

Qualitative Research, Theory Development, and Evidence-Based Corrections: Can Success Stories Be “Evidence”?

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Evidence-based policy and practice has been described as the new millennium’s “big idea” (Pawson 2006: 1), generating a groundswell of support that has been described as a “social movement” (Dixon-Woods, et al. 2006). The radical vision of this movement is that public policy should be “based on” (or, more accurately, “informed by”) evidence of “what works” rather than being dictated by politics, ideology, or intuition, as it has for hundreds of years. In his presidential address to the American Psychological Association in 1969, Donald T. Campbell (1969: 409) argued for the creation of an “experimenting society”:

The United States and other modern nations should be ready for an experimental approach to social reform, an approach in which we try out new programs designed to cure specific social problems, in which we learn whether or not these programs are effective, and in which we retain, imitate, modify, or discard them on the basis of apparent effectiveness on the multiple imperfect criteria available.

This vision has particular urgency for the field of correctional services, which has long been said to be dominated by a form of “quackery” unsupported by science (Latessa, Cullen, and Gendreau 2002). Research suggests that many popular approaches to reforming wrongdoers—from “scared straight” type deterrence measures to military-style “boot camp” prisons—can actually trigger more, not less, recidivist behavior (MacKenzie 2012). The remarkable achievement of the evidence-based movement has been to seek to hold all correctional interventions responsible for these outcomes, challenging even the most popular programs to substantiate their claims to effectiveness empirically.

As the logic of this sort of accountability and transparency in justice work is highly persuasive, the movement toward evidence-based policy is increasingly receiving support across the political spectrum in the United States and other jurisdictions. Who, after all, could publicly oppose doing “what works”? The only problem has been defining what counts as being “evidence-based” in actual practice. The label is utilized in countless different ways by different parties, often with clear political motivation. Donaldson (2009: 5) describes the basic “formula” thusly:

Mom + The Flag + Warm Apple Pie = Evidence-Based Practice

Indeed, precisely because evidence-based justice sounds so indisputably desirable, intervention designers and policy makers have been scrambling to claim that their particular program (and ideally none of their competition’s) is “evidence-based”—in ways that appear suspiciously similar to the non-evidence-based era of correctional “quackery.” Dodge and Mandel (2012: 526) write,

The intervention community must generate consensus and endow the label “evidence based” with reliable and valid meaning. A failure to do so would place politicians in a position similar to consumers when shopping for “natural” foods; they would be forced to study a program’s jargonistic packaging to understand how and to what degree it is “evidence based.”

As real as this risk is, there is an equal threat of defining “evidence” too narrowly, excluding core, traditional forms of knowledge generation, with potentially disastrous impact on skewing social scientific research in the name of commercial interests. That is, if research budgets are limited (and they of course are), it makes sense to concentrate that funding on research that has been decreed “certifiable” in the “evidence-based” sweepstakes. This is reasonable if it means cutting funding for poorly designed, haphazard research studies, but it would be harmful if it resulted in ending research that could lead to core, fundamental discoveries of value in correctional practice. Most obviously, defining “evidence-based” too narrowly to include only evaluation research—or even only experimental evaluations—runs the risk of creating real disincentives for the funding and support of traditional forms of theory development, including qualitative and process-focused studies, thought to be at the heart of the scientific enterprise.

In this essay, I make the case for a broadened definition of this contested and open-ended term “evidence-based” in the field of corrections that would include not just aggregate-based evaluations of interventions but also studies of individual lives over time. Following other critics, I argue that, although crucially important, evaluation research is limited in what it can contribute to understanding a complex process like offender rehabilitation. Most evaluations are explicitly atheoretical, seeking to answer the question of “what works” (or actually whether a particular intervention “worked,” past tense, or not, with a particular cohort). Yet they provide little understanding of how or why a process

works for some individuals and not for others. In order to be truly “evidence-based,” correctional practice also needs a rich body of theory and research on how and why the rehabilitative process works, a field of study that is sometimes known as “desistance” from crime, and that by necessity involves a multiplicity of methodologies, well beyond evaluation research.

The term “desistance” first appeared in the “criminal career” research of the 1980s (e.g., Blumstein, Farrington, Moitra 1985), although the research has its origins in the work of Glueck and Glueck (1945) and other criminology pioneers (see especially Glaser 1964; Matza 1964). Today, there is a thriving body of research on the topic; indeed, Paternoster and Bushway (2010: 1156) have recently argued, “Theorizing and research about desistance from crime is one of the most exciting, vibrant, and dynamic areas in criminology today.” Moreover, the term “desistance” (if not always the more subtle meanings behind it) has become familiar among policymakers and practitioners. For instance, the US Department of Justice recently developed a \$1.5 million pilot test of “desistance-based practices,” and desistance research also featured strongly in the evidence report of the UK Ministry of Justice’s green paper “Breaking the Cycle,” announcing the original plans for the UK government’s “rehabilitation revolution” (Ministry of Justice 2010).

These two momentums—around “what works” on the one hand and “desistance” on the other (to use shorthand labels)—appear to sit somewhat uncomfortably together, and some have argued that they are contradictory in ways (see, e.g., Farrall 2004). Certainly, the work appears different on the surface. “What works” typically involves program evaluation research privileging randomized controlled trials (RCTs) (see Gendreau, Smith, and French 2006; Latessa et al. 2002; Lipsey and Cullen 2007; MacKenzie 2012; Welsh and Farrington 2007). Desistance research, on the other hand, focuses on individual lives or journeys over time. Particularly prominent among this work is (mostly) qualitative research that focuses on the self-narratives of individuals who have moved away from crime (see, e.g., Fader 2013; Gadd and Farrall 2004; Giordano, Cernkovich, and Rudolph 2002; Halsey 2006; King 2013; Leverentz 2014; Marsh 2011; Maruna 2001; Vaughan 2007; Veysey, Martinez, and Christian 2013).

At the same time, “what works” and desistance perspectives have much in common as well. Both perspectives emerge out of rigorous criminological research. Both utilize the same dependent variable: a reduction in levels of recidivism. Finally, both perspectives share a fundamental belief that people can change, rejecting the pessimistic notion (and potentially self-fulfilling prophecy) that “once a criminal, always a criminal” (Cullen and Gilbert 1982; Maruna and King 2009). As such, for whatever genuine differences they may have (see especially McNeill 2006), both approaches to research are clearly on the “same side” (Cullen 2012).

Indeed, there is emerging evidence that, in the “real world” of rehabilitation work, practitioners and policy makers have had no difficulty drawing from both

perspectives. For example, in a recent review of *Evaluating in Practice*, Nigel Elliott (2012) writes,

[F]ifteen years ago . . . probation was locked in a positivist What Works paradigm, such as is described by Shaw as a linear “dominance of social science and research ‘experts’ over practice ‘beneficiaries’” (p. 20). . . . Today, tentatively, we have a more nuanced situation. . . . [F]or example desistance research [has] restored the concept of narrative to practice that had been so grievously lost. Desisters “need to make sense out of their past lives” and they do this by the stories they tell and reframe (Maruna et al., 2004, pp. 228–229).

In what follows, I argue that (mostly qualitative and theoretical) desistance research and (mostly quantitative and applied) program evaluation research are not just compatible but also strongly complementary. I outline a realist synthesis (Pawson 2006) between these apparently contrasting approaches that could draw on the strengths and contributions of both “what works” evidence and desistance research, under the banner of “evidence-based corrections,” if the term “evidence” is understood more broadly. In particular, desistance research offers the hope for developing much-needed theoretical models for change necessary in developing correctional practice.

Evidence-Based Corrections and Its Discontents

Although its origins are clearly in the field of medical research, the movement for evidence-based policy has particularly close links to the field of criminology (and offender rehabilitation in particular) and owes a particular debt to Robert Martinson’s (1974) article “What Works? Questions and Answers About Prison Reform.” As Pawson (2006: 20) writes, “Evidence-based policy is dominated by one question. Attend a conference, read a textbook, peruse a proposal, buy a tee-shirt on the said topic and somewhere in headline font appears the phrase ‘what works?’” This simple question has had a powerful impact, for better and for worse, on the way that criminology is practiced. In the sections below, I review both the achievements and the criticisms of this approach to crime research.

What Is “What Works?”

A group of researchers known as the Evidence-Based Medicine Working Group, who first met at McMaster University in Ontario, Canada, in 1992, became the inspiration for something known as the Cochrane Collaboration, which “focuses exclusively on questions concerned with effectiveness and almost exclusively on RCT’s as a means of answering the question of whether something ‘works’” (Dixon-Woods et al. 2006: 26). The Cochrane Collaboration became the model for a parallel effort in the social sciences, which took the name the Campbell Collaboration, whose vision, from the start, has been to “do for evidence-based policy what Cochrane has done for evidence-based medicine” (Smith 1996). Indeed, this desire has a long pedigree. One of the founding

fathers of positivist criminology, Enrico Ferri (1908/2004), once wrote, “This is the fundamental conviction at which the positive school arrives: That which has happened in medicine will happen in criminology.”

At the heart of this model is the systematic review or meta-analysis². Pawson (2006: 11) writes, “The systematic review has grabbed the methodological mantle of evidence-based policy.” Systematic reviews, or comprehensive syntheses of existing research on a topic, are said to be the means through which “science takes stock” (Hunt 1997) and “the most reliable evidence on what the science says about a particular question” (Campbell Collaboration website). Unlike traditional literature reviews in the social sciences, systematic reviews utilize transparent strategies for identifying, screening, assessing, and interpreting relevant studies. These systematic reviews rely on a hierarchy of research evidence, such as the Scientific Methods Scale (SMS), to appraise and score the “quality” of research evidence in a review (see, e.g., Harper and Chitty 2005).

These scales almost all elevate randomized controlled trials (RCTs) to the highest ranking (or “gold standard”). Randomized experiments are widely considered the most reliable method for linking causes and effects in science, as RCTs can reduce spurious causality and some of the biases inherent in nonexperimental research, allowing for greater confidence in the outcomes (Weisburd 2010). Qualitative research is, for the most part, screened out of such reviewing processes; hence is not included as “evidence,” because it typically addresses issues of process rather than cause and effect. To achieve the label of “evidence-based” in such a framework, a program needs to have been positively evaluated in research that meets the specific methodological standards specified in the review.

Numerous systematic reviews and meta-analyses have now been completed on various intervention types in correctional services (see, e.g., Smith, Cullen, and Latessa 2009; Smith, Gendreau, and Swartz 2009) contradicting earlier, non-systematic conclusions that “nothing works” to rehabilitate prisoners (Martinson 1974). McGuire (2002) reviewed thirty different meta-analytic reviews (themselves each a statistical review of dozens of studies), published between 1985 and 2001, and concluded that the interventions typically reduced reconviction rates by between 6 to 15 percent, with some programs sometimes achieving reductions up to 20 percent. The strengths and advantages of this methodological approach over traditional literature reviews and other forms of evidence-gathering are many. Too often, evidence is marshaled in a post-hoc and partisan way, whereby conclusions are reached first (based on ideology or intuition), supportive research findings are cherry-picked to defend it, and contradictory evidence is dismissed, undermined, or ignored (see Nickerson 1998). The result is that for nearly any policy, one can identify an expert who says it is effective and another who says it is not, and policy makers and practitioners are left confused (Dodge and Mandel 2012). The great advantage of the Campbell approach to research synthesis is that it provides transparent guidelines for knowledge accumulation

that have the potential to provide clarity to policy makers and practitioners as to effective practice.

Some argue that the high point for evidence-based policy, to date, was the 2003 Education Sciences Reform Act by George W. Bush's Department of Education (Donaldson 2009). In criminology, the most profound achievement of the "what works" movement so far has been the US government's commissioning of the comprehensive and groundbreaking report *Preventing Crime: What Works, What Doesn't, What's Promising* (Sherman 1997), a first-of-its-kind, independent review of effective crime control strategies employing "rigorous and scientifically recognized standards and methodologies." This research, although based almost entirely on US data, has had a pronounced impact on the United Kingdom as well, prompting Tony Blair's new crime adviser, Louise Casey, to remark, "If [someone] says bloody 'evidence-based policy' to me one more time, I'll deck them" (Bowcott 2005). The UK's Home Office commissioned a parallel review of effectiveness research on crime prevention in the UK context (Goldblatt and Lewis 1998), which became the foundation on which the Blair government built its highly ambitious Crime Reduction Program (CRP). The CRP, which ran from 1999 to 2002, assigned an "exceptional degree of importance—initially at least—to evaluation" (Maguire 2004), setting aside at least 10 percent of expenditure for this purpose. In their detailed independent review, Homel and colleagues (2005) describe the CPR as "the most ambitious, best resourced and most comprehensive effort for driving down crime ever attempted in a Western developed country" (p. v)³.

Beyond "What Works"

Although there is widespread agreement (especially among researchers, who admittedly are not unbiased in this regard) that research evidence is the key to better policy making and practice, "there ironically appears to be much less agreement, even heated disagreements, about what counts as evidence" (Donaldson 2009: 5–6). As Marks (2002) argues, the phrase "evidence-based practice" is really "not a synonym for using research in practice," as many practitioners and supporters assume, "but is in fact a set of epistemological assumptions that include evidence hierarchies and systematic reviews." In other words, the primary criticism of the medical model of systematic review involves the assumptions the method makes about research quality and, in particular, the considerable amount of scholarship that is excluded from consideration as "evidence" in favor of methodologies borrowed from the medical sciences. These assumptions regarding the superiority of various forms of research are made explicit both in the use of terms like "gold standard" and in the writings of reviewers (see Sampson 2010). Farrington (2003: 50) has written, "People whose [research] projects are excluded from systematic reviews correctly interpret this as a criticism of the methodological quality of their work."

Critics of the “what works” paradigm argue that this “institutionalized quantitivism” (Booth 2001) is “brutishly destructive of some of the most important aspects of research and scholarship” (MacLure 2005). Holmes and colleagues (2006) argue, “The evidence-based movement . . . is outrageously exclusionary and dangerously normative with regards to scientific knowledge.” Likewise, in criminology, Stenson (2010) argues that “the broad church of criminology risks capture by a narrow sect”:

The advocates of experimental criminology . . . borrowing the legitimacy of medical science . . . favor big datasets, big funding (leaving less available for non-Campbellites), self-referential systematic literature reviews, causal statistical analysis and a methodological hierarchy with random control trials at the apex, as if conscious human conduct is as determinable and predictable as branded prescription drugs in experimental trials. In this vision, data gathering with real people carries low status.

Such broad critiques can be rejected by proponents of “what works” research as part of a wider opposition to positivist research. However, criticisms from leading quantitative methodologists are not as easily dismissed. For instance, in “Gold Standard Myths: Observations on the Experimental Turn in Quantitative Criminology,” Robert Sampson (2010: 490) argues that “criminological randomistas⁴ have overreached in their claims and generated their own folklores.” He notes a variety of unacknowledged limitations to the RCT method and concludes that “[c]riminologists should at the least dispense with the use of the ‘gold standard’ language (even if in quotes!) and get on with the hard business of doing good research” (p. 499; see also Berk 2005; Cartwright 2007).

Likewise, in “Demythologizing Causation and Evidence,” Michael Scriven (2009: 136) argues that “to insist that we use an experimental approach is simply bigotry—not pragmatic, and not logical. In short, it is a dogmatic approach that is an affront to scientific method”:

The current mythology receiving most public attention would have it that scientific claims of causation or good evidence, either optimally or universally, require evidence from randomly controlled experimental trials (RCTs). The truth of the matter—the reality—is very different, as many readers with good general scientific knowledge will realize upon reflection (p. 135).

Both authors argue that even when they can be designed and delivered properly, RCTs are often compelled to sacrifice low external validity (which limits our capacity to make good use of their results) for high internal validity (which allows us to be confident about their results) (see also Hollin 2008). Meta-analyses and systematic reviews of evaluation research, by their nature, ignore the context or setting in which each program takes place by aggregating results across different studies—even though “everyone understands that what works in Dulwich might not go down so well in Darlington, still less in Detroit” (Pawson 2006).

Moreover, even though RCTs are thought to be superior to other methods in terms of internal validity, Sampson (2010) and others point out that they are not immune to threats to internal validity themselves. Experimenter effects/demand characteristics (expectancies in favor of the treatment group leading to differences in how participants are treated) appear to be a particular threat, especially when combined with allegiance effects between the experimenter and the program being evaluated (indeed, in some cases the experimenter and the program designer are the same person). For instance, Luborsky et al. (1999) found a correlation of .85 between investigator allegiances and differential treatment outcomes in a review of twenty-nine studies of therapy types in clinical psychology. Such biases have led the long-standing editor of the highly prestigious *New England Journal of Medicine* to reluctantly resign her post after two decades as editor, concluding, “It is simply no longer possible to believe much of the clinical research that is published, or to rely on the judgment of trusted physicians or authoritative medical guidelines” (Angell 2009: 12).

Finally, critics suggest existing “what works” research can answer only a very limited number of questions leaving considerable gaps in our knowledge base, even on key questions of causality (Clear 2010). In particular, critics have argued that evaluation research of this type is largely atheoretical and hence leaves the cause of those effects (what elements or aspects of the treatment led to the differential effects observed) unclear from RCT results. Although this is not as problematic with pharmaceutical trials or other very compact and self-contained treatments, critics suggest that with interventions as complex and multi-faceted as offender rehabilitation, this is a substantial gap.

In other words, critics have two key problems with the idea of “what works”—the first involves the “what” and the second involves “works.” That is, the “what works” movement has been criticized for both failing to understand and characterize the “what” of rehabilitation—the process involved in this work—and also for its conception of causality explicit in the concept of interventions “working.” So-called “treatments” like restorative justice, humanistic therapy, and engagement with the arts involve complicated, individual experiences unlike most medical treatments.

The framing of tight operational hypotheses in evidence-based medicine is made possible by the fact that medical treatments tend to be highly reproducible. . . . In the case of medications, getting the active chemical ingredients into a capsule in the right proportion and to reliable quality standards is hardly an impediment to the replication of medical trials (Pawson 2006).

Rossi (1987) and others have argued that it is less easy to believe that rehabilitative interventions will be the same each time they are delivered.

Raynor (2003: 339) argues that one of the implications of the “what works” research movement is the belief that “only programs matter”—or what the Chief Probation Inspector of England and Wales described as “program fetishism”

(Morgan 2002; see also Travis 2003). The reason for this is that manual-heavy “programs” following standardized workbooks are the most likely to be consistently implemented (critics would say a “cookie-cutter fashion”), they are the easiest to evaluate and have the best chance of demonstrating clear effectiveness in random control trials and other evaluations. This does not mean, however, that such programs are the most likely to actually reduce crime. Only that other, more organic interactions and interventions are more difficult to evaluate⁵. Likewise, Hope (2005: 277–278) argues that effectiveness research is biased toward institutional programs (e.g., interventions in schools or correctional settings) and against interventions that seek to “change holistically the structure and organization of communities and . . . prevailing institutional arrangements” (see also Clear 2010).

Additionally, at the heart of the medical model research is a key causal assumption that programs “work” by “causing” change. The Office of Justice Programs in the United States “considers programs and practices to be evidence-based when their effectiveness has been demonstrated by causal evidence (generally obtained through one or more outcome evaluations).”⁶ In other words, the model implicitly presumes that the key causal agent is the program and not the individuals who participate in them. In some (but not all) aspects of medicine, such assumptions make perfect sense:

[T]he ointment reduces the rash, the vapor unblocks the airways, the antibiotics act on the micro-organisms, the radiotherapy kills the cancerous cells. Medics tend to describe their deeds using terms like treatments, therapies, remedies, correctives, cures, . . . and so forth. All of these terms indicate that the active agent resides in the intervention (Pawson 2006: 45).

Critics argue that this is not the case for corrections, arguing that greater attention is needed to developing theories of change more aligned to the complexity of the rehabilitation process. Mike Hough (2010: 19, emphasis added), for instance, argues that in the United Kingdom:

There has been over-investment (both financially and intellectually) in a technocratic model of reducing offending that attaches too much importance in accredited programs, and *under-investment in models that see the process of ‘people changing’ as a complex social skill*. The technocratic model seriously underestimates this complexity and its advocates wrongly assume that experimental research can readily identify the causal processes at work in helping people to stop offending.

Likewise, Frank Porporino (2010: 63) writes:

In this exuberant momentum towards a directed and prescriptive *change-the-offender* agenda in corrections . . . we may have blinded ourselves to other ways of approaching the challenge. . . . We may have narrowed in on too few approaches, too prematurely, and with too much uncertainty about the real process of change that offenders move through.

This argument regarding the problems with the “what works” question was eloquently summarized recently by Michael Martinson, the son of the late Robert Martinson whose 1974 article arguably instigated the quest for “what works” in correctional services in the first place. In response to the challenge that “what works?” is a “meaningless” question (Maruna 2012), the younger Martinson (2012) writes:

The word ‘works’ certainly means something, and what that is is not hard to discover. Simply: what will have a reliable result? What government expenditure ‘y’ will produce result ‘z’? ‘What set of inputs will produce a consistent set of outputs?’ Explicating the question this way teases at the larger problem inherent in the research enterprise of ‘what works?’, which is that impersonal forces and persons remain definitionally different, that sociology is not physics – or at least not Newtonian physics, to predict the discrete behaviour of objects under stress – because the human is a subject which only violence can reduce to an object. To [Robert] Martinson’s surprise, his generation decided that violence (mass incarceration) was the only reliable tool to get Newtonian results in a world of stubbornly quantum human individuality. They solved the non-scientific problem of human choice and freedom by refusing individuals both en masse. They answered his question definitively, at least for his generation. But the question was not meaningless, it was loaded.

In summary, although the evidence-based movement in corrections is wise in many ways to draw upon the parallel evidence-based movement in medicine for inspiration and guidance, adopting the research methodologies wholesale from medical sciences to address complex criminological issues may have been a mistake (Sampson 2010). Pawson (2006: 38), for instance, argues that “it would have been better to start from scratch and to produce a strategy for systematic review that befits our subject matter – the labyrinthine, mutating entanglement that is social and public policy.”

The Challenge Of Desistance

Offender rehabilitation research is notorious for its pessimism and the specter of “nothing works” is never far away in the difficult efforts to help repair highly damaged lives. However, there is one piece of positive news emerging from evaluation studies that is not often acknowledged in “what works” studies. As Hans Toch (1997: 97) states, the “most salient finding in therapeutic research is that the control group members tend to improve too.” That is, even in the bleakest of evaluation results (when nothing appears to “work”), the findings do not suggest that no ex-prisoners are able to rehabilitate. Just the opposite, the research finds only that those in the “no treatment” group succeed just as much as those in the “treated” group.

For most individuals known to the criminal justice system, participation in “street crimes” generally begins in early adolescence, peaks rapidly in the late teens or early twenties, and dissipates before the person reaches 30 years of age (see Figure 16.1). Official conviction statistics, like those represented graphically in Figure 16.1, are not easy to interpret and might be skewed by any number

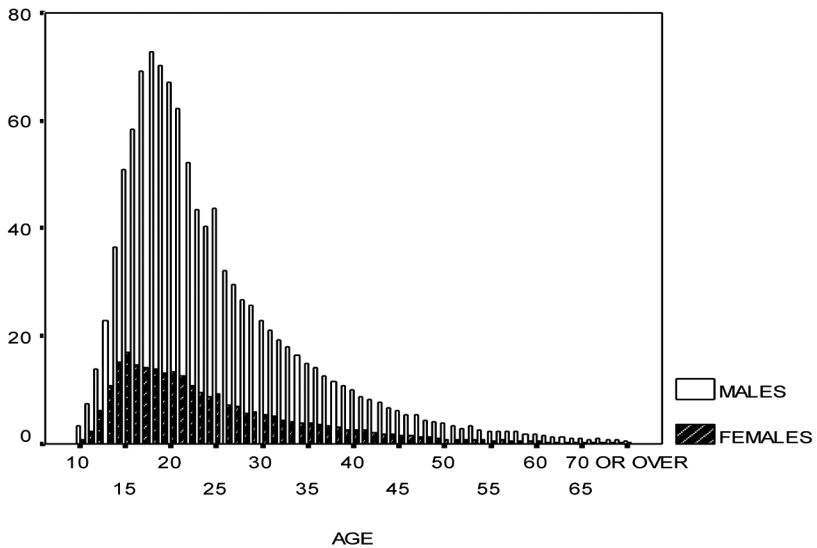


Figure 16.1. Recorded Offender Rates per 1,000 Relevant Population by Age and Sex, England and Wales, 2000 (from Bottoms et al. 2004).

of factors. However, longitudinal cohort studies such as the Cambridge Study in Delinquent Development consistently confirm that the primary reason that relatively few street crimes are committed by older persons is that they have moved away from these behaviors. Farrington (1992), for instance, found that for the Cambridge sample, self-reported criminal behavior peaks at around age seventeen or eighteen and decreases sharply as the young adults progressed through their twenties.


Criminologists label this process “desistance from crime,” understood as the long-term absence of criminal behavior among those who previously had engaged in a pattern of criminality. Desistance research is therefore the effort to understand how and why so many individuals formerly labeled as persistent “offenders” are able to desist from these behaviors. Desistance research is widely varied but tends to involve primarily longitudinal cohort studies where individual lives are tracked over time (e.g., Blokland, Nagin, and Nieuwbeerta 2005; Farrington 1992; Sampson and Laub 1993; Warr 1998) and, increasingly, in-depth qualitative research focusing on the self-narratives and self-understandings of ex-prisoners and former offenders (e.g., Burnett 2004; Gadd and Farrall 2004; Giordano, Schroeder, and Cernkovich 2007; Halsey 2006; Healy and O’Donnell 2008; Hundleby, Gfellner, and Racine 2007; Maruna 2001, 2004; Runggay 2004; Vaughan 2007; Veysey, Christian, and Martinez 2009) or some combination of those two strategies (e.g., Bottoms and Shapland 2011; Sampson and Laub 1993; for reviews, see, e.g., Farrall and Calverley 2006).

Rather than asking what “works,” qualitative and mixed-methods research on desistance typically asks how individuals with criminal backgrounds are able to construct new, prosocial identities for themselves. How does one reconcile a new prosocial identity with a criminal past? What motivates such changes? What processes hinder such efforts to change? Such questions are often best answered using narrative methodology, for obvious reasons (for a review of the contributions of qualitative studies of desistance, see Veysey et al. 2013). Below, I review the core challenge that this self-change research presents for “evidence-based corrections,” drawing on parallels in cognate areas such as recovery from drug addiction and stuttering. Then I present a challenge to the core assumptions of “self-change” or “spontaneous remission” underlying the desistance approach. I argue that the distinction between professional rehabilitation and desistance is largely an illusory one based on the same problematic premises as the “what works” medical model.

The Self-Change Paradigm

How many psychiatrists does it take to change a lightbulb? Only one, but the lightbulb has to really want to change, (Old joke.)

The study of desistance originally emerged out of something of a critique of the professionally driven “medical model” of corrections. To explore desistance (sometimes referred to as “spontaneous desistance” in early writing, drawing on the notion of “spontaneous remission” or “natural recovery” in medicine) was to study those persons who change *without* the assistance of correctional interventions. In this conceptualization, one *either* “desists” on one’s own accord or else one is “rehabilitated” through formal counseling or treatment.

Although this is no longer the way the term “desistance” is utilized (see Maruna, Immarigeon, and LeBel 2004), it remains the case that most desistance research focuses primarily on factors beyond formal criminal justice intervention in seeking to explain the process. For instance, key research findings have included the roles of marriage (Sampson and Laub 1993), employment (Uggen 2000), and peer networks (Warr 1998) in desistance, with relatively little focus on criminal justice actors—a factor ely missed by critics of “medical model” approaches to corrections. Bazemore (1999: 170), for instance, writes, “Johnny was not ‘rehabilitated’ because he . . . gained some new insights through counseling, but because an employer, his wife, his uncle, and other adults eventually provided him with a job, family ties, and a network of support.”

In leading theories of desistance, “personal agency looms large” (Laub and Sampson 2003: 280) and desisters are framed as “active participants in constructing their lives” (281). As one ex-prisoner wrote,

Although I believe people make the biggest difference, they can do little more than help you find the desire to change; they can help you see reasons for changing. However,

the real battle still rests within the self, and the really hard work must be carried on alone (Thornton 1988: 28).

Some observers suggest that this more agentic vision of the change process has “the potential to revolutionize” the way we think about rehabilitation (Shaffer 2007: xii). Adams (1997), for instance, writes,

Thinking of criminal reform as self-initiated socialization highlights a side of the equation often ignored by researchers. Substantial and lasting changes in criminal behaviour rarely come about only as a result of passive experience, and such changes are best conceptualized as the outcome of a process that involves significant participation by the offender, who, in many respects, acts as his or her own change agent (334–5).

Research on self-change has certainly had an important impact on the science of recovery from addiction. For instance, self-change is thought to be the rule, not the exception, among smokers (Floter and Kroger 2007). Although most attempts to quit smoking on one’s own will fail, over three-quarters of ex-smokers eventually achieved this desistance with no formal treatment (Sobell 2007). The possibility of self-change from addictive behaviors had been recognized by the early nineteenth century in the pioneering work on alcoholism by Benjamin Rush and others, yet it only became a formal area of study in the past fifty years with pioneering work by Winick (1962), Drew (1968), Calahan (1970), and Zinberg and Jacobson (1976), among others, followed by second-wave researchers like Biernacki (1986), Waldorf (1983), and Sobell, Ellingstad, and Sobell (2000). Vaillant and Milofsky’s (1995) remarkable fifty-year longitudinal study of the “natural history” of alcoholism and Lee Robins’s (1993) groundbreaking study of heroin addiction among returning Vietnam veterans have had particular impact on the international understanding of the dynamics of addiction. Robins, for instance, found that one in five of all returning Vietnam veterans admitted to having been “addicted” to narcotics (heroin and opium) during the war, yet almost all recovered upon returning to the United States. “Spontaneous remission was the rule rather than exception” (Price, Risk, and Spitznagel 2001). Moreover, Robins (1993: 1051) argued that soldiers’ brief period of addiction followed by a swift recovery “was not out of line with the [general, non-military] American experience, only with American beliefs.”

Yet research in the area has especially blossomed in the past decade, triggered in part by the first International Conference on Natural History of Addictions in March 1999 (Klingemann, Sobell, Barker, Blomqvist, Cloud, Ellingstad, et al. 2001). Sobell et al. (2000) identified thirty-eight studies on self-change from addiction written in the previous four decades. Most of this research involves small samples of self-changers recruited through advertisements in the media and in-depth qualitative analysis of their self-narratives and personal experiences (see Bottorff, Johnson, Irwin, and Ratner 2000; Burman 1997; Hanninen and Koski-Jannes 1999; Sobell, Klingemann, Toneatto, Sobell, Agrawal, and Leo 2001).

Nonetheless, for many treatment professionals and members of the general public, “self-change has been met with disbelief,” as it contradicts our beliefs about the biological nature of addiction as a disease (Sobell 2007). Resistance also comes, of course, from those with vested interest in the leading treatments of the day. For those peddling the latest miracle cure, there is something inherently threatening about the idea that control group members are spontaneously recovering as well (Shaffer 2007). Peele (1990) writes, “One of the best-kept secrets in the addiction field is that people often quit drugs or alcohol without entering treatment or support groups like AA. The treatment industry repeatedly and erroneously claims that no such self-curers exist” (p. 7). Indeed, even though it is known that anywhere from 80 to 90 percent of smokers beat their addiction without formal treatment, research that suggests that alcoholics and other addicts can do the same is routinely dismissed and heavily criticized (see Sobell, Ellingstad, and Sobell, 2000). Chiauzzi and Liljegren (1993) describe natural recovery from addiction as a “taboo topic in addiction treatment” that threatens to diminish the many gains made by the drug treatment movement over the last fifty years.

On the other hand, recovery advocates suggest that self-change research could potentially play a supportive role in efforts to develop better addiction treatment services. Orford and Edwards (1977: 3) write, “The way ahead in alcoholism treatment research should be to embrace more closely the study of ‘natural forces’ that can then be captured and exploited by planned interventions.” Indeed, even research on drug treatment is shifting from a focus on evaluating individual treatment outcomes to a focus on “drug treatment careers,” which understands cycles of treatment in terms of the natural history of the person’s life. Hser and his colleagues (1997) write, “While it is important to determine outcomes for any single intervention, a research approach that evaluates patterns and outcomes of multiple, sequential interventions provides a fuller understanding of the effectiveness of treatment over time.” By modeling recovery as a long-term, cyclical process involving a series of different interventions, this research is able to identify life course “successes” (i.e., people who recover) who might appear to be “failures” after exiting a particular treatment program (Lewis 1991).

Likewise, in an influential essay, William White (2000) argues persuasively that treatment (e.g., professional work with addicts) should be understood as a component of the larger process of recovery:

[P]rofessionally-directed addiction treatment may or may not be a factor in [the recovery process] and, where treatment does play a role, it is an important but quite time-proscribed part of the larger, more complex, and more enduring process of recovery. Treatment was birthed as an adjunct to recovery, but, as treatment grew in size and status, it defined recovery as an adjunct of itself. The original perspective needs to be recaptured. Treatment institutions need to once again become servants of the larger recovery process and the community in which that recovery is nested and sustained. Treatment is best considered, not as the first line of response to addiction,

but as a final safety net to help heal the community's most incapacitated members. The first avenue for problem resolution should be structures that are natural, local, non-hierarchical and non-commercialized.

Another fascinating parallel can be found from outside criminology, but this time in speech pathology rather than addiction studies. One of the most common problems leading families to turn to the help of professional speech pathologists is the phenomenon of stuttering. The onset of stuttering is usually between two and five years of age, and it afflicts around 5 percent of all children at some point (Finn 2007). Somewhere between half and three-quarters of these children will desist from stuttering within a few years of onset without any formal treatment, to the point that they become as fluent as never-stuttering peers (Mansson 2000). Even those who continue stuttering beyond this point also frequently desist in adolescence or young adulthood with no formal treatment (Finn 2007). A sample of 103 adults who did so found that the average age for the onset of desistance was around seventeen years old (Finn and Felsenfeld 2006). Finn (2007: 146) writes, "It is clear that self-managed late recovered speakers are achieving long-term improvements in their stuttering that even clinicians would envy."

Research into stuttering, then, has not focused only on treatment program effectiveness when exploring "what works," as doing so would be to miss the vast majority of recovery experiences. Instead, research has sought to better understand how former stutterers desist from this practice without formal treatment.⁷ It would also miss an obvious opportunity for improving formal treatment interventions themselves. For example, one finding from the self-change research was that parents whose children desisted from stuttering would encourage their children to "slow down" or "stop and try again" whenever they heard stuttering occur (Wingate 1976). What good is such a finding for professional practice? A number of treatment interventions (e.g., Onslow, Costa, and Rue 1990) explicitly drew upon this "natural recovery research as the basis for their treatment approach" (Finn 2007). So, for instance, the Lidcombe program involves a manual for parents with guidance on how to intervene effectively, using just the sorts of techniques that parents used organically in successful instances of desistance from stuttering. This and similar desistance-based interventions have subsequently been positively evaluated using "what works" methodology, including RCTs (Bothe, Davidow, Bramlett, Franic, and Ingham 2006; Finn 2007), in a perfect example of a pluralistic model of evidence-based practice.

A similar movement emerged in criminological dialogues in the past ten years to give the "rehabilitation" process back from the expert to the desister (McNeill 2006). This argument has been variously labeled as a "self-change" or "empowerment" model (Harris 2005), "strengths-based" or "relational" approach (Maruna and LeBel 2003; Raynor and Robinson 2006), or more recently

“desistance-focused practice” (see especially Farrall 2004; Halsey 2006; Maguire and Raynor 2006; Maruna, Immarigeon, and LeBel 2004; McCulloch and McNeill 2008; McNeill 2006; Porporino 2010; Rex 1999; Weaver and McNeill 2010). McNeill (2006: 46) explains this “desistance paradigm” thusly: “Put simply, the implication is that offender management services need to think of themselves less as providers of correctional treatment (that belongs to the expert) and more as supporters of desistance processes (that belong to the desister).” Likewise, Porporino (2010) writes, “The desistance paradigm suggests that we might be better off if we allowed offenders to guide us instead, listened to what they think might best fit their individual struggles out of crime, rather than continue to insist that our solutions are their salvation” (p. 80).

Essentially, the desistance model starts by asking what is empirically known about why some individuals persist in criminal behavior over time and others desist from criminal behavior. Then it seeks to determine how interventions can support or accelerate approximations of these “organically” occurring processes. After all, a recent article on the crime drop in the United States put this argument concisely: “It has been said that the most effective crime-fighting tool is a 30th birthday” (Von Drehle 2010: 24). In other words, maturation, and whatever it entails (socially and psychologically as well as biologically), appears more powerful than any program designed by experts when it comes to reducing crime. Maruna and colleagues (2004: 16) argue, “The lesson of desistance research is that correctional interventions should recognize this ‘natural’ process of reform and *design interventions that can enhance or complement these spontaneous efforts.*” This idea has animated desistance researchers at least since Sheldon and Eleanor Glueck (1937: 205), who wondered, “Can educators, psychologists, correctional workers, and others devise means of ‘forcing the plant,’ as it were, so that benign maturation will occur earlier than it seems to at present?” Of course, criminal justice interventions can work the other way as well: impeding the normative processes of maturation rather than speeding it up. Indeed, arguably, the majority of criminal justice interventions derail rather than facilitate the normative processes of maturation associated with desistance from crime (see Lieblich and Maruna 2004).

Beyond “Self-Change”

The desistance perspective is a bold one and presents an important challenge to the medical model of change inherent in “what works” research. However, this perspective is not without its own epistemological problems. Indeed, ironically, the confusion stems from the original “medical” metaphor inherent not only in the “what works” concept but even in the concept of “spontaneous remission.” In the medical sense, treatment implies the systematic application of some particular remedy for a measurable and well-defined symptom (Schneider 1999: 206). Whatever correctional intervention is, it is surely not that. Even in the medical literature,

researchers have struggled with the notion of what constitutes a “treatment” versus a “non-treatment” experience (see Sobell et al. 2000). Sobell (2007: 4–5) writes,

Do brief physician interventions, often involving a single session and sometimes lasting less than 30 minutes, constitute formal treatment? . . . [What about] advice by laypersons such as ministers, rabbis, and friends, or a trip to a detoxification center or emergency room for whatever reason (e.g., traffic accident, but no psychotherapy provided)? . . . [What about] respondents who had attended one or two self-help group meetings? . . . [Or] those who were unsuccessfully treated, but later resolved their problem on their own?

As such, the very notion of “self-change” is a misnomer, and words like “natural,” “spontaneous,” and “self-initiated” are even more misleading. It is simply never the case that people change on their own. Human beings live intertwined in social networks, and our identities, meanings, and actions are mutually constructed within these milieus (Klingemann et al. 2001). The child who learns to stop stuttering does so with the help of a parent or sibling, in the same way that the child who becomes involved in street violence does so with the help of peers or family. Indeed, research that has asked why treatment succeeds, when it does (see, e.g., Billings and Moos 1983; Klingemann 1992), finds that the “single most notable factor associated with positive outcomes” is positive family/social support (Sobell 2007). Yet professionals like teachers, police officers, and treatment practitioners also play a role, not as change agents but as fellow humans also adept in forming relationships (Schneider 1999). In fact, a growing body of largely qualitative research (e.g., Burnett and McNeill 2005; Farrall 2004; Liebling, Price, and Elliott 1999; Rex 1999;) has begun to explore the qualities of relationships that are most successful in the rehabilitation process.

When treatment is understood as just another relationship between people, the distinction between receiving an intervention and not receiving an intervention (i.e., the “treatment” versus “control” group distinction) may be less important than understanding the quality of the actual relationships, processes, and pathways that desisting persons experience (Farrall 2004; Toch 1997). After all, “individuals assigned to a control condition can seek non-program assistance or training on their own, whereas individuals assigned to an experimental condition can refuse to show up for training or subsidized employment” (Bushway and Apel 2012).

In summary, the problem with the notion of self-change is that it evades the larger epistemological critique of the medical model. People do not change “naturally” or “on their own.” As Mulford (1988: 330) argues, the notion of “spontaneous remission” is just “a euphemism for our ignorance of the forces at work.” In fact, before there was a body of research that was recognizable as the “desistance literature,” leading criminologists made this same argument. In their hugely influential *A General Theory of Crime*, for instance, Gottfredson and Hirschi (1990: 136) wrote, “Crime declines with age. Spontaneous desistance is just that, change in behavior that cannot be explained and change that

occurs regardless of what else happens.” The two decades of desistance theory and research that followed this claim have sought to show that desistance can, in fact, be fruitfully examined and understood. In the final section, I conclude that in order to be “evidence-based,” policy and practice could utilize this growing body of theoretical work, including the large number of qualitative studies it includes.

From “What Works” To “How It Works”: Theory-Driven Evidence-Based Policy

If there is no essential difference between rehabilitation and desistance, then the focus of evidence-based policy might move from “delivering verdicts and best buys” (Pawson 2006: 71) to a framework for exploring the social mechanisms at work in the change process. In other words, research would ask not only “what works” but also “how exactly does change work?” (Lin 2001). Indeed, the originator of the “what works” question, Robert Martinson (1976), began to ask just this question four decades ago in his challenge to early rehabilitation proponents:

But what specifically is the method? Probation-like placement? Small case loads? Unadulterated love? What is it? What is the actual process that takes place by which “recidivism” is reduced? If [one of the rehabilitation supporters] knew which “element” or “dimension” of [the treatment] was having whatever effect he thinks he has found, he surely would not keep it such a secret. He would patent it, sell it around the country to our administrators, be given the Congressional Medal of Honor, and retire to the Bahamas, an honored and wealthy man. [The average rehabilitation supporter] can talk for twenty pages in the special language we all know so well, but he cannot bring himself to say in plain English to my neighbors, who are waiting with bated breath, just what this process is.

Such a reimagining of evidence-based policy has been laid out in detail by Pawson (2006) and others under the rubric of a realist (or “realistic”) or theory-driven (or “theories-of-change”) evaluation framework (see, e.g., Chen and Rossi 1983; Mark, Henry, and Julnes 2000; Pawson and Tilley 1994; Tilley 2000, 2004). This shift in thinking involves two primary moves from the way evidence-based policy is typically understood. First, the evidence of interest would need to become “theory-driven” rather than leaving questions of process locked inside the “black box” of evaluation methodology. Second, a pluralistic approach to what counts as evidence would be required, one that accepts that social sciences require multiple ways of understanding human lives. Both issues are addressed below.

Kurt Lewin’s famous truism “There is nothing quite so practical as a good theory” applies well in the field of correctional services. Too often interventions are developed without devoting any attention to the theory behind how they might work. As Cullen (2002: 283) writes, “Although criminology is rich in contemporary theories of crime, true theories of correctional intervention are in short supply. One searches in vain in mainstream criminology journals and

textbooks for new systematic theories of intervention.” Likewise, Gendreau, Goggin, and Cullen (1999) argue,

The sad reality [is] that so little is known about what goes on inside the “black box” of prisons and how this relates to recidivism (Bonta and Gendreau 1990). Only a mere handful of studies have attempted to address this matter (Gendreau *et al.* 1979; Zamble and Porporino 1990). Analogously, could one imagine so ubiquitous and costly a procedure in the medical or social services fields receiving such cursory research attention?

Tilley (2000) argues persuasively that what should be evaluated in future research are not “programs” but rather theories of change. Once we have improved our theories of how change works, we will be ready to develop interventions on this basis. As it stands, programs typically precede theory, and if any theory is even at play in our interventions, these are typically pasted on in a post hoc fashion (see Latessa *et al.* 2002).

Fortunately, there are a number of rich veins to tap in this regard, including in the psychological literature (e.g., Bandura 1997; Prochaska and DiClemente 1992) and in the growing literature on procedural justice, compliance, and legitimacy (Hough 2010; Robinson and McNeill 2008; Tyler 1990). However, an obvious starting point is the research on desistance from crime. If there are no real differences between desistance and rehabilitation, then this body of evidence is essentially theories of rehabilitation. Raynor and Robinson (2005: 158) point out, “Far from rendering correctional approaches redundant, theory and research in the areas of restorative justice, desistance and resettlement potentially enrich the ways in which we theorise, practice and, more generally, seek to encourage offender rehabilitation.”

Next, answering the question of “how” rather than “what works” would also require a pluralistic and pragmatic reimagining of “evidence-based policy” that recognizes that “science is catholic on method” (Sampson 2010: 492). Hough (2010: 19) argues that

The right strategy for getting closer to answers is not to invest in a huge program of randomized controlled trials, but to construct and test middle-level theories about how to change people’s behavior. . . . The research strategy for testing such middle-level theories needs to be as multi-faceted as the subject is complex. Evidence in support of them may *sometimes* be found in experimental research, sometimes in quantitative surveys, sometimes in qualitative work. In my view this is the real contribution that criminology—whether theoretical or empirical—has actually made to policy [*italics in original*].

Likewise, in his American Society of Criminology presidential address, Todd Clear (2010: 7) put this argument concisely:

We need to take a broad view of what constitutes evidence. This new outlook means we are interested not only in controlled experiments but also in policy studies, qualitative work, natural experiments, and problem analyses. Even the strongest proponents

of experimental criminology readily recognize the value of these other forms of knowing. We have to make sure that studies of these types find their way into the policy-informing literature.

It is certainly worth recognizing that Donald T. Campbell himself, the namesake of the “what works” movement in the social sciences, came to largely share this view toward the end of his career (see Howe 2004). Campbell (1984: 36) wrote,

Qualitative knowledge is absolutely essential as a prerequisite for quantification in any science. . . . We failed in our thinking about program evaluation methods to emphasize the need for a qualitative context that could be depended upon. . . . The lack of this knowledge . . . makes us incompetent estimators of program impacts.

Structured and well-designed evaluations and RCTs are, by far, the best way to measure the contribution of structured interventions in criminal justice. Too many widespread correctional practices have no basis in evidence, do real damage to individual lives, and are never questioned or evaluated. The “evidence-based” movement therefore is enormously welcome in the field of correctional services and has played a crucial role in challenging such harmful practices, insisting that correctional services be held accountable for outcomes beyond the delivery of pain and punishment. Yet evidence is needed throughout the correctional process, and the evidence-based movement should not stop at identifying effective programs but should also contribute to the theoretical understanding of how people change. The qualitative desistance literature provides one potential avenue for improving the theoretical foundations upon which correctional services are based.

Notes

1. School of Criminal Justice, Rutgers University-Newark, 123 Washington St., Newark, NJ 07102.
This work is based on an unpublished review commissioned by the Correctional Services Advisory and Accreditation Panel for the United Kingdom. The author would like to acknowledge the valuable feedback of Dr. Ruth Mann on that original report.
2. Meta-analysis, a method once labeled as the “holy grail” of rehabilitation research (see Logan and Gaes 1993), appears to have quietly fallen out of favor in recent years. However, meta-analysis can be thought of as simply a special case of systematic review involving the statistical synthesis of data from a set of comparable studies.
3. Unfortunately, the CPR “eventually came to disappoint many of those who played a part in it” (Tilley 2004: 255–256). According to one of the program’s key evaluators, “[b]y common consent, the CRP fell well below expectations. . . . The triumphs were few and the failures many” (Tilley 2005). Another evaluator, Mike Hough (2004), writes, “The Crime Reduction Program was ambitious by any standards—well-funded, with a clear and impressive commitment to evaluation. [Those who] originally embraced its objectives with enthusiasm . . . were disappointed when its aspirations failed fully to materialize” (p. 212).

4. Smith and Pell (2003) likewise have recently parodied the “randomista” brigade by pointing out that the effectiveness of the parachute has never been tested in a peer-reviewed RCT. Ruling out the use of observational methodology as too unsystematic to prove the value of parachutes, the authors conclude, with tongues firmly in cheek, “We think that everyone might benefit if the most radical protagonists of evidence-based medicine organized and participated in a double-blind, randomized, placebo controlled, crossover trial of the parachute.”
5. In a previous article (Maruna 2012), I compared this to “the drunk who looked for his car keys under the lamppost, not because he thought they were there, but because that was where the light was best.”
6. See <http://www.crimeresolutions.gov/GlossaryDetails.aspx?ID=15>.
7. This is not to say that self-change in this area (like every field) is not controversial. Finn (2007) argues that although it is a well-documented phenomenon, untreated late recovery is rarely mentioned in textbooks on stuttering, at least partially because its existence challenges widely favored views about the need for professional treatment.

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