

# Gold standard or fool's gold: the pursuit of certainty in experimental criminology<sup>1</sup>

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This article has been prompted by that of Larry Sherman (2009) which advocates a top-down system for developing experimental evidence about what works in reducing crime. The vision of a criminological equivalent of the National Institute for Health and Clinical Excellence (NICE) is seductively attractive to politicians and practitioners. It also contains an implicit criticism of mainstream criminology: how could we have failed so badly to have answered those questions that so obviously need answers? I want to suggest that the promise of experimental criminology in Sherman's article is an over-promise, and to explain why. I have restricted my discussion only that part of the evidential jigsaw that covers the reduction of re-offending – attempts to reform, rehabilitate or deter people who are known offenders. I hope that what I have to say builds on responses to Larry Sherman's article by Tim Hope (2009) and Nick Tilley (2009), both of whose critiques I find very persuasive. And I should say that like Tilley, I admire Sherman's research work as much as I am troubled by what I see as his oversell of the experimental method.

To anticipate the article's conclusions, there has been over-investment (both financially and intellectually) in a technocratic model of reducing reoffending that attaches too much importance in accredited programmes and packages, and under-investment in models that see the process of 'people changing' as a complex social skill<sup>2</sup>. The technocratic model seriously underestimates this complexity, and its advocates wrongly assumes that experimental research can readily identify the causal processes at work in helping people stop offending. They mistakenly suggest that *clinching* evidence<sup>3</sup> about 'what works' can be accumulated when in reality this is a field where evidence is perennially tentative and where knowledge is perennially labile. This is not to deny that there is a place for experimental methods in this field. They constitute one form of evidence about what works in reducing reoffending, and in some circumstances this can be very important evidence.

## **What works? What research has told us**

For as long as I have been doing criminology, there have been tensions between researchers at the 'applied' end of the spectrum and their more traditionally academic colleagues. Government research, and government-sponsored research, tends to be largely a-theoretical, or rather, it implicitly accepts the conceptual frameworks within which political and governmental debate about crime and its control are conducted<sup>4</sup>. This body of work tends to be narrowly focused, and addresses specific policy questions. In general it is empirical, quantitative and increasingly incorporates a cost-benefit assessment. It is overwhelmingly short-termist – designed to answer the question whether whatever is being evaluated is having an immediate impact; and it tends to be uncritical, in the sense that it does not (or cannot) question general government policy. Rather, it assesses whether the evidence favours investment in one policy tactic as opposed to another. Traditionally this sort of research has either been carried out by Home Office (and now Ministry of Justice) researchers, by the NAO or the Audit Commission, or by academics on contract to government departments. Increasingly, though, 'niche consultancies' are also carrying out this sort of work.

In contrast, there is a growing body of academic research which is much more theoretically orientated, and substantially detached from policy dilemmas – even when crime policy is the focus of its attention, as indeed is often the case. Typically this work is concerned with conceptual rather than empirical analysis, and in so far as it engages with empirical work is as likely to draw on qualitative as quantitative work. Probably the best known and the most cited British criminologist at the moment is David Garland (eg Garland, 2001, 2002)<sup>5</sup>. His work is an extended commentary on government crime control policy that is nevertheless largely detached from the day-to-day questions with which politicians and their advisors have to grapple.

There is no necessary reason why this divergence should have occurred. Research can be both theoretically engaged and empirically grounded. Indeed it should be a source of concern if there were not a vigorous interplay between theoretical and empirical work. In practice, however, criminology feels as uncomfortably polarised as it has been at any time since the 1970s.

Over the last seven years or so this polarisation has become more obvious, because the a-theoretical empiricism of government criminology has become more obvious. Research managers at the Home Office and especially at the Ministry of Justice have nailed their colours to the mast of the Campbell Collaboration<sup>6</sup>. The basic idea behind this initiative, itself modelled on the Cochrane Collaboration in the healthcare field, is that one should be systematic in assembling and reviewing research evidence, admitting only those studies that achieve acceptable methodological standards<sup>7</sup>. The threshold for inclusion of studies should be set individually for each review, in the light of available evidence. For example, the Maryland Scale of Scientific Methods is often used as a filtering device<sup>8</sup>. In this, randomised controlled trials are rated as the gold-standard – the highest quality research. The ambition is that with sufficient investment in high quality research, a body of knowledge will be built up over time that would tell criminal justice managers what works in crime control in much the way that the National Institute for Health and Clinical Excellence (NICE) can give guidance to health managers about what treatments represent a good investment.

There are, of course, a wide variety of systematic reviews. Whilst those that are accredited by the Campbell Collaboration website have a bias toward evaluative research with experimental research designs, there is no reason why totally different approaches could not be adopted to the inclusion or exclusion of studies in reviews. It is hard to take issue with the principle of *system* in carrying out reviews of research evidence. It is important for reviewers to be clear and explicit about their rules for searching for evidence, and about their rules for admitting or rejecting evidence. It is systematic reviews that follow rules *inappropriate to the object of study* that are objectionable<sup>9</sup>. An important question to ask is whether the rules for sifting evidence that have gained currency in the Campbell Collaboration and in governmental criminology are the right ones.

### **What systematic reviews of evaluative research have told us**

The most consistent finding to emerge from those examining programmes for reducing reoffending is that insufficient research has been conducted of high enough quality to say much with any confidence. The second most consistent finding – which is, of course, linked to the first – is that knowledge about what works is inconsistent and incomplete. The third finding that consistently emerges is that some programmes sometimes work. Those that have emerged as successful most frequently are cognitive-behavioural programmes. This

probably reflects the fact that they have been developed on a firm theoretical foundation – but also that in their nature, they are amenable to evaluation.

Advocates of the Campbell approach such as Sherman (2009:16) argue that it is still early days, and that if we take the right decisions now about research strategy, in time the evidence base will develop. I would not want to belittle the importance of carrying out evaluations that demonstrate that a particular approach to reducing re-offending *can* work. This is a necessary building block in developing theories about how best to reduce reoffending. However an important series of studies carried out by the Home Office in the mid-1990s graphically demonstrated the limits in generalisability from evaluative research in this field. The first study (Friendship et al., 2002), examining the first four years' experience of a cognitive skills programme working with offenders in prisons, showed considerable impact. However in a later replication of the evaluation (Falshaw et al., 2003; Cann et al., 2003) when the programme was rolled out to prisons on a larger scale, the effectiveness of the programme appeared to evaporate. The authors suggest, amongst other things, that changing levels of motivation amongst prisoners or staff could explain the difference<sup>10</sup>.

These studies graphically illustrate a potential problem for evaluative research: evaluations with robust methods of causal attribution, such as RCTs and quasi-experimental designs with properly matched control groups, tend to have high internal validity, but often have limited external validity. Let us accept, in this case, that the initial study had established incontrovertibly a link between the cause and the effect for the particular groups of offenders and workers under evaluation. However strong this internal validity, the study's external validity – the ability to generalise to other circumstances – may be quite limited. Even if the initial study reached the right conclusion, there are plenty of reasons for doubting whether the programme will work the same magic with other offenders and workers in other settings. In this respect, experimental research into criminal justice differs fundamentally from pharmaceutical trials, where generalisability, at least within broadly similar settings, is usually high<sup>11</sup>.

The implications for criminal justice research strategy are important. It is of great value for evaluative research to establish that something *can* work in reducing reoffending, but this is only the beginning of any serious evaluation. If a programme has been shown to be effective in one setting, the important next research step is to identify the mechanisms by which this impact was achieved<sup>12</sup>. The sort of evidence that one needs to search for in this enterprise

may be distinctively different from that which one needs to establish whether a programme can work.

What is missing from systematic reviews in this field – as currently practiced – is the development of ‘middle level theories’ to explain why some things work and others don’t. The available systematic reviews are such disappointing documents to read, precisely because of their failure to grapple with the real evaluative issues. The main theory of evidence that underpins work of this sort is that the production of ‘clinching evidence’ based on RCTs and similar evaluative methods can result in an accumulation of generalisable hard knowledge about what works in reducing reoffending. There is a failure to recognise that work with offenders is a highly reflexive process in the sense that the meanings attributed to the process by those involved in it will affect the outcomes<sup>13</sup>. This means that the effectiveness of interventions will be highly context-specific. What works in one culture at one time may well be ineffective in other settings and at other times.<sup>14</sup>

### **What descriptive research has told us**

More theoretically engaged descriptive criminology has contributed a great deal to academic understanding of the issues, but the impact on policy has been more limited than is desirable. I do not propose to offer a comprehensive review. Rather, I shall consider just two examples. One is concerned with the development of ‘procedural justice’ concepts in thinking about prison regimes; the other relates to desistance theories. The former has had a very marked impact on thinking within the Prison Service – but less within the Ministry of Justice and NOMS. The latter has achieved very little impact on policy thinking to date, but I expect this to change – not least because there are some important commonalities between this body of work and that concerned with procedural justice, relating to concepts of institutional legitimacy. I have labelled this research ‘descriptive’, to suggest that it is empirical but not experimental.

#### *Procedural Justice*

Procedural justice theories are concerned with explanations for why people *comply* with the law, thus inverting the problem usually addressed by criminologists and criminal justice strategists. The key insights to emerge from writers such as Tom Tyler (eg Tyler et al, 2002; Tyler, 2003; Tyler, 2007) are that people comply with the law for normative as much as instrumental reasons, and that preparedness to obey the law is a function of perceived

institutional legitimacy. The implication is that to promote compliance, criminal justice managers need to identify the drivers of institutional legitimacy, and to maximize the impact of these drivers. The survey evidence put forward by these researchers is good – but not incontrovertible – that preparedness to comply with the law is a function of the perceived fairness of procedures and the personal style of the officials carrying out these procedures as much as the perceived fairness of outcomes.

In the context of working with prisoners, there is an important body of British work in this tradition, by Richard Sparks and Tony Bottoms (Sparks, 1994, Sparks et al., 1996), and in particular by Alison Liebling (2004). These researchers have been successful in describing institutional regimes and the important dimensions of difference that can be found across institutions. Regime quality (including levels of civility and the respect accorded by staff to prisoners) have a clear impact on prisoner perceptions of regimes, and there is some evidence that regime quality is linked to reconviction outcomes. The body of work has been important in supporting senior Prison Service managers' drive to push forward the 'decency agenda'. The evidence for this set of middle-level theories about institutional regimes could not be presented as incontrovertible 'clinching' evidence. It is circumstantial, but cumulative and persuasive. It is significant – and disheartening – that many systematic reviews would exclude this body of work from the 'admissible evidence' about approaches that work in reducing reoffending.

### *Desistance studies*

Desistance studies focus, as their name implies, on the factors that lead offenders to stop offending. The empirical base on which their theorizing rests is typically but not exclusively qualitative in-depth interviews carried out with ex-offenders (eg Laub and Sampson, 2001; Farrall, 2002, 2006; Maguire and Raynor, 2006; McNeill, 2006, 2007; LeBel et al., 2008). This body of work stresses the importance of understanding the centrality of offenders' *agency* (or capacity to exert control over their lives) in deciding to stop offending, and to appreciate that criminal justice agencies are only one of the many factors that impinge on offenders. Desistance theorists tend to stress that the process of change should be *offender-led*, with those helping offenders providing offenders with empathetic support to sustain their motivation to stop offending. Drawing on McNeill (2006), I would summarise the lessons from desistance theory for practice as follows:

- Work with offenders should aim to promote and support offenders' own efforts to stop offending
- This work needs to respect and foster offenders' agency
- It has to be grounded on legitimate and respectful relationships
- It needs to focus on the provision of practical support as well as the development of motivation and capacity
- It needs to adhere to certain 'practice virtues' that support the legitimacy of the enterprise in the eyes of the offender.

Procedural justice theory and desistance theory provide two examples of empirical criminological research playing an important role in developing and testing middle-level theories about criminal behaviour. It is a function of the complexity of the topic, however, that the theories rarely allow for conclusive verification – or even for conclusive (Popperian) disconfirmation. In this respect, knowing how to get offenders to stop offending is simply a special case of the general question about making other people do what we want. We know that this sort of knowledge is problematic. It is not easily acquired or easily stated or readily tested. I find it puzzling that this – fairly obvious – reality is not reflected in the research strategies of those responsible for reducing reoffending. I also find it worrying that a top-down experimental evaluation strategy as proposed by Sherman might be blind to the subtleties of both procedural justice and desistance theories.

### **A conceptual framework for thinking about reoffending**

Let me first simply assert here that the effectiveness of any work to reduce reoffending is likely to be shaped by several interacting factors, including:

- The programmes (structured activities or interventions) to which offenders are exposed
- The quality of the regime of the prison or the ethos of the probation office
- The personal qualities and skills of the staff
- The morale of staff, and the quality of leadership
- The characteristics and mix of offenders under the supervision of the prison or probation office
- Funding and other resources available to the institution

- The additional resources upon which prison and probation staff can draw from elsewhere to support resettlement
- The economic and social environment from which the offenders are drawn.

It is reasonable to expect that the interventions to which offenders are exposed will have *some* impact on their subsequent behaviour, but these are unlikely to be the major determinant of institutional outcomes. If one accepts the work cited above by Liebling, Sparks and colleagues, the quality of the regime will be a major factor in prison settings. Similar considerations are likely to apply in probation work, in that different probation areas and within them, probation offices, will have widely varying styles and work cultures. Perhaps more important, the (positive or negative) impact of friends and family is likely to be greater than the relatively brief encounters that offenders have with probation staff. Levels of resourcing within the prison and probation services will be relevant, as will be the external resources upon which they can draw, and the socio-economic environments in which offenders live.

#### *Models of working with offenders*

There are at least three discernibly different approaches to work with offenders:

- The case-manager/interventions model
- The therapeutic relationship model
- The caseworker/craft model.

The case-manager/interventions model is the one which is currently favoured by government. According to this, the most skilled of prison and probation staff assess offender needs and decide on a package of interventions tailored to this need. The interventions are then delivered by other staff under the overall direction of the case-manager. Within this model, it is the programmes or interventions which are assumed to be the things that really make the difference to outcomes processes so that they can be delivered by less highly trained and qualified staff (see Raynor and Maguire, 2006, for a discussion). The more weight that policy gives to this model, the greater the focus on a narrow sub-set of evaluation questions about 'treatment effectiveness' – equivalent to those answered by NICE-type systematic reviews.



This is not the only model of practice with any currency, of course. At the other end of the spectrum to the case-manager/interventions model is the therapeutic relationship model, which the relationship *is* the intervention. In this model, styles of engagement define the intervention. The prototype for this model is Freudian and Post-Freudian psychoanalysis, of course; its popularity has substantially waned in social work and cognate professions.

Between the two extremes is the casework model, where the 'craft' of the key worker is in managing a process of effective moral persuasion. In this model, caseworkers may well deploy interventions in support of their work, but they will not regard interventions as the primary means of effecting change. Clearly the casework model can be combined with the case-manager model up to a point. Caseworkers may often need to supplement their own skills by referral to specialist workers, but will continue to be the main player in the process.

There is very little narrowly defined *evaluative* evidence against which to test current policy preferences for the case-manager/interventions model – though plenty of useful evidence exists against which to evaluate the model. The National Offender Case-management Model (NOMM) has been subject to process evaluation (Matrix, 2006), but there has been no formal outcome evaluation. To my mind, the casework/craft model conforms more closely to our everyday experience in persuading others to do what we want. And it privileges a different set of evaluation questions about craft skills.

### *Craft skills*

Too little attention has been paid to the *craft* of working with offenders. Within any preferred model for working with offenders, prison and probation staff have to choose tactics for engaging with offenders and for getting them (or helping them) to change. It is useful to think of these as constituting the *craft* of people-changing. These skills are obviously cognate to those required of managers in the workplace, of teachers and lecturers in the classroom and of police on the streets.

One can identify various levels of craft skill. For example there are *tricks of the trade*. These are common-sense tactics for working effectively with offenders, such as taking account of benefits pay-day in arranging meetings, or tailoring appointment times to their early waking hours (1.00 pm - 4.00 pm) rather than to the convenience of the worker (10am – 1.00 pm). Then there are *skills in social interaction* refer to those techniques for asserting control and

authority – which can range from detailed attention to language, body-language and personal presentation to strategies which rely on analysis of the power dynamics in a relationship.

Third there are *casework styles or orientations*, by which I mean approaches such as Motivational Interviewing (see eg, Prochaska and Diclemente, 1986; Miller and Rollnick, 1991), pro-social modelling (cf Trotter, 1999) or client-led approaches. Compliance theory, or Procedural Justice theory, provides a related but separate set of orientating principles (cf Bottoms, 2001; Tyler and Huo, 2002; Tyler, 2003); what is distinctive about these is the emphasis on securing normative as distinct from instrumental compliance.

Finally there are issues of *responsivity* and of *sequencing* to consider. The principle of Responsivity is an important one (Andrews and Bonta, 2003). It is well established that different offenders have different learning styles – both at the individual and group level – and that work should be tailored to these differences. Sequencing decisions relate to the fact that offenders typically have multiple needs, and that effective practice is often a matter of arranging the right *sequence* of support (cf McSweeney and Hough, 2006). To our knowledge there is very little research that throws much light on sequencing, but we would expect to find a fair amount of professional wisdom on the topic.

### **The promise of experimental criminology?**

What I have tried to do here is to demonstrate the complexity of questions relating to work with offenders. Sherman (2009:16) sketches out the evidential utopia that the Campbell Collaboration will create as follows:

Whether you are a crime victim, a police superintendent, a Magistrate or a probation officer, you will be able to go to [www.campbellcollaboration.org](http://www.campbellcollaboration.org) to find out exactly the same kind of information [as is provided by the Cochrane Collaboration in relation to health treatments]. What is the most effective strategy to prevent auto theft? Do burglar alarms work? What can I do to protect my daughter from chronic domestic violence by her partner? What sentence is optimal for a chronic burglar? All these questions deserve to have answers from the Campbell Crime and Justice Group.

I doubt that we shall ever find answers to these questions which are good for all places and good for all time – or good even for the next ten years. Experimental criminology can provide valuable evidence of ‘proof of concept’ – for example that restorative justice meetings can reduce reoffending for some groups of offender in some settings (cf Shapland et al., 2008). However I doubt that it can offer with any certainty the answers to Sherman’s questions, as the honest answer in all four cases are surely, “It depends on the context.”

These questions are much more complex than those about the impact of pharmaceutical treatments. The right strategy for getting closer to answers is not to invest in a huge programme of randomised controlled trials, but to construct and test middle-level theories about how to change people’s behaviour.<sup>15</sup> Choices about strategies and tactics needs to be made on the basis of middle level theories about what is likely to work best.

What counts as a middle level theory? Well, this article is full of examples of partially-evidenced claims about approaches to changing people’s behaviour: that achieving change is a human process, in that the quality of relationship will be a key determinant of outcome; that the personal qualities of the agent of change will thus also be critically important; that for change to occur the offender should confer legitimacy on the agent of change and on the process for achieving change; that legitimacy flows from fair and respectful treatment; and that structured programmes can sometimes be useful in helping the process of change.

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. If criminology is to remain healthy, it needs to do what it does best – which is to construct and test middle-level theories about the maintenance of social order. These middle-level theories can bring insight and perspective to policy. In my view this is the real contribution that criminology – whether theoretical or empirical – has actually made to policy.

Let me end with a political point. As we fall under increasing pressure to demonstrate impact and social utility, criminology needs as a discipline to develop a more inclusive and shared narrative about the value of criminology. This narrative needs to recognise the importance of questions about instrumental impact – ‘what works?’, in the narrow terms set by Treasury scrutineers. But we also need to recognise the narrowness of such questions, set against the breadth and diversity of the contribution that is actually made by criminology. If we over-

promise clear answers to questions that are more complex than they appear, we shall pay heavily for this in the middle term.

## Notes

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1 This article is based on a paper commissioned by the National Audit Office, and I am grateful to the NAO for funding this work.

2 Intellectual fashions ebb and flow. This article has some resonances with the backlash against 'positivist criminology' which occurred in the late 1960s and early 1970s – though I do not share the 'radical non-intervention' pessimism that critics of that era expressed.

3 I borrow the term from Nancy Cartwright, who makes a useful distinction between evidence that clinches conclusions and evidence that vouches for conclusions. She argues that in reality the most widely used methods for warranting causal claims – even in scientific work – involve vouching, not clinching (Cartwright, 2007).

<sup>4</sup> See Morgan and Hough (2007) for a fuller discussion.

5 Despite being based in the US for many years.

6 See Hollin (2008) for a discussion. I and colleagues have argued elsewhere that this is best understood as an understandable response to the disappointing results of a very large-scale research programme established in the first years of the current administration for evaluating the Crime Reduction Programme. See Hough (2003), Maguire (2003).

7 See <http://www.cochrane.org> for details of the Cochrane Collaboration, and <http://www.campbellcollaboration.org/> for details of the Campbell Collaboration.

8 The Maryland Scale assigns evaluative studies into one of five categories, according to the form of experimental control that is used to help to attribute causality. The highest score is reserved for studies that use randomised controlled trial methods. Systematic reviews usually exclude all studies that fall into the lowest two categories, and some include only the top, or the top two, categories. The scale provides a measure of internal validity, but does not take account of external validity.

9 The most common form of mismatch is to specify inappropriately restrictive rules. Our Home Office-funded evaluation of Drug Treatment and Testing Orders (Turnbull et al, 2000; Hough et al, 2003) is typically excluded from systematic reviews using the Maryland Scale, as lacking adequate comparison groups. So too is the Scottish equivalent (McIvor, 2004). In

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combination the two studies say a great deal about the order's effectiveness, however. But reviews which wastefully throw away such evidence necessarily exclude such comparative insights.

10 There are other possible explanations. There might have been problems of matching in the – quasi-experimental – design that biased the first study towards success, for example.

11 For example UK doctors would probably accept the clear US evidence about the effectiveness of statins in lowering cholesterol as a good guide to outcomes in this country.

12 The Home Office very sensibly mounted a qualitative study to get at the reasons for the differences between the two evaluations (Clarke et al., 2004).

13 There is a discussion to be had about the extent to which the prescription of pharmaceutical drugs is a similarly reflexive process, but *on the whole* it is safe to assume that statins – or paracetamol or aspirin – will achieve their intended effect regardless of the meanings that participants construct of the process.

14 I suspect that cognitive behavioural programmes may have proved so successful specifically with offenders over the last 25 years because they typically – but not always – provide an apparently morally neutral framework within which offenders can think about their offending. They facilitate a covert moral dialogue, at a point in our cultural development when a more explicit moral dialogue would fail. Some CB programmes, such as ART (Aggression Replacement Therapy) specifically address moral values.

15 Ironically, Sherman's own work is full of very creative examples of such middle-level theories, as Tilley (2009) argues.

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