Do the Policy Sciences Exist?

This book's central goal was to explore the core research questions of public policy scholarship with an eye toward gaining the tools necessary to make a decision on whether there really is, or ever can be, such a thing as an academic field of policy studies. The preceding chapters, we believe, marshal considerable evidence supporting an integrationist conception of the field of public policy studies.

In Chapter 1 we identified the basic characteristics that identify an academic discipline as things like a core research question or a central problem, a unifying theoretical framework, a common methodological framework, and a general agreement on epistemology. Some of these characteristics clearly apply to the field of the policy studies. True, the field does not have a central research question, nor is it oriented to a single overarching problem. Still, given what's been presented in the preceding chapters, we believe there is a strong argument that public policy does have a set of clearly identified research questions, and that these questions roughly define distinct scholarly domains. We do not always see a dominant theoretical framework within these domains, but we do see considerable evidence of theory construction. In areas such as policy

process and implementation, there are general notions of what a conceptual framework needs to do, even if no one has—yet—figured out how to perfect that framework. At a minimum, we see lively debate over theory-building, and constructing explanatory frameworks is progressing at least episodically.

But what connects these domains? What stitches them together into something that can be defined and defended as a distinct field? Perhaps the best answer to this question is that whereas policy studies is not oriented toward *a* particular problem, there is a legitimate case that policy studies has anchored itself using the problem orientation foundational to the Lasswellian vision of the policy sciences. What can be drawn from all areas of policy studies is a deeper applied understanding of how democracies deal with, have dealt with, or might deal with whatever problems society or a group within society believes is worth addressing. This is the common thread that connects all areas of policy studies, even in areas such as the policy process literature, which to the novice can seem an overwhelmingly academic exercise where knowledge is pursued for its own sake. At a minimum, work such as Kingdon's (1995) and Baumgartner and Jones's (1993) can be mined for a wealth of practical advice on how to get a democratic system to pursue a particular solution to a particular problem. Have a solution ready, be ready to attach to another problem, change indicators, be alert to focusing events, breach the subsystem monopoly, and seek a shift in venue—though not written as how-to manuals for policy advocates, these sort of works can be mined for exactly that sort of systematic advice.

Although the problem orientation is arguably a pretty thin way to connect the disparate research questions that orient different policy domains, it is no weaker (and perhaps a good deal stronger), than the bonds that hold together varied subfields in disciplines like political science, public administration, or sociology. The same defense can be made for public policy's lack of distinct methodology or its running epistemological battle between the rationalist project and its post-positivist critics. Whatever balkanizing influences such issues have on the field of public policy, they are not so different from those in related social science disciplines.

This same line of argument, however, also advances the perspective that policy studies do not add up to a coherent academic field. If the primary claim for policy studies as a distinct discipline boils down to "we're no worse than political science or public administration," then the field is

in trouble. To stand on its own it must make a positive claim to be making unique conceptual, theoretical, methodological, epistemological, and empirical contributions; the negative defense that public policy is not any better or worse than related fields is ultimately not only unsatisfying but condemning. If the field of policy studies conceives of itself as a decentralized patchwork of a discipline, content to be borrowing bits and pieces of whatever is useful or fashionable in other social sciences, it deserves its already-commented-on inferiority complex. The core case against treating public policy as a unique discipline boils down to this basic critique: what has the field of policy studies done that adds to the cumulative store of knowledge that has not been borrowed from some other academic home? We believe the contents of this book suggest a reasonable answer to this questions is "quite a lot."

The Theoretical Contributions of Policy Studies

As detailed in Chapter 1, policy studies are viewed as a taker and user of theory rather than a producer. It is bad enough that this view is broadly shared among those outside of policy studies, but it is accepted and affirmed by many within the field as well. The lament for better theories comes from those identified with the rationalist project (Sabatier 1999) and its post-positivist critics (Stone 1988, 3). The inability to construct general conceptual frameworks is blamed for the lack of progress in areas like implementation, and the reliance on theories adapted and taken from other fields is seen as a key reason why policy studies is seen as parasitic to disciplinary hosts like economics and political science. Even in areas where policy studies indisputably generates unique theoretical frameworks, these are seen as too limited and tied to specific times and events to count as a real contribution (program theory being the obvious example).

It is true that policy studies has not yet produced a single generalizable framework that ties together all the causal relationships that fall within its area of interest. Even if we divide the field by central research questions, as we have done in this book, we find little in the way of a guiding conceptual framework within any of them.[1] While ceding this argument, we believe criticisms from this quarter miss the point. No social science, with the potential exception of economics, has managed to establish a central theoretical orthodoxy. And even in economics there is considerable

controversy about this theoretical orthodoxy's ability to adequately describe, explain, and predict the phenomena it is supposed to. There is no reason to expect the field of policy studies to be any different in this regard; indeed, given its sprawling subject matter, it is perhaps more expected in this field than any other matter. What is remarkable about policy studies, and what has been a constant theme throughout the preceding chapters, is the astonishing array of theoretical efforts and accomplishments the field has generated.

Consider policy typologies, generally reckoned (as detailed in Chapter 2) to have reached an explanatory dead end because of an inability to overcome the classification problem. What is important to keep in mind about the policy typology project is that it was not simply a theory of public policy, it was (and is) a general theory of politics. It did not borrow from political theory—it was not constructed by adapting preexisting theories from other disciplines—it was an original conception of the political realm that stood on its head the conventional wisdom on causal relationships in politics. Its failure to live up to its tantalizing promise was not due to a failure of logic or an empirical falsification of its key axioms. It failed primarily because of a universal difficulty found in the study of politics, i.e., the inability to separate facts from values or perceptions from objective reality. Though this inability doomed the framework as a predictive theory, for two reasons it is unfair to label typologies as a theoretical failure.
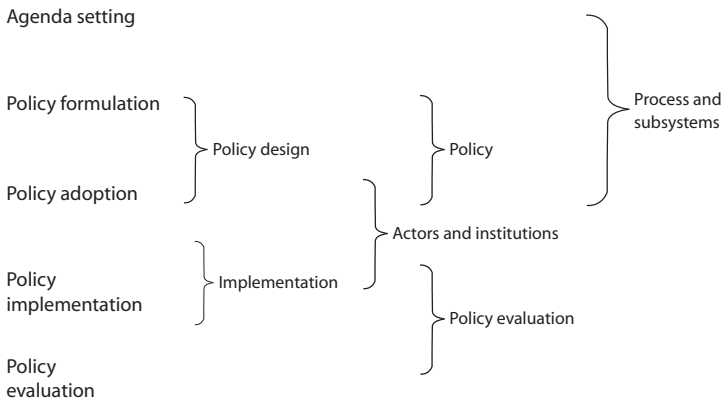
First, typologies continue to provide a useful heuristic for making sense of the political and policy world. Categorizing policy as regulatory, distributive, or redistributive is a quick and intuitive means to make sense not just of policy outputs or outcomes but politics in general. It is conventional wisdom to accept that redistributive policies produce different power relationships than regulatory policies, even if objectively classifying policy into these categories is all but impossible. Second, policy typologies continue to develop as a theoretical construct and have proven to be a remarkably resilient and useful way to conceptualize process, behavior, outputs, and outcomes in a broad swath of the political arena.

To take one example, a significant literature in morality policies developed over a decade or so, beginning in the mid-1990s (e.g., Tatalovich and Daynes 1998; Mooney 2000). Morality policy attracted the attention of scholars because of its increasing centrality to politics at this time; issues such as abortion, gay marriage, and the death penalty became ideo-

logical and electoral rallying points. These types of policy issues seemed to produce a particularly virulent form of political conflict, one that mobilized large numbers of people and resisted the sort of compromise typical of the democratic process. Morality policy scholars were interested in whether there was a particular form of policy issue that bred this sort of politics, and if so, could it be systematically described, put into a coherent conceptual framework, and used to explain (or even predict) political behavior and policy outputs. At its heart, the morality politics literature is oriented by a classic typology strategy: the attempt to systematically classify policy issues into morality and nonmorality types and to assess whether these classifications had predictive power. Though this literature ultimately stumbled over the same issue as Lowi's (1964) original framework—the problem of objectively classifying policy types—it provided some unique insights into why public policies fail, why public policies orient themselves to some problems over others, why certain policies have such powerful mobilization characteristics and are resistant to compromise—and it even provided some evidence that policies could be classified systematically and empirically if not wholly objectively (e.g. Meier 1994, 1999; Mooney and Lee 1999; Smith 2002). These are significant achievements that drew their conceptual power from a framework developed and refined within the policy field.

Perhaps the classic "failure" of policy theory is the stages heuristic. The harshest critics of the stages approach are almost certainly policy scholars themselves, who argued that the stages theory was not a theory at all (e.g., Sabatier 1991b). As detailed in Chapter 2, these criticisms are not without justification. The stages approach is not predictive and does not generate falsifiable hypotheses; it is descriptive rather than explanatory in any real sense. Yet even if it is only a heuristic guideline to the policy process, it is a remarkably succinct means to impose meaning and order on an incredibly complex undertaking. Understanding public policy in all its dimensions is a daunting task when undertaken as a primary academic career; yet the basic gist can be conveyed to an undergraduate class in ten minutes using the stages framework. Whatever its drawbacks as a grand conceptual theory, this is not an insignificant achievement.

Moreover, the stages heuristic still serves as a useful means to conceptualize what the entire field of policy studies is all about. Figure 10.1 shows how most of the dimensions of policy studies discussed in this book might map onto the stages heuristic. All these dimensions are connected

FIGURE 10.1 **The Stages and Fields of Public Policy**

through the larger stages framework, each subfield focused on a particular element or set of elements that together constitute the stages approach. To be sure, there is overlap and redundancy, and no single dimension encompasses every single stage of the policy process, but the stages framework serves as a useful umbrella to demonstrate what the field of policy studies is all about.

Typologies and the stages framework, in short, have made and continue to make useful contributions toward helping scholars understand the complex world of public policy. More to the point, these conceptual frameworks were produced and developed primarily within the policy field; this is hardly the record of an academic discipline as theoretically devoid as policy studies is routinely described to be.

We have taken some pains to point out that the theoretical contributions of policy studies are not limited to these two frameworks. Some policy theories build off of conceptual foundations from other disciplines. Notables in this category include Kingdon's (1995) concept of policy windows, which builds from Cohen, March, and Olsen's (1972) garbage can model of organizational behavior. They also include Baumgartner and Jones's punctuated equilibrium framework (1993), which builds from the bounded rationality concepts pioneered by Herbert Simon (1947) in public administration as well as work by Stephen Jay Gould in evolutionary biology.[2] Kingdon and Baumgartner and Jones, however, do consider-

ably more than simply borrow an existing conceptual framework and apply it to a different dependent variable. In both cases, there is considerable theoretical refinement going on. Starting from the fairly raw materials of a new perspective on organizational process (Kingdon) or a well-established notion of how humans make choices (Baumgartner and Jones), these scholars considerably refined the starting concepts and emerged with original contributions to our understanding of where policy comes from, why government pays attention to some problems more than others, and why policy changes.

Even in an area like implementation, where hope of generalizable explanation has been all but abandoned, we still see policy scholars making steady contributions. These range from the basic conceptual tools needed to understand what makes policies work (or not work), things like the complexity of joint action, to full-blown hypothesis-generating, empirically falsifiable theoretical frameworks like the one produced by Mazmanian and Sabatier (1983). The fact that a generalizable theory of implementation has not emerged should not obscure the fact that we know more about what is and is not important in putting a policy into action thanks to three or four decades of implementation research. Pressman and Wildavsky would be hard pressed to make the same lamentations about the lack of research or insight into implementation today that they did when their book first appeared in 1973.

Ironically, it is probably where policy scholars have made the fewest theoretical contributions that policy research has the most settled conceptual frameworks. Policy analysis, at least compared to other areas of the policy studies field, has something approaching a general theoretical gyroscope in the form of welfare economics. Though policy scholars have certainly refined the conceptual materials and made a number of contributions in terms of the methodology, it is reasonable to describe rationalist policy analysis as largely consisting of applied economic analysis (see Munger 2000). Policy evaluation, at least on the rationalist side of the ledger, can appear more concerned with empirically demonstrating causality rather than theoretically explaining it (e.g., Mohr 1995). Evaluation is guided primarily by program theory, which in most cases is a set of beliefs about causality traced to policymaking intentions. As such, program theory tends to be limited to specific programs in specific circumstances and requires no assumptions or fundamental truths about how the world works. Yet policy analysis and evaluation are considerably less

likely to be the target of (rationalist) lamentations about the lack of theo-
retical production in the policy field. This is perhaps because of their
more applied nature, which sometimes leads to a focus more on methods
and situational tractability rather than grand and universal conceptual
frameworks.

Some of the harshest critics of policy theory (or the lack thereof) have
come from post-positivist policy scholars, who are either skeptical that
theories in the scientific sense are capable of explaining the world of poli-
tics and policy, critical that such theories and their associated methods
undercut democratic values in the policymaking process, or some combi-
nation of both (e.g., P. deLeon 1997; Stone 1998; Fischer 2003). Yet the
post-positivists are not antitheory; it's just that as a whole they tend to ar-
gue that normative theory (as opposed to the positive theories of the ra-
tionalists) should provide the guiding framework for policy studies.
Creating normative democratic frameworks for systematically under-
standing the complex world of public policy is not an undertaking for the
faint-hearted. Stone's polis model and the epistemological cases made by
Fischer and deLeon have nonetheless made a significant impact on policy
studies as a field that perceives what it is doing and why. If nothing else,
the post-positivists have served to remind the theory-building rationalists
that public policy in democracies must ultimately be judged not just by
scientific values but also by democratic ones.

Overall, we believe there are plenty of examples to counter the argu-
ment that the field of policy studies has contributed little to the system-
atic understanding of the political world. From our perspective, the real
problem is not the field's inability to generate conceptual frameworks
that result in genuine insights so much as the field's sprawling subject
matter. Policy scholars have made significant advances since Lasswell first
envisioned the policy sciences. We know considerably more about agenda
setting, decision making, implementation, impact, and evaluation than
we did a half-century ago. Much of this book has been devoted to making
exactly that point. Yet in the policy field, progress seems to be measured
by what we have not done rather than by what we have. We have not pro-
duced a robust and generalizable theory of implementation. We have not
reconciled the paradox of science and democratic values. We have no
overarching framework for the policy process. This list of failures is all
true enough. We have, however, provided a decent understanding of the
reasons why implementation succeeds or fails. We have been engaged in a

serious and long-running debate over how science and democratic values can and should be balanced in policymaking. We have produced a wide array of empirically testable conceptual frameworks (punctuated equilibrium, advocacy coalition frameworks, policy windows) that cover multiple stages of the policy process, even if they do not cover all of them. This list of successes represents important contributions, and any discussion of the policy field's failures should rightly be balanced with an account of its successes.

Key Problems

Although we clearly believe a spirited defense of the policy field's intellectual contributions is more than justified, this should not be used to distract attention from the field's intellectual challenges. The purpose of this book was to demonstrate that policy studies did have a set of core research questions, had constructed useful conceptual frameworks to answer those questions, and had used these to accumulate a useful store of knowledge. Yet our examination has also clearly shown that the policy field consistently stumbles over a set of key conceptual and epistemological challenges.

Conceptual Challenges

The field of policy studies suffers considerably because of the continuing vagueness over what it actually studies. As discussed in some detail in Chapter 1, no precise universal definition of "public policy" exists. A central problem here is making "policy" conceptually distinct from "politics." In languages other than English, "policy" and "politics" are often synonyms. In German, for example, "die Politik" covers policy and politics. In French "politique" does the same.

In English we have fairly precise definitions for politics. In political science the most commonly used are Easton's (1953), "the authoritative allocation of values," and Lasswell's (1936), "who gets what, when and how." Both of these essentially capture the same underlying concept: the process of making society-wide decisions that are binding on everybody. There is little controversy in these definitions or the underlying concept, and they are widely accepted by political scientists as defining the essence

of what they study. But if this is politics, what conceptual ground, if any, is left for policy? This is a fundamental question for policy studies—indeed, it is probably *the* fundamental question—and it has never been satisfactorily answered. Astonishingly, the field as a whole seems to have lost all interest in seriously grappling with this question.[3]

If we distill the various definitional approaches summarized in Chapter 1, we end up with a concept that can roughly be thought of as "a purposive action backed by the coercive powers of the state." This definition (and those similar to it) conveys the two basic concepts at the heart of policy studies: 1) public policy is goal-oriented; it is a government response to a perceived problem; 2) public policy, as Lowi (1964, 1972) argued, fundamentally rests upon government's coercive powers. What makes a policy public is the fact that, even if you oppose the purposes of policy, the government can force you to comply with it.

We fully recognize that the validity of this definition is debatable (e.g., what about purposive inaction? Should that not count as policy too?). Our purpose here is not to end the definitional debate but instead to point out its importance to distinguishing policy studies as a distinct academic field. If this definition is, at least for the purposes of argument, accepted as a reasonable expression of the concept at the heart of policy studies, how is it really different from the concept of politics? Does it not simply restate, perhaps in a more narrow and focused way, "the authoritative allocation of values"? Purposive decisions no doubt allocate values—they are expressions of what society considers important and what is going to be done about it. If these decisions are backed by the coercive powers of the state, they are certainly authoritative. Perhaps a key implication of this line of reasoning is that the study of public policy is really the study of the reason for, or the end goals of, politics. If so, it is not at all clear how politics and policy can be conceptually disentangled. Yet there is also the argument that this conceptualization has causal order backward. Lowi (1964) argued that policy begat politics, not the other way around. It is the nature or the type of purposive action that shapes the struggle over whose values get authoritatively allocated.

The larger point here is that the lack of conceptual clarity is a big reason why it is legitimate to question whether the field of policy studies has any legitimate claim to be a distinct academic enterprise. For the past half-century, policy studies has been mostly content to claim the problem orientation á la Lasswell as its raison d'etre, or (more commonly) to ig-

nore the issue altogether. Given that the field has never fully or forcefully articulated its reason for being, it is little wonder it is not even sure what to call itself. We have used terms like "policy sciences" as synonyms throughout this book as handy descriptive terms for the general field of policy studies. Yet it is not at all clear that these terms should be treated as interchangeable. A term like "policy sciences," for example, may carry epistemological and philosophical implications that some policy scholars (especially if they are of a post-positivist bent) are at odds with.

Our approach to the conceptual fuzziness lurking at the heart of policy studies is to seek more clarity by looking at key research questions and using these as a basis for defining such terms as "policy analysis" or "policy evaluation." We obviously believe this is at least a partially effective way to impose theoretical and epistemological coherence onto policy studies. Yet these terms are not always used in the way we describe them, and in some ways we have drawn artificially clear conceptual lines. The *ex ante* and *ex post* division we use to distinguish analysis and evaluation, for example, is blurred in practice by a considerable amount of *in media res*, studies that by definition blur the pre- and post-decision markers we have used. Perhaps if there were a clearer understanding of the core concept of policy, these divisions could be made sharper and with less reliance on the individual perspective of a given researcher or writer.

The bottom line is that public policy must find a way to make the concepts at the heart of the field clearer. At least, it must do so if it is ever to justify itself as an academic undertaking distinct from fields such as political science and public administration.

Epistemology

A running theme throughout this book is a central split in philosophy, a difference over how policy should be studied. This split was virtually ordained by Lasswell's original notion of the "policy sciences of democracy." The key problem with that vision, of course, is that science is not particularly democratic, and democratic values seem to leave little room for the positivist leanings of the scientific approach.

The result has been two camps that often imply the two approaches are contradictory and mutually exclusive, camps that we have termed throughout this book as the rationalists and the post-positivists.[4] This is an accurate enough claim within some narrowly defined limits. Rationalists, for

the most part, do make assumptions about an objectively knowable state of the world, a world that can be empirically described and analyzed. Post-positivists, for the most part, argue that whatever is objective about the physical world does not imply a similar state of affairs in the political and social world. Reality in those domains is a heavily constructed reality, with "truth" and "fact" varying with perspective and context. These two radically different assumptions about the nature of the political and social world naturally lead to radically different notions of how to go about understanding those worlds.

Yet these differences are sometimes overblown. Many of the self-described post-positivists are not necessarily anti-rationalist in the sense that they see the whole enterprise as pointless (a good example is P. deLeon 1997). Mostly what these scholars are arguing for is epistemological pluralism, a place in policy studies where subjective experience is considered at least as meaningful as a regression coefficient. Similarly, self-proclaimed rationalists have recognized that the failure to account for values is a key weakness of their work and thus have developed methods to incorporate—or at least account for—subjective values in their work (e.g., Meier and Gill 2000; Smith and Granberg-Rademaker 2003; Smith 2005). In practice, then, what we see in public policy is less two warring camps in a fight to the philosophical death than a general recognition by everybody that effectively studying public policy means figuring out ways to combine values and empiricism.

Still, it has to be said that the differences here are significant and deep enough to act as a break on pushing the field as a whole forward. The harsh truth is that the scientific method that orients the rationalist project is in fundamental ways incompatible with democratic values. Rationalist policy research is not participatory, does not give contradictory outcomes equal weight, and does not submit the validity of its conclusions to a vote. From the post-positivist perspective, this makes the rationalist project misleading (or even dangerous) in democratic terms. Yet in its defense, the rationalist project is enormously informative; it is probably fair to say it has produced more useful knowledge (both in applied and academic senses) than its post-positivist opposite.

Part of the problem for the post-positivists is that the rationalists have a practical and utilitarian epistemology in the scientific method, and the post-positivists simply have no equivalent. The alternate methods of gaining knowledge about public policy pushed from the post-positivist

perspective—hermeneutics, discourse theory, and the like—take relativism as virtue. Pile this on top of the conceptual vagueness that characterizes the policy field, and what you tend to end up with is an approach to public policy that confuses as much as it illuminates (at least it is if our experience teaching graduate students is any guide).

Post-positivists recognize this problem and have sought to construct practical approaches to studying public policy. A good example is participatory policy analysis, which springs from the notion of deliberative democracy. The latter is a values-based conception of democracy whose basic premise is that public policy is best legitimated by public deliberation. Participatory policy analysis (PPA) in various forms is championed by scholars such as Fischer (2003), deLeon (1997) and Durning (1993). PPA rests on a fundamental assumption that the problem, the most appropriate policy solution, the impact of the policy, and the relative success of the policy are all at least partially determined by perspective. PPA begins with the basic premise that the perspective of all stakeholders must be given equal consideration if democratic values are going to be taken seriously in the policy realm.

To make this practical, the central component of PPA is to create something like juries: panels of citizens that study a particular policy problem and seek to come to some consensus on what should be done about it. PPA methodology would require policy analysts to select people, "randomly chosen from a broadly defined pool of affected citizens (possibly formulated to take sociocultural variables in account) so as to avoid the stigma of being 'captured' by established interest and stakeholders, to engage in a participatory analytic exercise" (deLeon 1997, 111). PPA has been tried in relatively limited circumstances. For example, deliberative polling, a sort of precursor to full-blown policy analysis, has gained considerable attention worldwide through the work of James Fishkin and Robert Luskin (1999). For the most part, however, neither PPA nor any other post-positivist–championed methodology has come close to providing a widely used alternative to the mostly quantitative toolkit championed by the rationalists.

The reasons for this failure of post-positivist methods to penetrate the mainstream are practical as well as theoretical. Drawing together random samples of citizens is not easy (and not cheap), and it requires a significant investment of time on the part of the analyst. Theoretically, PPA strikes many in the rationalist camp as having internal contradictions.

PPA basically creates mini-legislatures with the aim of forging more consensual policymaking. It is not entirely clear, however, why these groups would be any more or less consensual or reflective of the public's true preferences than the standard-issue legislature of representative democracy. What about the scope of the problem or policy issue? Does PPA work as well for, say, national defense as it does for local traffic problems? What is the mechanism that promotes greater levels of cooperation in PPA? Any decision or policy recommendation that comes out of a PPA process is just as likely to create losers as well as winners; this is an unavoidable characteristic of government decision making. There seems to be a general assumption in PPA that participation itself will promote consensus, or at least greater levels of acceptance. Yet there is considerable evidence that citizens are not yearning to participate, and that when they do, disagreement does not disappear (Hibbing and Theiss-Morse 2002).

It is not even clear that such methods would be more democratic. A panel of citizens that sits for an extended period of time (deLeon 1997, 111, has suggested a year), informing itself about a particular policy issue, distinguishes itself from fellow citizens by the same characteristics post-positivists find troubling about technocratic policy analysts. They become, in effect, policy experts, experts whose informed judgments may differ significantly from those of the public at large (deliberative polling results provide empirical evidence of this possibility). Arguably this leads straight to the very problem that post-positivists are trying to address: elites making decisions on behalf of the public. Beginning with a random sample of citizens does not guarantee its ultimate policy judgment will reflect a consensus that the public will support, anymore than it guarantees its policy recommendations will effectively address the targeted problem.

When it comes to differences between the rationalist and post-positivist camps, there are clearly strengths and weaknesses on both sides. It is our view that the rationalist project, at least thus far, does the better job of identifying problems, probabilistically assessing the likely effects of alternative responses to those problems, identifying the impact of the alternative chosen, and systematically assessing how and why policy changes. It also has the most practical analytical tools. The reasons supporting this perspective are detailed at length in other chapters in this book (but see Chapter 9 for serious limitations). The rationalist project, though, has failed miserably in its effort to separate values from facts,

and post-positivists are quite right to point out that any notion that the rationalist project can make political decision making less political is, to put it mildly, highly unlikely. Post-positivist approaches embrace the messy, perspective-driven political realm of policy and use the values of the stakeholders as the lenses to examine problems; the relative worth of proposed solutions; and the process of deciding, changing, or implementing policy; as well as assessing what a policy has actually done. In short, the post-positivists provide a considerably richer picture of politics in policy studies. The problem with this approach is that it is comparatively more difficult to put into practice and it is harder to assess what the end result really means.

Is it possible to find and build from some common ground between these two approaches? Perhaps. post-positivists, at least for the most part, are not calling for things like the wholesale rejection of regression analysis in the field of policy studies. Rationalists, at least for the most part, recognize the importance of values and perspective. The problem is incorporating these acknowledgements into something that can be practically used as a way to study and understand public policy. Epistemology is something policy studies will struggle with for the foreseeable future. The scientific method and the generally positivist framework of the rationalist project is going to continue to be the primary means of gaining knowledge in the policy field. For all its flaws (and these should not be underestimated), it is still more practical than any alternative. Post-positivist criticisms of the mainstream approach will remain valid because they rightly force the field to continue examining the uneasy paradox of rationalist epistemology and democratic values.

Conclusion: Whither Policy Studies?

The central conclusions reached thus far are that policy studies has made and continues to make important and lasting contributions to our cumulative understanding of how the political and administrative world works, and that policy studies has struggled and continues to struggle with conceptual and epistemological difficulties that are a long way from being resolved. Efforts are also being made to take a more interdisciplinary approach to public policy in the hopes of providing a richer and more

powerful way of conceptualizing the way policy decisions are made (see Chapter 9). So where does this leave public policy as a field of study? Does such a thing really exist?

Based on our explorations throughout this book, we believe a strong case can be made for the affirmative. There is such a thing as policy studies and, at least in general terms, we can describe it.

Distilling the message of this book, we propose that the field of policy studies is the systematic search for answers to five core questions: 1) what problems does government pay attention to and why?; 2) what government response represents the most effective response to those problems and why?; 3) how are solutions chosen?; 4) how are those solutions translated into action?; and 5) what impact has policy made on those problems? Out of necessity, these questions demand a theoretical and methodological pluralism; there is no grand theory that ties them altogether (though some policy scholars have given this a pretty good effort, as typologies and the stages heuristic demonstrate).What clearly connects these questions, and the various domains of policy study they generate, is the problem orientation that powered Lasswell's original vision of the policy sciences.

Policy studies is also a field struggling with key conceptual and epistemological issues. Most notably there is a significant philosophical divide between rationalists and post-positivists, the former favoring objectivism and quantitative methodologies and the latter favoring subjectivism and qualitative methodologies. This rift is not really fatal to the field. The differences are no more serious than they are in most other social science fields. As members of both camps recognize the legitimacy of each other's claims, this is less a philosophical fight to the death than a difficult search for common ground.

Policy studies has a strong element of art and craft (as opposed to science), but this is to be expected in a field whose core research questions have such clear applied implications. Public policy is more than a mood, though. Perhaps it is not (yet) a science, but it can stake a legitimate claim to being a field of study.

Notes

1. The one potential exception to this that we can see is the use of the welfare economics paradigm in policy analysis. The welfare economics paradigm, though, is

obviously not a unique product of policy studies. And even here, there is strong resistance to using economics-based frameworks as a primary theoretical vehicle to answer questions of "what should we do?"

2. Kingdon's (1995) framework also owes a significant debt to bounded rationality.

3. We could not find a single citation in any major policy, political science, or public administration journal of the past two decades whose primary subject was defining the concept of public policy, much less one that proposed a conceptual distinction between politics and policy that justified a separate academic discipline to focus on the latter. Such articles may exist, but our search makes us confident that they are not a primary focus of policy scholars.

4. This may be another case of the sloppy and unclear labeling so characteristic of policy studies. We suspect many we have lumped under these classifications have rejected the titles, arguing that, for example, "empiricist" or "deconstructionist" were more accurate and descriptive of their particular perspectives.