

WHAT WORKS: RHETORIC, REALITY AND RESEARCH

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Abstract

The What Works initiative in England and Wales has been vaunted as an example of evidence-based policy/practice. Yet despite such claims, the research base is limited and not strong enough to support a national programme. Various reasons for the limited use of research in criminal justice are examined, most of which are related to misunderstandings of what research can offer and how it is used. The purchaser-provider model of commissioning research consigns researchers to a lesser role. Given the current political climate and the rise of the Campbell Collaboration, there are good reasons to be anxious about the well-being of imaginative, innovative research.

Introduction

The What Works initiative represents a major investment in the community treatment of offenders. It has been vaunted as the most substantial ‘experiment’ (Merrington and Stanley, 2000) of its kind in the world and the term is instructive. We may not all agree with the natural science connotations of the term ‘experiment’ with its overtones of positivism and determinism, but as an ‘experiment’ we are entitled to expect a rigorous approach to research and evaluation. Experiment, however, also has suggestions of an approach being tested out on a small scale, in a limited way – yet What Works is a national programme and the framework within which the National Probation Service (NPS) works with offenders. Given the significance of What Works for the NPS, it is difficult to see it as an ‘experiment’. What Works has also been claimed as exemplifying an evidence-based approach to policy and practice, although not all commentators would go along with this view (see, for example, the essays in Mair, 2004a; Merrington and Stanley, 2000; Merrington and Stanley, 2004). In reality, the What Works initiative has been characterised by academic in-fighting, politics, and the entrepreneurial power of a few individuals. Ironically, for an initiative that claims to be evidence-driven, the input from research has been limited.

In this paper, I will reflect on the role of research generally in the What Works initiative. An examination of this role will help to highlight the gap between the rhetoric of What Works and the reality, to show up the tensions and contradictions that exist at the heart

of this key policy and practice development. First, the immediate research base for What Works will be examined briefly. Following this, the various obstacles to research being utilised effectively will be discussed. In the final part of the paper, the possibility of improvements in the situation will be explored.

An Unholy Trinity?

In research terms, three strands of research have been crucial to the official discourse of What Works in England and Wales: meta-analysis, two reports sponsored by Her Majesty's Inspectorate of Probation (Underdown, 1998; Chapman and Hough, 1998), and a report from the Home Office (Goldblatt and Lewis, 1998).

Underpinning official acceptance that 'treating' offenders in the community could be effective in terms of reducing offending, was the sudden appearance of meta-analysis to criminal justice research. Meta-analysis, which might well be accurately described as old wine in new bottles, is 'the statistical analysis of a large collection of analysis results from individual studies for the purpose of integrating the findings' (Glass, 1976: 3). It has been widely used in medical research, but only began to appear in criminology from the mid-eighties with meta-analyses examining the effectiveness of interventions for juveniles delinquents (Garrett, 1985; Gottschalk et al, 1987). The two most significant meta-analyses for What Works were carried out by Don Andrews and his colleagues (Andrews et al, 1990a) and by Mark Lipsey (1992). The first of these claimed to show that treatment worked, but it should not be forgotten that it did not go unchallenged, as it was partly a response to an earlier meta-analysis that claimed that treatment had little impact upon recidivism (Whitehead and Lab, 1989) and a debate ensued (see Lab and Whitehead, 1990; Andrews et al, 1990b). The second (Lipsey, 1992) was a mammoth study incorporating 443 studies which, despite its positive findings, demonstrated the difficulties involved in trying to synthesise the results of many disparate studies:

- The primary delinquency measure was not always recidivism
- Follow-up periods varied, with almost half of the follow-up studies having follow-up periods of less than six months
- Almost half of the studies were coded as having low treatment integrity
- Almost two-thirds of the programmes analysed were less than 24 months old
- Half of the studies covered non-juvenile justice interventions
- Those who administered programmes varied from criminal justice staff to mental health personnel to lay persons

These meta-analyses concentrated almost exclusively on juvenile offenders in the USA and many of the primary studies they relied on were taken from the 1950s and 1960s. The applicability of their findings to adults in the England and Wales thirty or more years later has simply been taken for granted. Perhaps most disturbing was the media take on the Lipsey meta-analysis; tabloid newspapers claimed that a huge research study covering more than 400 projects (and in this case size was assumed to reflect quality and reliability) had proved that employment was the most successful form of treatment for juvenile offenders.

This was not quite the case, however, for the press reports forgot to mention (or were not aware) that this finding did not emerge from more than 400 studies but in fact was based on only four.

While the meta-analyses of Andrews and his colleagues and Lipsey are certainly suggestive, it is difficult to see how a national programme could be constructed out of them – especially a programme that claims to be evidence-based. And meta-analysis is not without its critics (see Copas, 1995; Mair, 1995).

The Probation Inspectorate generally – and the late Chief Inspector of Probation, Graham Smith specifically – played a key role in building up the ‘evidence’ for What Works. HMIP sponsored the Underdown report ‘Strategies for Effective Supervision’ (1998) which at least clarified the extent of the problem: 267 programmes were initially claimed to have been subjected to outcome evaluation by 43 probation services (out of the then 55); 210 were identified as potentially suitable for follow-up, of which 109 were actually followed-up; 50 of these were selected for a more detailed follow-up, of which 33 merited further examination; of these, only four programmes were judged to have ‘demonstrably effective outcomes’ (Underdown, 1998: 107). In other words, only 1.5 per cent of programmes which were claimed to have been evaluated, had been satisfactorily evaluated; and it should be noted that all of the four programmes involved small numbers and one was more than 10 years old.

The other HMIP report, ‘Evidence Based Practice’ (Chapman and Hough, 1998) presented an overview of the principles that might be involved in effective practice, but just how these were to be brought down to the messy level of working with offenders, and how they were to be woven together into a coherent framework, was not addressed. Once again, neither of these two Inspectorate reports can be said to constitute a solid foundation for the national What Works initiative. Nor can the final part of this trinity, the Home Office report ‘Reducing Offending’ (Goldblatt and Lewis, 1998). At best, this report sets the stage for developing effective practice, but it is sorely lacking in detailed prescriptions about effective programmes and how they worked. Describing a handful of disparate research studies that found reductions in offending is not a recipe for a national policy.

It would seem, then, that research has had little to contribute to the development of the What Works initiative. While the rhetoric behind What Works would have us believe that it is firmly grounded in evidence, the reality is rather different. Why should research have such a minor role?

The Role of Research in Criminal Justice Policy/Practice

While we could all name examples of ‘good’ and ‘bad’ research (how far we would agree about which was which is another matter), the reasons for research studies not punching their weight in criminal justice have relatively little to do with their being badly

conceived and/or executed. They do, however, have everything to do with the politics of research and the status of researchers. The following list of reasons for research being marginalised are specific to criminal justice and my own experiences over a period of 25 years; I would suspect that they would also apply to most other areas of policy/practice.

1. Researchers are specialists, while those who commission research tend to be either generalists (policy makers/civil servants) or practitioners. Neither of these two groups has much time for researchers, who are viewed with some suspicion as academics inhabiting a very different world from the day-to-day difficulties of policy or practice. To policy-makers, researchers are under-labourers who are called in at times to provide support to the more aristocratic tasks associated with the policy process. They are usually perceived as a necessary, though by no means sufficient, aspect of policy planning intended to justify a new initiative, to demonstrate that the proposed policy is not simply the brainchild of an ideologically driven government minister.

For practitioners, research is used not so much to justify a policy but to try to show that a practice is effective. In criminal justice this means evaluating the success of a new community penalty for young drug misusers, or a new model of policing deprived neighbourhoods. For the most part, the practitioners involved in the scheme being researched will be keen on it and do their utmost to demonstrate that the initiative is a good one; managers especially will wish to end up with a positive evaluation which they can use to demand further funding and bask in success. Other practitioners who may not be so keen on the initiative will try to ensure that the researcher takes on board their point of view, thereby muddying further waters that would not have been particularly clear in the first place.

In neither case do policy-makers or practitioners fully appreciate the abilities of researchers or the limitations of what they can offer; nor, conversely, and despite claims to the contrary about their knowledge and expertise, do researchers appreciate the worlds of policy-makers or practitioners. Researchers are not policy-makers or practitioners; the three types of activity are very different, the cultures each group inhabits overlap only slightly. The tensions among the three groups need to be recognised and acknowledged, not swept under the carpet and kept out of sight.

2. Research tends to be perceived as an optional add-on after a new initiative or policy has been delineated and is about to be, or is in the process of being, implemented. This is often too late for research to be involved. By the stage of implementation the aims and objectives of the policy will have been decided and research is bolted on in order to keep the Treasury satisfied that evaluation will be carried out. Questions about the clarity of the policy objectives, their prioritisation and whether the objectives can be easily operationalised are therefore not discussed with the researcher, who is put into a difficult position. This model also means that what is evaluated is (at best) the initiative as implemented, thus ignoring the development of the policy and its implementation – key aspects of the policy process which can

contribute to the failure of a new initiative.

The simplistic model of the policy process implied here does little justice to the complexities of the policy process or the potential input from research. Hogwood and Gunn (1984) suggest a more realistic set of stages for the policy process, but emphasise that it 'is not intended as a straightforward description of what actually happens to every issue; rather it is a framework for organizing our understanding of what happens – and does not happen' (Hogwood and Gunn, 1984: 4):

- Deciding to decide (issue search or agenda-setting)
- Deciding how to decide (issue filtration)
- Issue definition
- Forecasting
- Setting objectives and priorities
- Options analysis
- Policy implementation, monitoring, and control
- Evaluation and review
- Policy maintenance, succession, or termination

Research could play a part in each of these stages, but for the most part its role is confined to the penultimate one of evaluation and review.

3. Research is too closely tied to the Home Office which either carries out in-house work or commissions academics or (increasingly) consultants to do so. While the Home Office desire for relevant research is commendable and perfectly understandable (and many academic careers have been built upon Home Office funding for research), there are problems with the definition of such research. First, as I have argued before, 'policy-makers are not adept at framing research questions' (Mair 2000: 262); they have problems with certain issues or they do not know what is happening with some programme, and they expect research to solve the problems or fill the gaps in their knowledge. While such expectations are comprehensible, they cannot always be fulfilled by research, and by the time a research proposal has been put together and agreed and the research has been carried out and written up, the problems may have disappeared, the gaps may no longer be apparent, and those who commissioned the research may have moved on to another post.

The second problem is the fragmented, short-term and reactive nature of Home Office research. Given the nature of Home Office business, such characteristics are comprehensible and legitimate, but they do not necessarily lead to relevant research. Home Office research is planned on an annual basis and policy priorities can change so that long-term, cumulative research is lacking. Research is fragmented insofar as individual studies are focused on single initiatives; different projects within the same area are carried out with little regard for the potential interconnections between them. Acknowledging possible connections between projects in different areas is even

more difficult. Basing a national programme on a few unrelated research studies is not evidence-based policy, and this is especially pertinent with regard to What Works.

4. The timing of research and the kind of research carried out are not especially conducive to the policy process. Policy tends to be driven to a tight time-scale; politicians require answers within a few days or weeks, they do not tend to work to a lengthy time-scale as a four-year period is their furthest horizon. A research project can easily take 18 months or longer and this can be far outside the parameters within which policy-makers work, so that by the time research is delivered the policy issue no longer exists (for whatever reasons) and the research becomes purely of academic interest. A major Home Office study carried out over a two year period in Hampshire (1981-82) aimed to explore a variety of possibilities to reduce the number of offenders going to custody. At the time the research was commissioned, prison overcrowding was a highly significant policy issue, but by the time the research was completed prison overcrowding was no longer at the top of the policy agenda and the research was marginalised (see Smith et al, 1984; Mair, 1985). Research does not sit neatly and tidily alongside the requirements of policy.

While matters do seem to have improved over the last five or six years, there is still scant interest in process evaluation research; the focus is on outcomes. Again, Home Office interest in outcomes is perfectly legitimate but by focusing on these the ways by which outcomes have been shaped are ignored or left on the periphery. Without a full understanding of why outcomes are as they are, it is impossible to replicate a successful initiative or improve a failing one. This is where meta-analysis is less helpful; its focus on what is 'effective' does not tell us the why, the with whom, or the how of an effective programme and these are questions that need to be answered. Increasingly, Home Office sponsored research includes what is called a process evaluation, but this is a shadow of the real thing and tends only to describe the operation of a programme rather than analyse in depth how it is operating in practice and linking this to outputs and outcomes. Pawson and Tilley (1997) list eight rules for what they term realistic evaluation, which cohere around the examination of what they refer to as Context-Mechanism-Outcome configurations. They argue that only by understanding the various possible mechanisms through which a programme might work, as well as the social context into which the programme is introduced, can one begin to understand the outcome patterns and how these are produced. While the Pawson and Tilley model of evaluation research is certainly an appealing one, it represents more an ideal model than one which can be easily attained in practice. However, it should be obvious even from this somewhat simplistic outline of realistic evaluation just how far the What Works initiative falls short of this ideal. Even if there were some outcome data (and I have argued that there was not nearly enough), there was nothing on contexts or mechanisms to support a fully designed programme.

5. Research is not the value-free, independent activity that it is all too often taken for granted as being. Researchers from the same disciplinary backgrounds argue over

methods and theories; and researchers from different disciplines argue over whose discipline is the more relevant for a policy. It could be argued that What Works has been a result of the successful marketing of a particular brand of psychology (cognitive behavioural theory) emerging from Canada and transferring to England and Wales at a time that was conducive to its being accepted as an effective way of dealing with offenders. As sociology and social work fell away in significance for community penalties (and the first had played a major role in research, while the second had been the basis of probation training), psychology took over as the dominant academic discipline. We now have the absurd situation where, instead of various disciplines being accepted as equally relevant to probation work, psychology (in the guise of cognitive behaviouralism) is in the driving seat and questions about where housing, poverty, employment and health are located have been marginalised (see Drakeford and Vanstone, 1996; it should be added that this imbalance *may* be in the process of changing).

6. Research is all too often commissioned to examine a single initiative or programme, and only one discipline tends to be involved. Initiatives do not exist in a vacuum, they are inserted into a social context which has traditions of working and cultures of practice. How often are these taken into account when a new initiative is developed and implemented? In the late 1980s when intensive probation (IP) schemes were being encouraged by the Home Office, ten probation areas were persuaded to participate in the initiative (and not all were equally keen). The origins of IP were various: to tackle prison overcrowding, to restore judicial confidence in the probation service, to reduce reoffending, to provide effective punishment for 17-25 year olds (and replicate the decrease in juvenile involvement in the criminal justice system that had occurred in the 1980s), and to cut costs by being cheaper than custody. How a single initiative was expected to deliver these expectations was never clarified. In addition, the IP initiative was associated with a slightly earlier initiative 'Tackling Offending' (Home Office, 1988) which led to some confusion on the part of Chief Probation Officers as to how IP differed from the earlier initiative. Not all of the ten areas took part: one dropped out prior to the start date; a second ran for only 12 instead of 24 months; and a third failed to get an IP scheme up and running. Two months before the official start date of IP, it was announced that the combination order (now the community punishment and rehabilitation order) was to be introduced in the forthcoming Criminal Justice Bill (which became the Criminal Justice Act 1991). This threw the idea of IP into further confusion as how IP would relate to the proposed new order was never clarified. And to cap it all, IP schemes were expected to make use of the services of voluntary organisations – not, one might think, an especially contentious idea. But probation staff were deeply suspicious at the publication of a discussion document 'Partnership in Dealing with Offenders in the Community' (Home Office, 1990), which many interpreted as a move by the government to take away certain tasks from the probation service and make these the responsibility of voluntary organisations. The social and political context for the implementation of IP, then, was complex and much of it was not conducive to the

successful operation of a new initiative. Indeed, the IP initiative fizzled out and is very much a footnote in probation history. While the research commissioned to evaluate the initiative tried to take some account of the context of the project, and to demonstrate its significance to a successful operation, its lessons were ignored and a planned reconviction study of offenders who received IP was scrapped (Mair et al, 1994; Mair, 1997).

A similar picture can be constructed for the context for What Works. What Works too had a variety of factors lying behind its introduction: the probation service was perceived to be not as effective as it might be; it was assumed to have lost the confidence of sentencers; it was seen as 'soft' on crime; it was accused of having no valid and reliable methods of working with offenders. Added to this appealing list of reasons, was the entrepreneurial flair of two significant individuals – Chris Nuttall of the Home Office Research and Statistics Directorate and Graham Smith of the Probation Inspectorate – who were fully aware of the research emanating from Canada and encouraged its reception in this country.

The context to which it was introduced was not designed to ease its implementation: probation training was in abeyance and when the new training arrangements were introduced they would require some time to settle down; the probation service had been starved of resources during the declining years of the Conservative government; staffing levels were low; sickness absence was high; a major reorganisation of the service resulted in the introduction of the National Probation Service (NPS) on 1 April 2001 which was faced with remarkably high targets to meet in terms of completions of What Works programmes; a number of new tasks were placed on the NPS including work with high risk offenders, released prisoners, with Crime and Disorder Reduction Partnerships, and a demanding new community penalty, the Drug Treatment and Testing Order. The research commissioned by the Home Office into What Works has focused upon separate projects; Basic Skills, General Offending programmes, community punishment, Think First and prisoner resettlement. The research into these projects has examined them in blinkers without taking account of the context into which they have been introduced – and as has been noted, that context was a complex, multi-faceted, shifting one. This 'silo' approach fails to appreciate the full picture as, for example, one probation programme may link up with another, and a programme only lasts for a part of a community rehabilitation order. In addition, key aspects of a programme-centric approach were not in place when What Works programmes were introduced; a national approach to risk/needs assessment (OASys) was being planned and designed but has only been implemented in the past 12 months (2003-2004), and the full significance of case management seems to have been realised belatedly (see Holt, 2000; Kemshall et al, 2001). Indeed, Kemshall et al. have identified other factors that require the attention of the probation service:

...organisational issues; leadership, culture and change management;
operational management and communication; staff development and

training; practice issues and case management; and capacity planning and performance management. However, in the rush to roll out the programme curriculum, it is these factors that have been overlooked...[which] leaves services with significant barriers to the adoption and implementation of effective practice...(Kemshall et al, 2004: 173-174).

There are, therefore, good reasons for the relatively minor role that research plays. Its place in the policy process is taken for granted rather than examined critically. Discussions between those who commission research and those who carry it out are all too often a series of dialogues of the deaf.

Glimmers of Hope?

Are there any glimmers of hope that research might become more fruitful in terms of its influence on policy or practice? In the immediate term, the answer is probably not – and for two key reasons, one of them directly political and the other more academic (though with political undertones).

Janet Newman has shown very clearly the salience of modernisation to the Labour government:

Modernisation was presented as being not just about short-term efficiency but about longer-term effectiveness...[it] was more strongly oriented towards the delivery of new policy agendas in health, education and social welfare, areas in which the delivery of policy outcomes was critical to Labour's continued political success (Newman, 2001: 52).

From this point of view, the What Works initiative was at least as much about modernising the probation service as it was about making it more effective in terms of reducing crime. Without the modernisation programme that was symbolised by the introduction of a National Probation Service, the What Works initiative would have been inconceivable; and without the What Works initiative, a national service would have been incomprehensible. Thus, What Works is a crucial political initiative and, as such, research into its criminological aspects may not be quite as important as we might expect.

It is worth recalling the example of the Intensive Supervision Probation (ISP) movement in the USA in the 1980s. The various programmes that made up ISP had stated aims that were to do with reducing prison overcrowding, cutting costs, and cutting crime. As Michael Tonry (1990) noted, for the most part the ISP programmes were failing to meet their stated aims but this did not lead to the demise of ISP – it continued and prospered. Tonry argued that while ISP was failing to meet its stated aims it was succeeding well in hitting what he termed its latent goals which were increased sentencer credibility in probation, greater visibility, acknowledgement and respect for probation as a public service, and closer alignment with the popular politics of law and order. And it was these latent, more 'political' goals which were at least as important as ISP's stated goals:

In the current climate, it cannot hurt the professional and institutional interests of probation to have launched programs that are politically salable because they accord with public and political calls for severe punishment of criminals (Tonry, 1990: 187).

The parallels with What Works are intriguing.

The academic factor is the fairly sudden emergence of the Campbell Collaboration and its interest in developing systematic reviews of research evidence. The Campbell Crime and Justice Coordinating Group (CJCG) sees its mission as being 'to oversee the preparation, maintenance, and accessibility of systematic reviews of research on the effects of criminological and criminal justice interventions' (Farrington and Petrosino, 2001: 39). While on the one hand, the CJCG can be seen as a positive development to encourage more rigorous and reliable research, on the other hand its emphasis upon meta-analysis and randomized experiments suggests an effort to colonise and control all criminal justice research. If a research study does not conform to the CJCG's demanding standards then it will not be considered in a systematic review; the CJCG focus upon quantitative research means that a considerable amount of relevant research is likely to be ignored. While some of the difficulties of meta-analysis are noted (Wilson 2001) and the potential problems of bringing research evidence to the attention of politicians is acknowledged (Welsh and Farrington, 2001) by members of the CJCG, there remains a somewhat naïve perception of the policy process and its relationship to research.

The CJCG is partly funded by the Home Office so that its ideas are likely to be welcomed there – with a possible impact upon Home Office specifications for research. The CJCG is an example of academic politics where a certain group or discipline try to attain privileged status thereby enhancing reputations and gaining access to funding and power. Perhaps most disturbing, however, is the possibility that the CJCG has maneuvered itself into a situation whereby it cannot lose. Because of the way it structures its reviews, positive findings will only emerge from studies that conform to its standards; and if positive findings are not found (either because there are not enough reliable studies to review, or the results are unclear) then the answer is better research that will follow CJCG requirements.

Another factor that is critical before research can be more directly relevant to policy or practice is dissemination. Research is written mainly for other academics to consume - even Home Office commissioned research which is, of course, expected to be policy relevant is still oriented to an academic audience. Efforts have certainly been made to try to rectify this situation with the introduction over the past few years of Research Findings, Development and Practice Reports, and Toolkits. These are steps in the right direction, but dissemination policy remains tied to a simplistic model whereby research is published (and the publication format is relatively unimportant) and therefore assumed to be available to the appropriate audience who will read it, understand its messages and take these on as part of their practice. If only matters were as simple.

The dissemination process is a fairly arduous one that only begins when a research report has been delivered. Research reports are not (usually) written to bring out their policy/practice relevance; indeed, a researcher may be unaware of how some of his/her results relate to current practice. Thus, research needs to be filtered in order that relevant results are highlighted. To do this may require the attention of someone other than - or perhaps in addition to - the researcher. While the researcher may not be fully aware of current policy or practice or possible plans, the 'other' may not be fully aware of some of the potential problems in pulling out what seem to be significant research findings. Even when this task has been successfully carried out, there remains the matter of how the relevant results are made available to practitioners (a report, a summary, an official government circular, etc.). During the 2000-2001 period when the probation service was being transformed into the National Probation Service, Chief Probation Officers were being bombarded with official circulars at the rate of almost one a day, and many of these required some kind of response. In such a situation, a report of relevant research findings would be of low priority, unless it came with a requirement that it be acted upon (and the period for action was usually a short one). This is not the most successful way to effect change; staff respond better to consultation, discussion, perhaps some negotiation (or at least the feeling that there is scope for this), and time to prepare. It is not difficult for an initiative to be undermined by practitioners being obstructive - which could render the best and most relevant research worthless.

Politicians use research to suit their own purposes. The Home Office study of the pilots for community service was designed officially to guide decisions about whether a national roll-out would occur, but, as Ken Pease, the lead researcher on the project has noted (1983), there was never any doubt that this would happen so the research was irrelevant in one sense (though in criminological terms it was an important study). A Home Office study of reconviction rates (Lloyd et al, 1994) was published with a press briefing that argued that the research demonstrated that prison was effective in terms of reconvictions - although the published report stated nothing of the sort and the decision to slant the press briefing was solely a political one, coming when Michael Howard was Home Secretary and arguing that 'prison works'.

Despite these rather gloomy prognostications it would be misleading to conclude with a vision of research as irrelevant. The fact remains that, for better or worse, a considerable amount of research is funded and consumed by the Home Office, and as purchasers how they treat the final product is very much up to them. Criminology has always been an applied science, flirting with policy/practice and perhaps has forgotten that if you sup with the devil you need a long spoon. The desire of criminologists for funding and to affect policy or practice inevitably means that expectations of influence will be raised. And just as inevitably, such expectations will be dashed as research is only a part of the policy process; it does not play the privileged role that researchers might like to think it does. On the other hand, however, a disinterested observer might well conclude that Canadian researchers (e.g. Robert Ross, Paul Gendreau, Don Andrews, James Bonta) have been significant in influencing the direction of the National Probation Service via the What

Works initiative. But, as I have argued elsewhere (Mair, 2004b), this is not so much a neat example of research influencing policy as of the results of some research being promoted effectively and then adopted for political reasons – and it is in this way that research is likely to impact upon policy.

Research generally (and perhaps even more so in the social sciences) is cumulative over a period of time; one study does not suddenly change the world. While What Works has not been built on a solid foundation of research evidence, we are currently collecting evidence from a variety of research projects that – if properly utilised and disseminated– should help to influence the future of community penalties. Rigorous research that uses reliable methods appropriately, depending on the subject matter and the objectives of the project; that is delivered on time and written in such a way as to be useful for its intended audience; and that is disseminated accurately to its target group with clear guidelines about how to use it, will always be needed. The specific methodology is not significant as fashions in research change. What is required is greater dialogue between researchers and those who commission research; a simple purchaser-provider model is not the best way to buy research. Greater understanding of what can be offered, as well as its limitations, will lead to better focused research that can be useful in contributing to policy or practice.

Even where research is more distanced from government, it can still examine policy and practice in a relevant way. We are often confronted with policy/practice decisions that are presented as the only way forward. But the policy process is about a series of choices, and research can help to highlight these choices and demonstrate why one course was taken rather than another. A range of questions lie behind policy decisions, such as: how is a problem ‘noted’, how is it brought to someone’s attention, who defines it as a problem worth attending to; how are ‘solutions’ canvassed, who is included in the canvassing and why, who decides to take on board some evidence and advice and not others; how are policies developed in practice, who formulates the policy, how is it implemented; who decides to have the new policy evaluated and how is it evaluated, what is done with the evaluation. Analysing the policy process, and attending to such questions and providing possible answers is a key part of the research task.

Several years ago I argued for the need for research into community penalties to address new topics:

We still know little about the organization of probation services and how that may relate to the delivery and outcomes of penalties, or how individual probation officers justify what they do and how they do it...A greater willingness to address the politics of community penalties as part of the research agenda is needed; the wider context is at least as important as a narrow-minded obsession with simplistic notions of effectiveness (Mair, 2000: 278).

With the government's proposals to set up a National Offender Management Service (NOMS; Home Office, 2004) a whole new set of issues arises for research into community penalties. More worrying, however, is the possibility that whatever research has been recently completed or is currently underway into What Works programmes could become irrelevant as more organisational upheaval takes place. It is difficult to be clear about precisely how the probation service will be affected by NOMS, but it is all too easy to envisage a very different context within which community penalties (or, more likely, a single community penalty as included in the Criminal Justice Act 2003) are delivered. With a completely different operating environment and a mix-and-match approach to community punishment, research will have to move quickly to keep up with policy. It is vital for the well-being of the probation service (assuming that such an entity remains in existence) that researchers react to the challenge positively, otherwise we risk being consigned to the sidelines. Whether we like it or not, research will always be at the whim of politics – the key is to be fully aware of this and work constructively with such knowledge.

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