

THE THEORY–PRACTICE ISSUE IN POLICY IMPLEMENTATION RESEARCH

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Applying implementation theory to practice has been rare. Reasons include the difficulty of the theoretical challenge, the varied needs of practitioners and the complicating normative issues at stake. Nonetheless, several approaches can contribute to the efficacy of implementation action. Building on points of theoretical consensus is one strategy. A second is the systematic probing of points in theoretical dispute, to sketch out practical implications. A third is the development of a contingency perspective to determine which theoretical strands may be appropriate in a given case. Finally, tapping the emerging ideas built on a synthesis of partial perspectives is ultimately likely to be the most useful approach. New methodological tools can help select out valid high-performing instances for systematic inspection and possible emulation. And some of the synthetic perspectives now available are amenable to heuristic application; these include approaches based upon reversible logic, game-theoretic notions and contextual interaction theory.

How, if at all, can theory inform practice? Surely, if theoretical knowledge should have practical ramifications on *any* subjects, policy implementation should make the short list. The very development of research interest in this field has stemmed from the practical disappointment following rounds of policy innovations in earlier decades. If the link between policy intent and policy action is problematic, as the accumulated evidence has now demonstrated beyond doubt, should not the acquisition of systematic knowledge meant to explain and perhaps predict this gap be helpful for those in the world of action?

Clearly, those who proclaimed the importance of scholarship on policy implementation, starting with Pressman and Wildavsky, intended to enlighten not only their academic colleagues but also the broad community of policy-interested parties (Pressman and Wildavsky 1984). That original, path-breaking study offered explicit reflections on the practical implications of an economic-development program's failure in Oakland, California. After significant expenditures, years of effort on the part of many actors, and apparent agreement on the part of virtually everyone about the wisdom of the effort, this project resulted in almost no useful outcomes. The authors of this influential analysis framed it as a cautionary tale: if even the apparently

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easy and well-supported instances can be expected to disappoint, it may be apropos to dampen enthusiasm for many forms of ambitious public action.

The initial impact of the Pressman and Wildavsky study was to invigorate the study of policy implementation and also catalyse attention to implications for practice. In one of the most optimistic assessments to be found in the lay literature about any study of public policy, the *Virginia Quarterly Review* pronounced of Pressman and Wildavsky's volume that 'the potential good that can come out of this study cannot be exaggerated' (see back cover to paperback edition).

More than a quarter century following that brave and promising opening to the systematic investigation of this topic, assessments on the theory-practice question have become considerably more sobering. Theories about policy implementation have been almost embarrassingly plentiful, yet theoretical consensus is not on the horizon. The number of variables offered by researchers as plausible parts of the explanation for implementation results is large and growing. Disputes among proponents of different perspectives on the implementation question have filled volumes. Different investigators pursue explanations for different kinds of dependent variables, with relatively little dialogue regarding what might be the most appropriate *explanandum*. After hundreds of empirical studies, validated findings are relatively scarce. Few long-term longitudinal studies have been completed. And, most telling of all, those who have specialized in studying implementation questions systematically have had relatively little to say to practitioners.

Some time ago, but still a number of years after implementation research had become a hot topic, Elmore commented: '[W]hen we look to the most influential implementation studies for guidance about how to anticipate implementation problems, we find advice that is desultory and strategically vague' (1979-80, p. 601). Fifteen years ago I undertook a systematic examination of what the research field had to offer to the improvement of practice. My assessment at the time was also critical and included the observations that 'few well-developed recommendations have been put forward by researchers, and a number of proposals are contradictory. Almost no evidence or analysis of utilization in this field has been produced' (1986, p. 181).

In some respects, a thumbnail, state-of-the-field assessment about the theory-practice issue today would look similar to these. And yet such a bottom line is partially misleading. In the pages that follow, I argue that any assessment of the research field's contribution to utilization begs the question of just what the world of implementation practice can and should expect from research, not to mention the complication that the practical universe is a large and diverse place. Second, normative differences, explicit or implicit, inevitably complicate any translation or influence process between scholarship and action. Third, research at this point can be said to be less consistently disappointing than many have recognized, and these signs of the field's maturity have been somewhat obscured by an overly literal interpretation of

‘the field’. And fourth, a number of strategies can be suggested for linking research results with practical considerations. This last task addresses most directly the theory–practice question for policy implementation in the context of contemporary developments and therefore merits more extended attention later in the article.

THEORY AND PRACTICE IN IMPLEMENTATION: WHAT KIND OF RELATIONSHIP?

Valid theory can inform and improve practice by offering knowledge that can be tapped by people in the world of action. This statement, in essence, is the core assumption that guides theory building on many kinds of practical questions. And yet the theory–practice link is not easily understood. Specialists in science policy, for instance, have explored the paths by which basic research in the physical or biological sciences shape technological innovation and advance. A key conclusion is that basic research indeed influences practice, but it is exceedingly difficult to know in advance which kinds of fundamental scholarship are likely to produce results that carry the most important practical implications.

Indeed, a venerable argument has it that the best thing to do to assist practical affairs with science is to support the best people doing the most basic fundamental research and refrain from imposing upon them concerns about applications. The ultimate fruits of that knowledge, goes the claim, will eventuate most rapidly if basic researchers simply ignore the theory–practice challenge. This argument is far from uncontroversial, but it does gain traction from the evident difficulty in planning scientific advance. Further, thoughtful arguments have been developed in the social sciences, most particularly in public management, to the effect that the community of basic researchers is small enough and the research issues tendentious enough that protection should be afforded those who seek to contribute to policy-relevant knowledge without chasing the issues *du jour* from the practical world of policy (Bozeman 1993; for a much more benign interpretation of the same issue, see Perry 1993).

Again, this argument is likely to provoke disagreement. But it is useful to note that, at a minimum, the research topics that form the core of a scholarly specialty in policy implementation are exceedingly difficult and contain far more nuance and complexity than the basic research questions in, say, the physical sciences. Getting clear answers is difficult enough without considering the relatively shorter times thus far available for development and the much smaller budgets provided for research. It is therefore heretical but not unreasonable to suggest that expecting quick, clear, general, practically useful answers to implementation questions from theory-focused research is almost certain to lead to disappointment.

The point can be put another way: to be primarily critical of theorists for not having solved the theory–practice challenge for policy implementation is to blame analysts for the nature of the challenge itself.

There is a further complication on issues of policy implementation. 'Practitioners' come in many flavours. Even for a given policy or programme many roles and positions are obvious. Practical advice for those crafting law is likely to be mostly irrelevant to middle managers seeking to operate and make progress within the set of formal strictures already adopted. Those at the 'bottom' of an implementation chain, and particularly those stakeholders with interests in policy and yet outside the authoritative policy apparatus, have need of very different kinds of knowledge than do those whose practical concerns are implicitly addressed by, for instance, Mazmanian and Sabatier's 'checklist' of items meant to provide a rough measure of implementability for a given mandate (1989). And those who think of implementation questions largely in terms of compliance are likely to need different kinds of information than those who translate their need for practical implementation guidance into a concern for advice about problem solving through policy.

This point, then, serves to illustrate the great variety of kinds of knowledge and expectations that could follow from research and may be 'needed' by various actors in the real world. Expecting some theory, any theory, to translate simply into a clear and uniform body of knowledge suitable for all such customers is to expect far too much. The theory–practice nexus is not a simple link in some translation belt from thought to action.

NORMATIVE DIFFERENCES

An additional dimension that complicates the theory–practice question for policy implementation is the issue of normative differences, or that 'knowledge' about matters of implementation inevitably carries a kind of normative twist.

Take, for instance, the implications bound up with the very proposition that implementation is a subject worth having systematic answers about. This apparently unexceptional position entails a set of normatively tinged implications. First, attending to implementation implies that improving the conversion of policy into action is likely to be a meaningful event. Radical critics might note that focusing on implementation questions instead of, for instance, issues of regime crisis, is akin to shuffling the deck chairs on the Titanic (see the contrasting positions of Offe 1984 and Scharpf 1977 on this theme).

Second, some might argue that unpacking the foibles evident in disappointing public programmes carries a neoconservative implication, thus suppressing energetic and ambitious public programme efforts altogether. Interestingly, this theme is evident from the very first well-known study on this subject. Pressman and Wildavsky's Rube Goldberg-cartoon metaphor for implementation conveys the clear message that programmes are typically convoluted, poorly designed, indirect, horrendously complicated, highly contingent on uncontrollable circumstances, and – as their probabilistic calculus (pp. 102–10) seemed to 'demonstrate' – virtually immune from

successful results. Their own sketch of the implications of their analysis is explicit in saying that government would be better off lowering its sights and 'keep[ing] it simple', even if there are political pressures to do otherwise.

Nor are Pressman and Wildavsky a lonely pair of voices. Bardach, another well-known scholar on implementation, was even more explicit. As the concluding comments to his most well-known study on the subject, he offered these thoughts:

The longer I watch the evolution of public policies and programs, and the closer I get to the process, the more attached I become to...two heresies...which currently threaten the ideology of liberal reform: government *ought* not to do many of the things liberal reform has traditionally asked of it; and even when, in some abstract sense, government does pursue appropriate goals, it is not very well suited to achieving them.... In the short run, it is essential to invest a great deal of energy in designing implementable policies and programs. For the longer run, however, it is equally essential to become more modest in our demands on, and expectations of, the institutions of representative government. (1977, p. 283)

These views are prescient, at least in that they predate the Thatcher and Reagan eras. And they are more remarkable for being conclusions of an implementation study that essentially offered detailed evidence of a largely successful case.

During the 1980s I interviewed some political appointees of President Ronald Reagan in the US government about implementation themes and found them enthusiastic supporters of implementation research. Their rationale was quite direct: more studies would reveal more failures and more reasons why programs cannot be expected to achieve, thus catalysing support for shrinking the agenda of government.

Third, some might argue, focusing on the implementation question carries a bias toward designing or improving one aspect of the policy process and implicitly suggests ignoring others. For example, in corporatist systems of governance, principal stakeholders are brought into direct negotiations and ultimately agreement at the policy-making stage, thus – so proponents of corporatism might argue – simplifying the implementation challenge considerably. This may be the case since actors to be influenced by a policy-under-discussion are clearly committed to its success from the outset of agreement. Some cross-national evidence does indeed suggest that corporatist systems have more difficulty adjusting quickly to new circumstances with policy innovation, but also have an easier experience with implementation, once the key actors are brought to consensus (Crepaz 1995). The point is that the mere notion of emphasizing the development of 'implementation theory' *per se* might be seen as offering a bias towards one form of system design or redesign rather than another.

Even if one were to sidestep these kinds of normative questions and concentrate on the development of valid theory for implementation as a step toward the ultimate improvement of practice, it is clear that this decision would not eliminate normative complications. The well-known difference between so-called top-down and bottom-up analyses makes this point clear. Quite typically, top-downers and bottom-uppers differ not just on some key puzzles of empirical theory (the former indicating that the most influential variables are controllable by the top or centre of the system, the latter arguing that the contextual or field variables are more important), and not simply on matters of research method (top-downers favouring studies testing for the influence of variables at the 'top', bottom-uppers designing studies to show the significance of complicated settings at the bottom), but also of normative orientation. Top-down analysts often express themselves clearly in support of a representative regime and the consistent execution of choices made by political leaders. They view any other position as a hijacking of democratic principle (for a classic statement, see Linder and Peters 1987). Bottom-up analysts, on the other hand, abjure the virtues of complex overhead representational schemes and endorse the emergence of meaningful policy effort in the discretionary choices of actors, including nongovernmental actors, far from the oversight of political principals (the work of Hjern represents the clearest major instance of this perspective in the research literature; see for instance Hjern 1982; and Hjern and Hull 1982). Is implementation primarily a matter of assembling action in support of the intentions and orders of political leaders? Or of mobilizing the energies of disparate stakeholders to make sensible choices in congealing problem solving around a complex, context-specific, and dynamic policy issue? Does the practical question essentially focus on issues of compliance and monitoring? Or of innovation, collaboration, and creativity? To the extent that there are different positions on this matter, the relevance of apparent 'knowledge' regarding implementation is likely to be a matter in dispute. Further research will not solve such fundamental normative differences.

The broad point is, then, that a cautionary note is essential in any discussion of the theory–practice question for policy implementation. Normative issues are ever-present and will not disappear if only better studies or valid propositions were to emerge from the academy. For these reasons, as well, it is important to frame expectations about the theory–practice link with considerable care.

IMPLEMENTATION RESEARCH AND THEORIES OF IMPLEMENTATION: THE STATE OF THE FIELD

Thus far, I have primarily suggested some of the nuances and complications in considering the theory–practice nexus for policy implementation. Here I leave these to the side and consider directly the state of the field itself and what it might offer. This coverage, then, sets up an assessment of what kinds

of help theoretical efforts might be able to provide to the world of implementation practice.

Considering the state of the field in implementation research, including a review of theory development and theory testing, would be much too large a task for this article, which must perforce emphasize the needs of practitioners. But I have quite recently completed a fairly comprehensive study of the state of the field (2000), one that updates and complements my earlier assessment (1986), and can draw here from the more detailed study to offer a summary sketch.

Within the last few years, some scholars have asked in print why the interest in conducting implementation research seems to have diminished and have debated whether a resurgence should be attempted (for instance, DeLeon 1999; Lester and Goggin 1998; Schneider 1999; Winter 1999). Others have bemoaned the supposed lack of progress in the development of validated theory. My own perspective is that the theoretical-and-research glass is half full:

Virtually all analysts have moved past the rather sterile top-down/bottom-up dispute, and some helpful proposals for synthetic or contingent perspectives have been offered. But consensus is not close at hand, and there has been relatively little emphasis on parsimonious explanation. The dominance of the case-study approach has receded, and a number of thoughtful larger-*n* empirical studies have been conducted – a point often missed by critics. But significantly more are needed; more importantly, the recent empirical work raises a question about appropriate modeling strategies and specifications. The context-dependent... feature of much earlier work has been exposed and theoretical efforts have become more self-consciously general, but solid cross-national investigations are still rare. A so-called third-generation approach to implementation research has been suggested, but relatively little such research has been stimulated by this call... Finally, and perhaps most importantly, the implementation problem has been reconceptualized in somewhat different fashions, and work has proceeded along a number of parallel, overlapping, and highly relevant lines of research. These promise to expand knowledge about converting policy into action, even as they diffuse attention away from implementation in the narrow. Far from signaling a failure of the research enterprise, this last development provides evidence of impact and advance. (O'Toole 2000, pp. 267–8)

A few of these points deserve brief elaboration for present purposes. Elements of both top-down and bottom-up approaches have been partially validated. But the research literature is still overpopulated by a mass of potential explanatory variables. Parsimonious general explanation has eluded theorists, and Meier has recently offered a sarcastic suggestion: 'I propose...[a]ny new policy implementation scholar who adds a new variable or a new interaction should be required to eliminate two existing

variables' (1999, pp. 5–6). Some methodological developments offer the potential to assist in this sorting and synthesizing challenge, but plenty of work remains. More encouragingly, several theoretical developments in allied lines of work have emerged in recent years, and these offer promise for contributing to the basic understanding of implementation issues. Among these are: scholarship on institutional analysis, the study of 'governance', investigations of networks and network management, and certain formal and deductive approaches (see O'Toole 2000, pp. 273–82, for coverage; see also Schofield 2000 for the relevance of research on organizational learning as well). In short, we should not be so narrowly literal in assessing relevant scholarship and theory.

In sum, this assessment of the theoretical state of the field is mixed. I am, however, mildly hopeful of significant improvement, particularly if scholars do not construe the relevant lines of theoretical work too tightly. The question remains, nonetheless: what can the field and its theoretical contributions thus far offer to those interested in assisting implementation practice, and in doing so in the near term? Here the challenge is tendentious, since the demands of practical action impose particularly heavy burdens on theory. Implementers need to take quick action, often in evolving circumstances. They face myriad contexts. And merely cataloguing, let alone interpreting, the scores of variables that have been introduced into the theoretical discussion would consume time and resources unavailable in practice.

Does this mean, then, that implementation theory, as presently arrayed, offers no utility? In the remainder of this article I argue, instead, that there can be considerable value for practice of the theoretical work that has been done and is likely to emerge soon. The utility, however, will typically not follow from some simple 'application' of a consensus theory. Rather, there are particular points of leverage for practitioners in the theoretical mass, and even in the set of disagreements, that can be seen in any careful review of the work thus far completed.

LINKING IMPLEMENTATION THEORY TO PRACTICE

For the most part, theories of policy implementation cannot be applied directly in a predictive sense by practitioners to diagnose dynamic situations and calibrate precise responses to their needs in action. But that does not mean the theoretical progress that has been made and continues to be made has no real-world value. In this section I outline points of leverage between theory and practice. In each case, I suggest ways that theoretical work can offer some help in practice.

Advancing theory, assisting practice

In an abstract sense, one can consider four general strategies to improve implementation theory. In each case, elements of that advance can speak as well to the needs of practical decision makers.

Recognizing and building on points of general agreement

There has been so much disputation and fragmented scholarship in the research literature that seldom have analysts noted there are actually other points of consensus. An obvious but not trivial example has to do with resources. The theoretical and empirical scholarship, virtually without exception, supports the propositions that more resources increase prospects of implementation success (almost no matter what one means by that latter notion); that resources are often not liquid, so that funding sometimes cannot be converted easily into (for instance) skilled staff, or vice versa; that therefore multiple kinds of resources may be critical; and that what matters for implementation is resources for the implementation tasks themselves, not simply size of budget or extent of subsidy to clients. The general point here is that we know much more than nothing, and some of what is known is relevant to the world of action. Distilling and communicating these points of substantial consensus makes sense.

Probing points of theoretical disagreement and communicating the results of empirical testing

This point may seem to be the converse of the preceding one. Actually, it reflects an additional way of gaining leverage without waiting for a full theoretical consensus to emerge. The research work done on policy implementation has generated – mostly implicitly – different if not contradictory injunctions. Probing with special care these points of conflict via additional research can prove to be particularly revealing and useful.

Once again, an illustration can make the point. Top-down theorists argue that, in the words of Mazmanian and Sabatier, ‘hierarchical integration’ makes for implementation success (1989, p. 27). Bottom-uppers disagree (for instance Hjern 1982). Which position is correct? In a study focused on this question (O’Toole 1989), I found that a careful dissecting of the top-down proposition reveals two points of fundamental ambiguity: the meaning of hierarchical integration and the notion of implementation success. As it turns out, the relationship between either hierarchy or integration and implementation success, however interpreted, is not so simple as the top-downers had suggested. On the other hand, it can be shown that integrating implementation action via means other than hierarchy can sometimes incur important costs. These points are directly relevant to practice.

One additional example can support the point further. Pressman and Wildavsky offer a famously pessimistic deductive ‘proof’ that the complexity of joint action makes implementation success almost impossible. Yet this conclusion is patently in conflict with the abundant evidence in the practical world that success occurs much more frequently and that there is a great range of variation to explain. This discrepancy has been labelled the ‘Pressman–Wildavsky paradox’ (Bowen 1982). Confronting a particularly clear theoretical logic with empirical evidence allows analysts to identify the several ways that Pressman and Wildavsky had misspecified actual

implementation settings. These exceedingly enlightening results carry direct implications for practitioners: suggesting such unambiguous points as the value of 'package deals' in complex situations, the importance of bandwagon effects, the ways that institutional arrangements can encourage or discourage agreement, and the value of influencing perceptions about both interests and probabilities among interdependent stakeholders.

Comparative advantage

Mazmanian and Sabatier (1989) recommend the virtues of a strategy of 'comparative advantage'. This notion might also be referred to as a kind of contingency perspective. If neither a top-down nor bottom-up perspective offers a monopoly of wisdom, might it be the case that each is particularly persuasive and useful in certain kinds of situations? If so, that knowledge – including the knowledge of what the choice between them really depends on – has high practical value. Mazmanian and Sabatier offer some tentative suggestions regarding which circumstances can be expected to be associated with which value for each perspective, and other scholars have elaborated this insight more fully (see for instance Matland 1995). The general insight is both strong and unexceptional. For instance, implementation issues arising from the execution of international agreements must rely on the ideas and suggestions of bottom-up analysts, since by definition there is no authoritative top and only a skeleton secretariat in typical cases. Similarly, implementation challenges in developing countries that lack effective central regulatory regimes and plentiful budgets need the help of bottom-up analysts who indicate ways of mobilizing stakeholders outside the official apparatus to lend legitimacy and catalyse effective collaboration. On the other hand, generating uniform action on an issue of known technology in stable circumstances can most easily be accomplished via bureaucratic means supplemented by oversight and the prospect of sanctions for non-compliance – as in the US South during the civil rights era.

Tapping synthetic perspectives in practical ways

Almost everyone in the scholarly community these days believes that some synthesis of the insights of different, even divergent theoretical perspectives offers the fullest, most valid perspective on understanding implementation. But which synthesis? And how can it be used? The fact of continuing, even overwhelming, theoretical complexity compounds the difficulty. Can synthetic perspectives of some sorts offer practical help, particularly at this stage of theory development? My answer is 'yes', and the following section unpacks the reasoning with some examples that may be of practical assistance.

SYNTHESIS FOR PRACTICE: A CHANNEL FOR THEORY-INTO-PRACTICE

Implementation theory offering a synthesis of different partial perspectives offers raw material for possible practical use, although there are important

barriers to be confronted. The most fundamental impediment derives from theoretical complexity, and dealing with or managing that complexity is a key theme of the discussion that follows.

Goggin refers to this issue as the 'cases-variables problem in implementation research' (1986), and research design is obviously a central concern in any effort to validate elements of a complex implementation theory. Good examples of synthetic theory for policy implementation are now available (for instance Goggin *et al.* 1990; Jennings and Ewalt 1998; Stoker 1991). Moving further, to implications for practice, may require even more than solid empirical work.

One way to begin to tease useful insights from maddeningly complex, even if valid, theoretical formulations, is to tap methodological tools designed to assist practical decision makers, rather than merely to explain patterns in existing data. In a series of important recent publications, for instance, Meier and colleagues have shown how multiple regression techniques as well as other statistical approaches can be modified with the explicit intent of sorting through combinations of variables to determine systematically just how success stories differ from standard cases or failures (Meier and Kaiser 1996; Meier and Gill 2000; Gill and Meier 2001).

This set of developments is a notable advance over so-called best-practices research, which has been plagued by serious problems of validity and reliability (Lynn 1996; Overman and Boyd 1994). Instead, standard estimation techniques are modified – by overweighting the high-performing cases in systematic fashions – with the goal of determining the different ways that key variables may influence performance in the cases that depart sharply from the norm. Such techniques can allow one to say, for instance, not simply that resources and political support matter for implementation results; but, more intriguingly, for those cases without benefit of much resources or strong political support, why certain instances outperform the statistical expectations by a substantial margin. These kinds of approaches can be combined with detailed qualitative study of a few cases to tease out the important and nuanced details of the particularly interesting examples, with an eye toward offering practical insights. So-called 'substantively weighted analytical techniques', or SWAT, offer prospects of bringing systematic, large-*n* empirical implementation research together with practical concerns in a direct, explicit way. (Meier and Keiser 1996 is the classic implementation piece in this regard. Another instance is Meier and O'Toole 2001.) By using the results of such analysis, practitioners can focus on variables they actually have some chance of influencing; and they can take advantage of complex, multivariate analyses without facing the 'paralysis by analysis' so common as a result of such theory testing.

In short, adapting methods to the needs of practice can leverage some theory based on the synthesis of partial perspectives to assist in improving practice. Still, methodological innovations are by themselves insufficient.

Theoretical complexity requires efforts to adapt substantively to the needs of active decision makers with limited time and information.

Is it possible for theory that synthesizes the findings of implementation researchers to be applied by busy decision makers in the midst of their own implementation challenges? Not directly and in multivariate detail, I believe. But there are promising possibilities of other sorts to pursue. For the remainder of this section, it is assumed that one defensible goal is to assist an 'implementation manager' operating in the midst of a programme towards better results.

Why not direct and detailed assistance from theory aimed at synthesis? The number of variables involved is simply overwhelming. Furthermore, as Meier and I have argued (1999), managers operating in public programmes typically face a nonlinear reality – in other words, aspects of their settings interact with each other, as well as with managerial effort, and such complicated interactions are difficult to model, let alone to analyse predictively. These circumstances are especially likely to prevail when public programmes involve complicatedly networked institutional settings spanning organizations, governments and sectors. These circumstances are common, probably increasingly so. (See Hall and O'Toole 2000 for systematic data; for evidence that such a nonlinear specification is valid, at least in some important public programme settings, see Meier and O'Toole 2001.)

What, then, can synthetic theory say that is useful in practice? Some versions of implementation theory reflecting synthesis of different and partially valid insights can be employed in a heuristic rather than rigorously predictive fashion, and heuristic use of certain types of theoretical formulation can offer implementation managers help, nontrivially and sometimes counterintuitively. In the next few pages some examples are offered.

First, it is useful to explain what is meant by heuristic application. My meaning is close to that of Lynn, who has written enlighteningly and encouragingly about the theory–practice issue in public management (1996). He argues forcefully for rigorous theory development and testing and is keenly interested in the challenges faced by practitioners. His injunction is to encourage analytical practitioners by communicating to them theoretical work that can have heuristic value in their own settings, as follows:

An analytic approach begins with the assumption that public managers confront 'a messy reality' of data, observations, opinions, facts and, not to be missed, human beings. A manager's intellectual task is to understand or explain messy reality toward the goal of gaining sufficient control over events to influence the future intentionally.... The question is, how can this kind of instrumental understanding be achieved?

The mastery being sought is the ability to use a repertoire of analytic models as heuristics, that is, as instruments for experimenting, in a trial-and-error way, with different hypothetical approaches to complex issues and problems, whether they concern the content of and rationale

for policies or the institutional and procedural means of accomplishing intended results...

...A useful heuristic has two important characteristics: (1) it yields disconfirmable propositions and is not merely 'a filing system for every possible event' (Bendor 1994, p. 37); and (2) when applied to particular situations, it will lead reasonably often to insights that will enhance a manager's effectiveness. (Lynn 1996, pp. 100–1)

In short, according to Lynn, 'training in heuristics... is the way to stock and condition the mind for its intuitive, creative work' (p. 107).

This view emphasizes analytical models – in the plural, as in repertoire rather than magic bullet. The reasons should be clear from the coverage in the first sections of this article regarding the complex relationship between theory and practice, as well as the normative dimensions of implementation analysis. It also calls for something other than a cookbook, and something much more analytically developed than a set of proverbs or, say, a metaphor (*contra* Bardach 1977, pp. 5, 57; see also Bardach 1998). Metaphors admit of too many ambiguities and misleading inferences to be, on balance, very helpful to practitioners facing consequential choices. The heuristic use of explicit theories and models is more useful and is also feasible. The choice of conceptual apparatus is surely crucial, but it is also important to recognize that this perspective begins from the premise that no fully developed theory, no one particular perspective, will suit the needs of even a single practitioner. Additionally, even the best heuristics only take one partially down the road towards effective action. Analytical approaches are key, but so can be other forms of learning, including from experience in the confounding practical world where 'policy and poetry' meet (Lynn 1996, p. 101).

Which set of tools should be put into the bag carried into implementation action by practitioners? There is no definitive list. In fact, clearly the short list offered below is far too incomplete and additions can and should be made. The main point is to suggest with a few plausible instances the ways that the world of theory can speak heuristically to the sphere of implementation practice.

Reversible logic

The short list begins with an approach that is itself explicitly heuristic rather than fully theoretical, although it draws upon theory building and testing conducted previously. Elmore's notion of 'backward mapping' (1979–80) and his more fully developed argument for 'reversible logic' (1985) are sets of ideas designed directly for the goal of assisting practitioners during policy design and implementation.

Elmore's arguments need not be capitulated here. The important point is that he offers a systematic way of thinking about implementation settings, one that encourages decision makers to analyse the channels of influence that are likely to be most efficacious in shaping the actions of other necessary

participants in the jointly produced policy output. The heuristic Elmore offers is most directly helpful for those engaged in policy design. Nonetheless, his full argument calls for practitioners to consider action possibilities from multiple perspectives, search for nonobvious points of leverage, and take care not to assume that policy initiatives are necessarily the only or primary motivators for policy-relevant action. At the same time, he does not dismiss the potential for centrally-initiated policy to carry ramifications through a complex multiactor system (1985).

In this fashion, Elmore offers a treatment that builds on a combination of top-down and bottom-up logics to convey a practically useful tool, without overwhelming decision makers with caveats and qualifications. His theoretical ambitions are limited, and indeed his ideas have generated no discernible empirical 'testing'. But such a result is not unexpected and should not be considered the appropriate evidence of successful, theory-grounded help for practice. Reversible logic represents a useful heuristic tool for practical actors.

Game theory as an implementation heuristic

Both top-down and bottom-up implementation perspectives have a tendency to caricature the influence of either central or contextual actors, respectively, as well as variables associated with these different portions of an implementation system. Research demonstrates that each argument presents weaknesses, particularly in complex, multiactor or networked instances. Variables and decision makers at the 'top' (a notoriously ambiguous notion) matter, and variables and actors at the 'bottom' (also ambiguous) can be highly influential. Treatments synthesizing both core insights tend to be very complicated. But some theoretical approaches, adapted from other social settings, provide templates.

One worth considering is game theory. The reference here is to formal game theory, a branch of applied mathematics used to analyse a variety of social situations. One point must be emphasized, however: I do not think that game theory in its rigorous, deductive, full-blown sense is of much utility in practice (for an explanation, see O'Toole 1993). Implementation settings in the real world typically face considerable uncertainty and complexity, particularly in networked contexts and especially at the initial stages of implementation, when routines for interaction are not yet in place, modes of coordination and many aspects of implementation action are under negotiation, and considerable learning must take place. Game theory is not likely to be of much direct help for actors in the midst of such flux.

Nevertheless, as a heuristic tool that can help decision makers sort through their points of leverage in a complex situation and identify options and trade-offs for consideration, game theory can be useful. This conclusion is likely to be particularly valid in settings of multiactor implementation: action for implementation involving two or more interdependent actors – individual and/or organizational – where the full set of actors are not linked

by an authoritative position, and where coercion is impractical or unacceptable as a way of congealing coordinated effort. In such cases, which are quite frequent, game theory provides a way of modelling implementation settings that accords with some of the fundamental elements of multiactor implementation. Game-theoretic logic builds into its modelling efforts features such as: instrumental rationality (and, as appropriate, limitations on rationality) on the part of the actors involved, joint production of decisions (so that what happens is a product of the interdependent choices of the distinguishable actors rather than controlled unilaterally), separate but potentially overlapping objectives (different games incorporate different degrees of conflict or congruence of interests), and strategic decision making (what actor A decides to do is conditioned by what A expects B to do, and vice versa). It is even possible to model connected or linked games, thus incorporating explicitly how more complex and multilateral agendas and patterns of interdependence can help to shape choices. Game-theoretic models, therefore, avoid some of the pitfalls of either the top-down or the bottom-up perspectives applied separately and incorporate ideas of both.

Many insights can follow from an implementation analyst's understanding of the logic of interdependent strategic choice. Two examples illustrate the value of such a heuristic. First, game theory can show how relatively small changes of even one actor's preferences or choices in a setting of interdependence can have very large consequences on what transpires collectively. This point fits the data from experience, even if it does not accord with many of the ways that analysts typically think about policy settings, particularly analysts steeped in the logic of incrementalism. Second, and even more revealingly, game theory can make clear the circumstances in which even fully rational actors, who desire to achieve their own goals and who even possess complete information about their own setting, can consistently suboptimize when faced with patterns of interdependence in which they fear being exploited by other actors. The collective result may be worse for all parties than another possible outcome, and all may be aware of this fact but find themselves trapped in unproductive patterns of interaction. The archetypical case of such a result is, of course, the classic Prisoners' Dilemma, a pattern that may be relatively common in settings where efforts are being made to convert policy into action among a collection of different parties (see Lynn 1993 for an analysis suggesting that such a pattern can be common in social service programs; Weber 1998 implicitly models implementation settings of environmental regulation as Prisoners' Dilemmas amenable to change under certain carefully specified circumstances). The general point, in the jargon of game theory, is that interdependent actors can find themselves in Nash equilibria that are, nonetheless, Pareto inferior to other solutions – that is, worse than at least one other option *for some or all parties, without being superior for any*. Understanding such circumstances can allow implementation managers to effectuate moves to create, in the jargon of today, win-win moves.

This point brings to the fore a key lesson about the use of game theory as a heuristic. To be of most use in the world of practice, a rigorous application of game theory should be avoided. The reasons have to do not only with the fact that the mathematics in multiactor settings can be highly complicated, but even more fundamentally, that solving the mathematical problem does not necessarily surmount any practical challenge. A Prisoners' Dilemma game is 'solved' by dominant strategies of defection for all parties, even if social welfare gains can be made by destabilizing the 'solution' and even if such a mathematical 'solution' signals a practical failure (that is, to cooperate). Game theory as a heuristic, on the other hand, can be practically quite revealing by offering a way of thinking about how different implementation circumstances differ from each other and by pointing to the kinds of variables that, if influenced, can make practical differences in a given case.

This last point is crucial. Game theory in a narrow sense helps one to explore the logic of interdependent choice, given set dimensions and players and preferences. But often these are the very features that, if altered, can provoke dramatically different outcomes. As shown elsewhere (O'Toole 1996), the very reasons why mathematical game theory faces severe limitations in saying much that is useful in practice are precisely the points that are most useful to consider when trying to identify points of leverage in actual implementation settings. The failures of deductive game theory are the opportunities for actors in practical settings, and therefore the perspective can offer a valuable heuristic for decision makers. Knowing how games can be coupled, uncoupled, and modified (changing players, preferences, options, etc.) can be a tool of considerable utility.

Although game-theoretic applications to implementation have not been common, they are available. Stoker, for instance, uses a nontechnical version to productive ends (1990), and I have also attempted to sketch its strengths and weaknesses, both for theory (1993) and for practice (1996). It is worth noting that the logic itself has seen application in other policy-relevant contexts, as demonstrated most fully by Scharpf (1998; see also Weber 1998).

Contextual interaction theory

One additional example can be offered to suggest ways in which theory built on a synthetic understanding of policy implementation might be put to heuristic use. The instance is the potential utility of 'contextual interaction theory' in aiding practical decision makers during implementation.

Contextual interaction theory refers to a line of work developed by and with Dutch colleagues (for instance, see Bressers and Klok 1988; Bressers, Klok and O'Toole 2000). The theoretical argument emphasizes that characteristics of policy instruments themselves that are adopted by governments can feed into (often ongoing) social processes between implementers and the target groups they seek to influence. The perspective is, in essence, a social process theory that incorporates some explicit considerations having

to do with, among other variables, the policy tools that are available as implementers and targets engage in strategic interactions over extended periods.

The theory has been elaborated on a number of particulars, to include the ways that target group characteristics themselves shape the kinds of instruments that are likely to be adopted by governments (Bressers and O'Toole 1998), as well as the more complex interaction patterns that might be seen when targets are multiple and complex (Ligteringen 1999). Even a brief sketch of core elements of the theoretical logic would require more space here than can be provided. Instead, a few comments can be offered about the general approach and why it may be amenable to heuristic use in implementation practice.

The theory is essentially a deductive argument that places emphasis on interdependent action between implementers and targets, with policy instruments one, but only one, element shaping what happens. The theoretical focus is on the ongoing interactions among those involved. Putting the focus here moves the characteristics of the actors into the central place in the theory's logic. Three kinds of actor characteristics are incorporated into the theory's deductive logic: their objectives, information, and power. Depending on how these combine, one could expect one or another kind of implementation process to ensue, such process in turn linked to other ongoing ones involving the same actors.

I omit even a sketch of the propositions derived from this kind of thinking (but see Bressers, Klok and O'Toole 2000) and instead emphasize a strategic choice in the theory building that may offer some assistance to practitioners. One of the major substantive impediments to a more effective theory-practice connection, as explained earlier, is the sheer complexity of the world of implementation. This complexity manifests itself in synthetic theoretical arguments that incorporate an array of variables. Even if valid, such complicated explanations impose serious obstacles to practical problem solving.

An important reason has to do with the typical form taken by theoretical propositions. For a theory incorporating, say, 15 independent variables, the typical propositional form includes statements such as: more of variable *x* increases the likelihood of implementation success, *ceteris paribus*. But in the real world, other things are virtually never equal, and implementation managers simply cannot hold the relevant range of other variables constant. This point means that to be useful, propositions somehow need to include values for all relevant variables. But to anticipate all combinations of all relevant variables is to stipulate overwhelming complexity. If one were to consider an implementation theory incorporating 15 independent variables, and even if one were to simplify by treating each of these as dichotomous, 32768 combinations would need to be considered.

The approach of contextual interaction theory, however, provides a route to a manageable heuristic. We distinguish 'core circumstances' from

'external circumstances'. Core circumstances are those basic sets of actor characteristics identified above: the objectives, information, and power of those involved in the implementation process. External circumstances, including for instance features of policy instruments, are conceived of as working through (and thus perhaps modifying) the core circumstances. In this fashion many contingencies can be incorporated, at least implicitly, without increasing greatly the complexity of the basic theory.

The heuristic use of such an approach can be seen as following from the simplification adopted in delineating the core circumstances (see Bressers, Klok and O'Toole 2000). The full set of predicted results emanating from various combinations of core circumstances can be sketched without creating a pattern of overwhelming complexity. An implementation manager, equipped with the basic logic, can consider a particular circumstance and identify which, if any, external circumstances might potentially alter the value of one of the central variables in the scheme. New circumstances represent either direct shifts in values of the core items or can be mapped onto the core circumstances through a straightforward logic (for instance, a policy increases or decreases the power of a particular actor, or shifts the balance of information available, and so on).

This sort of schema does not magically eliminate complexity; but used judiciously, it can offer a systematic way of sorting through complexity that moves past the *ceteris paribus* approach of most theory building while also providing a reasonable framework for practitioners to consider the context and dynamics of their particular settings.

CONCLUSIONS

This article has offered a sampling of approaches that, in sum, suggest a cautiously optimistic response to the theory–practice challenge for policy implementation. All the *caveats* sketched early in the article still apply. There have not been striking successes evident thus far in finding ways of linking theoretical efforts with practical advice. To some extent, that observation is a logical consequence of the difficulty of the theoretical challenge itself, the relatively brief time that analysts have been tackling it, the varied needs of different practitioners, and the complicating features of the normative issues at stake. Still, it is possible to identify several lines of effort underway or feasible in the short term that can help to contribute to the efficacy of practical implementation action.

Building on points of theoretical consensus is one route to useful advance. So too may be the systematic and critical probing of points in theoretical dispute, with a view toward sketching out practical implications. Developing further a contingency perspective, or strategy of comparative advantage, can help practitioners determine which brands in the theoretical bazaar, if any, are appropriate for use in a given case. And tapping the emerging ideas built on a synthesis of partial perspectives is ultimately likely to be the most useful approach.

Theory developed from synthesis can be overwhelmingly complex in practice. But new methodological tools can help select out valid high-performing instances for close and systematic inspection, with a view toward lessons that can be applied elsewhere. And some of the synthetic perspectives available at this point are amenable to heuristic simplification or adaptation in ways that can increase the quality and range of tools for implementation practitioners to consider.

Much work, indeed, remains to be done. But it would be a mistake to conclude that, on the theory–practice challenge, the effort must begin from square one.

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