successful in curing or even curbing juvenile delinquency" (p. 4). Indeed, they characterized as "naive" the officials dealing with wayward youths:

> How can it reasonably be expected that a brief judicial contact with a delinquent, whether it be entirely sympathetic or accompanied by a threat of punish-

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> ment if the boy does not "turn over a new leaf," will change an emotional and conduct pattern that is often deeply anchored? (pp. 3-4)

They called for continued scientific study of the multiple causes of crime that will help reveal "the numerous factors and forces, which, in their dynamic interplay, make for persistent delinquency" (p. 286). This knowledge will then make it possible to "concentrate psychologic, psychiatric, and sociologic activities in those areas in which action gives the highest promise of good results, instead of galloping in all directions at once" (p. 286). Effective intervention will require "specialized training" and the "scientific experiment with therapeutic models based on sound and early diagnosis" (pp. 288, 289). Importantly, they noted the need to move beyond correctional interventions because they often occur when it is "too late for effective results" (p. 287). "Among the forces that count most in whether or not a boy will be conditioned to antisocial behavior," the Gluecks reminded us, "is the home atmosphere, and especially the intimate emotional relationships of the parent and child and their psychologic deposits in the personality and character of the boy" (p. 287). Therefore, good science compelled the conclusion that

> little progress can be expected in the prevention of delinquency until family life is strengthened by a large-scale, continuous, pervasive program designed to bring to bear all the resources of mental hygiene, social work, education, and religious and ethical instruction upon the central issue. (p. 287; see also Laub & Sampson, 1991, pp. 1428-1429)

We will offer one more illustration of the prevailing professional ideology—an essay authored by Donald Cressey (1958). With his penetrating mind, Cressey was quick to see the problems plaguing correctional rehabilitation. He noted, for example, how virtually any activity in an institution—"whatever is done with prisoners to keep them occupied and/or productive and quiet"—can "be called a correctional measure" (p. 763). He revealed how the main goal of those running intervention programs—although seemingly in favor of evaluation research—was to ensure that "research results can be interpreted as 'conclusive' if they favor continued utilization of the technique and as 'inconclusive' if they do not" (p. 759). In the latter regard, any unfavorable finding from an evaluation could be rendered "inconclusive" by subjecting it to a range of spurious methodological critiques, thus ensuring the continued funding of questionable interventions (pp. 761-763). Furthermore, he ended his essay with "the rather obvious conclusion that most of the 'techniques' used in 'correcting' criminals have not been shown to be either effective or ineffective and only vaguely related to any reputable theory of behavior or of criminality" (p. 770).

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Still, despite this skepticism, Cressey (1958) did not favor abandoning rehabilitation (see also Cressey, 1978, 1982); science still held answers for him. "If correctional work were scientific," he claimed, "each correctional technique would be established on a rational basis" (p. 764). As the paradigm suggests, "we would be reasonably sure that men commit crime in certain describable circumstances and not in others, and then we would set out to modify their crime-producing circumstances." As the caretaker of Sutherland's differential association theory, Cressey proposed that interventions would be more effective if they focused less on personality traits and more on a "group-relations" approach in which the goal is to modify the group or social relations of offenders. He then deduced that many prison programs—such as education, vocational training, and prison labor—will be effective only to the extent that they serve as a conduit through which "the offender's postinstitutional group relationships" are changed (p. 767).

In short, the positivist professional ideology could accommodate a diverse lot—sociologists and psychologists, skeptics and supporters of rehabilitation. Program failures were not a reason to reconsider the whole enterprise but occasions to point out how treatment might be done better—that is, more scientifically. Indeed, the ideological hegemony was sufficiently tight that, as described by Gibbons (1999),

> it seemed to many criminologists that they were about to become "scholar princes" who would lead a social movement away from punitive responses to criminals and delinquents and toward a society in which treatment, rehabilitation, and reintegration of deviants and lawbreakers would be the dominant cultural motif. (p. 272)

In Toby's (1964) words, the authors of criminology textbooks felt comfortable suggesting, implicitly if not explicitly, that "punishment is a vestigial carryover of a barbaric past and will disappear as humanitarianism and rationality spread" (p. 332).

At least on the surface, the "real world" was not inconsistent with this academic vision. Although the rehabilitative idea was never implemented as intended (Rothman, 1980), the language of "corrections" became commonplace. In 1954, for example, the American Prison Association changed its name to the American Correctional Association. Prisons became known as "correctional institutions" (Irwin, 1980; Rotman, 1995). But real programmatic changes also ensued. Prison labor, industrial/vocational training, and basic education—long the staple of prison rehabilitation—were increasingly supplemented with psychological classification systems, therapeutic milieus, token economies, work release and furloughs, and college educa-

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tion. "During the 1960s," observes Keve (1991), "many of the country's correctional systems instituted special treatment programs, in an optimistic hope that the behavioral sciences could diagnose individual criminal cases and devise corrective strategies. It was a time of widespread creative experimenting in rehabilitation" (p. 216). It was also a time when calls were being made to explore community-based treatment programs, spurred on by theories such as differential association that stressed the curative benefits of prosocial reintegration into local neighborhoods.

Reflecting the tenor of the times, the Task Force on Corrections (1967)—part of the President's Commission on Law Enforcement and Administration of Justice—officially sanctioned the professional ideology of criminologists. "Excessively harsh penalties," warned the task force, "may simply backfire by fostering hostility and despair." The challenge is to achieve community safety "by reducing the incidence of crime." And the "rehabilitation of offenders to prevent their return to crime is in general the most promising way to achieve this goal" (all quotes from p. 16).

"NOTHING WORKS" AS A PROFESSIONAL IDEOLOGY

In 1974, Robert Martinson published his celebrated assessment of the effectiveness of correctional treatment, "What Works? Questions and Answers About Prison Reform"—an article that would have both substantive and symbolic significance. Distilled from a larger coauthored report published a year later (see Lipton, Martinson, & Wilks, 1975), Martinson (1974a) presented the results of an analysis of 231 program evaluation studies conducted between 1945 and 1967. "With few and isolated exceptions," he pessimistically concluded, "the rehabilitative efforts that have been reported so far have had no appreciable effect on recidivism" (p. 25). Importantly, Martinson then went on to pose a provocative question. "Do all of these studies," he asked, "lead irrevocably to the conclusion that *nothing works* [italics added], that we haven't the faintest clue about how to rehabilitate offenders and reduce recidivism?" (p. 48). He refrained from answering in the affirmative, but it was clear what he was suggesting. Soon after, it became a criminological wisdom that "nothing works" in the correctional system to change offenders.

Instead of the ready acceptance of the "nothing works doctrine," we might have expected a more detached appraisal of this strongly put finding (for an exception, see Sechrest, White, & Brown, 1979). As Merton (1973) points out, a core norm of the scientific enterprise is "organized skepticism" in

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which the community of scholars subjects claims of truth to withering scrutiny. If criminologists had adhered to this normative prescription, they might have noticed that the Martinson review covered only 138 measures of recidivism—not 231—and that in these studies, fewer than 75 of the evaluated interventions could be called "treatments" (Cullen & Gendreau, 2000, p. 127). They also might not have chosen to ignore Martinson's (1974b, p. 5) admission that his review did not include a category for behaviorally oriented programs—even though readily available literature at this time showed that this treatment modality had achieved promising results with other problem populations and had demonstrated successes in some correctional settings (see Gendreau & Ross, 1979; Milan & McKee, 1974).

If the traditional positivist professional ideology had been reigning undisturbed, Martinson's (1974a) report would have received such scrutiny—such organized skepticism—and warnings would have been forthcoming not to read too much into it. The technical finding of "no appreciable effects" would have been treated as regrettable and then used as evidence that correctional treatment was still based on faulty theories of crime, was using scientifically inappropriate intervention technique, and/or lacked quality in its implementation. A call for more research and experimentation would have been made. The idea that "nothing works" would not have been broached or, if put forward, accepted.

But the traditional paradigm was not left undisturbed. It had been shattered, and thus a very different response to Martinson's (1974a) study was forthcoming. Indeed, a profound transformation in criminologists' professional ideology was well under way when the "nothing works" essay appeared. Between the late 1960s and mid-1970s, the paradigm organizing how criminologists thought had been increasingly abandoned, and "new criminologies" were now flourishing (Cressey, 1978). Michael Gottfredson (1979) captured how this paradigmatic shift changed views on rehabilitation in his amusing but poignant essay, "Treatment Destruction Techniques."

Writing 5 years after Martinson, Gottfredson (1979) introduced his essay with the observation that "the conventional wisdom in criminology is that rehabilitation has been found to be ineffective" (p. 39). In fact, this view was so widespread that "the lack of demonstrated effectiveness is agreed upon by criminologists of nearly every persuasion and theoretical orientation" (p. 39). This consensus, however, rested on shaky empirical ground. As Gottfredson pointed out, about half of the studies with the most rigorous methodological designs in the Martinson report showed *positive* treatment results (see also Palmer, 1975). But criminologists were no longer interested in defending rehabilitation but rather were ideologically predisposed to prove that "nothing works." Accordingly, in the face of promising results, they used "treat-

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ment destruction techniques"—biased methodological critiques and ex post facto arguments—to show why promising evaluation results were, in fact, inconclusive, if not misleading (see also Gendreau, 1996a). He characterized these techniques and their use in defense of the new professional ideology in this way:

> There is a variety of general principles (or, more accurately, pseudoscientific criteria) that may be invoked to fend off any attack on our conventional wisdom regarding the effectiveness of treatment. In fact, there are at least five distinguishable methods that may be used to demonstrate the ineffectiveness of any and all treatment modalities in the criminal justice system. Individually (but, most effectively, in combination), these methods are capable of destroying any positive results that might appear in the literature. Perhaps more important, each can be used to show that continuing research in the area would be a mistake. . . . Because no study or research proposal can withstand an assault from a carefully chosen arsenal of these destruction techniques, their value is obvious. (p. 40)

A number of factors had coalesced in a short period of time to prompt criminologists to relinquish one professional ideology for another. Like others in the United States, criminologists experienced the social and political upheaval of the decade spanning the middle parts of the 1960s and 1970s. Civil rights marches, urban unrest, the Vietnam War, Kent State, Attica, and Watergate—these events and more created a "confidence gap" between the public and the government (Lipset & Schneider, 1987). This rapidly declining trust in the government had direct implications for how scholars came to view the so-called "welfare state," of which corrections and, more generally, the "therapeutic state" were a part (see Kittrie, 1971).

Previously, the expansion of government interventions into the lives of the poor and deviant had been portrayed as benevolent paternalism—as well-intended efforts to address problems in people's lives. But the events of the day fostered the view that the state wielded its power not benevolently but abusively. It was now possible to see welfare programs for mothers and children not as assistance but as an attempt to "regulate the poor" (Piven & Cloward, 1971) or to see "hospitals" for the mentally ill and mentally retarded as a means both to inhumanely incapacitate them and to make them undergo "enforced therapy" (Kittrie, 1971). Vulnerable Americans did not need help from the state but protection from it.

As Rothman (1980) points out, the issue of the discretionary power of the state was integral to the correctional paradigm first initiated in the Progressive Era. Indeterminate sentencing, community supervision, and parole release gave correctional officials unfettered discretion to individualize treat-

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ment. The bargain was that this discretion was to be exercised responsibly and according to scientific criminological knowledge. In the prevailing context of the state waging an unpopular war, shooting down college students and inmates, and plotting illegal political schemes, such a bargain seemed strangely and naively trusting.

Indeed, with minorities crowding the courts and prisons, criminologists now worried about whether the unfettered discretion accorded criminal justice officials was an invitation for race and class discrimination—that is, for "extra-legal" factors to shape decision making. They worried as well that release from prison was not earned through true rehabilitation but linked to effective playacting before the parole board (i.e., to "prove" one was cured) and to coerced compliance to institutional rules while locked up (i.e., unless one obeyed, there would be no release). Academic studies, media exposés, and inmate accounts seemed to show that, in fact, benevolence had been corrupted (Irwin, 1974; Rothman, 1980). For many criminologists, offenders needed legal rights to protect them from a powerful state that had its own, not their, interests in mind. In this context, the idea that "nothing works" in correctional treatment was awfully appealing and not grounds for "organized skepticism" (Cullen & Gilbert, 1982).

Within criminology, exciting intellectual developments—"new criminologies" that challenged old ways of thinking—were reinforcing this rejection of rehabilitation. Starting in the first part of the 1960s, labeling theorists warned that stigmatizing "societal reaction" triggered a "self-fulfilling prophecy" that would trap offenders in criminal roles and amplify their offending (Becker, 1963). The logical extension of this thinking was Schur's (1975) call for "radical nonintervention"—a call to keep juvenile offenders out of the justice system whenever possible.

Critical criminology, with its roots in Marxism, offered an even harsher portrayal of rehabilitation as its adherents unmasked the "good intentions" of correctional "reformers" as efforts to reinforce existing privileges and structural inequalities. For example, the "child savers" who created the juvenile court were now portrayed as embarking on a moral crusade that legitimated their class values and extended control over the lives of poor children (Platt, 1969). From Foucault (1977), we learned that penitentiaries and youth reformatories were not civilized advances over public whippings and hangings but insidious, covert means to discipline the disadvantaged. More broadly, rehabilitation was seen as an effort to adjust offenders to a society that was, at its core, unjust and criminogenic. The solution to crime was not fixing people but fixing social injustices.

These intellectual and social influences combined to create a new set of interrelated principles or shared understandings that were now the lens

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through which criminologists viewed their world and subject matter. The traditional positivist model did not lose all its adherents, but the emergent professional ideology dominated scholarly discourse and publication. Indeed, even today this ideology shapes academic criminology, as succeeding cohorts of criminologists have been socialized, to varying degrees, into it. On the broadest level, the core feature of this ideology is the rejection of science as a means of building correctional knowledge and, instead, the use of science—and, if need be, rhetoric—to show that virtually nothing related to the correctional system reduces crime. More specifically, this professional ideology of "knowledge destruction" rests on five main principles.

First, nothing works to rehabilitate offenders. Martinson's (1974a) study is seen as providing final and definitive proof that correctional rehabilitation is a failure. It is not, of course, necessary to actually read this study—only to cite it and perhaps one or two more (e.g., Whitehead & Lab, 1989). The wealth of contrary evidence is to be systematically ignored, especially the empirical studies of Canadian psychologists (see, e.g., Andrews & Bonta, 1998; see also Gendreau, 1996a). When discussing rehabilitation, it is essential to unmask its hypocrisy by noting that "good intentions have bad results" and that "benevolence is corrupted"; ignore as well the possibility that the policy results are even worse when the intentions are simply nakedly "bad." Whenever possible, refer to probation officers, correctional counselors, and the like as "state agents of social control" and refer to rehabilitation as "state-enforced therapy"; this will ensure that their coerciveness will not be missed (Binder & Geis, 1984). In contrast, when studies report that fortuitous events—such as job stability or marriage—cause persistent offenders to change and desist from crime (Sampson & Laub, 1993), cite this research uncritically and repeatedly. Meanwhile, retain the assumption that *planned* interventions—even well-designed, high-dosage, targeted treatment programs—are incapable of changing offenders.

Second and closely related, nothing that the state does in the criminal justice system will reduce crime. This is the corrections "nothing works" criticism writ large. There is no need to conduct more research to verify this principle empirically, but studies are always welcome that show the ineffectiveness of new "get-tough" laws, diversion programs, drug crackdowns, policing patrol strategies, and so on. There are two exceptions to this generic nothing works principle: White-collar offenders, especially corporate executives, and perpetrators of domestic violence are good candidates for deterrence, apparently because they are exquisitely sensitive to criminal sanctions.

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Third, prisons have virtually no effect on the crime rate and should only be used for serious violent offenders—of which there are few. This principle is important because it is prima facie evidence that the United States overincarcerates its citizens. After all, if prisons have “no effect” and few people inside them are dangerous, why are we locking up so many folks? Relatedly, it is always better to place an offender in the community than behind bars. Even though “nothing works” that we do in the community either, at least this intervention “does less harm” than incarceration. Meanwhile, do not discuss the risks to public safety posed by placing felony offenders into local neighborhoods (see Petersilia, Turner, Kahan, & Peterson, 1985). If pressed on this issue, defend community corrections by saying that it is more “cost-effective.”

Fourth, engaging in “penal science” is a bad thing for criminologists to do. Using criminological knowledge to show how to make control exercised by the criminal justice system more effective is to become a “tool of the state.” Especially if paid by the state to do this work, the criminologist is suspect ethically. The real danger is not so much that penal science will be effective but that it will lend an air of legitimacy or academic respectability to the repressive measures of the state.

Fifth, crime is caused by structural factors, especially forms of social and economic inequality. This principle has several functions. Thus, it explains why rehabilitation and other types of state control are ineffective: They cannot change the structural “root causes” of crime. It also explains why individual theories of crime are wrong and can be safely ignored. In fact, because these astructural theories divert attention to individual pathology, they run the risk of acquitting the social structure of its complicity in crime. As such, individual differences theories are likely to blame the victim and foster repressive crime control policies.

CRIMINOLOGISTS AND THE PUNITIVE STATE

To select a somewhat arbitrary line, the “nothing works” professional ideology was fairly entrenched by 1980. Although this ideology arose largely as a critique of the “therapeutic state,” it was clear by this time that rehabilitation—although still seen as ineffective—was the least of criminologists’ problems. Until the early 1970s, the rate of incarceration in the United States had remained relatively stable for half a century (Blumstein & Cohen, 1973). But by the time the Reagan era arrived, the steep increase in prison

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populations was the major concern of criminologists studying corrections and broader issues of social control. The policies and practices of the 1980s ensured a full transformation from a "therapeutic state" to a "punitive state." Worries about the use of rehabilitation to mask coercion were replaced by worries about policies explicitly formulated to deliver, as Clear (1994) aptly puts it, "penal harm."

Of course, most attention was given to the inordinate numbers of offenders behind bars. Then there was the attempt to transform community "corrections" into community "control," with an emphasis on intensive supervision, drug testing, electronic monitoring, and home confinement (Cullen, Wright, & Applegate, 1996). But equally disquieting was the sheer punitiveness of the rhetoric surrounding crime policy and, at times, the gratuitous attempts to inflict suffering on offenders (e.g., eliminate access to college courses, reimplement chain gangs). Corrections had entered a "mean season," and criminologists could not simply sit by as the human toll mounted. In response, criminologists did what they knew best: They wrote books and articles showing the foolhardiness of the punitive state (see, e.g., Beckett, 1997; Clear, 1994; Currie, 1985, 1998; Irwin & Austin, 1994; Mauer, 1999; Petersilia, 1992; Tonry, 1995; Welch, 1999).

Most of these works carted out a laundry list of familiar criticisms as to why prisons and harsh penalties "don't work." These arguments varied in their scholarly merit—some convincing, some questionable—but they functioned, in essence, as "punishment destruction techniques." It was often argued, for example, that America had engaged in a two- or three-decade "punishment experiment" that had proved to be a failure; that prison populations had to be doubled to achieve even a small percentage reduction in the crime rate; that other nations imprisoned less and had less crime; and that community sanctions were more "cost-effective" than building and operating an endless flow of new institutions (cf. Bennett, DiIulio, & Walters, 1996).

Although a worthy battle to have fought, the downside of the attack on the punitive state was that criminologists continued to spend enormous amounts of time showing what did not work. Knowledge destruction, not construction, remained the dominant theme of these writings. To be sure, exceptions existed (see, e.g., Currie, 1985, 1998), but most works that had offered splendid critiques of punitively oriented correctional policies and practices ended with half-baked or vacuous recommendations for an *alternative way of doing things*. Mostly, there was a call for using prisons less and communities more as places where less harmful punishments might be exacted. Meanwhile, most criminologists declined to build a *science of corrections*, based on criminological knowledge on the causes of criminality, that might show what

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worked to reduce recidivism, improve the lives of offenders, and foster public safety (Gendreau, Goggin, Cullen, & Paparozzi, in press).

THE "WHAT WORKS" MOVEMENT IN CORRECTIONS

Any attempt to characterize the professional ideology of an entire discipline, such as criminology, or a group within a discipline, such as corrections scholars, is open to the charge of ignoring intellectual richness and nuance of opinion. Although admitting that we undoubtedly underplay the diversity of viewpoints among criminologists, the portrayal of the "nothing works" professional ideology captures, we believe, assumptions that are pervasively held and that tend to "load on the same factor." Again, core features of this professional ideology are not without merit or empirical support. Still, belief in this ideology is typically not a rational choice that follows a careful assessment of competing empirical realities. Instead, this way of thinking is socially transmitted in graduate school, in texts, and in journals. At professional meetings, the ideology is reinforced. Here, we learn which criminological statements are disciplinarily "correct" (e.g., "structural reform is needed to reduce crime") and which are "incorrect" (e.g., "prisons reduce crime").

Take, for example, the reception given to the writings of Wilson and Herrnstein's (1985) *Crime and Human Nature* and Gottfredson and Hirschi's (1990) *A General Theory of Crime*. Because these works were provocative in tone, they brought some criticisms onto themselves. Nonetheless, their preference to explain crime by individual differences, not structural circumstances, earned them a disproportionate level of harsh written and unwritten criticism. In contrast, we can point to the warm reception accorded Sampson and Laub's (1993) *Crime in the Making*. Although truly a wonderful book—a contemporary classic—this work is now uncritically cited as definitive proof that persistent individual differences in criminal propensity can be changed when offenders, virtually by chance, acquire quality jobs and marriages. It is accepted that the delivery of "social capital" and "informal social control"—whose conceptual components are broadly defined and measured indirectly— are integral to crime causation. Sampson and Laub's findings certainly are plausible, but we suspect that criminologists' ready embrace of their interpretations is because they are consistent with conventional disciplinary wisdom. Even better, Sampson and Laub's views have the special benefit of rebutting central claims of Gottfredson and Hirschi's "general theory"—a perspective that antagonizes the field's conventional wisdom (see Sampson & Laub, 1995).

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In this context, it is perhaps not surprising that the challenge to Martinson's (1974a) "nothing works" doctrine came disproportionately, if not primarily, from scholars from outside criminology and often from outside the United States—that is, from Canadian psychologists (see, e.g., Andrews, 1995; Andrews & Bonta, 1998; Andrews et al., 1990; Gendreau, 1996b; Gendreau & Ross, 1979, 1987; see also Lipsey, 1992; Lipsey & Wilson, 1998; Palmer, 1975, 1992). Note that the collateral movement to discover what works in early interventions with children and adolescents at risk for crime also is rooted largely outside traditional criminology (see, e.g., Catalano, Arthur, Hawkins, Berglund, & Olson, 1998; Henggeler, 1997; Yoshikawa, 1994; see also Farrington, 1994).

As social scientists embracing the norm of organized skepticism, psychologists engage in a fair amount of knowledge destruction, falsifying commonsense myths and academic nonsense. It is a discipline, however, that also is devoted to constructing knowledge about what works to change people. In part, of course, this is because they have an economic interest in doing so: If no one could be helped through psychological treatment, much legitimacy and many jobs might be threatened. In any case, the discipline of psychology—especially its clinical component—largely believes that behavioral change among troubled individuals is possible and that interventions can bring this about. The data, it appears, are supportive of this assumption (Lipsey & Wilson, 1993).

How psychologists view corrections also differs from how mainstream criminologists, especially those with a sociological orientation, view corrections. Criminologists see prisons as a "society of captives" that is characterized by power relationships, racial conflict, violence and victimization, inmate insurgency, and subcultural formation. The study of corrections thus is the study of social problems writ small; the task is to take the social domain of corrections, especially the prison, and to detail what is wrong with it. In contrast, although not unmindful of these phenomena (Haney & Zimbardo, 1998), clinically oriented psychologists tend to focus on the *individual* within the prison or other correctional settings. An important reality for them is "the program" or "intervention" through which psychological treatment is delivered. It is the quality of this intervention that counteracts the offender's criminality and that, implicitly, outweighs the effects of what else is going on in the correctional setting. Individual change of the offender, not structural change of prison dynamics, is their concern.

Given these considerations, we can perhaps understand why Canadian (and other) psychologists, especially those working in correctional agencies, did not jump on Martinson's (1974a) "nothing works" bandwagon. Coming from a different discipline and society, Martinson's results did not resonate

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with them as being "obviously true" but as "obviously false." And providing treatment to offenders did not strike them as a form of "repression" but as a genuine attempt to help these individuals overcome behavioral patterns damaging to themselves and others.

Indeed, the findings reported by Martinson (1974a) were anomalies within the psychologists' disciplinary paradigm and inconsistent with their professional ideology; they "couldn't be true." In response, they published reviews of the extant empirical literature revealing the effectiveness of treatment, which they then hoped would provide "bibliotherapy for cynics" (Gendreau & Ross, 1979; see also Gendreau & Ross, 1987). They also developed "principles of effective intervention" that, if followed, promised to achieve meaningful reductions in recidivism. For the most part, however, these empirical reviews and theoretical insights changed few minds, again because the message being conveyed was incompatible with the "conventional wisdom" of criminologists' prevailing professional ideology.

In the latter part of the 1990s, however, the "what works" view within corrections seemed to capture more adherents and to be accepted as "possibly true" by criminology in general. Why this is so, we do not really know, but we can speculate about two factors that may have played a role. First, with a Democratic administration in the White House and heading the National Institute of Justice, doing research—especially funded research—aimed at reducing crime became more acceptable. Scholars now might be seen as "chasing money," but at least they were less likely to be stigmatized as "tools of the state."

Second and more consequential, the data from meta-analyses of tens, if not hundreds, of studies confirmed that the Canadians might, in the end, be right. With rather consistent results drawn from meta-analyses conducted in North America and Europe, it appeared that across all types of treatments, rehabilitation programs worked to reduce recidivism. However, the key finding from these analyses was that treatment effects were *heterogeneous*. Some interventions—especially those that were punitively oriented (e.g., boot camps)—did not work, but others—especially those reflecting the principles of effective treatment—did work to cut recidivism meaningfully (Cullen, in press; Cullen & Gendreau, 2000). Scholars might interpret these findings with a different degree of optimism (cf. Cullen & Gendreau, 2000, with Losel, 1999, and Gaes, Flanagan, Motiuk, & Stewart, 1999), but the case against the "nothing works" doctrine has become pretty convincing (see also Lurigio, 2000; MacKenzie, 1997; Pearson & Lipton, 1999; Pearson, Lipton, Cleland, & Yee, 2000; Prendergast, Anglin, & Wellisch, 1995; Redondo, Sanchez-Meca, & Garrido, 1999; Taxman, 1999).

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In many ways, the professional ideology underlying the "what works" approach overlaps with that of the earlier days of modern American criminology. The key theme is that science is a way of knowing the world and changing it for the better. More specifically, the "what works" professional ideology encompasses five principles or core beliefs—ideas that inform, explicitly or implicitly, the scholarship of those now working in this approach (see, e.g., Andrews & Bonta, 1998; Cullen & Gendreau, 2000).

First, scientific criminology is the basis for effective correctional intervention. As early criminologists understood, interventions can only be effective if they target for change the causes or known predictors of recidivism. Understanding what causes persistent criminal involvement, however, requires research into the factors that place offenders at risk of recidivism. Theory plays an important role in this enterprise by directing research into potentially salient risk factors.

Second, scientific criminology should be used to "destroy" knowledge that is not evidence based. Many correctional interventions are based on faulty theories and employ treatment techniques that are inconsistent with decades of social psychological research on behavioral change (e.g., "boot camps," "Scared Straight" programs). Resources should not be allocated to these interventions. Furthermore, if such ill-advised programs are initiated, evaluation research should be undertaken to test the claims being made. In all likelihood, such studies will show that these programs are hopelessly ineffective (see, e.g., Gendreau, Goggin, Cullen, & Andrews, 2000).

Third, scientific criminology should be used to "construct" knowledge about what does work. This is the central premise of the "what works" ideology. There is a belief that offenders are not trapped for life in their criminality but can change. There is a belief that such change is more effectively accomplished by interventions that use treatment modalities (e.g., cognitive-behavioral treatment) that are capable of reducing the known predictors of recidivism. And there is the belief that sustained research and the systematic accumulation of knowledge will improve our ability to discern how best to deliver effective treatments in correctional settings. Indeed, knowledge construction is every bit as important as knowledge destruction. More generally, this approach assumes that scientific criminology is capable of producing knowledge about "what works" in others areas, such as in policing and in situational crime prevention.

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Fourth, although pure research is a noble activity, it is equally legitimate for criminologists to produce knowledge that can reduce crime. The failure to make correctional policies and practices "evidence based" merely ensures that ineffective interventions will abound, as they now do. Unless criminologists can provide a persuasive blueprint for what should be done in corrections, we will be saddled with programs—such as boot camps—that potentially damage offenders and harm public safety. Furthermore, criminologists who help to develop effective interventions are not "tools of the state" but servers of the commonweal. The exercise of state control is not an inherent evil but a resource that should be used to foster effective and humane interventions. This task is daunting, but it is precisely why criminologists should not leave it exclusively to other people.

Fifth, "what works" scientific criminology is not limited, a priori, to any one level of analysis or theoretical perspective. The project is to use science to build criminological knowledge that is the basis of more effective interventions. The "nothing works" professional ideology assumed a priori that theories focusing on offenders' individual differences were incorrect and thus incapable of forming the basis for effective interventions. In contrast, the "what works" paradigm believes that there is heterogeneity in individual propensities to engage in crime—propensities that reflect both traits and differential exposure to social circumstances. At present, the research is strongest in showing that proximate causes of crime—such as antisocial attitudes and relations, faulty cognitions, low self-control, and dysfunctional family relations—are more amenable to intervention than are causes that are more distal (e.g., concentrated disadvantage in neighborhoods, society inequality) (Andrews & Bonta, 1998). This state of affairs, however, may reflect the failure of criminologists to investigate systematically and empirically how interventions targeted at social arrangements beyond the level of the individual offender might affect criminal involvement (e.g., specific housing, welfare, or employment policies) (but see Currie, 1998). There is no inherent reason, though, why a "what works" approach could not operate at, or across, different levels of analysis.

Sixth, scientific criminology will result in more "good" in the world than a criminology that ignores what really works. The "nothing works" doctrine is a fiction that is not based on science. The "cost of ignoring success," to use Van Voorhis's (1987) words, is that we are consigned either to inaction—doing nothing—or to supporting faulty actions—doing the wrong thing. It is particularly disquieting that criminologists would prefer to call for more social justice—to "preach to the choir" and receive accolades from their pro-

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fessional colleagues—even when such statements do not materially change the lives of offenders now in the correctional system. In contrast, constructing knowledge about what does work can foster programming that can "do good" in the real world and in real people's lives.

CONCLUSION: CHOOSING THE FUTURE

What set of beliefs—what "professional ideology"—should guide criminology and the study of corrections in the 21st century? We have suggested that in the late 1960s and early 1970s, criminology experienced a paradigm shift in its underlying set of operating assumptions. The professional ideology of positivist criminology, which hoped to use knowledge to improve the correctional system, was replaced by a criminology that sought to show that "nothing works" when "state control" is exercised and to argue that larger social justice is the solution to crime. This paradigm shift was made possible, at least in part, by the inability of the older criminological paradigm to "deliver the goods"—to make matters better. But it also was a product of events outside and inside the discipline that shaped how a new generation—and subsequent generations—of scholars saw their work.

The new professional ideology was attractive. It seemed ennobling because it placed criminologists on the side of the casualties of social injustice and lined us up against the entrenched powers of capitalism. It implored us to become collective deconstructionists devoted to unmasking the hypocrisy of good intentions that justified repression, to showing how inequities flourished under the cover of claims of equal justice, and to deflating the many myths in society that legitimated inane "get-tough" policies. We were in a fight with powerful forces, and knowledge destruction was our weapon.

The limits of this professional ideology, however, are now clear: It was a recipe for never being able to provide a positive agenda for corrections—of never being able to tell anyone of consequence "what really works." We largely became a discipline of naysayers—people who could tell what not to do but not what to do. Indeed, in the absence of knowledge construction—knowledge whose persuasiveness comes from data and not from mere ideology—we have been able to offer only value-laden opinions about the need to "do less harm" to offenders. In short, we have been largely bereft of any scientifically based answers as to how we can reduce crime effectively and humanely.

The professional ideology of the past quarter century is hardly diminished, never mind dead. It continues to inform how nearly all of us approach correctional issues. In part, this is a good thing. Knowledge destruction is

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part of criminologists' role. Even so, we believe that the hegemony of this professional ideology has outlived its utility and has become counterproductive. The challenge that lies ahead is to use science to develop "evidence-based" corrections that not only tells us what not to do but also what to do (Cullen & Gendreau, 2000; MacKenzie, 2000). At its core, this challenge involves using what is unique to our field—our capacity to undertake scientific criminology—to construct knowledge both about the nature of crime and about "what works" to change offenders.

We are not unmindful of the criticisms of "logical positivism" or of the assumption that cumulative scientific knowledge ineluctably produces social progress. Any choice regarding what criminology should be about, however, has its costs and benefits, its dangers and its possibilities. Even so, if we are to experiment with one choice or the other, we will cast our lot with those who believe that criminology is a discipline that should use scientific knowledge to improve our world. Already within corrections, we can see that the efforts of a rather small cadre of researchers have produced a "what works" knowledge base about the "principles of effective treatment" that is shaping how correctional officials in a variety of jurisdictions view and implement interventions. We are convinced that a large-scale "what works" movement would increasingly construct powerful sets of knowledge about controlling crime that would meaningfully influence policy debates and actual practice. Not to make this choice is to consign criminologists to irrelevance and to forfeit crime policy and practice to those whose knowledge—and often whose motives—are suspect.

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