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# Research for the front lines

David Thacher*

*Public Policy and Urban Planning, University of Michigan, MI, USA*

It is a truism in the sociology of science that scientific knowledge bears the imprint of particular perspectives, interests, and values. In social science, it is especially common to find that research serves the needs of managers and policymakers better than it serves the needs of front-line workers. This paper analyzes the traces of that tendency in police research. By examining three features of front-line police work (the need to improve programs rather than assess them, the need to attend to an enormous number of situational details, and the need to cope with ambiguous and contradictory goals), I argue that common approaches to police research address managerial and policy concerns better than line officer concerns. To help rectify this imbalance, I discuss three variations on an alternative research strategy that deserves more emphasis in policy-oriented police research—one grounded in concrete case study description and analysis that often eschews causal generalizations.

**Keywords:** philosophy of science; qualitative methods; practice

It is a truism in the sociology of science that all knowledge bears the imprint of particular perspectives, interests, and values, so that any particular scientific program serves the priorities of some groups rather than others. From a normative viewpoint it is not even clear that this influence is always nefarious. Aristotelians once valorized a disinterested approach to scientific inquiry, but the idea that knowledge *should* strive to advance specific human interests is at least as old as Francis Bacon, who insisted that 'it is by witness of works, rather than by logic or even observation, that truth is revealed and established.' As Bacon himself went on to note: 'It follows that the improvement of man's mind and the improvement of his lot are one and the same thing' (Farrington 1948: 93). From this perspective, scientists' views about what it means to 'improve man's lot'—and about whose lot in particular they should try to improve—unavoidably influence the scientific agenda.

To claim a connection between truth and (non-cognitive) values is faintly scandalous for the obvious reasons, but at the general level I have invoked this claim it is impossible to deny. At minimum, no one doubts that the questions scientists ask are legitimately shaped by human interests, so which truths they discover (if not the very fact that those truths are true) depends on the interests to which they and those who influence their work are committed (e.g., Taylor 1985). That conclusion is so undeniable that the distinguished and decidedly non-radical philosopher of science William Newton-Smith (1984) dubbed it 'Boring Interest Thesis 1', and it is amply illustrated by agricultural and health research that serves first-world needs more effectively than third-world needs (e.g., by developing cures for diseases associated

*Corresponding author. Email: dthacher@umich.edu

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with obesity and old age, and by emphasizing research about the impact that expensive capital inputs, such as mechanization, chemical fertilizers, and pesticides, have on agricultural yields) (Tiles 1987: 295).

However obvious, the point always bears repeating in a context like this one because it raises two further issues that deserve attention. First, while the bare fact that interests shape the development of scientific knowledge is obvious, the specific ways in which they do so can be subtle, so in any particular context we should investigate those influences with care. Second, the fact that scientific research typically serves some interests better than others implies that we should evaluate any scientific research agenda partly based on whether it addresses the *right* mix of human interests. That question is a matter of ethics and political theory. It raises questions about the claim that different groups and interests ought to have on social development—specifically, on the development of relevant knowledge—or, put differently, it raises questions about the extent to which there is a social interest in satisfying the knowledge interests that each group has.

This paper examines these two issues in the context of police research in order to ask how such research can and should serve line officer priorities, and what exactly that imperative would mean. My thesis is that the dominant approach to police research in the US today embodies the interests and perspectives of police management and unduly neglects the interests and perspective of line officers. To make this case, I will describe three distinctive features of the line officer perspective, analyze the kind of research that is relevant to each, and note the limitations of dominant research models in this regard.

## Research for policy or for practice?

Policing is hardly the only field where research tends to neglect the needs of front-line practitioners. David Laws and Martin Rein describe this tendency in an analysis of knowledge for policy and practice that draws its major examples from education.

Laws and Rein (1997) begin by sketching a three-part model of the relationship between knowledge and action. Many analyses of usable knowledge rely on a dyadic framework—one that distinguishes between knowledge and action and asks how the former can serve the latter—but Laws and Rein argue that in fact at least three distinct perspectives can be identified: the perspective of front-line practitioners who deliver services directly to clients; the perspective of policymakers and upper managers who define agency goals through their interactions with the political process; and the perspective of researchers who attempt to develop knowledge that is both useful and generalizable (1997: 53).

In Laws and Rein's account, the 'practice' leg of the triad is particularly susceptible to neglect. They illustrate that pattern with a case study of Michigan's initiative to reform the way reading was taught and evaluated in the mid-1980s. On their account, the reform effort ultimately had at most a modest effect on teaching practice, and they attribute that failure partly to the neglect of the front-line practitioner perspective in the research program the reform was based on. Teachers rarely met with researchers and had no opportunity to discuss their practice problems with them. Instead, they received brief handouts of research findings or listened to summaries of the findings from administrators.

Policing scholars will undoubtedly find this basic pattern familiar, inasmuch as police research seems to be developed in interaction with policymakers and upper management. Examples from many other fields might be cited. What drives this common pattern, in which the research-policy dyad crowds out the perspective of practice?

### *Program assessment and program improvement*

Laws and Rein themselves do not answer this question directly, but they suggest one aspect of a possible answer in their discussion of evaluation research. On their view, evaluation research as currently practiced emphasizes questions that address policy concerns more readily than practice concerns:

> Evaluation research is sponsored by oversight agencies asking a different question than that of the practitioners they are evaluating. These agencies face policy concerns about how to allocate scarce resources among different strategies for dealing with a problem. This is very different from the practice question of how to make a particular strategy work. Where practice asks how to act and how best to fix a program within the limits of what you know, what you have, and what you can get agreement on, policy asks whether it is worth supporting the program at all. (Laws and Rein 1997: 53)

Thus, for an officer assigned to foot patrol, the major question is not whether foot patrol works but how best to carry it out. The same is true for an officer assigned to directed patrol, community engagement, problem solving, hot spots gun policing, order maintenance, traffic duty, or any other policing task.

This front-line interest in refining and expanding the possibilities of practice contributes an essential ingredient of intelligent policing. Where the policy interest described by Laws and Rein motivates rational choice among existing alternatives, the practice interest motivates the generation of new alternatives to evaluate in the first place. The essential role of this second task in truly rational choice is often overlooked, but Robert Nozick has described it lucidly:

> A choice of action is made among alternatives. Better choosing among the existing alternatives is one way to improve the results. Another way is to widen the range of alternatives to include promising new ones. An imaginative construction of a new alternative, heretofore not thought of, might be what makes the greatest improvement possible. . . . In some situations, much more might be gained by generating new alternatives and choosing among them roughly than by choosing finely and with perfect discrimination among the existing alternatives only. The second best among the new alternatives might be far superior to the very best among the old ones. It is as important to cultivate the relevant imaginative powers as to sharpen the discriminative ones. Without the exploration and testing of other imaginative possibilities, the procedures of rationality, by focusing only upon the *given* alternatives, will be myopic. (1993: 173)

In this respect, Laws and Rein's 'practice' interest contributes an essential ingredient to rational policing. It cannot be reduced to or replaced by the policy interest in determining whether to support a strategy at all.

The 'what works' movement in police research, although it has had many worthwhile effects, tends to obscure this distinction because it emphasizes only one

aspect of instrumental rationality—the evaluation of existing alternatives to determine their relative merits. In particular, the randomized field experiments that some policing scholars view as the only truly compelling source of knowledge often provide no information that can be used to improve practice, and as a result they contribute nothing to Nozick's second prong of instrumental rationality. Enthusiasts for randomized controlled trials sometimes dismiss this kind of concern as anti-scientific sentimentality, but in doing so they display their ignorance of the best scientific literature about field experiments. Economist James Heckman, who won the Nobel Prize for his contributions to econometric methodology, has repeatedly stressed the weakness of evaluation practice I am describing. For example, in joint work with economist Jeffrey Smith, he wrote:

> Policymakers often do not care solely about whether or not a particular program "works" in the sense of having benefits that exceed its costs. When programs fail, it is important to understand why they do not work. Without this information, which is not available from typical black-box experimental analyses, the only alternative open to politicians is to eliminate one program completely and start fresh with another. (Heckman and Smith 1995: 94)

Heckman and Smith conclude with a harsh assessment of field experiment practice: 'The end result of a research program based on experiments is just a list of programs that 'work' and 'don't work', but no understanding of why they succeed or fail' (1995: 108). Here I mean to emphasize that the negative consequences of this gap bear especially on front-line practitioners. In a division of labor that assigns the 'assessment' prong of instrumental rationality to policymakers and the 'invention' prong to practitioners, black-box evaluations serve policy needs but not practice needs.

It is not inevitable that evaluation research will address policy questions better than practice questions. To fill the knowledge gaps they identify, Heckman and Smith advocate for theoretically grounded experimental and non-experimental quantitative analyses that attempt to estimate invariant structural models. A complementary approach, particularly relevant for fields that lack the kind of formal theory that invariant structural models demand, relies on qualitative observation and analysis. That approach has been emphasized by the Manpower Demonstration Research Corporation—arguably the leading institutional expert on randomized social experiments in the world—which now incorporates a qualitative component in its policy evaluations as a matter of course (Sherwood and Doolittle 2003). These descriptive examinations of program operations not only make it possible to assess program components at a more fine-grained level; by providing detailed descriptions of program operations, they also contribute to the grounded use of imagination to identify new possibilities of practice (cf. Mead 2004; Laws and Rein 1997: 52). In that way, they provide information that practitioners can use to debug and improve ineffective programs.

In keeping with the points made at the outset of this paper, the problem Laws and Rein identify is not that common approaches to evaluation research generate incorrect conclusions. It is that only a subset of possible questions receives attention in the first place—the subset that addresses policy concerns rather the subset that addresses practice concerns (or more precisely the aspects of policy and practice concerns that I have just highlighted). An alternative approach to program

evaluation would supplement attention to policy questions about the impact of a program in its current manifestation with attention to practice questions about how that program might be improved. Such an approach would treat programs as continually evolving, reshaping them in response to emerging assessments of program components and of specific actions taken in the program's name. An important version of this approach relies on case description and on reflection on program operations, which can not only assess the effectiveness of existing programs but also help to identify and imagine new programs. In this way evaluative questions about what works (the policy question) are supplemented by diagnostic questions about how to make programs work better (the practice question). These contrasting approaches to program evaluation might be called *evaluation-as-assessment* and *evaluation-as-debugging*.[1]

The desire to evaluate the effectiveness of existing programs does not simply ignore this second task: It may undermine it, in at least two ways. First, excessive zeal for evaluation-as-assessment can make efforts spent to use research to help generate new alternatives seem wasteful. 'It is too easy, and tempting, for rationality to become a device that views the imaginative generation and testing of new possibilities as irrational,' Nozick observes. 'The process of exploring new opportunities will be imperfect and apparently wasteful; many of the possibilities explored will turn out to be useless. Yet rationality must be tolerant of this and not demand guarantees of success in advance' (1993: 173). Second, excessive zeal for evaluation-as-assessment can stifle innovation by encouraging premature evaluations of the minor modifications made in the process of debugging a program. Because many policing scholars draw an analogy with medicine to analyze the form their research should take, it may be helpful to consider an example of this danger from surgery—arguably a better field to compare with policing than the field of pharmaceutical research (the most commonly discussed example of medical research in policing literature) because policing and surgery both rely on complex and hard-to-standardize interventions.[2] Researchers in surgery have repeatedly raised concerns that premature evaluation will stifle innovation; as one group explained:

> RCTs [randomized controlled trials] consume substantial resources and are therefore not justified for some questions about small modifications to treatments. Surgical technique typically progresses via such modifications, which individually are unlikely to produce detectable benefits, but which collectively may do so. During the historical progression through hand washing via the use of antiseptics to the aseptic surgical environment, the change in morbidity from surgical infection was huge, but the increment with each step was small enough to allow persistent scepticism. Small randomised trials of components of this progression showed no benefit. If a positive RCT were required before adopting each small improvement, most would be rejected, and progress would be slowed. RCTs are appropriate where a clear, clinically important choice exists between contrasting alternatives. For smaller changes, an industrial paradigm may be needed. (McCulloch et al. 2002: 1449, citations omitted)[3]

Similar concerns have been raised about complex policing innovations such as problem-oriented policing and community policing, as scholars have warned that premature attempts to 'evaluate' these reforms may stifle attempts to debug and further develop them (e.g., Kennedy and Moore 1995).

In these respects, the 'industrial paradigm' (McCulloch et al. 2002: 1449) embodied in evaluation-as-debugging provides a necessary complement to the 'what works' judgments offered by evaluation-as-assessment. Note that the point here is not simply Donald Campbell's injunction to 'evaluate no program until it is proud'; it is that evaluation itself can be used as a tool to help make a program more worthy of pride. However, it can only do that if it goes beyond black-box experimental designs.

This analysis suggests one important and readily recognizable way in which practice concerns differ from policy concerns, and it is clear how dominant approaches to policy research have a greater affinity for the latter than the former. At least two other differences between policy concerns and practice concerns reinforce this tendency to bring research and policy together at the expense of practice.

### *Situated knowledge and universal knowledge*

Epistemologists (particularly feminist philosophers of science) have stressed a distinction between situated knowledge and universal knowledge. Very roughly, one aspect of the distinction involves the difference between features of the world for which knowledge is closely tied to the identity, situation, and background understandings of the knower and those that can be discerned in the same way by anyone (Diamond 1991). Another involves the difference between contextualized knowledge of particular events and universal generalizations not bound by the conditions that prevail in particular times and places (Toulmin 1990).

The second kind of knowledge dominates contemporary scientific practice. Indeed, many scientists and philosophers insist that anonymous, generalizable knowledge is the *only* type of knowledge that can truly be called scientific. This approach to scientific inquiry emphasizes abstract information gathered using standardized processes that do not depend on the identity, background abilities, or sentiments of the investigator. It is anonymous in the sense that it is accessible to, and it has the same meaning for, any observer (Nagel 1974),[4] and it is generalizable in the sense that it pursues timeless truths that hold across a wide range of contexts.

For present purposes, the important point is that the existence of such knowledge may serve the interests of some groups in society better than others. Granted the existence of impersonal, generalizable knowledge—abstract generalizations that can be stated and understood without regard to the personal characteristics or background understandings of the speaker or hearer—we should still ask: 'For what groups is it useful or desirable that there be such impersonal, anonymous, generalizable knowledge?' Cora Diamond illustrated this concern with a playful distinction between tourists and natives:

> Take what one might call tourist knowledge: the capacity to reply to the tourist who wants to know, what is in the soup? Is this handmade? What time does the tourbus leave? Where is the post office? And how much is airmail postage to the United States? In Northern European countries that knowledge is available in highly impersonal forms: the practices of impersonal knowledge there are all that "we" might want. As we tourists travel south, things change, the practices of impersonal knowledge-accumulation are less developed. (1991: 1011)

Tourist knowledge is authentically objective knowledge, but such knowledge serves the needs of some groups better than others: 'What the right answer is to 'Where is the post office?' is independent of the particular person who answers, but there being practice in handling such questions is useful to tourists rather than natives' (1991: 1011).

Compared with situated knowledge, universal knowledge typically serves policy interests better than practice interests. A full defense of this thesis would require more detailed analyses of the concept of situated knowledge and of the nature of practice than I can undertake here. Nonetheless, in the case of policing, several analyses of patrol officer expertise and the conflict between street cops and management cops suggest its plausibility.

Bittner, in particular, argued that the knowledge base that meets the demands of front-line police work must take a situational form: 'Policing is not technical in the sense in which engineering is,' he wrote. 'Instead, it makes great demands on experience and judgment. Experience is accumulated knowledge on which is based the understanding of practical necessities and possibilities. This understanding guides judgment' (1982: 11). Elsewhere he argued that the importance of detailed experience and situated judgment (as opposed to generalizable rules) results from the nature of the situations that police contend with. In those situations, interventions that fail to take account of complex situational details in favor of generalized 'by-the-book' responses often backfire (1990: 174–175).

Similarly, in *The Two Cultures of Policing* Reuss-Ianni concluded that officers' emphasis on situation-specific knowledge makes them skeptical of the decontextualized, abstract, universalistic information that Diamond associates with scientific analysis. For example: 'Routine data collected, including activity logs, arrest and crime coding sheets, and statistics on response time, provide standardized means of gathering information on the job. But field personnel frequently dismiss the data and the findings derived from them as irrelevant to their problems' (Reuss-Ianni 1983: 19). Police managers, by contrast, place great value on such information (ibid.: 18–19).[5]

We have a primitive understanding of what is involved in the kind of context-specific, situated knowledge that officers value. As a result, the very fact that it *is* knowledge is easily missed. Bittner warned about this problem four decades ago:

> What the seasoned patrolman means . . . in saying that he 'plays by ear' is that he is making his decisions while being attuned to the realities of complex situations about which he has immensely detailed knowledge. This studied aspect of peacekeeping generally is not made explicit, nor is the tyro or the outsider made aware of it. Quite to the contrary, the ability to discharge the duties associated with keeping the peace is viewed as a reflection of an innate talent of 'getting along with people'. Thus, the same demands are made of barely initiated officers as are made of experienced practitioners. . . . This leads to inevitable frustrations. . . . The license of discretionary freedom and the expectation of success under conditions of autonomy, without any indication that the work of the successful craftsman is based on an acquired preparedness for the task, is ready-made for failure and malpractice. Moreover, it leads to slipshod practices of patrol that also infect the standards of the careful craftsman. (1967: 715)

He and others raised particular concerns about the fact that officers' experiential knowledge is often not shared (Goldstein 1977: 150; Muir 1977).

It can appear to be unshareable. Indeed, the transformation of knowledge into more abstract, propositional forms arose partly to facilitate its dissemination through education (Toulmin 1982). However, we do have models of other kinds of knowledge—knowledge that contributes to perception and judgment rather than to logical deduction—and yet can be shared.

One such model is knowledge about chess. As Adriaan de Groot and others have shown, the most striking difference between chess masters and chess novices is the large database of chess scenarios they have stored in their memories (De Groot 1965, 1966; Chase and Simon 1973). Chess masters and novices both select their next move by considering the possible moves open to them and examining what implications each possibility will have, and as they make these calculations, masters and novices consider roughly the same number of possible moves; they also look about an equal number of moves ahead in the game. Chess masters, however, draw from a much wider repertoire of scenarios that they have learned through both experience and study (e.g., through the study of sample chess games published in weekly newspapers). Evidence of this advantage is vividly illustrated in the fact that chess masters do far better than novices at remembering the placement of chess pieces on a board stopped in mid-game but equally poorly at remembering randomly placed pieces—i.e., in a configuration that might never arise in actual play.

In short, the key advantage that chess masters apparently have over novices is their familiarity with an extremely wide repertoire of real situations similar or identical to those that they may encounter in future play. This knowledge seems to provide them with an enormous advantage over chess amateurs even though it has the form of familiarity with particular situations rather than the form of universal generalizations. Herbert Simon (1979) has argued that many kinds of professional expertise (and human expertise more generally) may resemble expertise in chess in this respect: They all arise out of extensive familiarity with the particular situations that are encountered in practice as much as (or more than) they require any generalizable problem-solving skills, generalizations, or principles. Scholars in a number of specific professional fields have made similar arguments, sometimes drawing explicitly from this line of cognitive science research (e.g., Behn 1987; Benner 1984).

More needs to be said about the nature, logic, and uses of this kind of knowledge, but I hope that it is clear even from this rough sketch how this conception of expertise relates to Bittner's analysis of police work. Indeed, Bittner himself repeatedly suggested the relevance of case-oriented analogical reasoning to policing. For example, in his most extensive analysis of policing practice, he concluded: 'Although [patrol officer] interest is directed to the accumulation of factually descriptive information, as opposed to the desire to achieve a theoretically abstract understanding, the ulterior objective is to be generally knowledgeable rather than merely being factually informed. That is, patrolmen seek to be sufficiently enlightened to be able to connect the yet unknown with the known through extrapolation and analogy' (1990: 9). De Groot and Simon's research provides conceptual and empirical support for this interpretation of practical understanding.

If police expertise consists at least partly in a familiarity with a wide range of practical situations that they are likely to encounter in practice, then police research

has good reason to value the development of detailed, contextualized case studies of well-chosen examples of those practical situations (Flyvbjerg 2001). Such vicarious experience apparently has benefits even when the cases are not explicitly used to identify generalizations about the consequences an action will have. This kind of knowledge is situated in the sense that its benefits, its significance, and even its meaning may only be accessible for those who have an extensive network of background understandings and powers of judgment that cannot be (or at least have not been) articulated.[6] It contributes to the development of professional expertise in the same way that real and vicarious experience of particular chess games contributes to expertise in chess. It meets an important need among front-line practitioners that the abstract and decontextualized knowledge that dominates police research today cannot fill.

### *The ambiguity of practical purpose*

It is a common phenomenon of organizational life that formal organizational goals often provide incomplete guidance for front-line practitioners. That is particularly true for public agencies, such as police departments (Wilson 1968, 1989). At the highest levels, removed from the insistent demands that concrete situations place on service delivery, managers and political overseers may be able to develop reasonably clear statements of organizational priorities. By contrast the concrete situations we encounter when we attempt to turn purpose into action tend to confound our understanding of the priorities we thought we had (Millgram 1997). As a result, we may be able to develop a clear statement about an organization's mission when we reflect on its tasks at a relatively abstract and stylized level, but once we become concrete, ambiguity and conflict are hard to avoid. Clarity of purpose is a particularly scarce resource at the front lines of an organization.

The first wave of contemporary police research that began with the American Bar Foundation studies of policing practice can be understood in this light: Concrete reports of what front-line officers actually did flew in the face of the clear and commonsensical view that police departments are responsible for crime control and/or law enforcement. Egon Bittner (1967, 1970, 1974, 1982) recognized this insight more clearly than anyone: Arguing that abstract statements about the police mandate (he called them 'specious programmatic idealizations') could not provide sufficient guidance for policing practice, he devoted the major thrust of his work to a sophisticated attempt to develop an alternative conception of the police mandate—one formulated at a lower level of abstraction based on a concrete examination of the situations that police officers actually encountered.[7] In this case, a close look at actual police work discovered a level of complexity in the police mandate that could not easily be captured in clear and simple statements about institutional goals.[8] Since this time it has become commonplace to observe that the police mandate is complex and ambiguous. My point here is that because this complexity and ambiguity becomes most pressing at the point of action, it is particularly salient for front-line officers.

Elsewhere I have discussed the implications of this kind of complexity for the form that police research should take (Thacher 2001b, 2005). Briefly, recent police research has been dominated by efforts to develop instrumental knowledge, or knowledge about the best means to a given end. Such knowledge can be very useful

in situations where we have a clear and focused sense of our priorities, since in that case all that matters is which means is most effective at promoting one clear and overriding end. By contrast when our goals are ambiguous, changing, multiple, and conflicting, the most vexing questions we face are not about which means will best realize a clear and given end but about how exactly each of many ambiguous ends is relevant in a particular situation, and which end deserves how much priority over the others. One model of scholarship that can shed light on such questions combines ethnographic description with moral analysis in order to develop something like a case ethics approach to policing practice. Such research develops detailed case studies of difficult practical dilemmas, examines our considered judgments about how police should handle those dilemmas, and compares those judgments to our convictions about other cases. That process of case description and analysis allows us to refine our normative understanding of police work. The focus on concrete cases rather than abstract analysis of ethical principles is valuable because we often have more confidence in concrete judgments about what should be done in a particular situation than in the abstract principles that purportedly justify them.[9]

I have elaborated on the types of research I have in mind elsewhere (Thacher 2001b, 2004a, 2006), and I have tried to illustrate it in analyses of order maintenance policing (2004b) and community engagement (2001a). Here I mean to extend that argument by suggesting that the case ethics approach I have outlined has special relevance to front-line policing. Insofar as police managers and elected officials are particularly concerned with the relatively clear goals that their agencies are formally committed to, instrumental knowledge may serve (what they take to be) their purposes well. But, insofar as front-line officers confront more normative ambiguity—an ambiguity pressed on them by the continually surprising situations they encounter, the concrete demands of which cannot be entirely captured in general statements of goals—it may be impossible to meet their needs without incorporating the alternative approach I have suggested.

## Conclusion

Science serves human interests, and human interests are diverse, so we cannot content ourselves with a single approach to science. Divergent interests exist even within a single institution like policing (Reuss-Ianni 1983), and although some of those differences may reflect error or bad faith, there is no reason to expect that they all do. As a result, we cannot content ourselves with a single approach to police research.

I have especially suggested that the classically scientific approach to scholarship that has recently come to dominate police research may be more relevant to management concerns than officer concerns (or, more precisely, to the specific officer concerns that I have highlighted in this paper). To complement that approach, I have discussed three variations on an alternative research theme—one grounded in concrete case study description and analysis of various sorts that often eschews causal generalizations.

I have referenced more extended descriptions of these approaches throughout this paper, but two points are worth adding here. First, although the approaches I have discussed draw from ethnographic and interview-based techniques, only some varieties of ethnographic and case study research will meet the needs I have

discussed. In order to develop the kind of situated knowledge that can successfully inform front-line practice, ethnographic description cannot remain at the level of generalized description, and research interviews cannot restrict their attention to generalized attitudes. Instead researchers must strive for concreteness, depicting particular people in particular places taking particular actions in response to particular situations (Flyvbjerg 2001). Second, description and value-neutral interpretation is not enough. To help resolve the normative ambiguity that stands out as a major challenge for front-line practice, police research needs to engage the normative questions posed by the situations that research subjects confront. One way to do that is to use case study material to test and refine overarching normative theories (Thacher 2001b, 2006). Another is to develop analogical comparisons with other cases in the literature (Thacher 2004a).

The research approach I have advocated draws from substantially different methodological and epistemological traditions compared with those that dominate policing scholarship today. Organizationally, however, it departs only modestly from existing research practice: In principle, the approaches I have described can be carried out by independent researchers who study police as outside experts. In practice, however, researcher-led scholarship may eventually run up against fundamental limitations, since situated knowers presumably have unique access to some kinds of situated knowledge.[10] In that respect, the participatory action model of research that has been recommended and practiced by several policing scholars (e.g., Goldstein 1990; Bradley et al. 2006; Wood et al. 2007; Wuestewald and Steinheider 2007) holds considerable promise as a model of research for the front lines.

I do not mean that the kind of research I have advocated as is all that officers need. I have identified three aspects of the practice problems that officers must solve that make this kind of knowledge valuable, but my picture is undoubtedly partial; undoubtedly there exist other characteristics of front-line practice problems that are better addressed by other kinds of knowledge. Here I have tried to focus on a few important aspects of the practice perspective that are not well served by dominant research strategies in order to suggest how police research might better support bottom-up reform. Undoubtedly these observations apply to many other policy fields as well, since the distinction between practice needs and policy needs that I have examined is hardly unique to policing.

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## Notes

1. I have of course not spelled out the details of either conception of evaluation here. Discussions relevant to the second approach include Schön (1997), Mead (2004), and Lennon and Corbett (2003).
2. In this connection it is worth noting that the use of randomized controlled trials to evaluate new surgical innovations is controversial and began to decline over two decades ago (Pollock 1993; Panesar et al. 2006).

3. Lawrence Sherman, the leading advocate of the 'what works' paradigm in police research, has discussed this same episode in the history of surgery, but his account is potentially misleading. Sherman (1992: 58) argues the lack of RCT evidence about antiseptic surgical procedures meant that many surgeons were too slow to adopt these lifesaving techniques, but McCulloch and his coauthors explain how overuse of RCTs would also have prevented the adoption of lifesaving techniques.
4. Taken literally no knowledge qualifies under this definition: Since Kant every serious philosopher of science has recognized that we grasp propositions against a background of assumptions and concepts that may vary from person to person. Nevertheless, the notion of anonymous knowledge is clear enough as a relative matter; as Nagel notes, the scientific aspiration is to develop progressively *more* impartial knowledge. (Nagel's own point is that the aspiration cannot succeed even in principle for features of the world that have a subjective realization, such as consciousness.)
5. If it needs to be said, I do not intend to romanticize officer knowledge preferences, which have their own drawbacks (q.v. Reuss-Ianni 1983: 18). I mean to argue that those preferences emphasize a valid and important form of knowledge, not that they exhaust all such forms.
6. As a chess novice, I scratch my head when I read the chess column in the weekly paper—and not because any specialized terminology is being used.
7. Bittner apparently vacillated between normative and descriptive conceptions of his task. Compare his statement of his objectives in 'The Police on Skid Row' (which he described as an attempt to 'disclose the conception of the mandate to which police feel summoned', insisting that 'it was entirely outside the scope of the presentation to review the merits of this conception') to that of *The Functions of the Police in Modern Society* ('the task we have set ourselves it to elucidate the role of the police in modern society by reviewing the exigencies located in practical reality which give rise to police responses, *and* by attempting to relate the actual routines of response to the aspirations of a democratic polity') (Bittner 1967: 715, 1970: 5, emphasis in original). For an argument that empirical research can in fact contribute to normative understanding—notwithstanding the maxim that it is impossible (unethical?) to derive ought from is, which Bittner himself invoked—see Thacher (2006).
8. Of course Bittner's own conception (especially Bittner 1974) was nuanced and influential. Important critiques can be found in Kleinig (1996) and Ericson and Haggerty (1997), though each of these analyses presents their own difficulties.
9. As Henry Richardson put it, 'Among moral judgments that stand most firm are some quite concrete, even particular ones—about the evil of the Holocaust and of the My Lai massacre, about the admirable character of Mother Theresa's work with the poor and sick of Calcutta, about the injustice of Idi Amin's rule, and so on. . . . Conversely, it is so difficult to frame satisfactory general principles for ethics or for practice that we are constantly indicating our doubts about them by hedging them in one way or another' (1997: 138; cf. Jonsen and Toulmin 1988).
10. In feminist epistemology, this idea underlies many arguments for stronger representation of women in science (Anderson 2006 provides excellent discussion of this point). A parallel argument in the present context would call for stronger representation of front-line officers.

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