

Factors and Conditions Influencing the Use of Research by the Criminal Justice System

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Whither We Are Tending

“If we could know *where* we are, and *whither* we are tending, we could then better judge *what* to do, and *how* to do it,” said Lincoln in his famous “house divided” speech in 1858 (Angle, 1991). While we might imagine that the debate over criminological research and its applicability to practice is inconsequential compared to the coming crisis Lincoln was addressing, such a judgment ignores the reason the debate is so important. On any given day, over two million people are being held against their will in jails or prisons in the United States and over twice as many more are under community supervision or otherwise entangled in the criminal justice system (Harrison and Beck, 2006; Glaze and Bonczar, 2006). Collectively, they represent tens of millions of victims. About 350,000 people a year are seriously injured in a crime and over the last decade an average of 20,000 have died violently each year (Federal Bureau of Investigation, 1990-2000; Rennison, 2001). One in three Americans are afraid to walk alone in their own neighborhoods at night (Gallup, 2000). These and many other statistics may define where we are, but the fact is that our own professional house has long been divided between researchers and practitioners and this has hobbled our society’s response to crime and violence.

What is most interesting about Lincoln’s remark is the distinction he drew between what to do and how to do it within the context of the *goals* we wish to set. This manner of framing the issue is a classic statement of Pragmatism; Lincoln was speaking at a time when that tradition was emerging as the dominant philosophical perspective in America (Menard, 2001). Much later, John Dewey (1929:7-8) captured the Pragmatic spirit of inquiry when he argued that knowledge should be tested by asking the questions

Does it end in conclusions which, when they are referred back to ordinary life-experiences and their predicaments, render them more sig-

nificant, more luminous to us, and make our dealings with them more fruitful? Or does it terminate in rendering the things of ordinary experience more opaque than they were before, and in depriving them of the having in ‘reality’ even the significance they had previously seemed to have?

For Dewey and the Pragmatists this test applies equally to any type of inquiry, including those that use the methods of science. Like everyday knowledge, science must begin and end with experience and its ultimate test is *how it can be used*. From the Pragmatist perspective, a science that begins with experience, but ends with a published report providing an explanation is incomplete. In this paper, we will discuss the contrast between this perspective and the more common practices of social science research inherited from a Positivistic view of the scientific enterprise to examine a number of issues that influence the relationship of research and practice.¹

Central to the Pragmatist critique of Positivism is the argument that the latter relies exclusively on an attenuated understanding of experience. In Pragmatism, the concept of “experience” joins the dual meanings of the term in ordinary language to include, 1) experience *of* something as when we observe the world around us and 2) experience *with* something when we participate in an activity (Dewey, 1925; Murphy and Rorty, 1990; Ratner, 1939). When we have observational experiences, including when our observations are systematic as in scientific research, it produces empirical evidence. When we have participatory experiences, we develop skills. We can say, for example, that a person has a great deal of experience in substance abuse programs and mean by it either 1) they have done many studies, 2) have run programs for many years, or even 3) have been treated for dependency several times. It is the union of these differing senses that constitutes the full meaning of experience in the Pragmatic sense. Keeping these multiple aspects of the concept in mind helps us think through the supposed divi-

sion between research and practice.

Largely for historical reasons, the social sciences have limited themselves to a language of experience-as-evidence and this has served to render its conclusions more opaque, to use Dewey's term. When social science emerged as a professional discipline, it embraced the Positivist view of a privileged language of science that assumed the verification theory of meaning. This value neutral vocabulary of operationalization has served social science well in developing and testing hypotheses on an empirical base. The obvious utility of this language argues against the view that it is simply a patina of "scientific" jargon designed to bolster the status of the discipline or a linguistic tick that amounts to an occupational hazard.

At the same time, there remains a residual practice of strictly following the language of operationalization when we present research results to audiences beyond the discipline. We will argue below that this language is a convention social scientists adopted because the use of precisely operationalized concepts and value neutral terms makes developing and testing hypotheses or engaging in scientific debate about research results more straightforward. While we may need to use operationalized language when we are doing research, however, there is nothing that *necessarily* restrains us from changing our language when we turn to the task of discussing the significance of our findings for practice. Our insistence upon staying within the strict confines of that language has led to a number of confusions about the relationship of research (and researchers) to practice (and practitioners).

For the purposes of the discussion below, a "practitioner" is anyone working within the criminal justice system, at any level, who has a decision-making role. When they are deliberately referring to research evidence in making such decisions, they can reasonably be said to be "using" it. If, for whatever reason, including political opposition, bureaucratic obstinacy, or simple lack of resources they are unsuccessful in implementing that decision, they have nonetheless used research to guide their efforts, no matter how disappointing the result.

Talking About What Works and How to Do It

When researchers talk about how something works, they are referring to the precise details of a causal explanation. When practitioners talk about how something works, they mean how can they actually *do* what a causal explanation implies should be the result of their actions. The bifurcation of our way of talking about what works as opposed to how to do it, which echoes the dual meaning of experience, is not typically a problem in everyday

life where we have little trouble making the translation. For example, if we wished to travel from Washington, D.C. to Boston and asked the best way to make the trip we would most likely be advised to fly. If instead we wanted to know the best way to get from Washington to Philadelphia, we might be told to take the train. So, we might inquire, what works better, train travel or air travel? The answer, of course, goes back to the "...where we are, and whither we are tending..." advice we received from Lincoln. Where you are and where you want to go determines the best way to get there.² It should also be pointed out that none of this information is going to be of any real use to you in actually traveling anywhere.

There is an entirely different answer to the question, "What is the best way to get to Philadelphia?" If the question were asked somewhere in downtown Washington, the answer would be, "Go to the closest Metro station and get on the Red Line train traveling to Union Station. At Union Station, take the escalator up to the main hall. Turn right and go to the Amtrak ticket counter and buy a ticket to Philadelphia. Follow the ticket agent's directions on where to get the right train..." There is nothing in this answer that is incompatible with the shorter one of, "Take the train." What is different is the vocabulary employed in making the transition from a "what works" language to a "how to do it" language.

In the simple case of getting from one place to another, the translation of what to do into providing the details of how exactly to do it seems straight forward. Yet if we ask, "What is the best way to deal with drug addicted offenders?" we are less likely to get a reply nearly so useful. We might be told, "Research has shown that offenders who receive substance abuse treatment are less likely to recidivate," or worse, that, "Research has shown a significant statistical association between recidivism and a self-reported history of drug use." These answers are decidedly opaque because they lapse into the peculiar language of social science explanation. This is why statements that begin with, "Research shows..." never seem to have an obvious translation into guides for action in the same way that taking trains seems to immediately imply train stations, tickets, and the like. As will be discussed below, explaining events, prescribing actions, or describing the details of how to carry them out requires diverse languages that are not necessarily incompatible nor is any one intrinsically superior to the others apart from its use.

Positivism and Social Science

Social science came of age as a profession under the influence of Positivistic approaches to measurement

that relies on a specific form of scientific nomenclature. Positivism, as it emerged during the early decades of the 20th century, was built on the central tenant that any statement of empirical fact has meaning only if it is possible to operationalize it through a precise description specifying the observational procedures for its verification. For example, Carnap (1953:47) wrote, “If we knew what it would be for a given sentence to be found true then we would know what its meaning is...Thus the meaning of a sentence is in a certain sense identical with the way we determine its truth or falsehood; and a sentence has meaning only if such a determination is possible.” (See also Bergmann and Spence, 1953). The Positivist language of science, therefore, has to be clearly distinguished from the everyday language we use to talk about the world and the many meanings we associate with its words and symbols.

Contemporary social scientists have enshrined this approach in standard methodologies that require every concept be operationalized in a way that allows it to be measured in an objective fashion. This approach is necessitated by our concept of causality. Levi (1959:331) opens his discussion of Positivism’s development by saying, “It has taken three hundred years to prepare the positivistic avalanche... [which came because]...the overwhelming successes of seventeenth-century science bequeathed to its philosophical successors the unsolved problems of the foundations of mathematics and of observational sciences.”³ The avalanche came as a delayed response to the Empiricist’s attack on the metaphysical theory of causality when Hume shifted the locus of analysis from necessary connection to constant conjunction. One of the keys to this approach is to recognize that causal explanations will always be probabilistic and conditional (Pearl, 2000).

This means hypotheses are never confirmed with certainty because they are based on observations of a correlation between variables and there is always the possibility that future observations will fail to detect the same correlation. Instead, hypotheses can only be disconfirmed when the null hypothesis is tested to determine if the observed association is significantly different from what might occur by chance (Popper, 1968). Disconfirmation as the basis for developing explanations depends on precise measurement to reduce to a minimum those occasions when measurement error or conceptual confusion results in a true hypothesis being disconfirmed or a false one confirmed.

Social science researchers have frequently argued that at this point, when they have developed and tested a casual explanation for a social phenomenon or behavior,

their job is done. Many consciously separate research as objective scientific inquiry from the realm of practical policy and their language serves to reinforce this separation. Researchers, as they engage in the scientific enterprise, expect to be objective, detached, nonjudgmental, or value-free as they endeavor to unravel the complexities of any phenomena and uncover the causes of such things as drug addiction, violence, and other criminal behavior. What to do with the knowledge—how to formulate criminal justice policy or develop intervention programs—is not a traditional part of the research process following Positivistic approaches.

As discussed above, there are compelling reasons why social scientists strictly adhere to this language when they are doing research or examining its findings. Using objective, verifiable, and value-neutral terms facilitates scientific debate and helps insulate it from other pressures. So far, this is familiar ground to social scientists, but the issue can be raised whether in pursuing this program of science we have not also created, among ourselves and others, a number of confusions that unnecessarily interfere with the translation of research into applications.

Explanations and Applications in Criminal Justice

The argument here is that this state of affairs is not an inevitable consequence of an empirical science. Instead, the value of the sort of objective, observational language we favor lies in its utility in developing hypotheses, testing them, and debating research results. If, however, the value of this language rests on its usefulness for the purposes of social science research, rather than some epistemological necessity, we cannot insist on adhering to it in situations where it is less useful. When we attempt to apply the results of research to the solution of concrete problems, the limitations of the language of research become obvious. In part, the issues surrounding the applicability of research to practice becomes obscured by concerns over value-free science as opposed to the value judgments required to make policy recommendations. Social scientists have tried to keep value judgments at arms length by arguing that they are better equipped to predict what consequences will result from each of the various policy alternatives available and that their role ought to be limited to what they do best.

In the area of criminal justice research, Moore (2002) has recently articulated the argument that a clear line needs to be maintained between research results and policy prescriptions. He sees an important overlap between social science efforts to understand and explain the world and policy analysis efforts to evaluate alternative

courses of action. The connection, according to Moore, lies in the fact that the scientific method is the best way for us to understand our world and that policymakers need the results of research to estimate the consequences of their action if they are to "...act both responsibly and effectively..." (2002:33) in making policy. The relationship is a tenuous one because the fundamental goal of social science is to produce verified theories which is "... a different project than the policy makers had in mind... [because they were]...less interested in general causal description than they were in specific policy prescription" (2002:33).

Moore goes on to argue that, as useful as they may be to predict the likely outcomes of a policy, the type of generalized explanations social science produces can not guide policy because, "As a logical matter, to decide on an action, one has to have a goal or purpose, or a way of evaluating whether the world is better or worse off than before one acted" (2002:34). Science, however, "...has always said it is incapable of making value judgments...It was capable only of making estimates of consequences" (2002:34). Social scientists typically draw this line between predicting the likely results of a particular policy as opposed to *recommending* one policy over other alternatives. Concludes Moore, "What constituted an important consequence of a policy from a normative perspective was left to philosophers, to political process, or to individuals who were free to have their own views about what was valuable" (2002:34).⁴

We can juxtapose this perspective on social science and its intrinsic limits with the criticism by the contemporary Pragmatist, Rorty, who takes issue with precisely this account of social science and its uses (1982:196):

Suppose we picture the 'value free' social scientist walking up to the divide between 'fact' and 'value' and handing his predictions to policy-makers who live on the other side. They will not be of much use unless they contain some of the terms which policy-makers use among themselves. What the policy makers would like, presumably, are rich juicy predictions...When they get predictions phrased in the sterile jargon of 'quantified' social sciences ('maximizes satisfaction,' 'increases conflict,' etc.), they either tune out, or, more dangerously, begin to use the jargon in moral deliberations.

This charge levied against social science research echoes, in different terms, the often voiced complaints made by policymakers and practitioners.

Rorty goes on, however, to trace the problem to the more fundamental issue of the strategy of social science. Social scientists argue that it is the utility of their explanatory models in making predictions that is the true value of research and, incidentally, why public funding ought to be used to support it. We assume that our casual explanations imply a set of alternative policies and allow policy makers to choose among them by predicting the likely results of adopting each. Rorty challenges this logic, arguing that, "...social science assumed that a thin 'behavioristic' vocabulary..." will allow reliable predictions, but

This assumption has not panned out very well; the last fifty years of research in the social sciences have not notably increased our predictive abilities...friends of value freedom, insisting that as soon as social science finds its Galileo (who is somehow known in advance to be a behaviorist)...[we will be able to predict and]... that it is our duty to start making policy in suitably thin terms— so that our "ethics" may be "objective" and "scientifically based." For only in that way will we be able to make maximal use of all the splendid predictions which will shortly be coming our way...It is a mistake to think that when we know how to deal justly and honorably with a person or society we *thereby* know how to predict and control him or her or it, and a mistake to think that ability to predict and control is *necessarily* an aid to such dealing. (1982:197-198)

While the above passage may make some of us wince, it does address the central issue with a directness few social scientists display. In abstaining from the evaluative debate about how our research ought to be used, researchers are evading this dilemma. Policymakers tend to be co-conspirators in this evasion as they seek to relieve themselves of the full responsibility for their decisions by trying to lean on research for support of their policy choices. As Rorty noted above, the ability to predict does not necessarily tell us how to deal justly and honorably with others.

This also raises the question of degree to which research and policy in criminal justice represents a special case of the larger problem of the relationship between science and practice. The American criminal justice system exists both to protect public safety and to dispense justice to victims and offenders alike and there are many instances in which these two goals come into sharp

conflict. Whether criminological researchers aspire to be value free or not, they inevitably work in a value-laden environment and it is disingenuous for us to claim that our neutral professional language will not have value significance. These are important distinctions. Often, when critics claim social science can not be value free they are confusing issues of bias in the conduct of research with the question of the significance of its results. Our training, the transparency of the scientific method, and the use of peer review are all designed to eliminate such biases. Practicing the *profession* of research requires the capacity to view an issue objectively, based on the available evidence and holding in abeyance considerations beyond the validity and reliability of the results.

At the same time, there is very little anyone can say about human behavior, no matter how objective or neutral in tone, that does not immediately become evaluative. Discussions about issues like addiction or crime and violence assume a value significance by their nature. Assuming a posture that the manner of talking about research results, (i.e., in operationalized or probabilistic terms), somehow absents the researcher from the value significance of the results is not a tenable position. This is not to say that becoming more engaged in policy prescriptions does not lead to role conflicts for researchers. There are many researchers, however, who have successfully navigated the inherent pressures of working in applied settings, in partnerships with practitioners, and on issues which arouse strong opinions. On the other hand, researchers usually then limit their involvement in policy discussions to the presentation of their findings coached in our preferred terminology and this leaves to others the interpretation of their results and its translation into evaluative terms.

As we have argued above, this is not necessitated by the dictates of our methodology nor does it have anything to do with debates over value-free science. Researchers, while we are doing research, must be unbiased and use all of the devices, such as precise operationalization of concepts, at our disposal to maintain neutrality. After the research has been finished, however, we can take positive steps to insure that our results will be properly understood and, when attempts are made to apply it, the results we do predict will follow.

The best evidence of this is that every debate in criminology is fundamentally complicated by the political battles among policymakers who make liberal use of their own interpretations and evaluations of research findings. Policy and programs within criminal justice are not unique in being influenced by political debates, but the language of research used by social scientists often makes

it vulnerable to manipulation and obfuscation. There are many recent examples of highly contested criminal justice issues in which the argument could be made that public fear and the absence of a clear understanding of the body of relevant research conspired to produce dubious policies: popular three-strike sentencing plans, mandatory sentences for drug offenses and the special attention given crack cocaine, some domestic violence interventions, general approaches to white collar and corporate crime, and correctional innovations such as boot camps or community programs like DARE. It is unlikely that a change in communication between researchers and practitioners will eliminate all the difficulties inherent in formulating justice policy, but we might hope it would help discourage the worst abuses.

This is part of the reason that communication, in and of itself, is seldom the solution. Researchers, for instance, frequently prescribe more communication with practitioners so researchers can convince practitioners of the value of research. Less often do researchers resolve to listen better in order to appreciate the value of practice. Equally unhelpful are practitioners who complain that they must work too hard to understand difficult issues, believing that complex information ought to be reducible to easily digestible “bullets,” sound bites, or one-page summaries. Holding up “evidence-based” practice as an ideal only exacerbates the problem. Evidence is what scientists collect by observing and this reduces practitioners to simple consumers who become the passive recipients of research results. This depreciates the importance of the second aspect of experience noted above—participation in experiences with the problem and the skills that develop as a result. Any effective process of knowledge building should be a union of these two, reflecting the dual meaning of experience, to produce an “experience-based practice” based on both.

Technology Transfer in Criminal Justice

Over the last few decades, federal agencies involved in funding research have pursued various strategies to transfer the results of research into applications, but these efforts have all tended to be driven by the orientations of researchers. Various known as technology transfer, research utilization, or “diffusion theory,” they have met with mixed success, although some, like the adoption of improved agricultural practices and the application of defense- or space-related technologies, have been more effective (Simpson, 2002; Backer, 1993; Rogers, 1995). The “transfer” of the results of behavioral science to practice has always been especially problematic. In review-

ing the area of drug abuse technology, Brown and Flynn acknowledge the general problem of technology transfer between researchers and the field, writing that

The past 30 years have seen a focus on substance abuse research in association with the creation of federal agencies specifically mandated to guide the effort. While research has been well supported and largely productive, there has been increasing concern with the slow pace of adoption of the findings from that research. (2002:245)

The authors develop a detailed model focused on the mechanics of dissemination, utilization, and evaluation with federal agencies playing a central role. (See also Brown, 1995).

In this model, federal agencies, such as NIJ and NIDA, occupy a unique position between the world of research and that of practice by managing a four-stage process. In the first stage, *technology development*, there is an emphasis on involving practitioners, "...the primary consumer group..." (Brown and Flynn, 2002:??) in the selection of research topics. In the second stage, *transfer preparation*, the body of research is reviewed by the research community to decide which results are most reliable and by the practitioner community to determine which are most relevant to their needs. In the third stage, *transfer implementation*, training and technical assistance is provided to practitioners to aid them in implementation. Finally, in the *technology stabilization* stage, ongoing support to the field is provided to avoid backsliding into the old practices.

In this model, interpersonal contacts among federal agencies and the field are pivotal to facilitate the necessary cooperation and planning for successful knowledge generation and subsequent transfer to the field. In his 1995 paper, Brown listed a number of general factors that serve as impediments to technology transfers, listing relevance, timeliness, clarity, credibility, replicability, and acceptance of research findings. The general tone of this and other similar models is a kind of marketing approach in which the agencies work with researchers to sell ideas to practitioners and concentrates on barriers to the implementation of evidence-based practices, including local resistance to innovation (Lamb, Greenlick, and McCarty, 1998; Martin, Turner, and Smith, 2000). All of these approaches focus on the problem of the transmission of research to the field as opposed to its translation in converting research results into directions for practice. They are all clearly constructive attempts, but they share a unidirectional quality in which practitioners, in their di-

minished role as consumers, are only consulted regarding their preferences. A full partnership between those with evidence-based knowledge and those with participatory-based knowledge has not been built into every stage of the process.

Knowledge Production

The essential Pragmatist thesis is that there is no real difference between theory and practice, and knowledge is always explicable in terms of what it means for human action. From this perspective, thinking always begins with a problem, some doubt about how in a particular context we could act, and it ends in a belief about how we might proceed. Science may, by virtue of the rigor of its methods, claim to be quantitatively superior to everyday experience as a means of gathering evidence, but not qualitatively so. It is in the nature of research to extract generalizations from the particular and in the nature of practice to apply the general to the specific. Each represents a conceptual posture toward our experiences, the one focusing on their common characteristics and the other on their distinctive qualities. The science of social research has a set of highly-developed methods to produce generalized findings, but there is no sister science of social practice equipped with its own methods to apply them to particular problems. This is why, when research names a policy or program to the list of "what works," we are only half done, because it begs the question of *how* it worked in the practical sense.

This implies that knowledge generation is a process that oscillates between an emphasis on experience in the first sense (observational to generate evidence) and experience in the second sense (participatory and skill-producing). The solution, however, is not to turn practitioners into researchers or vice versa, it is to develop effective partnerships between them so each can bring their respective strengths to the process through a constructive division of labor. Understanding the dual sense in which we use the idea of experience suggests a broader strategy toward the process that produces new knowledge. The *accumulation* of research results often fails to produce *cumulative* knowledge useful in applied settings because our efforts are routinely truncated, as when they end with the generation of only a research report.

This is because there are a number of interdependent processes that make up the structure of knowledge production and each process has a unique mix of roles between practitioners in the criminal justice system and the criminal justice research community. For the purposes of this discussion, knowledge building can be separated into

stages that define an arc from the definition of a problem through to the application of a solution. The analogy most often used to describe this process is “bridging the gap” between research and practice. The process becomes essentially an engineering problem in which information is collected, made sense of, a prototype is constructed and tested, others are taught how to recreate similar structures, and the solution is applied to similar concrete situations.

In the early stages, researchers and their methods dominate, but practitioners are not relegated to a passive role. Comparable to other models of technology transfer, this process begins with knowledge *Generation*, the familiar process by which basic and applied scientific research on crime and the criminal justice system is conducted. In this model however, genuine collaborative relationships are required and include active participation in research projects, rather than advisory roles, and equal representation on peer panels. In the second stage, knowledge *Organization* occurs, again through collaborative efforts, by which a body of empirical evidence in a field is made meaningful by placing it within a coherent conceptual scheme and relating it to practice in a manner that suggests a model policy or program. This differs from the recent attempts to synthesize the body of research in a particular area, a process that is controlled and guided by researchers in an attempt to package the results in a more digestible form. This tends to perpetuate the basic opacity of the results because they are still presented in the typical language of social science explanation.

In the third phase, knowledge *Testing*, a model policy or program is implemented in a field test designed both to demonstrate the feasibility of the model and to evaluate its effectiveness. In practice, this process involves iterative steps between basic research or evaluations and field tests. The National Institute of Justice has experimented over the last decade with a strategy of carrying out field tests as joint efforts between the Institute, evaluation researchers, and demonstration sites. There are a number of difficulties involved in the successful execution of such a strategy that are discussed elsewhere, but the central goal of such projects is the attempt to achieve a union between the two aspects of experience as a vehicle for the translation of basic research findings into broader applications (Innes, 2003).

As this process continues, the relative positions of the research community and the practitioner community reverse. Knowledge *Translation* is the process in which the accumulated organized body of research and the results of its testing are translated for a broader audience in the most accessible language and formats. Practitioners assume the central role in articulating the body of knowl-

edge into “how-to language” by developing program plans or manuals and providing training or technical assistance. When knowledge *Application* occurs, control over the process has passed into the hands of the practitioner community as a policy or program is implemented to scale in an organization and incorporated into its routine operations. While researchers play a central role in generating research results, they become increasingly peripheral to the processes of organizing or disseminating those results beyond the research community as practitioners assume greater responsibility.

In a real sense, the relationship between researchers and practitioners tracks the transition from observational experience to participatory experience as their essential roles are exchanged. At the beginning of the process, when basic and applied research is conducted, researchers take the initiative and the practitioners serve as a “reality check” to their theories and explanations. At the back end, it is the practitioners who take the lead in translating and applying knowledge, while the researchers rain on their parade by pointing out what is not based on the evidence or not working. The keystone, to continue with the bridge analogy, is field testing, when the roles are most nearly balanced and the collaboration most equal.

What to Do and How to Do It

The discussion above has contrasted the Pragmatist perspective on the scientific enterprise with social science methodology following a more Positivist model. That model, with its reliance on operationalized concepts and probabilistic statements of casual inference, has clearly been successful in advancing empirical research. The utility of this approach notwithstanding, its use by social scientists has led to a preference for a language that is grounded directly on an observational foundation. We have argued that in practice this has produced an empiricism biased toward one side of the dual meanings of “experience” as both the evidence we gain from observation of and the skills we develop from participation with the world around us. The exclusive emphasis on the first aspect of experience results in the gap between it and the second one that shows up in the difficulty of translating the findings of research into practice.

Social scientists have frequently sought to restrict themselves to their central concern of developing and testing explanations. They have tended to assume that their contribution to solving social problems is the understanding of its nature which, in turn, makes it possible to predict the likely effects of any proposed solution. That promise has not been realized to date and the ability of the

social sciences to make predictions in nontrivial cases has proven limited. The solution to the dilemma, in our view, is for the research community to reduce its isolation and recognize the need for the full involvement of the practitioner community in our work. This will mean accepting that the process of knowledge production must embrace both aspects of experience and that means there must be a division of labor between researchers and practitioners through each stage of the process.

Although we have discussed the issue in terms of differing languages, one an evidence-based “what works” language and the other a skill-based “how to” language, we have also argued that the gap between research and practice is not just a communication problem. More than anything else, the problem is an example of professional culture clash. If there is a defining difference between practitioners and researchers it is that the former love success too much and are always impatient to claim it. For researchers, it is really the thrill of failure that attracts them because it sets up the *next* research problem and this leads them to focus on critical questions awaiting investigation rather than those already answered. The solution is not to teach practitioners how to think like researchers, nor should it be for researchers to abandon their hard won methods and practices simply to make their results more digestible or palatable to practitioners. Instead, the solution lies in the acceptance by both the research and practitioner communities of the hard work involved in sharing the control and responsibility of the entire process of building new knowledge in the service of the public good.

Endnotes

1. The reader familiar with Gottfredson and Hirschi’s 1990 book will recall that it contains a lengthy critique of “positivistic” theories of crime. Unfortunately, as Akers has pointed out in his original review of the book, Gottfredson and Hirschi misuse the term when they contrast their version of control theory with other perspectives on the grounds that the others are Positivist while their theory is not. Akers notes that, “Positivism is supposed to be deterministic... [and] is quantitatively oriented, emphasizes measurability, utilizes statistical analysis, and measures variables with objective, empirical indicators,” and points out that their theory is thus as positivistic as any and more so than many. (See also Akers, 1991 and Akers, 1997.)

2. The same argument would apply to any questions about the cost effectiveness of flight versus rail travel; it

may be cheaper to take the train to Philadelphia than it is the fly to Boston, but that is no help if you are determined to get to Boston. In fact, the easiest and cheapest way to get either place is probably to steal a car and force someone to drive you there, but that option is unlikely to occur to most people who are not criminals, or at least criminologists.

3. Levi’s succinct account of the essential issue merits quoting at length:

The metaphysical theory of causality assumes that inductive generalization is possible because there is an order of nature expressing real relations which hold between the real things which compose the natural world. Such identities of patterns as disclose themselves in these mutual relations are the laws of nature, and from these uniformities or necessary connections we are entitled to trust in inductive inference. But Hume shifted the locus of his analysis from the necessary connection of things to the connections of ideas in the mind. He finds that although between such ideas there is a ‘constant conjunction’, there is no necessary connection, and that constant conjunction itself is a habit of belief and not necessity imposed by the texture of nature’s connectedness. Thus originates the positivistic doctrine of scientific ‘explanation’. Laws of nature are the observed identities of pattern disclosed in a series of comparative observations, but the pattern is a mere *description* wholly uninterpreted and without metaphysical implication. The doctrine is attractively simple, and it gives to scientific methodology imperatives admirable in their clarity: ‘Keep to things observed’ and ‘Aim at descriptive simplicity.’ But it leaves induction suspended in mid-air, cut off from its roots in the natural world. (1959:334)

4. Moore notes that evaluations of programs comes closest to unifying the methods of science with those of policy makers, because the goals of the program or policy has been set a priori by the policy makers. He makes clear, however, that such evaluations “...always seemed like a second rate, applied activity...” (2002:35) because it only shows that a how a particular program works in a particular place and leaves the underlying causal mechanisms hidden.

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