IDEAS AND POLITICS IN SOCIAL SCIENCE RESEARCH

EDITED BY
Daniel Béland and Robert Henry Cox

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OXFORD
UNIVERSITY PRESS
2011

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Published by Oxford University Press, Inc.
198 Madison Avenue, New York, New York 10016

www.oup.com

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Library of Congress Cataloging-in-Publication Data
Ideas and politics in social science research / edited by Daniel Béland and Robert Henry Cox.
p. cm.
Includes bibliographical references and index.
ISBN 978–0–19–973643–0; 978–0–19–973687–4 (pbk.)
1. Political science. 2. Social sciences. 3. Policy sciences.
I. Béland, Daniel. II. Cox, Robert Henry.
JA76.I33 2010
300.72—dc22 2010002406

9 8 7 6 5 4 3 2 1

Printed in the United States of America
on acid-free paper

Preface

"What we need is a manifesto for ideas and politics, a book that surveys the literature and charts a direction for future research." This statement, uttered over lunch at a conference in Washington, D.C., was the start of this project. What followed was a typical back-of-the-napkin jotting of names of people we thought were among the principal figures in this emerging approach to research. It took us all of ten minutes to arrive at a list longer than we needed. And we expressly strove to identify people who were working in a variety of diverse fields of research. That we were able to compile the names quickly indicates how widespread the interest in the study of ideas has become. After the conference, we contacted everyone on the list and were pleasantly surprised when almost everyone enthusiastically agreed to contribute to the project. Evidently, we were not the only ones who craved such a book.

The idea was to examine the broad collection of literature that addressed ideational approaches. This literature is vast and exhibits a broad variety in terminology, foci of study, and levels of analysis. In part, this is because ideational approaches have settled into a number of disciplines and subdisciplines of the social sciences. In some of these subdisciplines, ideational approaches are new, being toyed with by scholars who seek to move beyond rationalist or structuralist approaches. In other subdisciplines, ideational perspectives are well established but seem in recent years to have become the dominant approach to research—all the more reason for a book that examines the phenomenon and offers an argument for what this trend means for social science research. Yet we needed to be selective, so we focused primarily on policy-related research. This is still a broad field that spans many subdisciplines of sociology and political science. But it does not include some of the other subdisciplines where the study of ideas has also been prominent. Moreover, even when focusing on selected issues and subfields, we could not feature all of the ideational approaches available in the literature. Yet we hope that scholars working in those other fields will find the statements we make here useful to their fields of inquiry.

This project began as a set of sessions organized at the 2006 and 2007 annual meetings of the American Political Science Association. Most chapters in the book were first presented on panels at one of these conferences. Since that

time, we have engaged in a few lively rounds of critiquing one another's essays and formulating a series of statements about the role of ideas in political life. Our message is simple: ideas are at the core of political action. They shape the way we understand interests, are the inspiration for the construction of political and social institutions, and are the currency of our discourse about politics. Of course, the message isn't so simple. This book is our exploration of the complexities of the subject.

Putting together this volume was one of the most exciting intellectual projects in which we have ever participated. Our contributors were highly committed to it, and we must thank them all for their hard work and the insightful comments they provided us and one another. The pleasure of working with them increased when Dave McBride of Oxford University Press threw his support behind the project. At Oxford University Press, we also thank Alexandra Dauler and the members of the manuscript production team. Because thier anonymous reports really helped us improve this volume, we thank the reveiwers for their comments and suggestions. Thanks also go to Phill Jo and Albert Schilthuis, graduate students at the University of Oklahoma, who provided invaluable editorial assistance. Over the years, we had countless discussions about the role of ideas in politics with dozens of scholars and students we met at conferences or in the classroom. A list of their names would be too long here, but we do thank them collectively for their insight. As this volume illustrates, scholarly work is a collective endeavor, and research is like the politics of ideas: a constant debate among people who often disagree on core issues but keep the conversation going, as it is part of what makes us full human beings living in a democratic society. Finally, we would like to thank our wives, Angela and Luz-Eugenia, who provided much intellectual and personal support as we worked on this project.

Daniel Béland and Robert Henry Cox

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Ideas and Politics in Social Science Research

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Introduction

Ideas and Politics

Daniel Béland & Robert Henry Cox

Across the social sciences, ideas are increasingly recognized as major factors in politics. One could go so far as to say, as we do in this book, that ideas are a primary source of political behavior. Our contention here is that ideas shape how we understand political problems, give definition to our goals and strategies, and are the currency we use to communicate about politics. By giving definition to our values and preferences, ideas provide us with interpretive frameworks that make us see some facts as important and others as less so. In turn, this has serious consequences for how we understand the role of interests in politics. Instead of seeing politics as the contest among people who have clear and stable interests and develop strategies to pursue them, this book develops a vision of politics as the struggle for power and control among people who are motivated by myriad ideas. These might include their perceived interests but also their ideals, their pride, their fears, and so on. In addition, the ideas people share in their communications with those around them inform not only their belief in what they want but what they deem to be appropriate, legitimate, and proper.

What, then, are ideas? Despite the attention to the subject, there seems to be ambiguity and disagreement about basic definitions. For us, ideas are causal beliefs. This simple definition involves many discrete dimensions. First, as beliefs, ideas are products of cognition. They are produced in our minds and are connected to the material world only via our interpretations of our surroundings. Our minds can create ideas from any of a multitude of sensory perceptions, or the mind can create ideas based on no connection to reality at all. How else could we know and believe in things we cannot see or touch? Second, as causal beliefs, ideas posit connections between things and between people in the world. These connections might be causal in the proper sense, such as suggesting that one event was responsible for bringing about a series of successive events. But ideas can be causal in more informal ways, by drawing connections between things or people that we believe are related to one another.

Finally, causal beliefs, or ideas, provide guides for action. Ideas help us to think about ways to address problems and challenges that we face and therefore are the cause of our actions.

Ideas are abundant in political life. To cite one example, as we were writing this chapter, many universities in the United States were celebrating the sesquicentennial of the publication of Charles Darwin's *On the Origin of Species*. Many of these celebrations prompted raging debate among contending ideas about the origins of species and even the meaning of existence. Within this debate, material facts, expert opinion, and the collective consensus of the scientific community supports the belief that natural selection, as outlined by Darwin, posits a compelling explanation for the evolution of life on earth. By contrast, and primarily in the United States, another group argues that intelligent design or an omniscient being, rather than random selection, guided the evolution of species, while yet another group asserts that creationism, or the idea that a monotheistic deity created all forms of life roughly as outlined in the biblical book of Genesis, fits better with their desired interpretation. In this debate, what the relevant facts are is in dispute. All of the theories are ideas in the sense that that they represent cognitive orderings of the material world, and all posit causal relations that guide people's decisions and preferences. Yet the fact that there is no single truth that allows us to choose one among these interpretations opens space for politics.

People can hold impossibly reconcilable beliefs, and these can outline different strategies to guide their lives. The result is that they will reach consensus with people who share similar beliefs and will run into conflict with those who have dissimilar beliefs. Sometimes conflicts will be resolved; sometimes they will not. And it will not always be the "true" ideas that win. Politics, as we know, is a struggle in which might does not always side with right.

If this general outline of the role of ideas rings familiar, it is partly because we write this book at a time when ideational approaches have become more prominent across the social sciences. For example, constructivist approaches to international relations, as well as ideational perspectives in comparative politics, American politics, political economy, and policy analysis, acknowledge the central role ideas play in shaping political outcomes (Abdelal, Blyth, and Parsons 2010; Albrekt Larsen and Goul Andersen 2009; Béland and Hacker 2004; Berman 1998; Berman 2006; Bhatia and Coleman 2003; Blyth 2002; Carstensen forthcoming; Checkel 1993; Cox 2001; Culpepper 2008; Fischer 2003; Genieys and Smyrl 2008; Gofas and Hay 2010; Goldstein and Keohane 1993; J. Hall 1993; Hansen and King 2001; Jacobs 2009; Jenson 1989; Kay 2009; Lieberman 2005b; McNamara 1998; Mendelson 1998; Parsons 2003; Philpott 2001; Schmidt 2002a; Schmidt 2002b; Schmidt and Radaelli 2005;

Seeleib-Kaiser and Fleckenstein 2007; Skowronek 2006; Skogard 1998; Stone 2007; Taylor-Gooby 2005; Walsh 2000; Weir 1992; White 2002; Yee 1996). Beyond these political science subfields, sociologists are paying renewed attention to the impact of ideas, culture, discourse, and framing processes and their effects on politics, political economy, social movements, and policy making (Anderson 2008; Béland 2005; Benford and Snow 2000; Camic and Gross 2001; Campbell 2004; Cruz 2005; Dobbin 1994; Mehta 2006; Padamsee 2009; Pedriana and Stryker 1997; Somers and Block 2005; Steensland 2006).

This book brings together scattered pieces of knowledge and offers social scientists a critical assessment of the theoretical and empirical issues raised by the ever-expanding social science literature on ideas and politics. Ranging across several fields of political science and sociology (American politics, comparative politics, international relations, political economy, political sociology, and public policy), the book documents central themes in the burgeoning literature on ideas and politics, while offering critical analyses to guide future social science research. We pay considerable attention to showing how ideas shape political behavior and outcomes and how focusing on ideas provides richer explanations of politics.

There is a clear theoretical, indeed philosophical, approach at the core of this project. Often labeled constructivist, this theoretical approach is dynamic rather than linear and iterative rather than mechanical. Ideas are constantly in flux, being reconsidered and redefined as actors communicate and debate with one another. Political action is motivated by ideas, but the goals people articulate and the strategies they develop have feedback effects that further shape their original ideas. The first half of the book explicates the theoretical, epistemological, and ontological dimensions of this focus on ideas.

The second half demonstrates how this perspective broadens the number of empirical questions we can answer. By focusing on ideas, we can explain, for example, why people adopt positions that contradict their apparent material interests, how political coalitions that bring together people with competing interests can be formed in the first place, and how policy ideas are produced, disseminated, and reproduced.

Interdisciplinary inquiry defines the path that led us to these answers. A key feature of this book is that it bridges distinct streams of scholarship about ideas and politics that are seldom discussed together in existing social science literatures. The lesson is that the most fruitful discoveries are made when scholars seek out and incorporate the insights of those working in other fields, rather than working diligently to perfect their understanding within a small field of inquiry. The result is a systematic assessment of the literature, provocative theoretical claims, and a broad research agenda for the social-scientific analysis of ideas and politics.

STREAMS OF IDEATIONAL RESEARCH

The study of ideas has a long and curious history in the social sciences.[1] On one hand, ideas are central features of the social and political world. People produce them, develop them, disseminate them, and consume them. This is especially true of scholars, for whom ideas are the coin of the realm. Yet as objects of scientific inquiry, ideas have held a beleaguered status, often derided as imprecise or placed lower in status than material interests as motives for political and social action. For much of the past half-century, this has led to the denigration of the study of ideas in favor of material interests or institutions (routine rules, conventions, and practices; Parsons 2007, 70) to explain why people do the things they do. The reason for this could be traced to two important trends in the social sciences. The first was the rise of behaviorism, which derided interpretive methods of inquiry for being less empirically rigorous than deductive methods. The second was the resurgence of neo-Marxist modes of inquiry that, though more receptive to interpretive methods, were sharply dismissive of nonmaterialist explanations of human action. The study of ideas was caught in the middle and fell victim to these trends.

Since the late 1980s, however, ideational analysis has moved back to the center of major social science debates about politics, and a consensus has emerged about what ideas are. Ideas are causal "beliefs held by individuals or adopted by institutions that influence their attitudes and actions" (Emmerij, Jolly, and Weiss 2005, 214). As John Campbell (2004) suggests, these ideas can take the form of high-profile public frames, discourses, and ideologies at the foreground of the political arena (Schön and Rein 1994; Schmidt 2002b) or constitute lower-profile assumptions and paradigms that often remain at the background of this arena (P. Hall 1993). Types of ideas commonly discussed in the social science literature on politics include "policy prescriptions, norms, principled beliefs, cause-effect beliefs, ideologies, shared belief systems, and broad worldviews. They can thus range from quite specific, concrete, programmatic ideas (for example, abolition of nuclear weapons) to broader, more general ideas" (Tannenwald 2005, 14) central to ideologies such as communism and liberalism.

Several factors explain the growing prominence of ideational analysis. First, the decline of behaviorism and system theory and the related emergence of "new institutionalism" during the 1970s and 1980s gradually increased the profile of ideational factors in political analysis. At the time, this "new institutionalism" crystallized in three main approaches: cultural, historical, and rational choice institutionalisms (Campbell 2004; Hall and Taylor 1996; Lecours, 2005). As Vivien Schmidt argues here in chapter 2, a number of scholars associated with these approaches gradually turned to ideas to explain specific phenomena that such approaches could not explain alone. This is particularly striking in the interdisciplinary field of political economy, where

scholars such as Mark Blyth, John L. Campbell, and Peter Hall have bridged new institutionalism and ideational analysis (Blyth 2002; Campbell 2004; Cox 2001; P. Hall 1993; Peters, Pierre, and King 2005; Schmidt 2002a).

Second, in political sociology, ideational analysis made a comeback by moving beyond system theory and by reevaluating the role of culture in politics (Dobbin 1994; Pfau-Effinger 2005; Mehta 2006; Padamsee 2009; Steensland 2006; Steinmetz 1993, 1999). Interestingly, even some scholars tied to the Parsonian tradition now support an empirically grounded form of cultural and ideational analysis that focuses on shared beliefs and public narratives (Smith 2005). Beyond this surprising intellectual twist, the ideational turn in political sociology is ever present in social movement research, where a growing number of scholars point to the central role of framing processes in contentious politics and social mobilization (Johnston and Noakes 2005; Oliver and Johnston 2000; Snow et al. 1986). Stressing the key role of ideologies and discursive frames in mobilization processes, this perspective on social movements breaks from neo-Marxist and other materialist approaches to mobilization. Finally, beyond social movement research, political sociologist Brian Steensland (2006) has recently attempted to rethink the role of culture in politics through a detailed analysis of the impact of "cultural categories" on policy development. Overall, the analysis of culture and framing processes is a prominent aspect of the contemporary sociological research on the politics of ideas.

Third, as Jal Mehta shows here in chapter 1, the field of policy analysis witnessed the development of new scholarship on issues such as agenda setting and problem definition, which helped move ideational analysis back to the center of policy studies. Beyond the English-speaking world, a number of French policy scholars have emphasized the role of ideas in policy processes (Jobert and Muller 1987; Merrien 1997; Palier and Surel 2005; Surel 2000).[2] Elsewhere in Europe, a number of researchers are exploring the role of ideas in public policy (Albrekt Larsen and Goul Andersen 2009; Braun and Busch 1999; Seeleib-Kaiser and Fleckenstein 2007; Taylor-Gooby 2005). For example, British political scientist Desmond King (1995a; 1999) has applied ideational frameworks to understanding the question of "American exceptionalism." More recently, in the United States, Frank Fischer (2003) has promoted a constructivist approach to policy analysis that stresses the role of ideas and discourse in policy making. Finally, one of the burgeoning topics in comparative policy analysis is the study of transnational policy diffusion, policy borrowing, and lesson drawing (Bennett 1991; Dolowitz and Marsh 2000; Orenstein 2008; Rose 2004; Stone 2008; Weyland 2005). Here, the main focus is on understanding how policy entrepreneurs draw from policy ideas adopted abroad and apply them to solving similar problems in their own countries.

Fourth, the field of international relations witnessed the development of an increasingly influential constructivist school that emphasized the structuring

role of ideas, discourse, and culture in foreign policy (Bull 1980; Checkel 1993; Epstein 2008; Haas 1990; Katzenstein 1996a and 1996b; McNamara 1998; Parsons 2003; Parsons in this book, chapter 6; Philpott 2001; Wendt 1999). A central theme in this literature is the demonstration that shared ideas constitute many of the things that scholars of international relations study. For example, the Cold War was in part an idea that caused leaders to act in distinct ways and created expectations about how other leaders would act. The study of these shared ideas, therefore, requires attention to the interpretive frameworks held by actors, not only to the circumstances they face.

Fifth, a new generation of political theorists and historically minded political scientists reclaimed the concept of ideology, which had long been associated with historical materialism (Berman 2006; Berman in this book, chapter 5; Freeden 1978; Freeden 1996). One of the merits of this new take on ideology is to move beyond a materialist vision of ideology grounded in the simplistic and misleading dichotomy between infrastructure and superstructure central to the Marxist tradition. Beyond these particular subfields and disciplines, the renewed interest in the role of ideas in politics is probably related to broad trends such as the development of interdisciplinary forms of "discourse analysis" across the social sciences and the humanities (Jorgensen and Phillips 2002).

Finally, students of politics focusing on racial and gendered identities and inequalities have made a direct contribution to the analysis of ideational processes (Bleich 2003; Jenson 1989; Lieberman 2002; Lieberman in this book, chapter 10; Mahon 2006; Padamsee 2009; White 2002). For example, the feminist literature on politics and policy features ideational perspectives that demonstrate how ideas and discourse are embedded in gendered beliefs and inequalities (for an overview, see Béland 2009; Padamsee 2009). A special issue of the journal *Social Politics* recently explored the contribution of gender research to ideational policy analysis (Orloff and Palier 2009).

COMMON GROUND AND ONGOING DEBATES

Common Themes

Despite these distinctions and the variety of approaches and topics, we see three distinct preoccupations in the new scholarship on ideas that are the central themes of this book: the relationships between ideas and institutions, ideas and interests, and ideas and change. Take the case of institutions, for example. This book underscores two related points about ideas and institutions. First, ideas are embedded in the design of institutions. This is already a well-established notion in the literature on ideas and institutions. For instance,

scholars have explored the role of ideas in explaining institutional change (Béland 2005; Blyth 2002; Campbell 2004; Cox 2001; P. Hall 1993; Peters, Pierre, and King 2005; Schmidt 2002a; Somers and Block 2005; Weir 1992). And in chapter 7 of this book, Daniel Wincott's examination of child care and early childhood education demonstrates how optimistic ideas about the impact of education on life chances led policy entrepreneurs in many countries to a flurry of new institutions and policies. Moreover, the chapters on the politics of expertise (John Campbell and Ove K. Pedersen, chapter 8; Andrew Rich, chapter 9) underscore the mutually reinforcing effect of ideas and institutions on the production of policy knowledge and the development of think tanks as influential policy institutions.

From this perspective, ideas are the foundation of institutions. As ideas give rise to people's actions and as those actions form routines, the results are social institutions. The ideas then are enshrined in the institutions. As people interact with institutions, the founding ideas are reproduced. Through repeated interaction with institutions, people are confronted again and again with the founding ideas. This confrontation can serve to reinforce and reproduce these ideas. To borrow a notion from David Easton (1953), institutions engage in the "authoritative allocation of values." Consequently, institutions do more than establish routines that rational individuals must negotiate; they also nurture people's identities, helping them to construct their fundamental values, which, in turn, shapes their beliefs and interests.

Although ideas are embedded in institutions, those ideas might not always be coherent. Numerous policy ideas lend themselves to many, sometimes conflicting, interpretations. Equality, for example, can be interpreted by some as requiring formal or procedural equality, while others might understand equality to apply in a material sense. This distinction is at the center of the difference between social democratic and neoliberal ideas about the purpose of welfare assistance. Ambiguity and incoherence in ideas opens space for politics as people seek to make policy decisions reflect their preferred interpretation.

Ideas, therefore, afford power to actors, and when the ideas are embedded in institutions, they also institutionalize, even legitimize, power differentials. Sometimes power differentials legitimize the oppression of specific populations, as was the case in the South of the United States during segregation. Studying institutionally embedded ideas can help explain the reproduction of inequalities and asymmetrical power relations. Because these institutionalized ideas participate in the definition and the reproduction of group boundaries and inequalities, the study of these ideas can directly contribute to our understanding of power and domination. As suggested above, this is a major issue that students of gender and race have long addressed (Fraser 1989; Jenson 1989; Lieberman 2002; Orloff and Palier 2009). The politics of ideas can

intersect with institutional struggles that participate in the making and the unmaking of political and social inequalities, which does not mean that ideas simply reflect such inequalities.

Moreover, ideas provide elegant linkages between institutions and political processes. In chapter 2, Vivien Schmidt outlines how ideas are the currency for discursive political processes. Discourse begins among people who hold different opinions and interpretations and who learn and refine their ideas as they share them with others. Viewing politics as a discursive process means that it is not a mechanical process whereby actors formulate a goal, devise a strategy to achieve the goal, and struggle with others as they employ their strategy. Rather, drawing on existing cultural and ideological symbols, actors develop a set of ideas and share them with others, who may challenge these ideas and provide some alternatives. The discursive interactions prompt them to refine, reframe, and reinterpret these ideas. Not only is this iterative and sometimes contentious discourse in play between actors, but it also informs the evolution of political institutions. The ideas upon which institutions are formed are also subject to discursive revision as actors reinterpret and debate the meanings of the ideas upon which existing institutions are constructed. The ideas that define institutions, as well as the ideas shared by political actors, are in flux, often at odds, and malleable.

A second major theme worked out by the contributors to this book is a specific notion of the relationship between ideas and interests, namely, that interests are one form of idea. From Marxism to rational choice theory, the orthodox, materialist view of interests is not compatible with ideational analysis. Materialism tends to consider ideas as mere epiphenomena that simply reflect underlying, supposedly objective economic interests. Considering the strong influence of the materialist tradition on social science research, ideational scholars have spent much effort distinguishing theirs from the materialist tradition. This is what Colin Hay does in chapter 3 on the relationship between ideas and interests. Colin Hay's assertion is that interests are not objective facts but are historical, social, and political constructions.[3] This constructivist vision of the ideas-politics nexus challenges the traditional vision of "material interests," which sees ideas as instruments used by actors to promote their interests.

Ideational perspectives do not reject the notion of interests, but they do differ fairly substantially from materialist perspectives on the questions of where interests come from and whether they are objective and fixed. Regarding the first question, ideational scholars believe that interests must be defined by actors before they become "real." The emphasis here is generally on the subjective interpretation of interest by a political actor (Hay 2004). This stands in contrast to materialist approaches that see interests as having an objective existence independent of the actors who hold them and as being powerful

enough to determine the actions people take. For materialists, cognitive processes have little significance other than to lead one to the discovery of his or her interests. For ideational scholars, by contrast, cognition is a process of interpreting the world, not simply discovering it. Human cognition, therefore, has its own independent force, and the ideas our mental processes create as we interact and communicate with others have much power over our decisions and actions.

On the latter point, ideational scholars have a different understanding of how interests change. For materialists, people's interests evolve as changes in their circumstances alter their situations. Ideational scholars, by contrast, assume that interests change as actors alter their understanding of their changing world and recalculate their priorities. The notion that ideas are cluster concepts, as outlined by Mark Blyth (2002), helps us understand this. As clusters, ideas embrace thoughts, emotions, and desires, as well as interests, all in delicate and fluid balance with one another. Changing emotions, especially in response to new ideas and circumstances, help us to reassess, and possibly alter, the manner in which we perceive our interests. This could be simply a strategic calculation of advantage, which would be consistent with a materialist explanation. But, as Hay suggests in chapter 3, it might also involve a fundamental reassessment of priorities, perhaps even of identity.

This point leads to the third theme of this book. Explaining political and policy change stands as a major concern for much of the scholarship on ideas, and we assert that ideational explanations are richer than other explanations of change. Besides the issue of incremental change discussed in Wincott's chapter 7, a key challenge for many approaches to the study of politics, and especially of public policy, is to account for radical, path-departing change (Weyland 2008). For institutional as well as rationalist explanations of politics, path-dependent change is assumed to be the norm, while radical change often is treated as exogenous.[4] Radical change happens when external forces disrupt the institutional stability and the balance of interests that keep the political system in a state of equilibrium. It is a reaction to changing, objective circumstances (Pierson 2000; for a critical discussion, see Streeck and Thelen 2005). By contrast, the ideational approach we outline in this book problematizes radical change as a response to new perceptions and ways of thinking. It might be reactive to changing circumstances, but it often is a proactive effort by political actors to reexamine their surroundings, reconsider their positions, and develop fresh new approaches.

Election campaigns provide a simple illustration of this. Opposition parties rarely campaign by promising modest change or perpetuation of the status quo. Their promises of a new direction for their government are rarely phrased in terms of material interests. Certainly, interests are part of their appeals, but they couch their appeals in new ideas, fresh perspectives. On the other hand,

incumbent parties frequently promise to maintain the government's current direction and carefully steer future change in incremental directions. Yet the outcome of the election does not always determine whether change will be radical. If the incumbent government is reelected, it might still pursue radical change. Or if the opposition wins, it might not be able to carry out the radical reforms it promised. An explanation that highlights the ideational originality of an opposition campaign promise is richer than one that reduces all rhetoric to interests.

This focus on change identifies ideational perspectives as agency-centered. What things change and how they change are all the result of what people choose to do in response to the world in which they find themselves. To be sure, things happen in the world, but politics is about how people interact with the world and with one another. There is no politics without human agency. The nature of change, whether it is slow or rapid, radical or incremental, is largely the result of choices people make. The unique claim of ideational scholars is that these choices are shaped by the ideas people hold and debate with others. These ideas, in turn, are based on interpretations people have of the world and of those around them. There is a material reality, but it lends itself to many interpretations that open endless options for human agency. For this reason, the outcomes of any process of change are contingent. They are not predetermined and cannot be predicted.

Drawing lessons from experience is an important part of agency.[5] Robert Lieberman's chapter 10 outlines how actors who desired to pursue racial equality developed an appreciation of targeting once they realized that their programs to promote blind equality had pernicious effects. Beyond this example, lesson drawing is an important theme in comparative studies of public policy (Dolowitz and Marsh 2000; Rose 2004). Within this literature, policy ideas are the central object of inquiry as scholars seek to trace how policy ideas adopted in one country are appropriated by policy makers in another country (Stone 1999). This literature recognizes myriad motivations for such "policy borrowing," ranging from a desire to solve a similar problem to an aspiration to emulate the policy profile of an admired country (Cox 1993). To say that all examples of lesson drawing stem from a calculation of interest forces one to embrace an awkwardly broad definition of interest. On the other hand, to say that all cases of policy borrowing are inspired by ideas, some of which have cynical content, does not require an awkwardly broad conception of an ideas yet allows for many analytical distinctions between ideas inspired by interests, ideology, or aspiration, for example.

The ideational perspective outlined in this book assumes that people develop sets of ideas to make sense of the world. These ideas guide our actions and shape our interactions with others. Shared ideas lend themselves to routine practices that give rise to institutions. Then lesson-drawing processes

reshape ideas, exposing and sorting out the tensions among competing ideas. Such processes offer lots of opportunities for conflict, misinterpretation, miscommunication, deception, and duplicity, as well as cooperation, enlightenment, and resolution. At the core of politics is the way ideas are packaged, disseminated, adopted, and embraced. The muddle of politics is the muddle of ideas.

Unsolved Debates and Tensions

In addition to these shared problems and themes, this book outlines debates and tensions that remain unsolved. The most significant of these is epistemological, because the ontology of the ideational perspective is fairly clear. In ontological terms, the basic tenet of the ideational perspective is that the world is socially constructed:[6] ideas form the foundation of this construction and are often the inspiration to act. But epistemologically, this raises a number of thorny issues. Ideas cannot be seen and are sometimes hard to track down. How, then, do we know that they have strong impacts on political behavior and outcomes? The answer to this question is that we know that ideas are essential when we can identify an idea and trace its influence on a particular political outcome. This relies on a few epistemological findings. It requires the utterance of an idea. Actors must utter "blind equality" (Lieberman in chapter 10) or "social democracy" (Berman in chapter 5), to mention two of the ideas examined in this book. Then these ideas need to be linked to a specific outcome, which also can be rigorously measured. The idea must capture the attention of actors who advocate for it and successfully use it to influence the observed outcome. A similar logic can be employed to examine ideas that fail. For example, Jacob Hacker (1997), in his study of President Bill Clinton's failed health-insurance proposal, examines the rise and fall of the idea of "managed competition," which never passed the legislative test and disappeared from the agenda after the proposal's defeat. The title of Hacker's book, *The Road to Nowhere*, illustrates the fate of this once popular idea.

An epistemological tension arises when we ponder whether ideas are central to political processes if no one utters them. This is an important concern when the ideas under investigation are part of a broad political ideology or public philosophy. As demonstrated by Jal Mehta in chapter 1 and Sheri Berman in chapter 5, the great advantage to this level of analysis is to draw attention to the "big picture." For example, the concept of ideology, associated for a long time with the Marxist tradition, becomes far more compelling when stripped from its materialist underpinning and the related distinction between the superstructure and the infrastructure so central to this tradition (Freeden 1996). This is exactly what Berman does in her analysis of the development of social democratic ideology in Europe. But studying political ideology in this way still

allows for the investigation of a concrete utterance. After all, political parties are eager to explain their ideology, especially at election time. Yet the concept of a cognitive lock, which Berman employs to explain how ideology can lead people to ignore certain types of policy solutions, is not measurable. Cognitive lock is one of the leaps of faith for ideational scholars.

As far as public philosophies are concerned, the concept of zeitgeist that Mehta puts forward poses a similar epistemological challenge. Sometimes a zeitgeist has a clear origin in the discourse of political actors. More often, its utterance is made by a researcher, who identifies the zeitgeist as a way to understand and interpret an event or a series of events. This is different from identifying an idea that prompted an individual or a group to act. Frequently, the zeitgeist is only clear in retrospect, and it is often difficult to say that the actors involved were consciously aware of their zeitgeist. The use of terminology is not inconsistent, but, rather, it demonstrates that ideational research operates at different levels of abstraction. To accept the significance of a zeitgeist requires a recognition that there are unconscious ideas at work in people's minds—or, at least, that not all ideas are conscious to every actor. This is not a challenging notion to understand, and, as Daniel Wincott shows in chapter 7, it is possible to use the concept of zeitgeist as an analytical tool in more concrete empirical studies. However, this concept might not be satisfying to many researchers whose epistemological frameworks are more empiricist and who believe that we can only make meaningful statements about things we can "see."

On this point, different opinions can be found in the literature. For example, in Craig Parsons's account of European integration (2003), agency and the autonomy of actors who frame new ideas are explicitly related. From this angle, ideas are the product of the agency of political actors who have the power to think and rethink the world in which they live. Implicitly, this vision of ideas as agency is challenged by some of the approaches reviewed in this book by Schmidt and Mehta.

To resolve this tension, social scientific research should strive to identify the ideas people use and show their effect on political processes and outcomes. The historical impact of a zeitgeist offers a challenge, because often the actors cannot be directly interrogated. This increases the demands on data collection, as alternative sources of corroboration must be uncovered and systematically compared, but it is basically an empirical rather than a methodological problem. A thing to avoid is imposing an order on the historical events that would not have been familiar to the actors themselves. The challenge is to distinguish between interpretations that would ring familiar to the original actors and interpretations that are used by the investigator to frame a contemporary debate. To put this in concrete terms, when sociologist T. H. Marshall (1964) argued that postwar welfare states favored the expansion of liberal rights beyond existing civil and political rights, his historical framing influenced what social

policy meant for several generations of students and policy makers. One of the consequences is that the policy debate about welfare policies became quickly about "social rights" (Ashford 1986). Also, when scholars such as Louis Hartz (1955) identified a dominant liberal tradition in the United States, they helped put powerful ideas into the minds of their students and others who read their books. To the extent that such understandings are meaningful to those readers, the ideas will have an effect on their actions. A zeitgeist operates at a different level of abstraction and consequently involves the observer as actor in its construction, but its dynamic is similar to the ideational dynamic we generally observe in political processes.

Another unresolved issue raised in this book concerns the status of ideas as either *explanans* (the explanation) or *explanandum* (what needs to be explained). On one hand, as stated above, this book provides strong evidence that ideas can become major causal factors that help explain major political processes. From this perspective, ideas can explain much on their own. On the other hand, it is clear from several chapters here that powerful institutional factors can constrain the production and the dissemination of ideas (Campbell and Pedersen, chapter 8; Rich, chapter 9). The same remark applies to the work of Vivien Schmidt (2002a and 2002b) on policy discourse, which shows that institutional frameworks create constraints and opportunities for political actors who disseminate such discourse. These examples show that although ideas and discourse might become powerful causal factors in their own right, other types of social and political forces can shape their production and the conditions under which they can affect behaviors and outcomes (Beland 2009; Orenstein 2008; Paclamsee 2009; Walsh 2000).

THE METHODS OF IDEATIONAL ANALYSIS

We argue that ideas are complex but not that they are incomprehensible. The type of ideational analysis we outline in this book lends itself to—indeed requires—empirical analysis. Half a dozen chapters illustrate what rigorous, empirically grounded ideational analysis should look like (Berman, chapter 5; Parsons, chapter 6; Wincott, chapter 7; Campbell and Pedersen, chapter 8; Rich, chapter 9; Lieberman, chapter 10). These authors pay close attention to concrete historical, political, and social processes that have little to do with the stereotypical image of "idealism" associated with the Hegelian tradition. Ideational analysis is not "idealist" in the traditional sense of the term, and it is not afraid of making causal arguments, either. Yet, as Mark Blyth suggests in chapter 4, ideational analysis necessitates the formulation of more subtle causal arguments grounded in revised assumptions about order and change that help grasp the complexity of the political processes under study. Consequently,

ideational analysis is both a challenge and an attempt to perfect traditional social science research on politics.

The chapters in this book suggest innovative methods for examining the role of ideas. For example, Craig Parsons (chapter 6) shows how case selection can allow one to isolate ideas in a process of change. His study of European integration focuses on actors who should have been staunch advocates of their own countries' national interests as they created the supranational institutions that became the European Union. What he discovered was that these actors built alliances around sets of ideas that were more important to them than the national interests they supposedly represented. In chapter 10 on race politics in the United States, Robert Lieberman explores the methodological issues stemming from the analysis of the potentially full interaction between ideas and institutions in political development. His empirical analysis shows how to "weave together two explanatory modes whose analytical vocabularies and technologies have largely diverged." Another key methodological issue addressed in this book is the question of how to study rigorously ideational processes such as political ideologies. This issue is directly addressed in Sheri Berman's contribution on the social democratic ideology (chapter 5), which offers a clear example of process tracing without which it would be impossible to understand the rise and the fall of political ideologies. Her chapter is a powerful reminder that ideas can have a long history and that tracing their development over time is a key aspect of ideational analysis.

In future research, more variety in research design would help to develop some of these new conceptualizations. Much of the current scholarship has involved single- or two-country studies that examine a single idea or political outcome to establish ideas as necessary conditions for that political outcome. This has involved selection on the dependent variable. Scholars look for cases in which ideas shaped specific political episodes and then examine how they came to shape them. While selection on the dependent variable is a useful and valid method for drawing inferences about necessary conditions (Dion 1998), there are still many skeptics who view this type of research design with disfavor. More research that selects cases on the independent variable would be worthwhile both to satisfy critics who target the dominance of a single research design as the reason for dismissing ideational perspectives and to give ideational researchers a greater confidence in their inferences, as well as a greater variety of issues and topics to examine.

Furthermore, more variety in comparative design would also be bolstered by more variety in the methods employed to investigate the role of ideas. The most ambitious scholarship seeks to verify its findings through multiple methods of analysis (Putnam, Leonardi, and Nanetti 1994) or the use of statistical as well as interpretive methods to examine the same question. When multiple methods reach the same result, our confidence in the finding is greatly

enhanced. Because the field is dominated by narrative, interpretive methods, the greatest challenge for research on ideas is to employ more statistical and quantitative methods. This will require examination of problems at different levels of analysis. Very little quantitative research into ideas has been attempted, because most ideas that have been examined are not repeated occurrences. The typical design that examines a single idea and traces its influence to a single outcome does not generate enough data points to allow statistical examination. Questions posed in this way lend themselves to qualitative methods of analysis. Here, much could be learned from King, Keohane, and Verba (1994), who suggest that reliable inference can be drawn from qualitative research by shifting the level of analysis to one that lends itself to statistical examination.

One of the rare ideational contributions in this regard is a recent study by Jeffrey Chwieroth (2007), who tested the role of neoliberal ideas in the changing policy priorities of the International Monetary Fund (IMF). To develop data, Chwieroth examined the educational backgrounds of key staff of the IMF. His hypothesis was that the ideas of key staffers would be formed during their years of university education. Neoliberal ideas can be traced to departments of economics at eight universities whose professors were the architects of neoliberalism. The neoliberal turn within the IMF, therefore, should correspond with the increase in the number of key staffers who hold degrees or otherwise were associated with these academic institutions. By casting the study at the individual level of analysis and examining an easily measurable indicator, Chwieroth was able to draw statistically reliable inferences. More such research, especially if combined with qualitative research in an effort to "triangulate" its findings, could increase our confidence in the robustness of ideational perspectives.

In short, the study of ideas has many fruitful directions in which it can go. The theoretical and empirical issues outlined in this book provide a firm foundation for research on ideas, and they offer many suggestions for research that others can do to expand and enrich this line of inquiry.

ITINERARY

This book systematically explores major theoretical and ontological issues raised by the recent turn to ideas (part I) before illustrating the diversity of the subfields and research areas in which this turn has taken place (part II).

The main objective of the first two chapters is to map major bodies of literature dealing directly with the role of ideas. In chapter 1, for instance, sociologist Jal Mehta offers a timely review of the existing literatures on ideas, politics, and public policy. For him, the claim that ideas matter has increasingly become common wisdom in social science research. After suggesting

what would happen if scholars failed to acknowledge the role of ideas in politics and public policy, he convincingly demonstrates that the major question to investigate for social scientists working on politics and policy is "less *whether* ideas matter than *how* they matter."[7] Then, in chapter 2, Vivien Schmidt explores the relationship between ideational and discourse analysis, on one hand, and various forms of new institutionalism, on the other hand. She makes the case for a discursive institutionalism, concerned with ideas and discourse, because it has the greatest potential for reconciling existing institutionalist approaches.

As stated above, ideas are typically understood in relation to interests, an issue explored by Colin Hay and Mark Blyth in chapters 3 and 4. The aim of Hay's contribution is to explore the relationship between ideas and institutional change, particularly the kind associated with disequilibrium dynamics. As for Blyth, he examines why, where, and how we know that ideas matter by drawing on work about uncertainty and probability, as well as evolutionary theory.

It is important, as suggested above, that ideational analysis has little to do with Hegelian "idealism," as it is dealing with concrete actors and political processes, and it can lead to rigorous causal arguments and empirical research. The chapters in part II stress the variety of the subfields and substantive research areas to which ideational analysis can make a direct contribution to empirical knowledge. Through the analysis of highly contrasted case studies, these six chapters offer major methodological and theoretical insight about issues ranging from the construction of political ideologies to knowledge regimes and comparative research design.

A major debate in the ideational literature concerns the relative importance of structural forces and country-specific factors in determining the rise and fall of political ideologies. Sheri Berman argues in chapter 5 that this is a false dichotomy: understanding the evolution of ideologies requires looking at both. Using the rise of social democracy during the late nineteenth and early twentieth centuries as a case study, Berman shows how the massive changes that rocked European societies during this time led many to question existing political ideologies and search for new ways of understanding and responding to the rapidly changing world around them. As she shows, the precise nature of political battles and their outcomes varied greatly from country to country, indicating that one needs to examine local political contexts and actors to understand fully the evolution of political ideologies.

As suggested above, the field of international relations has considerably enriched the ideational literature. In chapter 6, Craig Parsons turns to this field to make a direct contribution to the debate on the role of ideas in politics. He demonstrates that both in broad historical and comparative perspective and in the details of EU history, it is strange to posit that people in the modern world

of nation-states could adopt extensive supranationality without deep shifts in meanings and rationality.

Students of public policy also have emphasized the impact of ideas on policy change. Daniel Wincott explores this issue in chapter 7 by focusing on public early childhood education and care provisions in Australia, the United Kingdom, and the United States. Engaging with the concept of zeitgeist formulated in Mehta's contribution, he draws attention to the issue of incremental change and the need for a more direct dialogue between policy analysis and institutionalist perspectives on ideas. As Wincott shows, ideational analysis can make a strong contribution to the analysis of policy making and concrete forms of policy development.

Likewise, in chapter 8, John L. Campbell and Ove K. Pedersen draw on comparative political economy to explore the comparative politics of expertise. According to them, comparative political economy in the 1970s and 1980s was largely about variation in the institutional arrangement of political structures, or what we might call policy regimes (e.g., liberal, statist, and corporatist). Campbell and Pedersen argue that what is missing is a comparable discussion of "knowledge regimes"—the institutions through which policy-relevant ideas are generated and percolate into the policy process in ways that affect economic performance. They address this omission by introducing and developing the concept of knowledge regimes and comparing knowledge regimes in liberal and coordinated market economies.

Students of American politics are increasingly entering the debate about the ideas-politics nexus. Taking a detailed look at the role of think tanks in American politics, Andrew Rich's chapter 9 complements the above discussion about knowledge regimes by taking a more concrete look at the production of policy expertise. Rich examines how public-policy think tanks package and present their ideas and research to inform—and influence—policy makers. In particular, he analyzes how the greatly expanded number of nonprofit think tanks have deliberately organized in ways to cater to demands from policy makers for supportive rather than original material.

The study of race relations in the United States helps shed new light on the role of ideas in what is known as American political development (APD). Adopting an APD perspectives, in chapter 10, Robert Lieberman explores the politics of race in the United States. According to him, this politics inherently involves both conflicts of interests (disputes over the allocation of power and resources) and conflicts of ideology (clashing commitments to color-blindness and various forms of race-consciousness). Conventional accounts of American race politics and policy have been typically oriented around one or the other of these axes. Lieberman proposes an alternative approach to explaining race policy making that combines both institutional and ideational elements in a single, comprehensive explanatory framework, and he demonstrates this

approach through a case study of the rise of affirmative action in the United States. His approach is compatible with Schmidt's attempt to bridge these two streams of literature.

As this book demonstrates, the study of ideas offers a comprehensive and cohesive focus for political analysis. The ideational perspective outlined here describes, explains, and enlightens our understandings of numerous political issues. We hope that the book will serve as a road map for those who would employ ideational approaches in their research and that it will give encouragement to those who already do.

NOTES

1. Considering the limited space available, this volume only engages with some of the ideational perspectives available in the scholarly literature. For instance, it does not revisit the "history of ideas" debate to the work of philosophers and political theorists such as Leo Strauss (1953), Quentin Skinner (1978), and Pierre Rosanvallon (2003). More generally, although this volume focuses on two social science disciplines (political science and sociology), it is crucial to note that scholars from the humanities and professional fields such as education and law have contributed to the literature on the role of ideas in politics, even when they do not explicitly write on the topic. "As for cognitive psychology, it is increasingly influential in economics and policy analysis (Thaler and Sunstein 2008) as well as in political science (Weyland 2008). Yet, as Craig Parsons (2007) persuasively argues, scholars must draw a clear analytical line between ideational and psychological explanations before combining them, when possible. As its title makes clear, this volume focuses on ideational factors." Finally, to return to the two disciplines at the center of this volume, some political scientists and political sociologists interested in ideational and discourse analysis have drawn extensively on the work of social theorists Michel Foucault (1980; 1989) and Bruno Latour (1987). A lot has already been written about these theorists, and we do not focus here on their work and that of their many followers.

2. Bruno Jobert and Pierre Muller (1987) are described as the founders of what is known as the French school of ideational analysis. A recent volume in English takes a critical look at their approach, which is centered on the concept of "référentiel" (Genieys and Smyrl 2008 for an overview in French see Faure, Pollet and Warin 1995).

3. For other perspectives on the construction of interests, see, for example, Jenson (1989), Steensland (2006), Stone (1997), and Wendt (1999).

4. A major issue here is how to define *change* (Capano and Howlett 2009; Weyland 2008).

5. On "lesson drawing" as applied to comparative policy research, see Rose 2004.

6. The idea of a "social construction of reality" is central to the sociological tradition (Berger and Luckmann 1966).

7. For a different perspective on this issue see Jacobs 2009.

Part I

THEORY

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1

The Varied Roles of Ideas in Politics

From "Whether" to "How"

Jal Mehta

The role that ideas play in politics has long been appreciated more by the newspaper reader than by the average political scientist or sociologist. Dismissed by Marxists and other materialists as a mere smokescreen that powerful actors use to mask their interests and seen by rational choice theorists as, at best, focal points for rationally self-interested actors (Bates, de Figueiredo, and Weingast 1998), ideas have begun to be taken more seriously in political analysis during the past twenty years (Stone 1988; P. Hall 1989; P. Hall 1993; Skrentny 1996; Skrentny 2002; Dobbin 1994; Rueschemeyer and Skocpol 1996; Berman 1998; Lieberman 2002; Steensland 2006; see Campbell 2002 and Berman 2001 for reviews). While Heclo introduced the idea that policy makers "puzzle" as well as "power" in 1974, it has been only recently that scholars have begun to examine in detail the various ways in which ideational factors can interact with interest group, electoral, or state-centered accounts (Weir 1992; Skrentny 1996; Zollars and Skocpol 1994).

Of course, cultural analyses of politics have not always been in such disfavor—one only needs to think of Weber's famous "switchman of history" metaphor. But for much of the postwar period, cultural explanations have been very much on the wane, for reasons that are worth an essay of their own. One reason for the anti-idealist turn was a reaction against Parsons's unified theory of culture, provoking a conflict school of political sociology that emphasized inequalities in power and the power of interests. Another was the growth of techniques for quantitative modeling and the associated emphasis on methodological individualism. The emergence of rational choice theory and the virulent antiempiricism of postmodern approaches to studying culture further weakened the position of those who would conduct empirical analyses of politics employing ideational variables (see J. Hall 1993 for one attempt to make sense of these changes).

IF IDEAS DID NOT MATTER . . .

For these reasons, scholars of politics who work with ideas have, until recently, taken a largely defensive posture, seeking to establish that "ideas matter" in an academic terrain that privileges neo-Marxist, structuralist, or rational choice modes of explanation. But while the idea of "ideas" may seem foreign to scholars working in other traditions, the notion that ideas matter is one that almost all of us routinely accept as we both participate in and think about everyday social and political life.

Consider what it would mean to assert that ideas did *not* matter. (To be precise, I define an idea to "matter" when it (a) shapes people's actions and (b) is not reducible to some other nonideational force.) At the broadest level, asserting that ideas do not matter would mean that shifting ideals about science, religion, democracy, slavery, colonization, gender, race, and homosexuality, to pick just a few salient examples, either have not appreciably affected how people act or were themselves the product of technological, economic, or other material forces.[1] Shifts of ideas widely considered foundational to Western civilization, such as the spreading of Enlightenment thought, would similarly have to be either irrelevant or reducible to other forces. Entire disciplines, such as intellectual history, would be obsolete. School boards and college committees would stop fighting about the curriculum, and movements to ban books would dry up. The tens of millions of dollars that are spent on think tanks to churn out ideas and public relations firms to market them would be largely wasted. The billions of dollars spent on market research and on advertising would be unnecessary. Consultants of all types, as purveyors of ideas about how to improve practice in different fields, also would be largely out of business. In politics, people would vote exclusively out of material self-interest. There would no longer be anything the matter with Kansas, and limousine liberals in Cambridge and Berkeley would become antitax crusaders. Job seekers would not choose careers because of their meaning; college students would stop trying to find themselves. Terrorists would not blow up buildings out of visions of religious glory or ideological triumph. There would be no such thing as a thought leader or a visionary.

One could imagine a critic's reply, but such a reply would likely have to concede some role for ideas. A critic might acknowledge that, yes, ideas matter, but in many cases they matter only instrumentally as means to ends that have been predetermined by other structural or self-interested reasons. A critic might acknowledge that ideas matter in the most fundamental way—as all of our social institutions are, in a sense, the embodiment of ideas—but claim that ideas offer little analytic purchase in explaining why a person, group, or polity did X instead of Y. A critic might acknowledge that the Enlightenment brought about considerable shifts in our views on science and religion but argue that

the shifts were themselves products of broader economic or technological forces. These are all debates that are worth having, but I would submit that given the ubiquity of ideas in all facets of everyday life, the burden of proof lies with the critics to show that ideas do not matter, rather than with proponents to show that they do.

IDEAS AND POLITICS: FROM "WHETHER" TO "HOW"

Given these realities, the key questions for scholars interested in ideas are less *whether* ideas matter and more *how* they matter.[2] And here is where social scientists have much to contribute, even working in areas that have been extensively plumbed by other disciplines such as intellectual history that have long incorporated ideas into their analyses. By specifying what kinds of ideas serve what functions, how ideas of different types interact with one another, how ideas change over time, and how ideas shape and are shaped by actors' choices, social scientists can provide greater analytic purchase on the question of exactly how ideas matter. In recent years, scholars in the field have begun this move from "whether" to "how," and I build upon many of these insights below.

Of course, specifying how ideas matter is still a considerable task, even if we restrict ourselves to politics, as this book does. Ideas, broadly defined, are central to questions about agenda setting, social movements, revolutions, diffusion, policy choice, the conceptual categories that underlie politics, path dependency and path-shaping change, institution building, institutional stability, institutional change, voter identity formation, interestgroup formation, and political coalition building. While much previous work in the domain of "ideas and politics" has been conducted by scholars of the welfare state, over the longer term, a thorough discussion of the topic would include more voices from other subfields of political science and sociology and would also incorporate work from cognitive psychology, linguistics, and even neuroscience.

My task here is more limited though still daunting in its potential scope. I am interested in policy ideas per se, by which I mean ideas of varying levels of generality that define how policymakers should act. Building upon and drawing together the best work in the field, I seek to offer a synthetic analysis of how ideas matter in politics: what is known, what is not known, and what areas are in need of further research.[3]

Drawing on Kingdon (1984) and others, I consider ideas at three levels of generality: policy solutions, problem definitions, and public philosophies or zeitgeist.[4] While *problems* and *solutions* are familiar terms in some parts of the literature, the broader notion of public philosophies or zeitgeist has been less frequently discussed. I also consider interactions between the levels of ideas,

with a particular interest in "upward-flowing" interactions, showing that not only does the conception of a problem constrain policy alternatives, but the fate of specific policy solutions also can have an impact on problem definitions or even broader public philosophies.

Within each of these categories, I seek to explore why some ideas triumph over others. While the literature is ripe with case studies that show that a particular idea was important in explaining specific outcomes, there has been much less work that has tried to discern systematically what differentiates victorious ideas from their rivals.[5]

Through answering these first two questions—what kinds of ideas matter and why some ideas win out over others—I also seek to address a critically important question that has received insufficient attention in the broader welfare state literature: considering the potential universe of policy options, why does policy take the specific form and content that it does? While this would seem as if it should be a central question of political science and political sociology, it has often been ignored by broader theories that are more concerned with the constraints that bound choice than with the content of the choices themselves. As Béland and Hacker write of one such theory, historical institutionalism: "the institutional perspective is considerably more instructive as an explanation of the *prospects* for policy reform than as an explanation of the specific *form* that policy change takes. If the question is merely why the American welfare state is 'smaller' or 'less developed' than European welfare states then it may be enough to cite America's distinctive framework of political institutions. But if the question is why the American welfare state has taken the structure that it has, then systematically unpacking the forces that shape actual policy choices seems unavoidable" (2004, 45). While historical institutionalism and other leading theories (such as interest groups or rational choice) offer accounts of the forces that govern policy making, a theory of ideas is needed to explain the content of policy choices.

Three caveats are in order. First, this is still an emerging field, and offering firm conclusions at this point would be premature. As a result, my discussion contains as many questions as answers, and I devote space to identifying areas and questions that the next generation of research could seek to address. Second, to make this sizable task more manageable, I draw most of the examples from the field I know best, American domestic politics, although I also incorporate some of the most well-known comparative research on ideas. Third, a comprehensive approach to addressing the above questions would need to include a more explicit theory of action, seeking to identify not only the types of ideas that matter but also how ideas affect actors and how actors affect ideas. I discuss these issues in passing, but they are worth an essay of their own (see Hay, chapter 3 in this book).

THREE KINDS OF IDEAS

Although scholars often talk about "ideas" as if they were one concept, there are at least three different levels of ideas that are relevant to understanding the policy process. In the narrowest conception, ideas can be *policy solutions*. Keynesianism is perhaps the most famous policy idea; other obvious examples are smaller class sizes or broken-windows policing. The implicit assumption here is that the problem is given (business cycle is too volatile, test scores are too low, crime is too high), the objectives are given (stabilizing the business cycle, raising test scores, lowering crime), and the idea provides the means for solving the problem and accomplishing those objectives. But as many scholars have pointed out, problems and objectives are not preestablished (Rein and Schön 1977; Schön and Rein 1994). To understand this process, we also need to understand the roles ideas can play as *problem definitions*. A problem definition is a particular way of understanding a complex reality. Homelessness, for example, can be seen as the product of a housing shortage, high unemployment, or a lack of individual gumption. The way a problem is framed has significant implications for the types of policy solutions that will seem desirable, and hence much of political argument is fought at the level of problem definition. Finally, ideas can function as *public philosophies* or as *zeitgeist*. These are broader ideas that cut across substantive areas. A public philosophy is an idea about how to understand the purpose of government or public policy in light of a certain set of assumptions about the society and the market (Heclo 1986). That the local government is more attuned to the needs of the people than the federal government is one such public philosophy. A related idea is the zeitgeist, which is a set of assumptions that are widely shared and not open to criticism in a particular historical moment. The zeitgeist includes a disparate set of cultural, social, and economic assumptions, which might not be as closely related to the purpose of government as a public philosophy. Keynesian economics in the years between the end of World War II and the early 1970s was an idea that had reached the level of the zeitgeist; the idea of holding people accountable has a similar status today. When public philosophies are in open contest, as is usually the case, neither has the status of zeitgeist, but when one emerges as overwhelmingly dominant (as in the New Deal), public philosophies can for a short time become the zeitgeist. These untouchable assumptions obviously have a broad influence on politics (and society) for the period in which they reign.

Dividing ideas into these three analytic categories allows us to ask questions about how each kind of idea works in politics and to think about interactions among the different levels of ideas. We will consider each of the types in turn and then together.

IDEAS AS POLICY SOLUTIONS

Policy solutions are both the narrowest conceptualization of the role that ideas play in politics and the most theoretically developed. The key question for these purposes is why some policy ideas become policy while others do not. The work on ideas and policy solutions differs from the more general political science task of explaining why policy choices are made, in that the analysis is tied to the properties of the ideas themselves. I consider three prominent models: Peter Hall's view that successful ideas combine policy, political, and administrative appeal; John Kingdon's view that policy ideas succeed when entrepreneurs link them to "problem" and "politics" streams; and the work of historically inclined scholars who argue that prevailing ideas are shaped by the contours of past policies.

Building on a cross-national corpus of work on the adoption of Keynesian policies, Hall (1989) points to three factors that are critical in the adoption of a policy idea: policy viability, administrative viability, and political viability. He implicitly suggests that for Keynesianism to be adopted, all three conditions must be met, and he notes that when one of these three is not in place, such as when the Treasury Department was strongly opposed to Keynesianism in interwar Britain, the policy was not adopted. In particular, Hall's work discredits the naive or functional view that the intrinsic worth of the idea in solving the problem (policy viability) is sufficient for a policy to be adopted. Other scholars who have studied the role that research plays in policy making have reached similar conclusions (Weiss 1977; Lindblom and Cohen 1979; Weaver 2000). In part, this is because researchers cannot offer the strong causal relationships that policy makers desire (although they often overpromise to deliver just that), and thus the predicted efficacy of a policy intervention must be considered along with a variety of other political and normative considerations (Rein and Winship 1999). Scholars working in other areas have also stressed the importance of administrative feasibility (Evans, Rueschemeyer, and Skocpol 1985) in domains as diverse as affirmative action (Skrentny 1996), deregulation of airlines and trucking (Mucciaroni 1992), and school standards (Murphy 1990). Particularly in comparison with debates about problem definitions or public philosophies, debates about policy ideas are more concrete and thus subject to considerations of cost and administrative feasibility.

Hall's third factor, political viability, is so obviously important that it needs to be specified further to be analytically useful. Under one scenario, political viability can simply mean the policy idea with the strongest interest groups backing it, in which case, the policy ideas themselves seem to be making little of an independent contribution. In a second scenario, an idea that has a strong policy rationale might triumph over an array of entrenched interests, as happened in the deregulation of the airline industries (Derthick and Quirk 1985).

Here, political leaders such as Jimmy Carter decided to champion an idea that was supported by research to the broader public in an effort to win electoral support. On issues of high visibility, policy entrepreneurs sometimes are able to triumph over concentrated interests when armed with a plausible idea that has potentially widespread benefits.

A third scenario that mixes the two is perhaps the most intriguing: a new policy idea creates its own backers, either by forging new coalitions or by causing groups to see their interests in a different and new way (see also Schmidt 2002b, 169–170). This might be an active process, paralleling Schattschneider's (1960) observation that the most effective strategy for the losers in any political debate is to expand the scope of the conflict to bring in more of the uninvolved players on the previously weaker side. Here, interests and ideas are not really in tension and are difficult to disentangle, because some of the most promising ideas will draw strong partners willing to back them (Blyth 2002). Overall, Hall's model effectively highlights three of the most important determinants of idea adoption, although he leaves largely unspecified the mechanisms by which these determinants come together to affect policy outcomes.

A second model for thinking about the role of ideas in the policy process comes from the seminal work of Kingdon (1984). Kingdon's question is "What makes an idea's time come?" and he answers that three critical streams must come together: problems, policies (solutions), and politics. The political stream—which includes factors such as the "national mood," the composition of Congress, and major events in the news—functions to open policy windows, and then savvy policy entrepreneurs step into these windows by linking favored solutions to current problems (see also Cohen, March, and Olsen 1972). For an idea (in the form of a policy solution) to succeed, it must fit within the prevailing political winds and have an energetic, well-connected, and (ideally) powerful person or group pushing it, and it must be perceived as a viable solution to an existing problem.

Kingdon's model makes two distinctive contributions to understanding the role of ideas in politics. First, by emphasizing the role of policy entrepreneurs, he makes a specific claim about how active agents use ideas to make policy. Second, he borrows from the earlier garbage-can models to show how solutions often precede the problems that they are supposed to solve. Support for Kingdon's model is widespread, and despite criticism, he remains the touchstone for any theoretical discussion about the role of ideas in the policy process (Mucciaroni 1992).

The work of historically oriented scholars adds another important dimension to understanding how some ideas come to be favored over others. These scholars emphasize how policies develop over time and rightly note that a focus on a short period of time (usually the present) can function to obscure variables that are constant over that period but would not be constant over a

longer period (Pierson 2003). Scholars interested in ideas have adapted historical institutionalists' emphasis on path dependence to encompass the "path dependence of an idea," arguing that long-standing models such as the Scandinavian welfare state model can continue to affect action and discussion, even as underlying circumstances change (Cox 2001). Taking the branching-tree nature of the path-dependence model seriously would suggest attention to the following two areas. First, it would urge attention to the role that ideas play in the "critical junctures" that are central to creating diverging paths. The "effects" of a given idea are likely highly context-specific (Skrentny 1996); therefore, any responsible investigation of the role of ideas with regard to a particular policy would require a careful reconstruction of the role that the ideas played at the time key decisions were made. Second, a historical approach would suggest attention to the way in which policy legacies feed forward in the evaluation of later decisions. This learning process can take place among elite policy makers (Heclo 1974), but it can also inform the positions taken by social groups that, in turn, are influential actors in later policy decisions (Pierson 1993). While the image of the branching tree might not apply quite as well to the world of ideas as it does to the material world—the repeated failure of universal health insurance in the United States did not, in the end, foreclose the possibility of greatly expanding health care in 2010—past policy legacies can affect the perceived legitimacy of particular policy options (Dobbin 1994).

STUDYING IDEAS AS POLICY SOLUTIONS: NEXT STEPS

Work to date has done an admirable job in discrediting the naive technocratic or functional view that ideas that are adopted by policy makers are chosen simply because of their ability to solve a policy problem. But while this literature has enlarged the stable of theories about which policy ideas are selected and why, this phase of theory generation is only the first step in understanding how policy ideas matter in politics.

One obvious next step is to try to develop some generalizations about how the processes by which ideas are chosen vary across issues, time, and space. In terms of variance across issues, one might expect that issues that were highly visible, such as schooling or welfare reform, would be more likely to be publicly championed (as with airline deregulation) than ideas that were highly technical or not immediately relevant to the public. In terms of change over time, some have argued for a general trend away from iron triangles and toward more inclusive networks of actors (Heclo 1978); integrating more contemporary work in this vein into the literature on ideas and politics would be helpful. Finally, research suggests differences in how national traditions or past collective

experiences affect the way nations interpret diffusing ideas (Katzenstein 1996a). Cross-national differences are also likely to exist in how and where expertise is produced, what kinds of expertise is produced, and the relationship between these producers and government (Béland 2005, 8–9). Schmidt (2001) provides evidence that differences in the configuration of political institutions can produce different structures of discourse, ranging from broadly inclusive to largely delimited to key policy makers. National differences in how expertise is processed and incorporated (Campbell and Pedersen, chapter 8 in this book) should become part of more refined ideational theories.

Methodologically, one central weakness of the literature is the massive selection bias toward ideas that ultimately become policy. The consequence is that the literature has been limited to identifying necessary factors (such as Hall's three factors model or Kingdon's three streams), but without equivalent negative cases, there is no way to delineate sufficient factors (Harding, Fox, and Mehta 2002). This is not simply an academic issue; an absence of negative cases can also mean an inattention to the second and third faces of power (Bachrach and Baratz 1962; Lukes 1974), because the analysis is limited to what actually appears on the agenda. This is not a problem that should be intractable; future studies could identify a range of plausible possibilities at a given point in time and then seek to isolate the reasons some ideas were more successful than others or, over a longer period of time, try to understand why some options remained on the table and others were excluded.

Much work also remains to be done in specifying the processes by which some ideas come to be favored over time. One way to see this is as a two-stage process: in the first stage, an old idea comes to be discredited, and in the second stage, a particular new idea comes to be favored (Blyth 2002; Legro 2000; McNamara 1998). Thus far, there has been much more attention to the latter than to the former. To put it another way, Kingdon's question of what makes an idea's time come has now been quite thoroughly examined; the complementary question of when an idea's time is up has received comparatively much less attention. One could imagine that this could happen through an exogenous external event that called the previous consensus into question. Another possibility is that an idea's time could lapse more gradually, as advocates manipulated indicators, symbols, and ordinary news events to create the political space for a new idea (Campbell 1998). Kingdon talks about the process of "softening up" or paving the road for a new idea; there likely often needs to be a slow and steady "wearing down" of the old idea. At the same time, those who are proponents of the idea will actively try to rebuff such efforts and ward off any attempt at agenda and policy change (Cobb and Ross 1997).

Even once a void has been created, the process by which another solution comes to the fore needs to be further investigated. While concepts such as policy feedback have drawn our attention to how these processes play out over

time, they do not specify *which* lessons policy makers will draw from previous experiences, sure always to be a heavily contested process (Béland 2005). For every story about power politics and legislative conflict, there is a back story that explains which ideas came to acquire the prominence, legitimacy, and backers that they did. It is these periods that are largely out of sight from the point of view of legislative conflicts that should command our attention if we are interested in how the agenda is shaped over time.

At the same time, seeing this as a process with definitive stages—failure of an old idea followed by a period of uncertainty and then consolidation of a new one (Legro 2000; Blyth 2002; McNamara 1998)—has its own limitations as a way to understand the rise of new ideas. In many cases, what happens is not the collapse of an older policy but, rather, simply the rise of a new set of considerations that make a different set of policies appropriate for approaching the issue area. Baumgartner and Jones (1993) illustrate this process repeatedly, showing, for example, how nuclear power shifted from an economic issue to one of health and safety, resulting in a much different policy regime. While Legro (2000) theorizes that it is the unfulfilled expectations of a policy that provide the opportunity for change, it also can be the sense that the expectations themselves change as the problem comes to have a different definition.

This brings us to the greatest limitation of the literature on programmatic policy ideas, namely, that it takes the problems for which these ideas serve as "solutions" for granted. How problems are defined has a substantial impact on which alternatives are chosen (Rein and Schön 1977), and so to ignore problem definition is to miss much of the debate. Kingdon does have a theory of problem definition—policy entrepreneurs redefine problems so as to meet their prearrived solutions—but he gives less consideration to the other ways that the problem-definition "stream" might be shaped by politicians, advocates, social movements, and media elites, much of the time in the absence of actual policy solutions.

IDEAS AS PROBLEM DEFINITIONS

The way in which political problems are defined is its own field (Rochefort and Cobb 1994), one that has generated a diverse set of case studies but not much in the way of theoretical development. Scholars of problem definition reject the idea that political choices are simply the sum of individual, interest group or institutional preferences and instead offer a model of politics in which actors are fighting over how a policy problem or collective purpose should be defined (Reich 1988; Mansbridge 1994; Stone 1988; Rein and Winship 1999). In comparison with many of the models discussed in the section on ideas as policy solutions above, those interested in problem definition see actors (at least some

of the time) less as strategic wielders of ideas and more as possessors of taken-for-granted assumptions (Berger and Luckmann 1966) that influence the types of problem definitions and solutions that they favor (Schön and Rein 1994).

Separating out the battle over problem definition from the battle over policy solutions is a critical step in understanding policy development. The distinction between problems and solutions should be familiar given the prominence of Kingdon's early work, but it is too often ignored in practice as scholars conflate policy paradigms or problem definitions with actual policy choices, as in much of the work on Keynesianism (Béland 2005). Problem definitions define the scope of potential possible choices, but within a given problem definition, there are still often multiple choices for policy. For example, in my work on education, I argue that standards-based reform, public school choice, vouchers, and charter schools all fit within an educational problem definition that emphasizes improvement on test scores and school accountability (Mekta 2006). At the same time, the battle over problem definition is critical for understanding agenda setting, because once a problem definition becomes dominant, it excludes policies that are not consistent with its way of describing the issue.

There are three definitional issues that I should mention upfront, in order to situate this discussion in the existing literature. First, problem definitions are at the same analytical level as what Peter Hall calls "paradigms," in that they describe "not only the goals of policy . . . but also the very nature of the problems they are meant to be addressing" (1993, 279). I prefer to use the term *problem definition* in this context, however, because while paradigms tend to evoke the notion of a single dominant idea that governs an area, problem definitions evoke the fluid nature of constantly competing ideas that highlight different aspects of a given situation. Second, the process of defining problems is different from *framing* as the latter term has been used in the literature. A problem definition is similar to a frame in that it bounds a complicated situation by emphasizing some elements to the neglect of others, but *framing* has been mostly employed as a term to describe how to package a preexisting set of ideas to win more adherents to one's position (Béland 2005; Campbell 1998). Consistent with this usage, framing is one element in a broader battle over problem definition.[6] Third, while some scholars have insisted on the analytic separation of normative and empirical or causal ideas (Campbell 1998; Goldstein and Keohane 1993),[7] I follow Putnam (2002) in arguing against the fact/value dichotomy. Problem definitions generally evoke both normative and empirical descriptions in ways that are usually mutually reinforcing.

Key questions for understanding problem definition are (1) how political problems get defined and (2) why one problem definition prevails over another in a particular dispute. Since there is no grand theoretical synthesis that answers these questions (Rochefort and Cobb 1994), I draw eclectically from literature in the construction of social problems, problem definition, and agenda change.

How Do Political Problems Get Defined?

In contrast to approaches that see political problems as either (a) imposed by hegemonic elites or (b) a reflection of the "social psyche" of the public (Gamson 1990), I argue instead that (c) problem definition is a contested process among players with varying levels of power and persuasiveness. This view remedies many of the drawbacks of the other two. In comparison with the social psyche view, it specifies a role for active agents, allows for a diversity of views within the population, and explains how some of the many social problems become political problems. In comparison with the hegemony view, it allows for a wider range of groups and forces to influence debate, while not ignoring the role of power differentials among the claimants.

This approach has roots in the "social construction of problems" school (Blumer 1971; Spector and Kitsuse 1977), which sees problem definition as a negotiated process among claimants with various points of view. The social problem constructionists are interested in how areas that are "conditions" move from being taken-for-granted aspects of everyday life to social problems that are worthy of government attention. The emergence of a social problem usually followed a model of "naming, blaming, and claiming" (Felstiner, Abel, and Sarat 1980–81). "Naming" a problem meant, for example, to take the phenomenon of traffic accidents and label it as one of "automobile safety," as opposed to unsafe driving. "Blaming" was to identify the causal agent (the car manufacturers), and "claiming" was to suggest that the government should act to ensure automobile safety. In turn, those opposed to the policy will make counterclaims, in an attempt to deny the existence of the condition, reject the claim of causal responsibility, or shift the emphasis to other causes or remedies (Cobb and Ross 1997). Although the "emergence of social problems" paradigm is too restrictive to cover the wider question of how political problems get defined and redefined, the model's emphasis on claimants and counterclaimants tussling over responsibility, emphasis, and issue definition is useful and broadly applicable.

It may, however, overstate the strategic aspects of the process of issue definition. While the conflict view of problem definition is more realistic than the consensus or elite manipulation models described above, I want to note two important caveats to this discussion as it has proceeded thus far. First, purveyors of problem definitions might not be consciously aware that they are contributing to a struggle over problem definition. Media actors, in particular, might do much to further one definition of a problem over another, without knowing that they are doing anything beyond "objective reporting." Even advocates might not be aware that there is a struggle over "problem definition" going on and might simply take for granted their own assumptions about how to see the problem (Berger and Luckmann 1966). Second, while the competing claimants model implies that participants' commitments to particular frames are fixed, they are in

fact often fluid and malleable. If individuals or groups are not committed to a particular problem definition for other strong reasons (e.g., established material or professional self-interest), their understanding of an issue might shift in the course of debate.[8] With these caveats in place, however, it is still the case that trying to understand how a particular problem definition (or set of problem definitions) emerges from a process of multiple sets of actors advancing initially conflicting problem definitions is the most fruitful approach advanced thus far. Note that this analytic framework does not require that the actors be aware of their role in this process.

It is also worth making a further distinction between the process of problem definition in an arena in which a political decision is at stake (Congress, the courts, school boards, etc.) and problem definition where this is not the case (the media, the academic literature, casual conversation among individuals), because the process differs in each context. Where a political decision needs to be made, the fight will usually be over the policy itself. Problem definition is generally in the background; it enters into the discussion surreptitiously as each argument for or against the policy implicitly privileges one problem definition over another. In contrast, discussions in the media or in the academic literature are more often explicitly about how to define an issue, in part because the goal might simply be to explore different positions and in part because there is no imperative to reach a decision. Arguments about policy in a decision-making arena might take place about the implications of a policy *within* a given problem definition (is the proposed solution efficacious, affordable, administratively feasible, etc.), or it may take place *among* problem definitions, as opponents highlight considerations not captured by the initial framing. In either case, those who advocate for a policy that is ultimately adopted can be said to be victorious in the battle over problem definition as well, for their definition (or definitions) of the problem are the ones that have been adopted by the majority of the decision makers. Because my interest here is in politics, the discussion below focuses largely on decision-making arenas.

What Determines Which Problem Definitions Prevail?

The previous section establishes that to understand political problem definition is to understand how a given problem definition (or set of problem definitions) is chosen from a larger universe of potential problem definitions, but it begs the most interesting question: what determines which problem definitions prevail? I have identified six sets of factors: (1) the power and resources of the claimants, (2) how claimants portray the issues (framing), (3) the venue or context in which the problem is heard, (4) which claimants establish ownership over the problem, (5) whether there is a policy solution for a given problem

definition, and (6) the fit between the problem definition and the broader environment. Some of these factors imply substantial intentionality on the part of the actors; others do not.

1. *Power.* It almost goes without saying that the resources each "side" can bring to bear are critical in any fight over problem definition. But it is also true that the power of the sides should not be seen as entirely exogenous to the issue-framing process; the reframing of an issue can change the way the balance of forces is arrayed. Schattschneider (1960) points out that in any fight, there are many more that are unconcerned than there are mobilized on either side and that therefore it is to the advantage of the weaker side to find a way to frame the issue that will bring in more of the uninvolved bystanders on their side. Power is also not limited to material power; in other work we show that being seen as possessing "moral power" can be a critical asset in political struggle (Mehta and Winship forthcoming).
2. *Portrayal of issues.* The rhetorical strategies that claimants employ in advancing a given problem definition are also key to their success (Stone 1988; Campbell 1998). A short (not nearly exhaustive) list of what I see as the most important and interesting of these strategies is below. None of these strategies is required for successfully defining an issue, nor does any guarantee victory. An additive model in which the more such strategies one employs, the more likely one is to win one's definition of an issue seems likely accurate.[9]

    - Establishing *causality* is often the central axis upon which problem definition rests.
    - Citing favorable *numerical indicators* is an important (if obvious) aspect of lending credibility to one's claims in a world of claims and counterclaims.
    - *Effective story-telling* is crucial in simplifying a complex reality in a way that is likely to be favorable to one's definition of the problem.
    - Shifting the *burden of proof* is another favored strategy that those who begin as losers on a political issue can use to their advantage.
    - Whose *metaphors* are accepted is another critical battleground in the fight over problem definition. As Stone (1997, 148) has pointed out, "Buried in every policy metaphor is an assumption that if *a* is like *b*, then the way to solve *a* is to do what you would do with *b*."
    - Invoking symbolic *boundaries* (Lamont and Molnar 2002) is often an effective way of advancing one's claims in politics.
    - Tying one's definition of a problem to a widely accepted *cultural symbol or value* is another way to advance one's problem definition (Gamson and Modigliani 1987).

One caveat to this discussion, raised initially by Winship (personal conversation): The factors listed above are essentially static attributes about ideas or the way they are presented. Actual debates are determined in part by these factors but also by the order in which claims are presented and rebutted and, more generally, the rules that govern the public conversation. These factors could profit from greater elaboration.

3. *Context or venue.* Baumgartner and Jones (1993) have stressed that a key aspect of how favorably a claim is received is the context or venue in which it is heard. They point out, for example, that "an agriculture committee in Congress is more likely to view pesticides as a way of increasing farmers' profits, while an environmental group is more likely to focus on the negative health effects of the same issue" (Baumgartner and Jones 1993, 31). Since pesticides are in fact both helpful to farmers and harmful to the environment, which issue definition prevails is largely dependent on where the claims are heard. Advocates' efforts to steer school equity away from Congress and to the courts is one obvious example of shifting venues in the hope of getting a better hearing.
4. *Problem ownership.* The social problems literature has stressed that sorting out who can claim "ownership" over a social problem is an important factor in problem definition (Gusfield 1981; Schneider 1985). Fights over problem definition are also often fights over who has authority to define the problem and what kind of expertise is relevant. Schools are often such a battleground, where politicians, business leaders, teachers, parents, and community representatives all claim to have distinctive and authoritative expertise.
5. *Availability of policy solutions.* Kingdon (1984) suggests that successful problem definitions are accompanied by viable policy proposals. (This might be particularly true in a decision-making arena and less true in media and academic arguments.) The opponents of war in Iraq, for example, were substantially weakened because their proposed course of action—presumably a mixture of continued sanctions and tough diplomacy—was perceived at the time as having been tried and failed. Revelations that sanctions were in fact effectively controlling Saddam Hussein's pursuit of weapons were significant precisely because they offered opponents of the war an argument that their preferred policy solution would have been effective at controlling the threat.
6. *Fit between problem definition and environment.* Whenever a problem definition put forward by a political claimant resonates with that held by the public or the media, its chances of success are enhanced considerably. But which comes first—how the public thinks about a problem, how the media frames it, or how political actors define it? Causality obviously runs in every direction. Baumgartner and Jones's

(1993) examination of smoking and pesticides suggests that changes in media tone produce changes in political problem definition (and policy), and public opinion follows only later. Of course, those pushing for change (particularly outsiders) often try to use the media to make social claims or redefine issues, so there is no reason to see media framings as necessarily the first link in the causal chain.

EMBRACING COMPLEXITY: NEXT STEPS IN PROBLEM DEFINITION

Political problem definition is an extremely complicated process. Any of the elements that I have touched upon above could profit from further elaboration. Here, I confine my discussion to three aspects of problem definition that run through the entire literature and are systematically underdeveloped:

Actor agency and cognition. There is a tension about intentionality contained in much of the discussion above. It is clear that, in general, actors are shaped by available cultural repertoires and also try to act strategically within these constraints (Sewell 1992). But this doesn't tell us how problem definition really works. Should we understand problem definition as a fight, with clear sides, which at the end produces a victor? Or should we see it as a process in which one problem definition emerges from the morass? The first version assumes too much actor awareness and intentionality;[10] the second specifies no actors at all. The truth clearly lies in between, with some actors more aware than others of their role in the process of problem definition. At one extreme, Winship and Berrien (1999) describe a process of cops and ministers trying to figure out how to take action on the crime problem in Boston as one of experimentation and only retrospective sense making. A problem definition emerged as a way to construe actions already taken. At the other, Zollars and Skocpol (1994) describe the way the early Social Security Board consciously worked to define Social Security as "self-reliant individualism" in order to win public backing for the program. Which one of these is the more common pattern? Is problem definition a process that can frequently be consciously manipulated? How often does a problem definition emerge that no set of claimants was initially pushing? What factors make one or the other more likely? These questions go to the heart of how much control human beings have in shaping our future.

A related question has to do with whether "problem definitions" is too broad a unit of analysis. DiMaggio (1997) has urged sociologists to pay attention to the findings of cognitive psychologists that suggest that humans hold very fragmented and contradictory beliefs, which are much less unified than the concept of problem definitions might suggest. An exploration of how people construct broader problem definitions out of these smaller fragments and how

stable these broader problem definitions are in the face of different kinds of challenges would shed interesting light on this field.

Linking across problem definitions. Scholars of problem definition have generally focused on how a particular issue was defined, either at a critical juncture (Skrentny 1996) or over time (Baumgartner and Jones 1993), at the expense of studying how the framing of one issue interacts with the framing of other similar issues. There is clearly much "cultural borrowing" in problem definition (Skrentny 2002)—consider, for example, the way gay leaders seek to utilize the civil rights frame developed by black leaders. This is captured, to some degree, in the concept of "metaphor," but to label it does nothing to answer the critical questions: At a micro level, when is the borrowing of a frame likely to be successful and when not? How do we understand when boundaries are going to be drawn that limit the broader applicability of a metaphor? At the macro level, does "cultural borrowing" mean that metaphorically similar issues will travel in similar cycles, or is the simple fact of a rhetorical connection too tenuous for that?

A related point is that even within an issue area, there are likely to be multiple potential questions, each yielding its own set of conflicting problem definitions. For example, in education, there are fundamental questions about the purpose of schooling, about what should be taught, about what methods should be used, about how the school system should be organized, and about who should receive what kind of training. How one set of these questions is answered might affect the way others are likely to be judged. Analyses of issues that suggest that only one (binary) problem definition is at stake (e.g., Baumgartner and Jones's [1993] argument that the tone of media coverage on pesticides shifts from economic benefits to environmental dangers) are too one-dimensional to capture these kinds of complex interactions.

Integrating the social and the political. Finally, the split between contemporary sociologists focusing on the construction of social problems (largely in the media and outside the political process) and political scientists focusing on political problem definitions largely within the political process has had unfortunate implications for our understanding of political problem definition. Any thorough understanding of how an issue developed would need to include understanding changes in the societal background, changes in public opinion, and changes in media portrayal, all of which feed into, and are informed by, decisions in the political arena. Kingdon's description of how policy alternatives are largely debated among specialists misses the fact that the way specialists' understandings develop is influenced not only by technical considerations but also by these broader currents.

Consider, for example, the Supreme Court's 2003 decision on affirmative action. A simplified version of the story might run as follows: Initially prompted by the civil rights movement and legal changes in employment law, companies,

schools, and other institutions such as the military initially employed affirmative action either under bureaucratic pressure (the EEOC) or out of a desire to right past wrongs. Over time, these rationales faded, as EEOC pressure was minimized under President Ronald Reagan, and the passage of time made the righting of past wrongs seem a less urgent priority. But over time, these institutions saw organizational benefits to affirmative action: specifically, a diversity of viewpoints and the organizational legitimacy conferred by a diversity of personnel (Dobbin and Sutton 1998). These organizational benefits led these institutions to advocate for continuing affirmative action, even if the initial rationales were no longer compelling. The Supreme Court, in turn, looked out at this changed landscape and, quoting briefs from these institutions, upheld the diversity rationale of affirmative action. The problem definition that the Court (the political actor in this case) ultimately embraced was heavily informed by societal changes and elite opinion about the consequences of those changes. There is no way to understand this process if societal changes are seen as somehow "exogenous"; any complete accounting must link changes in society, politics, and the intermediaries that link the two.

IDEAS AS PUBLIC PHILOSOPHY OR ZEITGEIST

Ideas also matter at an even broader level, that of public philosophy or zeitgeist. Recall that I have defined a political philosophy as a view, often voiced by political parties, about the appropriate role of government given certain assumptions about the market and society, whereas the zeitgeist is a disparate set of cultural, social, or economic assumptions that are overwhelmingly dominant in public discourse at a given moment in time. Both public philosophies and the zeitgeist are widely influential and difficult to study, precisely because they are so interrelated with the dominant events of the moment. Explaining why there was a "revolt against big government" in the 1980s, for example, seems equivalent to explaining the origins of the tax revolt, the shifting approach of the Republican Party to racial politics, and the changing view in popular culture of Wall Street, to name just a few potentially salient factors. Perhaps for this reason, most scholars have simply modeled the "national mood" as an exogenous independent variable (Kingdon 1984) and have not made it the central *dependent* variable (but see Heclo 1986), although it does sometimes appear as an outcome variable in studies of communities (Rieder 1985), political parties (Baer 2000), social movements, or think tanks (Smith 1991). Journalists have been the authors of the most holistic efforts that I know of to make sense of the impact of social and political changes during the past thirty years in America on the prevailing public philosophies (Edsall and Edsall 1991; Dionne 1991). Given that there is not much scholarly literature to critique, I will limit myself here to a few

general comments on what seem to be the most promising avenues going forward on two questions: (1) how these meta-ideas shift over time, and (2) how these meta-ideas affect specific policy debates.

How Do Public Philosophies or the Zeitgeist Shift over Time?

The most prominent theories of shift in the national mood are cyclical (Schlesinger 1986; Hirschman 1982). These scholars have noted that American history cycles between liberal periods, in which the role of government expands and business is viewed skeptically, and more conservative periods, in which business is viewed more favorably and government significantly less so. In another variant, Huntington (1981) posits that America is characterized by a gap between its ideals and its institutions and that reformers periodically try to make the country live up to its ideals, only to be slowly thwarted, creating a vacuum into which the next generation of reformers steps. Scholars have also suggested that given that the underlying terrain of these battles has often shifted considerably, a more appropriate metaphor might be that of a spiral (Schlesinger 1986; McFarland 1991; Tyack and Cuban 1995)—each cycle brings the battle not back to where it started but to a similar point on a different dimension (or historical period).

Theories that invoke cycles or even spirals might be simultaneously too imprecise for prediction and too deterministic to allow for unanticipated developments. While spirals have the advantage of parsimony, more complete explanations would leave more room for indeterminacy and specify in more detail the mechanisms of change. As imprecise as it seems, explaining the emergence of a particular public philosophy or zeitgeist requires careful historical reconstruction and process tracing. This analytical strategy allows for the interplay of various material and ideational factors, seeking to chart how they were influential in the development of a set of meta-ideas and also to incorporate the possibility of creative agency on the part of those who helped to bring about the transitions. To take one well-documented example from the United States, the Democratic Leadership Council explicitly tried to recast the public philosophy of the Democratic Party more to the center after national election defeats in 1984 and 1988 (Baer 2000). These politicos were, in turn, influenced by their reading (influenced by media and academic intermediaries) of who voted, what they valued, and how these voters perceived the Democratic Party vis-à-vis these values. One could ask why the voters valued what they valued or why the media emphasized certain issues over others. As with all social science, this could become a process of infinite regress; the expertise of the researcher lies in part in suitably delimiting the analysis.

The process of explaining a shift in the zeitgeist is even more difficult, because it has no natural hook such as a political party, but the methodological process is similar. If one's question, for example, was how accountability

became such a dominant metaphor for framing policy, one would have to go backward in time to when that wasn't the case and trace it forward. One might use media citations or content analysis to understand where and why it emerged first, chart how it diffused across substantive areas, and then analyze the broader social changes that contributed to its rise.

How Do Public Philosophies or the Zeitgeist, Once Established, Affect Policy Choices?

If the previous section is about the public philosophy as a dependent variable (how one comes to reign), there are also interesting questions about it as an independent variable (how it affects subsequent political action). Within a given period, it is preferable to see a public philosophy or the zeitgeist not as a stream that sweeps up everything in its path but, rather, as one central input that has significant influence in various different aspects of the policy process. The stream metaphor is overdetermining; it fails to explain how *Brown v. Board of Education* could emerge out of the "conservative" 1950s or how the 2003 *Lawrence v. Texas* decision overturning the ban on homosexual sodomy could recently be affirmed in another relatively conservative period.

To be sure, there are similarities that run across policy areas within a given period. It is no coincidence that, for example, the indexing of Social Security benefits, proposals for an income floor, the Environmental Protection Act, and the Occupational Safety and Health Act all happened during the first Nixon term. This bunching occurs because meta-ideas affect the policy process in at least three ways. First, the prevailing public philosophies or zeitgeist can significantly affect who gets elected, which, in turn, affects what is considered and passed on the legislative docket. Second, a public philosophy can serve as a kind of meta-problem definition for political actors, providing a way of seeing the public issues that are on their docket. When a new issue arises, these meta-ideas provide a heuristic that tells political actors what aspects of the issue to emphasize and what side to take. Third, meta-ideas can function as a kind of changing cultural touchstone to which actors can appeal in their efforts to advocate for a particular policy or symbol. Similarly to central values such as liberty or equality, these meta-ideas (such as that markets are more efficient than government programs) provide a way for political actors to gain legitimacy on specific topics that for the audience (public or media) might be unfamiliar terrain. Overall, while this conceptualization suggests that the balance of power among public philosophies or the prevailing zeitgeist can have an important effect on (a) who is at the table, (b) how those actors think, and (c) the types of actions that will be seen as desirable or legitimate, it also leaves room for various other inputs into the policy process and for the emergence of ideas that run counter to the prevailing winds.

UPWARD-FLOWING INTERACTIONS AMONG IDEA TYPES

No chapter that lays out a typology would be complete without a discussion of the interactions among the types. It is relatively clear how more general ideas affect more specific ones. Public philosophies are meta-problem definitions that shape how more specific problems are defined, which, in turn, affects which specific policy ideas seem to be viable solutions to the newly defined problem. For that reason, I will focus here on "upward-flowing" interactions—how policy can affect problem definitions, how problem definitions can affect public philosophies or the zeitgeist, and, finally, how policies can affect public philosophies or the zeitgeist. These thoughts are illustrative, drawing on examples of how this has or might work; I do not claim to have a theory that would explain when these outcomes are likely.

Policies to Problem Definitions

Policies provide the battleground for fights over political problem definition, so the success or failure of a policy can make the accompanying problem definition look more or less desirable. The perceived failure of the Great Society programs, for example, makes future efforts to frame the poverty problem as one of federal government responsibility a tough sell. Current school-voucher experiments are being watched closely and their results heavily debated because they have broader implications about the role of choice in education. Policies assert their influence on problem definition when the perceived success or failure of a policy extends beyond the policies itself and is used to make conclusions about entire approaches of which the given policy is just one example.

Effective policies can also *expand* problem definitions, if they find a way to reconcile multiple goods that previously seemed incommensurable. Community policing provides one example. The primary role of the police, most would agree, is to prevent crime and apprehend those who commit it, but if this can be accomplished through means that do not result in the shakedown of innocent community residents, than this is preferable to more draconian methods. Successful community policing effectively expands the goal of policing to include both crime fighting and creating decent community-police relations; once this transition has happened, there is unlikely to be a return to the more narrow definition of policing, unless there is a perception that community policing has become ineffective at keeping the crime rate down.

Problem Definitions to Public Philosophy or Zeitgeist

While a public philosophy or the zeitgeist cuts across substantive areas, it draws its credibility from its interpretation of more specific problem areas. For example, the idea that markets should generally operate without government

interference is given a heavy blow by the recent failures of large banks. If the banking problem is defined as an aberration, than the broader public philosophy can remain intact; if it is seen as a systematic problem in the regulation of capitalism today, then the market-oriented public philosophy is dealt a significant blow. One could argue that the leading public philosophy at any given moment in time is the one that can best encompass the area-specific problem definition on the leading half-dozen issues of the day, although the reverse is obviously also true. It would be interesting to see if one could create a kind of tipping-point model, in which after a certain number of issues that are prominent in the media are inconsistent with the leading public philosophy, the public philosophy itself gradually changes or is replaced by another.

When actors actively set out to create a new public philosophy, they both utilize and are constrained by problem definitions of existing key issues. I mentioned above the role of the Democratic Leadership Council in recasting the public philosophy of the Democratic Party after electoral defeats in 1980 and 1984. In trying to remake the party, taking a tough stance on the specific issues of crime and welfare was perceived by these "New Democrats" as a kind of litmus test for a group trying to separate itself visibly from the failures of the "Old Democrats" (Baer 2000). These positions, in turn, helped to shape the emphasis on personal responsibility that marked various aspects of the New Democratic philosophy. While, of course, it is also true that the New Democrats highlighted these issues precisely because they thought the older Democratic public philosophy was insufficiently oriented toward personal responsibility, the relationship between issue-specific problem definitions and broader public philosophy is a reciprocal one.

Policies to Public Philosophy or Zeitgeist

The best policies literally remake the public philosophy in their wake. The most obvious example is Keynesianism; there is no better way to advocate for the importance of a mixed economy with substantial government intervention than to show that such intervention can successfully manage the ups and downs of the business cycle. By the 1960s, Keynesianism had achieved such a transcendent status. But when Keynesianism proved to be an ineffective remedy for stagflation in the 1970s, its legitimacy as a remedy was undercut, and the public philosophy of untrammeled capitalism was given a boost. It should be noted, however, that the idea that the government has some level of responsibility for the broader economy has remained—even libertarians justify their claims with the supply-side rationale that these economic policies will produce the greatest good for the greatest number. In this sense, Keynesianism is another example of how good policies can produce broader problem definitions that last beyond the policies themselves.

More commonly, policy outcomes have to be interpreted before they can pass into a public philosophy or the zeitgeist. Zollars and Skocpol (1994) show how the

Social Security Board very successfully played this role in transforming Social Security from a precarious start to the third rail of American politics. Of course, these processes are also open to counterclaims, and politically savvy actors might try to kill entire policies because of their potential consequences for the public philosophy. Right-wing strategist and pundit William Kristol did exactly this in the debate over health care in 1994. He warned his Republican colleagues that if middle-class Americans saw the benefits of a universal health-insurance program, the forces of "big government" might be revived for another generation.

CONCLUSION

In an age in which terrorism forcibly reminds us that some people radically disagree with many of the ideas that govern much of Western life, it is no longer possible to pretend that people are only motivated by an individualistic utilitarian rationality or that ideas are merely a cover for deeper interests. Social scientists, whose professional and often personal identities are largely staked on their ability to traffic in ideas, have ironically been remarkably resistant to the notion that ideas play an important independent role in political and social life. This chapter moved from the rather rudimentary observation (at least to a lay person) that ideas matter to flushing out the much richer set of questions about *how* different kinds of ideas, interests, and institutions combine to produce political outcomes. What I have outlined above is only the very beginning of an attempt to theorize about this very complex process.

As societies become ever more complicated and the range of normative and empirical ideas proliferates, it will only become more important to capture which ones become dominant and why. In the welfare state literature, for example, it seems simplistic and one-dimensional to continue to speak only in terms of whether the welfare state is expanding or retrenching, when in the past decade, parties from across the political spectrum have adopted a variety of approaches to "modernizing" the welfare state. Many of the most interesting and influential policies today, from welfare reform to school accountability to asset development, have roots on both the left and the right; understanding how these ideas evolved, picked up backers, and were selected over other competing approaches is likely to be as central to understanding the "new politics" of the welfare state as power approaches were to understanding the old.

To reduce politics solely to material interests and strategic calculations is not only to be willfully ignorant of how the world actually works, but it is also to deny a significant part of what it means for individuals to be human and for societies to be democratic. Individually and collectively, it is in the exchange of ideas that we define who we are and what we hope to become. If, as Louis Wirth (1936) once said, the "most important thing to know about a man is what he takes for granted," then the most important things to know about a

society and its politics are its prevailing assumptions. Understanding how these assumptions become dominant, what role they play in determining policy while ascendant, and why they are replaced by other sets of assumptions should be at the heart of political science and political sociology. In an age of academic specialization, these are the kinds of broad but still tractable questions that represent the best of what our disciplines can offer to the wider public.

NOTES

Thanks are due to Christopher Jencks, Theda Skocpol, Jennifer Hochschild, Chris Winship, Daniel Béland, Robert Cox, Michèle Lamont, Frank Dobbin, and the members of the Harvard sociology workshop on culture for their comments on an earlier draft of this chapter. Particular thanks go to Chris Winship for our series of open-ended conversations on the role of ideas in politics.

1. I would argue that even if these developments were a product of other structural forces, such as economic imperatives, these forces were themselves shaped by ideas such as individualism and progress.

2. This point is increasingly being embraced by scholars of ideas in recent years. In his study of the embedding of changing economic ideas, Blyth writes: "This study, in contrast, aims to demonstrate not only that ideas matter, but precisely when, why, and under what conditions they matter" (2002, 18). Similarly, in their study of rhetoric that enabled antiwelfare policy, Somers and Block observe that "once we acknowledge that ideas *do* exercise this independent role, it becomes clear that many battles over social and economic policy should be redefined as conflicts not over *whether* but over *which* ideational regime will do the embedding" (2005, 264–265).

3. See Berman (2001) and Campbell (2002) for other analyses of these issues.

4. This tripartite distinction is similar in two of its three parts to Kingdon's (1984) three streams of policies, problems, and politics. The primary difference in the typology is that my discussion of public philosophy or zeitgeist is focused on meta-ideas that affect debate, while Kingdon's politics stream includes electoral results, the timing of policy windows, the balance of interest-group power, and other primarily nonideational forces. My discussion of how problems and solutions work also differs from his in a number of ways that will be outlined in the text, but the concepts are the same.

5. One exception is Legro (2000), which will be discussed below.

6. See Oliver and Johnston (2000) for a critique of reducing the role of ideas to frames.

7. Goldstein and Keohane do note that causal and principled beliefs can be "mixed" (1993, 25), but their typology separates the two types of ideas.

8. Of course, material and professional self-interest are also constructed; how groups come to see their self-interest is critical to understand in any ideational analysis.

9. The first five items on this list draw heavily on Stone (1988), who offers the most thorough discussion of these issues.

10. This critique speaks more broadly to the question of active framing, a notion that appears frequently in the social movement literature (Benford and Snow 2000).

2

Reconciling Ideas and Institutions through Discursive Institutionalism

Vivien A. Schmidt

During the past three decades, the "new institutionalism" has become the main methodological battleground among political scientists. This is because political scientists differ in their preferred "new institutionalist" approach to political science. There are four basic institutionalist approaches: three older new institutionalisms—rational choice, historical, and sociological institutionalism—plus a fourth newer new institutionalism, which I call discursive institutionalism (Schmidt 2002a; Schmidt 2006; Schmidt 2008; see also Campbell and Pedersen 2001) and which is close to what Colin Hay in chapter 3 of this book calls constructivist institutionalism. Rational choice institutionalism focuses on rational actors pursuing their interests and following their preferences within political institutions, defined as structures of incentives, according to a "logic of calculation." Historical institutionalism concentrates instead on the history of political institutions and their constituent parts, which have their origins in the (often unintended) outcomes of purposeful choices and historically unique initial conditions and which develop over time following a "logic of path-dependence." Sociological institutionalism sees political institutions as socially constituted and culturally framed, with political agents acting according to a "logic of appropriateness" that follows from culturally specific rules and norms. Finally, the newest of the new institutionalisms, discursive institutionalism, considers the discourse in which actors engage in the process of generating, deliberating, and/or legitimizing ideas about political action in institutional context according to a "logic of communication."

I use the term *discursive institutionalism* as an umbrella concept for the vast range of works in political science that take account of the substantive content of ideas and the interactive processes of discourse that serve to generate those ideas and communicate them to the public (Schmidt 2000; Schmidt 2002a, ch. 5; Schmidt 2006, ch. 5; Schmidt 2008; Schmidt and Radaelli 2004). On

the substantive dimension, this includes different types of ideas (whether cognitive or normative), different levels of ideas (going from policy ideas to programmatic ideas or paradigms to philosophical ideas, which is the focus of Jal Mehta's chapter 1 in this book; see also Schmidt 2008), and different representations of ideas through discourse (whether frames, narratives, scripts, myths, collective memories, stories, and so forth). On the interactive dimension, discursive institutionalism covers all works that focus on the discursive processes by which such ideas are constructed in a "coordinative" policy sphere by policy actors and deliberated in a "communicative" political sphere by political actors and the public.

The *institutionalism* in the term, moreover, suggests that this is not only about the communication of ideas or "text" but also about the institutional context in which and through which ideas are communicated. Most important, the institutions of discursive institutionalism are not the external rule-following structures of the three older institutionalisms that serve primarily as constraints on actors, whether as rationalist incentives, historical paths, or cultural frames. They are instead simultaneously constraining structures and enabling constructs internal to "sentient" (thinking and speaking) agents whose "background ideational abilities" explain how they create and maintain institutions at the same time that their "foreground discursive abilities" enable them to communicate critically about those institutions, to change (or maintain) them (Schmidt 2008). Discursive institutionalism, in consequence, shares with the other institutionalisms a core focus on the importance of institutions, but it differs not only in its definition of institutions but also in its objects and logics of explanation and in the ways in which it deals with change (and continuity) (see table 2.1).

These four institutionalisms have a core focus on institutions, then, but they otherwise differ along a wide variety of continua: from structure to agency, positivism to constructivism, universalism to particularism, statics to dynamics, and more. Many of the debates among them have the characteristics of war: battles for territory, for control, for dominance. In what follows, I suggest that rather than continuing with the methodological wars, we should declare peace and consider instead how these very different approaches interrelate—how they complement one another, where they contradict one another, and what they contribute to our knowledge of political social reality. Because the three older new institutionalisms are well known, with the battles among them well documented, I provide only brief sketches of these. I concentrate on the latest new institutionalism, discursive institutionalism, because this type of institutionalism explicitly incorporates ideas into the analysis. Here, ideas stand as forces that help individuals formulate their preferences and are the currency for the discursive interactive processes that help produce policy change.

Table 2.1 The Four New Institutionalisms

	Rational Choice Institutionalism	Historical Institutionalism	Sociological Institutionalism	Discursive Institutionalism
Object of Explanation	Rational behavior and interests	Historical rules and regularities	Cultural norms and frames	Ideas and discourse
Logic of Explanation	Calculation	Path-dependency	Appropriateness	Communication
Problems of Explanation	Economic determinism	Historical determinism	Cultural determinism or relativism	Ideational determinism or relativism
Ability to Explain Change	Static: continuity through fixed preferences	Static: continuity through path dependence	Static: continuity through cultural norms	Dynamic: change and continuity through ideas and discursive interaction

THE THREE OLDER NEW INSTITUTIONALISMS: RATIONAL CHOICE, HISTORICAL, SOCIOLOGICAL

The three older new institutionalisms all share a commitment to bringing institutions back into the explanation of political action. But beyond this, they differ in their objects of explanation, whether the behavior of rational actors for rational choice institutionalists, institutional structures and practices for historical institutionalists, or norms and culture for sociological institutionalists; and in their logic of explanation, whether interest, path-dependency, or appropriateness. They all confront similar problems, however, albeit for different reasons: they are overly deterministic, whether economically, historically, or culturally; and they are largely static, having difficulty explaining institutional change (for a fuller account, see Schmidt 2005; Schmidt 2009).

Rational choice institutionalism posits rational actors with fixed preferences who calculate strategically to maximize their preferences and for whom institutions represent the incentive structures that reduce the uncertainties resulting from the multiplicity of individual preferences and issues (Hardin 1982; Ostrom 1990). Critics point to a number of problems with this approach. Although it produces generalizations that might be good at capturing the range of reasons actors would normally have for any action within a given set of institutional incentive structures, the approach cannot explain anomalies if they depart radically from interest-motivated action. It might lead to overgeneralization where there is a push toward universalistic generalizations (Scharpf 1997).

It misses out on the subtleties of human reasons for action (Mansbridge 1990). It has difficulty explaining any one individual's reasons for action or any particular set of real political events (Green and Shapiro 1994). Where it emphasizes the self-interested nature of human motivation, and especially when this is assumed to be economic self-interest, it is value-laden and can appear economically deterministic, as individuals are predicted to respond in a limited number of expected ways to external incentive structures (Immergut 1998, 14; but see Elster 1989 for a less narrow take on interests). Its definition of institutional incentive structures as neutral involves either a "naive rationalism," when credible institutions are assumed to emerge from utility-maximizing agents' rationally self-interested behavior, or a kind of idealistic normativism, when they instead explain credible institutions by reference to social norms (Rothstein 2005, 137–141). Finally, because rational choice institutionalism assumes fixed preferences and is focused on equilibrium conditions, it tends to be static and can only account for change exogenously, as the result of external shocks (Levi 1997), which makes for difficulty in actually explaining why institutions change over time (see Green and Shapiro 1994; Blyth 1997).

Historical institutionalism focuses on how institutions, understood as sets of regularized practices with rulelike qualities, structure action and outcomes. It emphasizes not just the operation and development of institutions but also the path-dependencies and unintended consequences that result from such historical development (Hall and Taylor 1996, 938; Steinmo, Thelen, and Longstreth 1992; Thelen 1999; Pierson 2000). Critics note that because it tends to emphasize structures and processes much more than the events out of which they are constructed, let alone the individuals whose actions and interests spurred those events, any "micro-foundational logic," as scholars of rational choice institutionalism put it, is generally missing from this macro-historical work. Change is largely described (rather than explained) from the outside (exogenously), whether by way of "big bang" theories about critical junctures (e.g., Gourevitch 1986; Collier and Collier 1991) or by path-dependencies with lock-in mechanisms and positive feedback effects (Mahoney 2000; Pierson 2000). As a result, historical institutionalism can appear historically deterministic or even mechanistic where it focuses exclusively on continuities and path-dependencies. Even recent attempts to put more history back into historical institutionalism, by focusing on incremental change through processes of drift, layering, and conversion (Thelen 2004; Streeck and Thelen 2005), do more to describe change from the outside than to explain it from the inside, through agency. And when scholars of historical institutionalism have sought to bring in agents to explain change endogenously, they have tended to turn to the two other older neo-institutionalisms (see Hall and Taylor 1996, 940–941). But although this might help with agency, it does not necessarily help with institutional

change, given the difficulties we have just noted for rational choice institutionalism and will see below with sociological institutionalism.

Sociological institutionalism instead focuses on the forms and procedures of organizational life stemming from culturally specific practices, with institutions cast as the norms, cognitive frames, and meaning systems that guide human action as well as the cultural scripts and schemata diffused through organizational environments, serving symbolic and ceremonial purposes rather than just utilitarian ones. Rationality for scholars of sociological institutionalism is therefore socially constructed and culturally and historically contingent, defined by cultural institutions that set the context within which purposive, goal-oriented action is deemed acceptable according to a "logic of appropriateness" (Scott 1995; DiMaggio and Powell 1991; March and Olsen 1989). Because such explanations are arrived at inductively rather than deductively, they can lend insight into individuals' reasons for action in ways that rational choice institutionalism cannot. Moreover, because such explanations account contextually for individuals' reasons for action, sociological institutionalism is better able to explain the events out of which explanations from historical institutionalism are constructed (Meyer and Rowan 1977; Hall and Taylor 1996, 953). This said, the emphasis of sociological institutionalism on macro-patterns might make it appear like "action without agents" (Hall and Taylor 1996, 954) or, worse, structures without agents (see Checkel 1998, 335). And, like rational choice institutionalism, it also can be too static or equilibrium-focused and therefore unable to account for change over time—although where it adds a historical perspective, it can also show how norms are institutionalized (e.g., Katzenstein 1996a). Finally, rather than appearing either economically or historically deterministic, sociological institutionalism can appear culturally deterministic where it emphasizes cultural routines and rituals to the exclusion of individual action that breaks out of the cultural norm, that is, rule-creating action (as opposed to rule-following action).

THE TURN TO IDEAS

In all three of the older new institutionalisms, how to explain change within essentially static institutions has been a fundamental problem. The turn to ideas has come as a natural progression, running the gamut from positivist approaches, in which ideas are mainly seen as reflecting the strategic interests of actors, to constructivist approaches, in which ideas are seen to constitute interests. But while for some the turn to ideas has meant staying within the initial constructs of their older new institutionalisms, others have moved beyond, into discursive institutionalism and a primary concern with ideas and how they are communicated through discourse.

Among rational choice institutionalists, the foray into the realm of ideas has remained rather circumscribed. In international relations, an early move to ideas was made by Judith Goldstein (1993), who suggested that under conditions of uncertainty, ideas behave like switches (or "road maps") that funnel interests down in specific policy directions, serving as filters, focal points, or lenses that provide policy makers with strategies (see also Goldstein and Keohane 1993; Weingast 1995; Bates et al. 1998). Here, ideas have not gone very far beyond interests, since they are little more than mechanisms for choosing among interests or as focal points for switching among equilibria (see critique by Ruggie 1998, 866–867). Douglass North (1990) went farther, first by using ideas to overcome the problem of how to explain institutional construction, then by casting ideas as "shared mental modes." However, as Mark Blyth's chapter 4 in this book insightfully argues (see also Blyth 2003, 696–697; Blyth 2002, ch. 2), the contradictions inherent in both such approaches might have been "a bridge too far." First, if ideas create institutions, then how can institutions make ideas "actionable"? But second, if instead ideas are "mental modes," then what stops ideas from having an effect on the content of interests and not just on the order of interests? This means that ideas would constitute interests, rather than the other way around. The problem for rational choice institutionalists, and the reason most quickly abandoned the pursuit of ideas, is that they could not continue to maintain the artificial separation of "objective" interests from "subjective" ideas about interests, that is, beliefs and desires. And such subjective interests threatened to overwhelm the objective ones that were at the basis of the rationalists' thin model of rationality, by undermining the "fixed" nature of preferences and the notion of outcomes as a function of preexisting preferences.

For the relatively few rational choice institutionalists who flipped over into discursive institutionalism, however, some of the most knotty problems could be addressed, such as assumptions about institutions as inherently good (or bad), actors as instrumental, and interests as objective. If one takes ideas seriously, as Bo Rothstein argues, institutions need no longer be treated as neutral structures of incentives or (worse) the immutable products of "culture" that lead to inescapable "social traps." Instead, institutions are better understood as the carriers of ideas or "collective memories," which make them objects of trust or mistrust and changeable over time as actors' ideas and discourse about them change in tandem with changes in their performance (Rothstein 2005, ch. 1, 7). Moreover, subjective interests replace the objective ones of rational choice institutionalism, as ideas about interests that bring in a much wider range of strategic ideas and social norms. For example, even where one's focus is primarily on strategic interests and instrumental action, as in Cornelia Woll's (2008) discursive institutionalist account of the deregulation of international trade in services, the emphasis is on agents' ideas about their (subjective)

interests, about which utility to maximize (interests), how to maximize it (strategies), and to what end (goals), rather than about the (objective) interests attributed to them as "rational" actors.

Relatively few dyed-in-the-wool rational choice institutionalists who considered the role of ideas have taken this last step, however. This is because taking ideas this seriously would force them to abandon the whole range of assumptions, in particular about objective interests, fixed preferences, and neutral institutional incentive structures, which make for the parsimony of the approach and everything that follows from it, including the ability to mathematically model games rational actors play as opposed to those "real actors play" (see Rothstein 2005, ch. 1; Scharpf 1997).

In the historical institutionalist tradition, the move into ideas has been more significant. Here, the question is really where the tipping point is between historical institutionalists who continue to see institutions as constitutive of ideas, determining which ideas are acceptable, and those who might better be called discursive institutionalists within a historical institutionalist tradition because they see ideas as constitutive of institutions even if shaped by them. Interestingly enough, even in the book that gave historical institutionalism its name (Steinmo, Thelen, and Longstreth 1992), the few chapters that were focused on ideas—those of Peter Hall, Desmond King, and Margaret Weir—take us beyond historical institutionalism. But whereas Hall's earlier edited volume on the adoption of Keynesianism ideas (Hall 1989) remained largely historical institutionalist because historical structures come prior to ideas, influencing their adoptability, his later article on the introduction of monetarist ideas in Thatcher's Britain (Hall 1993) crossed the line into discursive institutionalism, since ideas are central to change and constitutive of new institutions. Similarly, Desmond King (1999) in his book on illiberal social policy in Britain and the United States made the move into ideas quite explicit, although he also retained a strong historical institutionalist emphasis on how institutional context made it easier for the British government to take up ideas and impose reform than in the United States.

More recent work in historical institutionalism, such as the edited volume of Streeck and Thelen (2005) focused on incremental institutional change, demonstrates a split between authors who look to rationalist interests for agency and those who look more to ideas and discourse. Thus, whereas the introduction to the volume tends to theorize the dynamics of change primarily in rationalist terms, explaining layering, drift, and conversion by way of rational actors engaged in "on-going skirmishing as actors try to achieve advantage" (Streeck and Thelen 2005, 19), a number of the authors in the volume emphasize the importance of ideas (Jackson, Deeg, Palier, Quack, and Djelic). Among these, Palier (2005) argues that French welfare-state reform underwent revolutionary institutional change without a "revolution" as policy actors "layered" new

"recipes" for social policy onto the old, while Quack and Djelic (2005) explain the "path generation," combination, and recombination of antitrust policies in Germany and the EU in terms of institutional entrepreneurs and epistemic communities with an advantage in the battle of ideas.

What defines work that is clearly discursive institutionalist within the historical institutionalist tradition is the focus on ideas as explanatory of change, often with a demonstration that such ideas do not fit predictable "rationalist" interests, are underdetermined by structural factors, and/or represent a break with historical paths. Examples include Sheri Berman's (1998) historical contrast between the German Social Democrats' capitulation before Nazism because they were trapped in their Marxist ideology and the Swedish Social Democrats' success in reinventing socialism; Kate McNamara's (1998) account of European monetary union through a learning process that led to a neoliberal consensus on monetarism following the German exemplar; Craig Parsons's (2003) detailed history of the ways in which French ideas about constructing EU institutions became the institutionalized ideas that constrained subsequent French leaders' ideas and actions; and my own elaboration of the ideas and discourse that help explain the different dynamics of change in the three (rather than just two) varieties of capitalism exemplified in the economic policies and practices of Britain, Germany, and France (Schmidt 2002a, ch. 5, 6).

In the sociological institutionalist tradition, one cannot talk about a move into ideas as such, since ideas have always been at the basis of the approach—as norms, cognitive frames, and meaning systems. However, there is also a tipping point here. On the one side are those "constructivist" scholars who see ideas more as static ideational structures, as norms and identities constituted by culture, and who therefore remain largely sociological institutionalist according to the earlier definition. These include constructivists such as Peter Katzenstein and his colleagues, who show how interests developed from state identities structure national perceptions of defense and security issues (Katzenstein 1996b). On the other side are those constructivists who more clearly fit under the rubric of discursive institutionalism. These are the constructivists who, in addition to putting ideas into cultural context, put them into their "meaning" context as well (e.g., Kjaer and Pedersen 2001; Hay 2006). They tend to present ideas as more dynamic, that is, as norms, frames, and narratives that not only establish how actors conceptualize the world but also enable them to reconceptualize the world, serving as a resource to promote change through "structuration" (Wendt 1987, 359–360), through the diffusion of international norms in developing countries (e.g., Finnemore 1996a), or through the reconstruction of state identities and ideas about European integration (Risse 2001).

It is important to note that discursive institutionalists, whether they engage with the sociological, historical, or rational choice institutionalist tradition, all define institutions very differently from scholars who remain squarely in those

traditions. For the three older neo-institutionalisms, institutions are structures external to agents that constitute rules about acting in the world that serve mainly as constraints—whether by way of rationalist incentives that structure action, historical paths that shape action, or cultural norms that frame action. For discursive institutionalism, by contrast, institutions are internal to sentient agents, serving both as structures (of thinking and acting) that constrain action and as constructs (of thinking and acting) created and changed by those actors. This internal capacity to create and maintain institutions derives from agents' "background ideational abilities" (Schmidt 2008). This is a generic term for what Searle (1995) defines as the "background abilities" that encompass human capacities, dispositions, and know-how related to how the world works and how to cope with it or for what Bourdieu describes as the "*habitus*" in which humans beings act "following the intuitions of a 'logic of practice'" (1990, 11). These background ideational abilities underpin agents' ability to make sense in a given meaning context, that is, to "get it right" in terms of the ideational rules or "rationality" of a given discursive institutional setting.

But how, then, do we theorize about the process through which sentient agents get it right or, better, manage to bring about change in the ideational rules? Theoretical approaches in discursive institutionalism for how to plot change in ideas remain underdeveloped, despite much empirical analysis of changes in ideas. The most popular theories of ideational change, those that focus on paradigm shifts, are arguably the most problematic; they fail to specify closely enough the process of ideational change, that is, how old ideas fail and new ideas come to the fore; the reasons for ideational change, that is, why certain ideas are taken up rather than others; and the timing of ideational change, since paradigm theory's emphasis on abrupt shifts in ideas rules out not only evolutionary change but also revolutionary change in ideas that is not abrupt (Skogstad and Schmidt unpublished). One promising way forward is to build on the work of discourse analysts (e.g., Kjaer and Pedersen 2001; Howarth, Norval, and Stavrakakis 2000), who theorize the process of ideational change in terms of how different elements may be added to ideas, thereby bringing about change in ideas incrementally even in times of stability and not just during paradigm shifts (see Carstensen forthcoming b).

There is one major problem with this focus on ideas: we have yet really to explain the dynamics of institutional change. Although concentrating on ideas gets us closer to why institutional changes occur, with the tracing of change in ideas over time that presage the institutional shifts, they still don't explain how such institutional changes occur, that is, how the ideas themselves promote institutional change. For this, we need to consider another aspect of discursive institutionalism, which is the interactive side of discourse. How ideas are generated among policy actors and diffused to the public by political actors through discourse is key to explaining institutional change (and continuity).

DISCOURSE AS INTERACTIVE PROCESS

Most discursive institutionalists deal mainly with ideas, leaving the interactive processes of discourse implicit as they discuss the ideas generated, deliberated, and legitimized by the various actors. Some scholars, however, have gone farther to formalize the interactive processes of idea generation, diffusion, and legitimization and to clarify how they are structured. They tend to see discourse not only as a set of ideas bringing new rules, values, and practices or as a resource used by entrepreneurial actors to produce and legitimate those ideas but also as the interactive processes by which such ideas are conveyed. In other words, discourse is not only about what is said but also about who said what to whom, where, when, and why.

Without discourse, understood as the exchange of ideas, it is very difficult to explain how ideas go from individual thought to collective action. We don't, after all, know what people are thinking or why they act the way they do until they say it. And we don't, for the most part, engage in collective action or in collective (re)thinking of our actions without the articulation, discussion, deliberation, and legitimization of our ideas about our actions. This is why, in addition to the background ideational abilities that explain the internal processes by which institutions are created and maintained, we need to identity the "foreground discursive abilities" through which sentient agents may change (or maintain) their institutions following a logic of communication (Schmidt 2008). This is a generic term for what Habermas (1996) calls "communicative action," and it is at the basis of theories about deliberative and discursive democracy (e.g., Dryzek 2000), about public debate (Art 2006), and about coordinative discourses of policy construction and communicative discourses of political communication (Schmidt 2002a, 2006). These foreground discursive abilities are essential to explaining institutional change because they refer to people's ability to think outside the institutions in which they continue to act, to critique, communicate, and deliberate about such institutions and to persuade one another to take action to change them, whether by building "discursive coalitions" for reform against entrenched interests in the coordinative policy sphere or by informing, orienting, and deliberating with the public in the communicative political sphere.

Scholars who focus on the coordinative sphere tend to emphasize primarily the individuals and groups at the center of policy construction who generate the ideas that form the bases for collective action and identity. Some of these scholars focus on the loosely connected individuals united by a common set of ideas in "epistemic communities" in the international arena (Haas 1992). Or they target more closely connected individuals united by the attempt to put those ideas into action through "advocacy coalitions" in localized policy contexts (Sabatier and Jenkins-Smith 1993). Others describe how ideas are conveyed through "advocacy

networks" of activists in international politics (Keck and Sikkink 1998) or single out the individuals who, as "entrepreneurs" (Fligstein and Mara-Drita 1996; Finnemore and Sikkink 1998) or "mediators" (Jobert 1992; Muller 1995), draw on and articulate the ideas of discursive communities and coalitions in particular policy domains in domestic or international arenas.

In the communicative sphere, discursive institutionalists emphasize the use of ideas in the mass process of public persuasion in the political sphere. Some of these scholars examine the processes of communication in electoral politics and mass public opinion creation (Mutz, Sniderman, and Brody 1996), when politicians translate the ideas developed by policy elites into the political platforms that are put to the test through voting and elections. Others are concerned with the "communicative action" (Habermas 1996) that frames national political understandings or with the more specific deliberations in the "policy forums" of informed publics (Rein and Schön 1991) about the ongoing policy initiatives of governments. Exemplary of this is David Art's (2006) investigation of the elite-led public debates about the Nazi past in Germany and Austria in the 1980s, which engendered very different political cultures and partisan politics by the 1990s, leading to highly contrasting results with regard to the rise of the far right.

The arrows of discursive interaction often appear to go from the top down, as policy elites are seen to coordinate the construction of ideas that political elites then communicate to the public and mediate the ensuing public debates. The arrows can also go from the bottom up, however, as in the discursive interactions of social activists, feminists, and environmentalists in national and international arenas, or remain solely at the level of civil society, as part of "deliberative democracy." Equally important, however, is that there might be no arrows between coordinative and communicative discourses when coordinative policy ideas remain out of public view in closed debates or if political elites choose to legitimate their policy ideas using arguments other than those used in the coordinative discourse (Schmidt 2008). Significantly, however, even when a discourse starts from the top, it very often escapes political leaders' control as a result of bottom-up influences. In the case of Germany, for example, Art (2006) shows that when conservative chancellor Helmut Kohl sought to "normalize" ideas about the country's Nazi past, the debate he launched quickly became an opportunity for a wide-ranging public reexamination of the country's understandings of its history and ultimately ensured that the discourse initiated by the left became the basis for a "political correctness, German-style" that silenced potential anti-Semitic and right-wing extremist speech.

In all of these discursive institutionalist approaches, the empirical analysis of the process of institutional change is very different from that found in rationalist, historical, or sociological institutionalism, since it is focused on who talks to whom about what, when, how, and why, in order to show how ideas are generated, debated, adopted, and changed as policy makers, political leaders, and the

public are persuaded, or not, of the cognitive necessity and normative appropriateness of ideas. Institutional context clearly matters here but not quite in the way that it matters for rationalist, historical, or sociological institutionalists.

Material interests, economic in particular, which are at the basis of much of the institutional incentives in the rational choice institutionalist literature, are not ignored. But in discursive institutionalism, scholars tend to separate material interests analytically into material reality and interests rather than to conflate them. Moreover, whereas they posit interests as constituted by ideas and discourse, such that interests cannot be separated from ideas about interests, they see material reality mostly as separate from interests and best understood as the setting within which or in response to which agents conceive of their interests (Schmidt 2008). Thus, discursive institutionalists problematize the rationalists' whole notion of "objective" material interests by theorizing interests as subjective responses to material conditions (see Blyth 2002 and discussion in Hay 2006). It is important that, in contrast to rational choice institutionalists, who extrapolate from expected responses their predictions about rational actors' "objective" and fixed preferences, discursive institutionalists take the actual responses to material reality as their subject of inquiry.

The kind of knowledge and degree of certainty agents might have with regard to their ideas about material reality also differ, depending on the aspect of material reality with which they are concerned. Illustrative of this epistemological point is Wittgenstein's (1972) little-noticed distinction between the language games based on our everyday experiences in the world, which tend to admit of few doubts or mistakes, and language games based on our (social) scientific pictures of the world, which might always allow for doubts, mistakes, and even gestalt switches. The problem with rational choice institutionalism is that it tends to develop social scientific picture games of the world that it treats as if they had the certainty of experience games (see Schmidt 2008). As Blyth (2002 and chapter 4 in this book) argues, rational choice institutionalism mistakenly assumes that most phenomena are explainable in terms of "Knightian risk" because they are part of a directly observable world that agents can perceive more or less well and in which they can calculate the subjective probability of the likely outcomes of their preferences, such as in the U.S. Congress. Such phenomena, Blyth shows, are in actuality better explained in terms of "Knightian uncertainty," because they are part of a world that is not directly observable, such as the global economy, in which agents are not simply unsure about how to achieve their interests but unsure of what their interests are, given that the uncertainties are too great, the moment unique, prediction impossible, and agents' interests always structurally underdetermined.

Equally significant, however, is that discursive institutionalists often go beyond the focus on the construction of interests alone to include values, whether understood as cultural mores, community morals, or ethics. This takes

them into the domain of sociological institutionalism, where culture and norms are at the basis of much of the institutional framing of the sociological institutionalist literature. But again, the ideas in discourse do not only reflect cultural norms; the discourse through which they are conveyed, if persuasive, can also serve to reframe such norms and create new cultural mores.

Finally, the macro-structures and regularized practices that are the subject of historical institutionalist analysis are also significant for shaping ideas. But ideas and discourse can also serve to reshape the macro-structures and regularized practices. This suggests another avenue for historical institutionalists who seek to go beyond description to explanation of "what happens." Instead of turning to rational choice or sociological institutionalism for human agency, both of which are still quite static, as we have already seen, they could turn to discursive institutionalism to help explain the dynamics of institutional change, with ideas and discourse providing another kind of micro-foundational logic to institutional development.

For example, in "simple" polities (or single-actor constellations), where governing activity tends to be channeled through a single authority, mainly the executive—primarily countries with majoritarian representative institutions and statist policy making and unitary states such as Britain and France—the communicative discourse to the general public tends to be much more elaborate than the coordinative discourse among policy actors. This naturally follows from the fact that a restricted group of policy makers, largely made up of or guided by government actors, tends to generate the policies that political leaders then seek to legitimate to all, including the most affected groups. The communicative discourse is therefore crucial, since without it, governments face sanctions ranging from interest-group protest to loss of public confidence and loss of elections (see Schmidt 2002a; Schmidt 2006).

By contrast, in "compound" polities, where governing activity tends to be dispersed among multiple authorities—countries with proportional representation systems and corporatist policy making and/or federal or regionalized states such as Germany and Italy—the coordinative discourse among policy actors tends to be much more elaborate than the communicative discourse to the public. This naturally follows from the large group of policy actors, including government officials, business and union representatives, and local and regional government representatives, all of whom may be engaged in the generation of policies. The coordinative discourse is therefore crucial not just with regard to reaching agreement on policy among the many policy actors involved but also in legitimating such agreement to those actors' different constituencies. The communicative discourse is, by contrast, likely to be quite thin, because political leaders' discourse tends to be very general, in order to avoid jeopardizing any of the compromises made in private among policy actors. An exception among compound polities is the United States, since it

has a strong communicative discourse as a result of its majoritarian politics and presidential system, along with a strong coordinative discourse as a result of its pluralist processes and federal structures. The highly compound European Union, by comparison, has the weakest of communicative discourses as a result of the lack of an elected central government—and its dependence on national leaders to speak for it—and the strongest of coordinative discourses, given its highly complex, quasi-pluralist processes and quasi-federal structures (see Schmidt 2006).

THE LIMITS OF DISCURSIVE INSTITUTIONALISM

Discursive institutionalism works best, in short, at explaining the dynamics of change (but also continuity) through ideas and discursive interactions. As such, it largely avoids the static determinism of the other three new institutionalisms. By the same token, however, it risks appearing highly voluntaristic unless the structural constraints derived from the three newer institutionalisms are included—whether rationalist interests, historical paths, or cultural norms. The appearance of voluntarism is especially problematic for scholars who focus only on ideas, for whom "text" appears without context, as in some postmodernist approaches. But even when the context is considered, other problems might arise.

In discursive approaches that follow in the sociological institutionalist tradition, there is always the danger that social construction goes too far and that material interests qua material interests are ignored in favor of seeing everything as socially constructed within a given culture (see the critique of Sikkink 1991 by Jacobsen 1995). This leads one to question whether there is anything "out there" at all, mutually recognizable across cultures. But while discursive approaches in the sociological institutionalist tradition might suffer from too much constructivism, those in the rational choice or historical institutionalist tradition might suffer from too much positivism, with political action assumed to be motivated by instrumental rationality alone, such that cognitive ideas about interests overdetermine the choice of ideas, crowding out the normative values that also color any conceptualization of interest.

Discursive institutionalists might also not take power and position seriously enough, by overdeterimining the role of ideas and discourse in political life. This might be an overreaction to rational choice and historical institutionalism, which tend to reify questions of power and position by assuming that power is a function of position and that agents' strategic interests derive primarily from their power and position. For discursive institutionalists, power is not solely defined by (objective) position, since ideas and values infuse the exercise of power, influence (subjective) perceptions of position, and often

give power to actors even when they might lack the power of position—as in the case of social movements or entrepreneurial actors who set the agenda for reform in policy or political spheres. Power itself, moreover, derives not only from position, meaning actors' ability to wield power, but also from purpose, since actors' ideas and discourse about how to wield power might reinforce or undermine the power they derive from their position, depending on the responses of their audience to their stated purposes. This is the essence of leadership.

But all discursive institutionalist approaches, whether positivist or constructivist, might also be overly deterministic or idealistic with regard to the role of ideas and discourse, seeing the influence of ideas and the persuasiveness of discourse everywhere in the way rational choice institutionalists see instrumental rationality everywhere or sociological institutionalists see cultural rationality. Often, critical ideas for change have little effect on crystallized ideas about interests or routinized patterns of interaction while critical discourse and deliberation do not persuade. Equally important is that "stuff happens." As the historical institutionalists remind us, processes of change are often unconscious, as people may act without any clear sense of what they are doing, creating new practices as a result of "bricolage" and destroying old ones as a result of "drift" (Thelen 2004; Streeck and Thelen 2005; see also Campbell 2004, 69–74). But even when there is conscious action, when people do have ideas about what they are doing, what they do most often has unintended consequences, not only in a historical institutionalist sense because the outcomes may be unanticipated but also in a discursive institutionalist sense because ideas may be reinterpreted or misunderstood.

Thus, the big question for discursive institutionalism in explaining change, once we have established that ideas and discourse do matter and how they matter, is *when* do ideas and discourse matter, that is, when do they exert a causal influence? And when don't they?

Establishing causality with regard to ideas and discourse can be problematic. The very question itself might seem inappropriate to constructivist discursive institutionalists who see causal logics of explanation as operating in a different domain from constructivist logics of interpretation (e.g., Wendt 1999; Bevir and Rhodes 2003). Other constructivists, however, argue that bracketing off questions of causality and explanation from those of meaning and interpretation is unnecessary (see Hay 2004a, 145). In fact, whether constructivist or positivist, most discursive institutionalists see their main explanatory task as that of demonstrating the causal influence of ideas and discourse (e.g., Hay 2001; Schmidt 2002a, ch. 5, 6; Blyth 2003; Parsons 2003). This approach to the causal influence of ideas and discourse is very different from that of historical institutionalists such as Hall (1989), whose Keynesian ideas are presented as objective and universally meaningful ideas, diffusing change across nations

and time, acting as causal forces coming from the outside. And it is even more different from those of rationalists such as Goldstein and Keohane (1993), in which ideas are "switches" or "mental modes" that determine preferences. But it is also different from those of constructivists who emphasize culture as constitutive of ideas. This is because, here, ideas are seen from the inside, as empirical subjects to be studied in terms of their development in interpretive meaning contexts (see Kjaer and Pedersen 2001). Thus, for discursive institutionalists, the question of causality is an empirical one of showing when ideas and discourse matter and when they don't.

Discourse, just as with any other factor, sometimes matters and sometimes does not in the explanation of change. The question is *when* does it matter, say, by redefining interests as opposed to merely reflecting them in rationalist calculations, by reshaping historical paths as opposed to being shaped by them, or by re-creating cultural norms as opposed to reifying them (see Schmidt 2002, 250–256)? And when are other factors more significant, say, where the creation of new institutional paths or cultural norms might be better captured by historical or sociological institutionalist analysis, because actors don't have any clear idea about what they are doing when they are doing it? Part of the reason many political scientists avoid explanations related to discourse is that it is difficult to separate it from other variables, to identify it as *the* independent variable. But instead of ignoring discourse because of the difficulties, because it might not be *the* cause, it is much better to ask when is discourse *a* cause, that is, when does discourse serve to reconceptualize interests, to chart new institutional paths, and to reframe cultural norms?

For this, we need to establish what criteria to use in evaluating whether discourse has a causal influence, that is, when it is "transformative" and when it is not. Generally speaking, in the realm of ideas, a "good" discourse depends on the relative strength of its cognitive arguments, the resonance of its normative arguments, the adequacy of the information on which the arguments build, the relevance or applicability of its recommendations, the coherence and consistency of its ideas, and more (Schmidt and Radaelli 2004). Factors such as timing, political salience, policy viability, and fit in terms of national values, tradition, and culture are equally important (Schmidt 2008; see also Mehta, chapter 1 in this book). In the realm of discursive interactions, who is speaking to whom in coordinative and communicative spheres also matters. And all of this can be investigated empirically, for example, through process tracing of ideas held by different actors that led to different policy choices (Berman, chapter 5 in this book; Berman 1998); through matched pairs of country cases in which everything is controlled for except the discourse to show the impact of discourse on welfare adjustment (see Schmidt 2002b); through speeches and debates of political elites that lead to political action (Wincott and Rich, chapters 7 and 9 in this book; Dobbin 1994; Art 2006); through

opinion polls and surveys to measure the impact of the communicative discourse (Koopmans 2004); through interviews and network analysis to gauge the significance of the coordinative discourse; and more.

Institutional context also needs to be taken into consideration, however. For example, in "simple" polities (or single-actor systems) in which the communicative discourse is most elaborate, the causal influence of discourse is most likely to be ascertainable in the responses of the general public over time, as discovered through protests and election results, opinion polls, and surveys. By contrast, in "compound" polities (or multiactor systems) in which the coordinative discourse is most elaborate, the causal influence is more likely to be seen in whether or not there is any agreed policy, with empirical investigation focused on interviews and reports of policy actors (Schmidt 2002a).

CONCLUSION

To get a sense of how the four new institutionalisms fit together in a very general way, figure 2.1 is a chart that arrays them along a horizontal continuum from positivism to constructivism—from interests to culture, with history in between—and along a vertical continuum from static to dynamic, with interests, history, and culture at the static end, ideas and discourse at the dynamic end. I put historical institutionalism between rational choice and sociological institutionalism, mainly because rational choice and sociological institution-

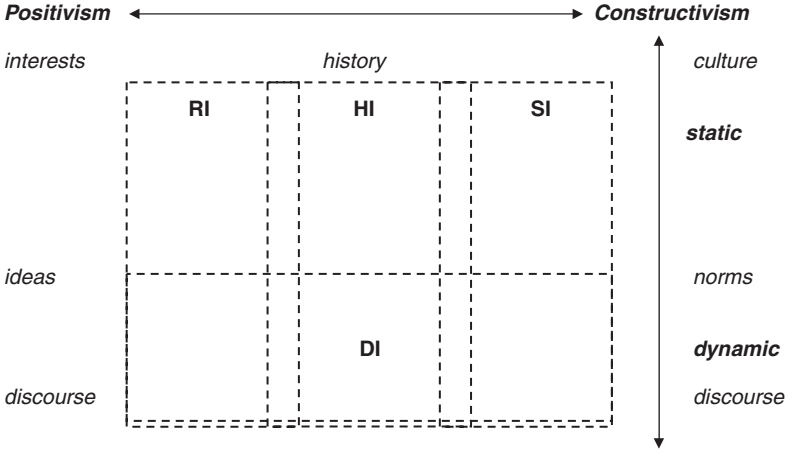

Figure 2.1 Spatial relationship of the four new institutionalisms: rational choice (RI), historical (HI), sociological (SI), and discursive (DI) (dotted lines represent border areas).

alism are largely incompatible, whereas historical institutionalism can go either to the positivist or to the constructivist side when it adds agency. I put discursive institutionalism underneath all three because, although it is distinctive, it can rest on the insights of any one of the three and because scholars often see themselves as continuing to fit into one or another of the traditions even as they cross the line into discursive institutionalism.

Discursive institutionalism thus lends new insights into the reconceptualization of rationalist interests, the reshaping of historical paths, and the reframing of cultural norms. It is a natural progression from the three older new institutionalisms and a progressive development beyond them. The other three new institutionalisms provide useful insights into the crystallized ideas about rationalist interests and cultural norms or the frozen landscapes of macro-structures and routinized actions prior to our investigation into the dynamics of change. Put another way, the old new institutionalisms could be seen as good shortcuts to the uncontested regularities and rationalities of institutionalized behavior and interactions. But to explain change, as well as to test the accuracy of such crystallized ideas and frozen landscapes, we need something more: discursive institutionalism.

NOTES

The first draft of this chapter was presented at the 2006 Annual Meeting of the American Political Science Association under the title "Give Peace a Chance: Reconciling Four (Not Three) New Institutionalisms." I would like to thank Kathleen Thelen, Mark Blyth, Colin Hay, Michael Lister, Fritz Scharpf, Paulette Kurzer, Bob Goodin, Peter Katzenstein, Martin Schröder, Daniel Wincott, and editors Daniel Béland and Robert Cox for their insightful suggestions at earlier stages of the manuscript.

3

Ideas and the Construction of Interests

Colin Hay

The proliferation of new institutionalist scholarship has, perhaps unremarkably, led to a corresponding proliferation in the adjectives used to characterize its variants. In 1984, James G. March and Johan P. Olsen spoke quite comfortably of the new institutionalism in the singular. By 1996, Peter Hall and Rosemary Taylor had eventually settled on three new institutionalisms (having toyed, in earlier iterations of the same now-classic article, with four). And by 1998, B. Guy Peters had identified no fewer than seven new institutionalisms. Yet none of these authors made any reference to constructivism, far less to a distinctive constructivist variant of institutionalism in its own right. Indeed, until recently, there has been little, if any, reference to what is now variously described as an ideational, discursive, or, as here, constructivist institutionalism. This is for three good reasons: constructivist institutionalism is by far the most recent addition to the family of institutionalisms, it arises out of an engagement with the limitations of the others, and, as a consequence and in contrast with the others, it is still very much in its infancy. On this point, I agree very much with Vivien Schmidt's characterization of contemporary institutionalism in the chapter 2 of this book. But where she sees continuity and progression in institutional approaches, I detect a rather greater incommensurability of competing ontological commitments. In what follows, I focus on the ontological, analytical, and methodological distinctiveness of constructivist institutionalism, assessing the challenge it poses to those institutionalisms in whose path it follows.

The aim of this chapter is quite simple: to outline the distinctiveness of constructivist institutionalism, to identify the nature of the challenge that it poses, and to discuss the problematic treatment of the concept of interests in much of the institutionalist research to date which ostensibly draws on a constructivist ontology. I aim to show what is at stake in developing a consistently constructivist institutionalism. This somewhat idealized position I counterpose to much existing discursive institutionalism, which, though frequently couched in constructivist terms, is invariably ontologically inconsistent on the question of material interests.[1]

The chapter proceeds in three sections. In the first, I discuss how constructivist institutionalism arose in response to problems of change in much of the previous institutionalist scholarship. In the second, I consider the ontological and analytical distinctiveness of constructivist institutionalism's turn to ideas. In the third and concluding section, I caution those who would adopt a constructivist approach against falling back on an essentially materialist conception of self-interest, documenting the potential pitfalls of such a move. I argue that a genuinely constructivist perspective must see interests as social constructions, rather than as materially given.

FROM HISTORICAL TO CONSTRUCTIVIST INSTITUTIONALISM

Constructivist institutionalism, as I label it, has its origins in the increasingly frustrated attempts of some to grapple with questions of complex institutional change, initially from within the confines of existing neo-institutionalist scholarship (see Schmidt, chapter 2 in this book, and Schmidt 2005). For such authors, the major problem with existing variants of institutionalism is that they have tended to be characterized by an emphasis on institutional genesis at the expense of an adequate account of postformative institutional change. Moreover, insofar as postformative institutional dynamics have been considered (e.g., Hall 1993; Hall and Soskice 2001; Pierson 1994), they have tended either to be seen as a consequence of path-dependent lock-in effects or, where more violently disruptive in nature, as the products of exogenous shocks such as wars or revolutions (Hay and Wincott 1998; Skocpol 1979; Tilly 1994). Traditional varieties of institutionalism, it seems, are incapable of offering their own (i.e., endogenous) accounts of the determinants of the dramatic institutional changes, or "punctuated equilibria" (Krasner 1984), to which they invariably point. This, at least, is the charge of many constructivist institutionalists (see, e.g., Blyth, chapter 4 in this book; Blyth 2002, 19–23; Hay 2001, 194–195).

If one follows Peter Hall and Rosemary Taylor (1996) in seeing existing institutionalisms as animated by actors displaying either "logics of calculus" or "logics of appropriateness" or, in the historical variant, some combination of the two, then it is perhaps not difficult to see why. For instrumental logics of calculation (calculus logics) presume equilibrium (at least as an initial condition),[2] and norm-driven logics of appropriateness (cultural logics) are themselves equilibrating. Insofar as an actor's behavior is norm-driven, and insofar as that norm is both context-dependent and accessible to us, the actor's behavior is rendered predictable to the analyst by virtue of the context in which it occurs. Accounts that see actors as driven either by utility maximization in an institutionalized game scenario (rational choice institutionalism) or by institutionalized norms and cultural conventions (sociological institutionalism) or,

indeed, both (historical institutionalism) are unlikely to offer much analytical purchase on questions of complex postformative institutional change. They are far better placed to account for the path-dependent institutional change they tend to assume than they are to explain the periodic, if infrequent, bouts of path-shaping institutional change they concede. The distinction between path-dependent and path-shaping logics and dynamics is a crucial one (see also Cox 2001). New institutionalists in general have tended to place far greater emphasis on the former than on the latter. This perhaps reflects the latent structuralism of the attempt to bring institutions back into contemporary political analysis (see Hay 2002, 105–107). For institutions, as structures, are invariably seen to limit, indeed delimit, the parameters of political choice. As such, they are constraints on political dynamism. This is certainly an important insight, yet there is a certain danger in tilting the stick too strongly in the direction of structure. For under certain conditions, institutions and the path-dependent logics they otherwise impose are recast and redesigned through the intended and unintended consequences of political agency. Given the importance of such moments, the new institutionalism has had remarkably little to say about these bouts of path-shaping institutional change. This is where constructivist institutionalism comes in.

THE ANALYTICAL AND ONTOLOGICAL DISTINCTIVENESS OF CONSTRUCTIVIST INSTITUTIONALISM

Set in the context of the new institutionalist scholarship out of which it has emerged, the analytical and ontological assumptions of constructivist institutionalism are highly distinctive. And in their capacity to inform an endogenous account of complex institutional evolution, adaptation, and innovation, at least, they represent a considerable advance on their rationalist and sociological predecessors.[3]

Actors are strategic, seeking to realize certain complex, contingent, and constantly changing goals (see also Béland and Cox in the introduction to this book). They do so in a context that favors certain strategies over others and must rely on perceptions of that context that are, at best, incomplete and that might often prove to have been inaccurate after the event. Moreover, ideas in the form of perceptions "matter" in a second sense, for actors are oriented normatively toward their environment. Their desires, preferences, and motivations are not a contextually given fact—a reflection of material or even social circumstance—but are irredeemably ideational, reflecting a normative (indeed, moral, ethical, and political) orientation toward the context in which they will have to be realized. For constructivists, politics is less about the blind pursuit of transparent material interest and more about the fashioning, identification,

and rendering actionable of such conceptions and the balancing of (presumed) instrumentality and more affective motivations (see also Wendt 1999, 113–135).[4] How consistently that core ontological premise of constructivism is applied in ostensibly constructivist institutionalist research is a question to which we will return in detail below.

Given such a distinct view of the motives of agents, actors are not analytically substitutable (as in rational choice or sociological institutionalism), just as their preference sets or logics of conduct cannot be derived from the (institutional) setting in which they are located. Interests are social constructions and cannot serve as proxies for material factors; as a consequence, they are far more difficult to operationalize empirically than is conventionally assumed (at least, in a nontautological way; see also Abdelal, Blyth, and Parsons 2006; Blyth 2003).

In common with other variants of institutionalism, the context is viewed in largely institutional terms. Yet institutions are understood less as functional means of reducing uncertainty, as in rational choice institutionalism, than as structures whose functionality or dysfunctionality is an open—empirical and historical—question. Indeed, constructivist institutionalists place considerable emphasis on the potentially ineffective and inefficient nature of social institutions, on institutions as the subject and focus of political struggle, and on the contingent nature of such struggles, whose outcomes can in no sense be derived from the extant institutional context itself (see especially Blyth 2002).

These are the basic analytical ingredients of constructivist institutionalism's approach to institutional innovation, evolution, and transformation. Within this perspective, change resides in the relationship between actors and the contexts in which they find themselves, among institutional "architects," institutionalized subjects, and institutional environments. More specifically, institutional change is understood in terms of the interaction between strategic conduct and the strategic context within which it is conceived and in the later unfolding of its consequences, both intended and unintended. As in historical institutionalism, such a formulation is path-dependent: the order in which things happen affects how they happen, the trajectory of change up to a certain point itself constrains the trajectory after that point, and the strategic choices made at a particular moment eliminate whole ranges of possibilities from later choices while serving as the very condition of existence of others (see also Tilly 1994). Yet pointing to path-dependence does not preclude the identification of moments of path-shaping institutional change, in which the institutional architecture is significantly reconfigured. Moreover, and at odds with most existing new institutionalist scholarship, such path-shaping institutional change is not seen merely as a more or less functional response to exogenous shocks.

Further differentiating it from new institutionalist orthodoxy, constructivist institutionalists emphasize not only institutional path-dependence but

also ideational path-dependence. In other words, it is not just institutions but the very ideas on which they are predicated and which inform their design and development that exert constraints on political autonomy. Institutions are built on ideational foundations that exert an independent path-dependent effect on their subsequent development (for an elaboration of this concept, see Cox 2004).

Constructivist institutionalism thus seeks to identify, detail, and interrogate the extent to which—through processes of normalization and institutional embedding—established ideas become codified, serving as cognitive filters through which actors come to interpret environmental signals and, in so doing, to conceive of their own interests. Yet, crucially, they are also concerned with the conditions under which such established cognitive filters and paradigms are contested, challenged, and replaced. Moreover, they see paradigmatic shifts as heralding significant institutional change.

Such a formulation implies a dynamic understanding of the relationship between institutions, on the one hand, and the individuals and groups out of whose practices they are made up (and on whose experience they impinge), on the other. It emphasizes institutional innovation, dynamism, and transformation, as well as the need for a consideration of processes of change over a significant period of time. In so doing, it offers the potential to overturn new institutionalism's characteristic emphasis on institutional inertia. At the same time, however, such a schema recognizes that institutional change does indeed occur in a context that is structured (not least by institutions and ideas about institutions) in complex and constantly changing ways, which facilitate certain forms of intervention while militating against others. Moreover, access to strategic resources, and indeed to knowledge of the institutional environment, is unevenly distributed. This, in turn, affects the ability of actors to transform the contexts (institutional and otherwise) in which they find themselves. Finally, it is important to emphasize the crucial space granted to ideas within such a formulation. Actors appropriate strategically a world replete with institutions and ideas about institutions. Their perceptions about what is feasible, legitimate, possible, and desirable are shaped both by the institutional environment in which they find themselves and by existing policy paradigms and worldviews. It is through such cognitive filters that strategic conduct is conceptualized and ultimately assessed.

THE PATHOLOGIES OF CONSTRUCTIVIST INSTITUTIONALISM: WHY IDEAS INTO INTERESTS DON'T GO

The above paragraphs set out, in effect, the relatively highly conserved ontological basis from which constructivism has developed as a distinct approach to

institutional analysis. Yet it is the argument of this chapter that despite the ostensible commitment of many institutionalists to constructivism, such ontological commitments are typically violated in the substantive analysis to which constructivist institutionalism has thus far given rise. In other words, few constructivist institutionalists to date have remained true to their ostensible ontological commitments in the substantive institutional analysis they have offered. In particular, much constructivist institutionalism, despite its emphasis on the explanatory and causal significance of ideas and its ostensibly constructivist take on the question of interests, falls back on an essentially materialist appeal to notions of real or genuine interest (see, e.g., Blyth 2002; Hansen and King 2001). In this respect, it is both ontologically inconsistent and, arguably, insufficiently distinct ontologically from its rational choice and sociological institutionalist antecedents for the task it sets itself. This is true even of its most accomplished, ontologically and epistemologically reflective, and explicitly constructivist proponents. Thus, even Mark Blyth, who has perhaps done more than anyone to establish the ontological and epistemological distinctiveness of constructivism as an alternative to rational choice, sociological, and historical institutionalism (see especially Blyth 2003 and chapter 4 in this book), seems to rely at key points in his more substantive work (2002) on the appeal to actors' material self-interest. This is all the more perplexing given Blyth's theoretical discussion of interests in the first chapter of *The Great Transformations* (2002; and again in this book), which, more closely than any other existing treatment, parallels that developed here. Blyth's core claim is that actors' conduct is not a (direct) reflection of their material self-interest but, rather, a reflection of particular perceptions of their material self-interest (see also Wendt 1999, 113–135).

The claim that it is perceptions of interest, rather than interests per se, that inform behavior might seem intuitive and obvious. It is nonetheless in some considerable tension to much neo-institutionalist scholarship. For, conventionally, it is actors' material interests, rather than their perceptions of those interests, that are assumed to be the key determinants of their behavior.

Yet there is some ambiguity and inconsistency in the manner in which Blyth operationalizes this important—constructivist—insight, which speak to a potentially wider ambiguity and unevenness within constructivist institutionalism to date. For on occasions, Blyth refers to interests as "social constructs that are open to redefinition through ideological contestation" (2002, 271; see also Abdelal, Blyth, and Parsons 2006). Yet at other points in the same text, interests are treated as materially given and as clearly separate from perceptions of interests, as, for instance, when he counterposes the "ideas held by agents" and "their structurally derived interests" (2002, 33–34). Here, Blyth seems to fall back on an essentially material conception of interests. And he is in extremely good company in doing so (see also Berman 1998; Best 2005;

McNamara 1998; Parsons 2003; Schmidt 2002b; Schmidt and Radaelli 2004; Wendt 1999). Obviously, it makes no sense to view "structurally derived interests" as social constructs. And the ambiguity this implies returns to haunt the argument he goes on to develop—as it does, more generally, constructivist institutionalism. The problem here, I suggest, is the ambiguity at the heart of the appeal to the language of interests, as I shall now seek to explain.

Why Material Interests?

To see why this might be so, it is instructive to ask why we appeal to the language of interests, real or material interests in particular, in contemporary social and political science. What work does the concept of material interest do? What purchase on social and political reality does it offer us?

The conventional and, in many respects, the best answer to that question is simple. The concept of material interest is an aid in the simplification of social and political reality, where the purpose of that simplification is, in turn, to make such a reality more amenable to the development of a naturalist and, here specifically, predictive science. In other words, and in short, the concept of material interest and the assumptions on which the concept is predicated make possible a naturalist science of politics (a science of politics in the image of the natural sciences) where otherwise it would not be possible.

The Context-Dependence of Political Behavior and the Irrelevance of Ideas

The core ontological difference between social/political arenas and their natural counterparts is that the units that make up the former are active and, indeed, proactive, as opposed to passive and responsive (Hay 2009a).[5] They are shapers of their own destiny, rather than bearers of systemic logics beyond their comprehension and control. They possess agency, free will, and the capacity to reshape the environment in which they find themselves—though, of course, not necessarily as they intend. This renders such arenas open and indeterminate in contrast with their natural counterparts, which are, by and large, closed and determinate.[6] As a consequence, the behavior of the units (agents) that make up the arena cannot so readily be derived from a series of generic laws said to govern the operation of the system as a whole. A naturalist science of the social and political seeking such laws is, one might think, inevitably likely to disappoint. But naturalists have not given up their animating conviction so easily. Instead, they have adopted a series of heuristic simplifying assumptions that serve to re-present social and political arenas such that they conform more closely to those in the natural realm. Such assumptions serve to render the behavior of actors predictable given the context within which it occurs.

The two central devices in this analytical naturalization of social and political arenas are those of rationality and material interest (see also Béland and Cox's introduction and Blyth's chapter 4 in this book). Here, and in what follows, I concentrate primarily on the latter (for an equivalent treatment of the former, see Hay 2002; Hay 2004a; Hay 2009a and Hay 209b).

In order to reconnect behavior and context, unit and arena, naturalists tend to assume (1) that actors' behavior is a simple reflection of their self-interest (since they act rationally in pursuit or defense of that self-interest) and (2) that such interests are both given by and hence a logical derivative of their material context.[7] Insofar as propositions 1 and 2 hold, or are assumed to hold (say, in the development of a stylized model of a social or political arena), an actor's behavior is rendered predictable by the context in which the actor is located. Or, at least, that is the case as long as one additional condition pertains. The context/arena must be, or must be assumed to be, in a state of equilibrium (proposition 3). If all three propositions hold, or can be assumed to hold, the aspirant naturalist's work is done. We need know nothing about the actor him or herself in order to know (or at least to claim to know) how he or she will behave under any given stimulus or set of conditions. The actor is, once again, restored to the analogue of the subatomic particle in a magnetic field. His or her behavior can be predicted entirely from knowledge of the context in which she or he is situated. This is the naturalists' world—the world, at first cut, at least, of neoclassical economics and rational choice theory. It is summarized schematically in figure 3.1.

For any given actor a, if we can assume the context-dependence of material self-interest, then

$$C_a \Rightarrow MI_a[1]$$

Moreover, assuming rationality—the sole motivation for behavior being the efficient maximization of self-interest,

$$MI_a \Rightarrow B_a[2]$$

$$\therefore C_a \Rightarrow B_a[3]$$

In the world circumscribed by such assumptions, ideas are both an irrelevance and a distraction. For the only ideas that could matter in such a schema are the

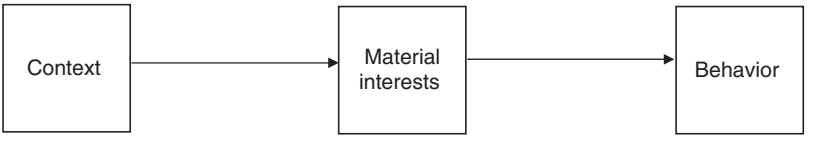

Figure 3.1 Naturalism: the context-dependence of behavior.

strategies of actors with respect to the context in which they are located. Yet such strategies are nothing more—and, indeed, nothing less—than efficient and entirely rational means to the end of securing and promoting an actor's self-interest. And self-interest is itself entirely determined by context. Thus, insofar as ideas in the form of strategies are carriers of behavior, they are entirely epiphenomenal—reflections of material necessity without any causal power of their own.

This is fine up to a point. But even in its own terms, there is one practical problem with it. Many—indeed, the majority—of game-theoretical renditions of such assumptions are indeterminate in the sense that their predictions are multiple rather than discrete. In other words, there is invariably more than one optimal strategy for the maximization of an actor's materially given self-interest. This has led many naturalists to acknowledge a second-order role for institutions and, indeed, ideas (in the form of the codified conventions and norms that constitute an institution) in selecting among such optima (see, e.g., Goldstein 1993; North 1990).

As this suggests, even naturalism's most ardent enthusiasts sometimes tire of the restrictions imposed by assuming social and political systems analogous to their natural counterparts. In so doing, they have come to soften, at various points, each and every one of propositions 1 to 3—usually only one at a time but sometimes in combination. Yet it is the softening of a fourth and thus far unacknowledged assumption that has done the most to open up the question of the relationship between ideas and interests that now so troubles contemporary political science.

That assumption—for consistency, let's call it proposition 4—is that actors are blessed with perfect information. It is not difficult to see why this is important. For it is only if actors are blessed with 20/20 vision when it comes to discerning the contours and nuances of the strategic terrain they inhabit that we can be sure that they will neither misperceive their materially given interests nor misidentify or fail to discern the strategies most effective in defending or advancing such interests through ignorance or lack of information. If actors are not quite so blessed, then the whole edifice comes tumbling down as—and precisely to the extent to which—the ideas actors hold acquire (or are seen to acquire) an independence of the context in which they arise. The more ideas mediate material interest, the more indeterminate social and political systems become.

Again, this is relatively easy to see. For if actors can misperceive or fail to recognize their materially given or "genuine" interests, and it is their perception of such interests rather than those interests themselves that informs, guides, and motivates their action, then the context that they inhabit is no longer an obvious guide to their behavior. As Judith Goldstein puts it, "only in a world of perfect information could interests be perfectly congruent with strategies"

(1993, 10). Although more intuitively plausible as an ontology—who, after all, would not admit to having come to reconceive their sense of their own self-interest as more information became available to them?—this concession comes at a considerable price to naturalism. For if the (ontological) independence of behavior and context is conceded, then the analogy between natural and social/political systems on which naturalism rests is profoundly challenged. Although it is rarely stated in quite such terms, this is precisely the point reached by mainstream attempts, notably from a number of prominent rational choice institutionalists (see, e.g., Denzau and North 1994; Goldstein 1993; North 1990), to show (or to deal with the consequences of acknowledging) that ideas matter. For to the extent that ideas might exert an independent influence on outcomes, the analogy between natural and social/political systems breaks down, and hence the appropriateness of naturalism as an epistemological guide for political science diminishes.

Rendering Interests Actionable: The Mediating Role of Ideas

The recognition of the significance of ideas and their independence from the context they seek to make sense of is, from a constructivist perspective, a very welcome development. And it has given rise to a rather different mode of political analysis from that which we have thus far discussed.

It can be outlined schematically as follows, and it is championed most clearly by Steven Lukes (1974; 2005) and Raymond Geuss (1981) (see figure 3.2). This, I suggest, is the conception of interests on which most ostensibly constructivist institutionalism falls back. Yet, as I will show, it is a far from consistently constructivist perspective.

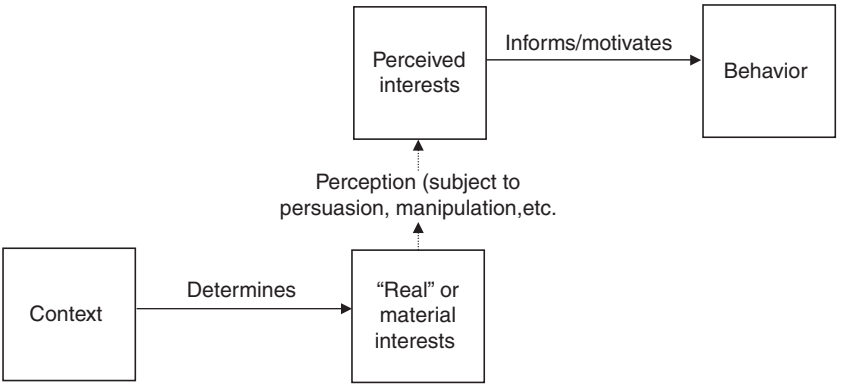

Figure 3.2 The Lukesian/Geussian conception of material self-interest.

In this conception, material self-interest remains context-dependent (as in figure 3.1). Consequently, for any given actor a,

$$C_a \Rightarrow MI_a[1]$$

Yet it is not material interest per se but the perception of material self-interest that motivates and informs behavior. So,

$$PI_a \Rightarrow B_a[4]$$

And since, in the absence of complete information,

$$PI_a \neq MI_a[5]$$

(the relationship between PI_a and MI_a being indeterminate),

$$MI_a \not\Rightarrow B_a[2']$$

$$\therefore C_a \not\Rightarrow B_a[3']$$

In other words, the relationship between context and behavior is indeterminate, and it is no longer possible to predict an actor's behavior from the context in which he or she is located.

As this already suggests, there are profound and substantial differences between the naturalist/rationalist conception of material interest outlined in figure 3.1 and the alternative conception of material interest outlined in figure 3.2. Yet in one key ontological respect, they are identical. Interests are held to be "real" and, in essence, to be materially given. The qualifier *in essence* is important. For there is, in fact, a fair amount of equivocation on this point in both Geuss (1981) and Lukes's revised formulation of the "radical" view of power (2005). Nonetheless, an essentially and explicitly materialist conception of self-interest is defended by both authors. Such a conception rests on a hypothetical counterfactual—the "complete information" or "perfect knowledge" condition, as Geuss terms it (1981, 48–49). This is nowhere more clearly stated than in William E. Connolly's work, on which Lukes draws explicitly. Policy x, Connolly suggests, "is more in A's interests than policy y if A, were he to experience the results of both x and y, would choose x as the result he would rather have for himself" (Connolly 1972, 472; Lukes 1974, 34). Geuss puts things slightly differently, suggesting somewhat paradoxically that "John's 'real' or 'true' or 'objective' interests are the ones he would have in the limiting case in which he had 'perfect' knowledge" (1981, 49). Both extracts are perplexing and illuminating in equal measure. And despite their ostensible similarities, they are somewhat at odds with each other. For Geuss, an actor's true interests are, in contrast to Connolly and Lukes, not those he or she would perceive himself or herself to have if he or she possessed complete information but those he or she *would* have if so blessed. This is extremely paradoxical, as it implies that in the absence of

complete information (i.e., in the "real do world"), an actor does not have do real interests. That is, presumably, not Geuss's intention, since he proceeds on the basis not only that actors in the real world have real interests but that the latter are, at least in their essentials, pretty much self-evident.

In addition, there is nothing very "objective" about interests that can be discerned only under conditions of complete information. Such a notion of material interests is an idealized extrapolation; it is unattainable and a purely theoretical construct.

This brings us to Connolly's definition of interests. A number of points about this are immediately important to note. First, as in Geuss's formulation, "real interests" are accessible, without theoretical reconstruction, only in a purely hypothetical scenario in which the actor has complete information of the consequences not only of acting on the basis of his or her perceived self-interest but also of acting on the basis of any and all possible conceptions of that self-interest. This is not so much complete information as a combination of omniscience about the present and perfect foresight. The actor must not only be blessed with a perfect grasp of the context in which he or she is situated, but he or she must also be able to replay, accurately, endlessly, and yet seemingly instantaneously, each and every possible future eventuality arising from each and every possible strategy for advancing each and every possible perception of his or her material self-interest. Needless to say, discerning as well as possible one's real interests is no simple task. Indeed, given the inherent difficulties of the exercise, one might be forgiven for expecting similarly located actors to reach wildly divergent perceptions of their own self-interest. Yet what is remarkable is that, by and large, they do not. This is, of course, conventionally taken as confirmation of the extent to which material interests are, for similarly located actors, essentially shared. Yet it is surely rather more plausible to see such similarities in perceived interests as a reflection of the prevalence, at any given point in time, of conventions for the evaluation of one's interests. Businesses, from this perspective, value profits not because it is innately good for them to do so or because it is structurally determined by virtue of their position in the relations of production to do so but because those responsible for corporate decisions are socialized in such a way as to evaluate business interests in such terms. In other words, it is conventional to conceive of corporate interests in this way. The more or less conventional character of interest perceptions is a point to which we will return.

Second, and rather more prosaically, in Connolly's formulation, my interests are given by the relative value to me (my preference ranking) of the results of all potential outcomes, regardless of the costs I may have to incur and the risks I may have to endure to achieve such outcomes. This works in a great many cases. For instance, I might recognize it to be in my interests to resist the offer of the next glass of wine, while not having the willpower to forgo the grat-

ification I am confident it will bring. In so doing, I effectively refuse to bear the cost (in terms of the anticipated gratification forgone) of realizing my interest. I act, quite consciously (and, arguably, quite rationally), in a manner I believe to be contrary to my own self-interest. Yet there are surely situations in which the conflation of desires and interests implied here is almost certainly unhelpful. To insist that results (in terms of relative merits and demerits to the individual) always trump the process by which such results are achieved in the determination of interests is, indeed, perverse on many occasions. I might very well perceive it to be advantageous and beneficial to myself to be fluent in numerous languages in the abstract, while recognizing that, here and now, I simply do not have the time, inclination, or linguistic capacity to bring about such a fortuitous situation. Is it irrational, in such a scenario, to conceive of acquiring greater linguistic competence as not being in my interests? Can I not value the outcome without believing it to be in my interests to do what I can to secure the outcome?

Third, and perhaps most significant of all, Connolly's (1972) conception of material interests is strangely immaterial. My material interests are those I would perceive myself to have were I better placed to see both the genuine nature of my current predicament and the full consequences of all possible courses of action available to me. Such information, were it available to me, would not render me, as Connolly and other defenders of a materialist conception of interests seem to assume, a mere bearer of my material context. Had I such information, I might well choose different, and more effective, strategies to seek to ensure the outcomes I value, but the things I value themselves cannot be derived from the context in which I find myself. However conventional they may be, the relative values I assign to my preferences are mine alone; they are not contextually determined and would remain indeterminate with respect to context even were I to have complete information. As such, any conception of material self-interest, whether that of the analyst or that of the actor and whether framed in a (hypothetical) condition of perfect information or behind the veil of ignorance, remains precisely that: a conception and a construction.

This, I think, is the constructivist insight. To think otherwise is to assume that similarly located actors blessed with complete information would conceive of their interests in an identical fashion and, if rational, would be motivated by such interests to behave identically. Conversely, to suggest that there is more to the seeming indeterminacy of human agency than lack of information is surely to concede the inherently subjective (and, indeed, intersubjective) character of interests, their value-dependence as well as their context-dependence. Lukes's radical review of the radical view of power is here somewhat perplexing. For in this major restatement and partial defense of his initial position, the author clearly concedes that actors similarly located might not share similar interests (see, e.g., Lukes 2005, 81). Moreover, Lukes emphasizes the

extent to which values and desires intermediate between context and interest (82, 145). Indeed, he freely concedes the "multiple and conflicting" character of any given actor's interests (145, 147), going on to state that "there is no reason to believe that there is a canonical set of … interests that will constitute the last word on the matter" (148). Yet he nonetheless continues to refer to interests as "real" (147), objective (80, 82), and materially given. Arguably, Geuss's position is more confusing still. To speak of an agent's interests, he suggests, "is to speak of the way that agent's particular desire could be rationally integrated into a coherent 'good life'" (Geuss 1981, 47–48). Surely, this renders interests as idealized abstraction based on both normative and inherently subjective/intersubjective evaluations (of desires and of "the good" such a life might hope to express). It is difficult to square this with Geuss's dogged defense of the "real," material, and objective character of interests. He goes on to say that "alcoholics can be said to have an 'interest' in giving up drink, even if they don't recognise it, because we know that health (and, in extreme case, life itself) is central to their conception of 'the good life' and that excessive drinking cannot be integrated into such a life" (48). This certainly does not make things any less confusing. Presumably, we "know" health to be central to a particular alcoholic's conception of the good life, since, putting to one side excessive drinking, the alcoholic appears to value healthier over less healthy options. Yet if this is Geuss's logic, then surely the converse also applies. By the same token, then, alcoholics might be said to have an interest in continuing to drink, since the pleasure they clearly derive from it is central to their sense of the good life, as their previous practice shows. Moreover, if my conception of the good life is central to the interests I have, then there is nothing very material or objective about those interests, though the strategies I devise in seeking to realize such interests must, if they are to prove effective, negotiate the material and objective contours of the terrain I inhabit. Nonetheless, as the example of the penitent alcoholic demonstrates all too well, it is not just material and objective constraints that must be negotiated. Oddly, then, for such a staunch materialist, his key analytical concept—that of interests—would seem to be little more than idealized extrapolation of subjective/intersubjective preferences—constructions, in fact.

This brings us to the nub of the matter—and to the value of a consistently constructivist take on interests. To conceive of the interests of an actor as real, material, and/or objective is to imply that they are discernible from—since they are ultimately determined by—the context in which the actor is located. This has an obvious appeal, as in rational choice theory, in that in combination with the assumption of rationality, it renders that actor's behavior predictable given the context in which it occurs. This, in turn, restores the equivalence between political systems and their natural counterparts, making possible a naturalist predictive science of politics. Yet it is to deny the agency, autonomy,

individuality, and identity of the agent. It is to reduce the agent to the status of a mere bearer, rather than a shaper, of systemic logics. It is, in short, to deny that agent's humanity. When that denial is defended in the name of parsimony, as in rational choice theory, it might well be warranted, especially where the irrealism of the resulting assumptions about political behavior is openly acknowledged and the correspondingly heuristic nature of the resulting models is emphasized. Yet that defense is simply not available to most institutionalists, concerned, as they are, with retrospective process tracing rather than with the development of a predictive and deductive mode of political analysis.

This suggests that it is time for constructivist institutionalists to dispense altogether with the concept of material self-interest in the explanatory political analysis in which they are engaged. This need not, of course, entail dispensing with the concept of interest altogether. But it does require the adoption of a (more) consistently constructivist stance with respect to this most ontologically, epistemologically, and normatively loaded concept. I turn to what such a conception might look like in conclusion.

CONCLUSION: TOWARD A CONSTRUCTIVIST CONCEPTION OF INTERESTS

Interests do not exist, but constructions of interests do. Such constructions are inherently normative and subjective/intersubjective conceptions of self-good—of what it would advantage the individual to do or to have done either on his or her behalf or inadvertently by others. They are idealized extrapolations of subjective/intersubjective preferences and, as such, are different from immediate and/or particular desires. I might, after all, desire that which I acknowledge not to be in my perception of my own best interest. Conceptions of self-interest provide a cognitive filter through which the actor orients himself or herself toward his or her environment, providing one (of several) means by which an actor evaluates the relative merits of contending potential courses of action. But such conceptions, though they arise out of an ongoing interaction with that context, are neither given or determined by it nor given or determined by the actor's knowledge of it. They have an autonomy from it.[8] They reflect, as much as anything else, subjective/intersubjective preferences regarding the things the actor values and the relative values the actor assigns to the desires he or she can imagine. Thus, however conventional my conception of my own self-interest might be, it is mine alone. It is, crucially, about what I value and the relative value, in the abstract, that I assign to the different conditions and possibilities that I can imagine at any given point in time. The penitent alcoholic might imagine a hypothetical and all too elusive healthy scenario in which he or she gives up altogether or significantly tempers his or her desires for vinous

gratification.[9] And he or she might also imagine a somewhat less hypothetical and altogether less healthy scenario in which the addiction continues. He or she might well assign a higher value to the former than to the latter in the abstract—indeed, if Geuss is right, then all alcoholics do (though I doubt that). But this does not, of course, prevent the alcoholic from drinking, though it might well lead him or her to desire no longer feeling the desire to drink.

How might such a conception of interests inform differently constructivist institutionalist analysis? Consider the rise of monetarism in the advanced liberal democracies out of the "crises" of the late 1970s. This topic has, perhaps unremarkably, attracted considerable attention from constructivist institutionalists. For it sees a profound and rapid change in the ideas informing economic policy in these democracies, which more conventional approaches have generally failed to explain—except by appeal to exogenous shocks (which, themselves, remain unexplained). The key question in this literature is why business in particular came to back monetarism, acting both as an ideational entrepreneur and as a financial backer for it. And the standard answer, insofar as one can be found, is that it did so because monetarism served the interests of business. Yet this proposition is itself never really defended, it presumably being assumed that it is largely self-evident. For what it is worth, I think it concedes far too much to monetarism to assume that it has led, for instance, to higher rates of profit than would otherwise have been the case. Yet the point is that we simply do not need to know whether monetarism was an expression of the "real" interests of capital in order to explain its origins and ascendancy.

The notion that monetarism was promoted by business because it served the interests of business to do so is simply wrong. It is not wrong because monetarism was not in the interests of business (that is almost impossible to adjudicate and, as I hope to show, an irrelevance here). It is wrong because monetarism was promoted by business not because it *did* serve business interests to do so but because it was *perceived* to serve business interests to do so. That might seem like a semantic distinction, but it is, in fact, crucial. And it is crucial because it changes the nature of the question we must now answer to explain the rise of monetarism.

For a genuine constructivist, to explain monetarism is not to explain how and why monetarism was a reflection of the material interest of business (and/or its other backers) but to explain how and why business leaders (and others) came to conceive (or, more accurately, to reconceive) of their interests in such terms. That might well not be a very difficult question to answer. But it is a question that largely remains unposed within the existing literature. Posing it in such terms is immediately suggestive of potential answers. It might well be, for instance, that business leaders backed monetarism, believing it to be in their interests to do so, because they had for a long time been accustomed to conceiving of their interests in a manner that gave a high value to the things

that monetarism prioritized (e.g., price stability) and a low value to those previous constraints on policy that it tended to discount (consensus, full employment, universal welfare free at the point of access, etc.). If this is correct, monetarism resonated directly not with a given set of extant material interests but with a particular conception of such interests—a conception that, to be sure, it partly came to reconfigure.

But the point is that in the absence of detailed substantive work informed by a constructivist take on interests, we are simply not yet in a position to answer the question of how and why business leaders came to conceive of monetarism as a set of economic policies that it would benefit them to promote. But it does hint at the way in which a more consistently constructivist institutionalism might provide answers to some of the thorniest puzzles in contemporary institutionalist analysis. It suggests, above all, that such answers do not lie in a materialist conception of self-interest and that the advantage of a consistently constructivist institutionalism is that it does not need to embroil itself in adjudicating the real interests of the actors whose behavior it studies. Finally, it is also suggestive of a research agenda for the further development of such a constructivist institutionalism. Such a research agenda would focus far more explicitly than it has to date on the social and political processes in and through which interests are identified, constructed, and rendered, in Mark Blyth's terms, "actionable" (see chapter 4 in this book). As this would suggest, the future of constructivist institutionalist analysis surely lies in detailed ethnographic research that maps and charts the development and redevelopment of interest perceptions rather than the abstract and deductive derivation from stylized assumptions of the "real"' interests of institutionally embedded actors.

NOTES

I am immensely grateful to Daniel Béland, Mark Blyth, Robert Cox, Vivien Schmidt, and Dan Wincott for their characteristically perceptive and incisive comments on earlier iterations of the argument presented here.

1. For useful reviews of discursive institutionalism, see, in particular, Schmidt 2005 and chapter 2 in this book.

2. This is, of course, not to deny that standard rational choice/neoclassical economic models can describe/predict disequilibrium outcomes (think of a multiplayer prisoner's dilemma game). Yet they do so by deriving such conclusions from models predicated on initial equilibrium conditions.

3. The caveat is, however, particularly important. Ontologies are not contending theories that can be adjudicated empirically, since what counts as evidence in the first place is not an ontologically neutral issue. Thus, while certain ontological assumptions can preclude a consideration, say, of disequilibrium dynamics (by essentially denying their existence), this does not in itself invalidate them. On the dangers of ontological evangelism, see Hay (2005; 2009b).

4. The affinities between constructivism in international relations theory and constructivist institutionalism are, perhaps on this point especially, considerable. And on the face of it, there is nothing terribly remarkable about that. Yet, however tempting it might be to attribute the latter's view of preference/interest formation to the former, this would be mistaken. For while the still recent labeling of constructivist institutionalism as a distinctive position in its own right has clearly been influenced by the prominence of constructivism within international relations theory (Abdelal, Blyth, and Parsons 2006), the causal and constitutive role accorded to ideas by such institutionalists predates the rise of constructivism in international relations (see, e.g., Blyth 1997; Hall 1993; Hay 1996). As such, constructivism in international relations and constructivist institutionalism are perhaps best seen as parallel, if initially distinct, developments.

5. So as to avoid the materialist connotations, I prefer the term *arena* to the more conventional *system*. The term is, however, used in much the same way to refer to the structured context in which social and political dynamics are played out.

6. The qualification is important. While all social and political systems are, by virtue of the agents that animate them, open, not all natural systems are closed. Indeed, Heisenberg's uncertainly principle is, in effect, a statement about the degree to which natural systems are (or might be held to be) open. It is nonetheless the case that naturalism in the social sciences invariably imports epistemological standards from the analysis of closed natural systems and imposes them on open social and political systems.

7. Such assumptions, it is important to note, are adopted not, for the most part, because they are genuinely believed to be true but because they are seen to be useful. The adoption of such assumptions, appropriately enough, is an efficient means to a particular end.

8. And not just a relative autonomy from it.

9. Both alcoholism and, indeed, penitence are, of course, constructions. By writing, in the abstract, about the "penitent alcoholic," I do not mean to imply that alcoholism is a self-evident description of an empirical phenomenon. Indeed, for a constructivist, it is rather better seen as a convention for labeling a range of behavioral phenomena. For present purposes, let us assume that the actor in question is happy to be described as an alcoholic though far from happy that his or her behavior lends itself so readily to that description.

4

Ideas, Uncertainty, and Evolution

Mark Blyth

"There is no perfectly static state in the history of life. Change is the norm."

—Orion Lewis and Sven Steinmo,
"Taking Evolution Seriously"

One way to make the case for ideational scholarship is, as many contributions to this book ably demonstrate, empirical. A scholar takes an outcome of interest and shows that ideas matter in explaining it (Berman, Parsons, Wincott, and Lieberman, chapters 5, 6, 7, and 10 in this book). Another way, as this book also shows, is through taxonomy. One examines a series of contributions, maps their distinctive logics of explanation, and thereby establishes that a particular school of thought has taken root (Mehta, Schmidt, and Hay, chapters 1, 2, and 3). There is, however, a third and perhaps more fundamental way to make the case for ideas.

This chapter argues that ideational scholarship rests, implicitly or explicitly, on a particular way of looking at the world, a distinct social ontology.[1] It then uses this understanding to make the further case that unless one is practicing a self-consciously ideational social science, one might be, as Keynes famously put it, a Euclidian geometer in a non-Euclidian world (Keynes 1937, 16). That is, to continue his geometric metaphor, we might live in world of knots and spheres, but without attending to ideas, social scientists are equipped to see only right angles and squares.

In making this case, this chapter and this book as a whole move beyond the claim that ideas matter and that scholarship should "take them seriously" (Blyth 1997; Blyth 2003) and suggests instead that practicing social science without viewing ideas as *fundamental* to both the nature of human action and causation in social systems produces seriously misleading explanations. The evidence on this score is now coming in (Taleb 2007; Tetlock 2005). Despite more than one hundred years of effort in this regard, political science, for example, has uncovered no overarching laws, has deduced and abandoned several dozens of theories, and is still consistently surprised by events despite all

of these efforts (Blyth 2006). The situation is, as far as I can tell, no better for other social sciences.

Recognizing this, I draw on work in uncertainty, probability, and evolutionary theory to argue that how the social world is put together necessitates a deep and systematic engagement with ideas, because without them, neither stability nor change in social systems can be fully understood. In referring to ideas, I draw attention to the frames that agents develop and deploy to make sense of the world, the weapons and blueprints that agents use to (re)structure their world, and the conventions that agents converge upon that give stability to that world (Blyth 2002, 34–45).

Ideas do not simply "matter" in that they mop up some unexplained variance in a particular outcome of interest, as much of the work in contemporary political science seems to assume. Rather, they matter in that they are simultaneously the media through which agents understand the world and the material that constitutes it. To appreciate this, we must first shake our foundations a little and "stress-test" a few of the assumptions that all too commonly underlie most nonideational social scientific theories. The point is not to discuss the appropriateness of what I term here the "normal science" model for studying the social world per se. Rather, the point of doing so is to show that most of the taken-for-granted assumptions that make nonideational theories work should not automatically be taken for granted.

WHY THE WORLD DOES NOT (OFTEN) CONFORM TO OUR THEORIES ABOUT IT

Four Fundamental Foundations

With few exceptions, nonideational social scientific theories have four taken-for-granted assumptions built into them. The first is that we live in a world of equilibrium (statics) rather than disequilibrium (dynamics). In plain English, we assume that the world hangs together most of the time and that change is pretty rare. Stability is thus seen as normal, and change is the exceptional thing to be explained. Characterizing change is therefore a project of explaining how the world shifts from one equilibrium to another—a question of comparative statics (Blyth 2001). For example, we might be interested in how feudalism (a stable equilibrium) gave way to a period of disequilibrium (the late medieval period, the industrial revolution, the rise of liberalism, etc.), which was then transformed into a new equilibrium (capitalism) (North and Thomas 1973). Or, to take an American example, we might look at how the Democratic Party, which began as a party of racial exclusion in the South (equilibrium 1), ended up becoming the party of African Americans in the North (equilibrium 2) (Marx 1998).

The second commonly held assumption is that causation in the world is linear. That is, for any and all factors (independent variables) X, under necessary and sufficient conditions (a-n), causation can be assumed to occur and affect factor Y (our dependent variable of interest) in a linear manner. One does not have to be a naive positivist to accept this premise. For example, commonly accepted explanations of the rise of capitalism (dependent variable) rely on the rise of urban merchant classes plus changes in the size of the optimal political unit (independent variables having certain critical values) (Moore 1966; Spruyt 1994). In such explanations, a + b (and maybe c) -> c/d, albeit through perhaps many intervening variables. Causes in this stable world are therefore assumed to be, with occasional nods to complexity theory (Jervis 1997), both linear and efficient.

Our third assumption, a corollary of the first two, is that change assumes a discontinuous function. Given stable equilibria and linear causation, explaining shifts of equilibrium requires the introduction of exogenous elements. Consider the literature on institutions. Institutions are seen as the very stuff of equilibrium. They are what structure politics and make the world hang together via path-dependence (linear causation).[2] As such, institutionalist theorists have typically relied on exogenous factors to "punctuate" their equilibrium models (Krasner 1984). For example, the Great Depression and World War II are often seen as the quintessential punctuations that restructured world politics in the mid-twentieth century (Weir and Skocpol 1985; Gourevitch 1986). Change is then discontinuous, rare, and, like the meteor that killed the dinosaurs, decidedly exogenous.

The fourth taken-for-granted assumption, given the above, is that outcomes in this world are normally distributed. That is, if the world is stable most of the time and if large changes to that world are rare and typically exogenous events, then such events would count as deviations; and the more destabilizing such events are, the more they should deviate from the mean. To return to the example of American politics, under Jim Crow rule, violence against blacks was a constitutive part of the political order of the South (Smith 1997). Large events, such as race riots, were rare. Revolutionary events, such as the civil rights movement, happen, it seems, once in a lifetime. If one were to map an "event set" of politically significant outcomes in the South during the past one hundred years, its shape would approximate a bell curve. Most of the action would be in the middle, with big events becoming increasingly rare the farther we deviate out from the mean.

What we have, then, when we add these four taken-for-granted assumptions together is a world that is usually in *equilibrium*, where causes are *linear*, where change comes from *exogenous* variables, and where outcomes are *normally* distributed (henceforth ELEN). These four assumptions form the implicit foundations of practically all social scientific explanations. They structure, at a most

basic level, the way we see the world and interrogate its mechanics. They are the taken-for-granted assumptions that serve as the basis of most of our theories about the world. There is, unfortunately, a problem with these fundamental assumptions: they might be fundamentally misleading.

Or Four Fallacies?

Let us take each assumption in turn and examine its plausibility.[3] Turning to our first assumption, is the world characterized by stable equilibria most of the time? As I sit in the developed West, typing on my Apple laptop, sipping a coffee that I just paid for by electronic debit, connected to the world wirelessly, the world around me seems pretty stable, regular, and predictable. Yet casual empiricism suggests that much of the world is far from stable. From Afghanistan to Zimbabwe, political and economic uncertainty and instability seem to be the default state of the world. Consider India's much-noticed equilibrium shift from underdevelopment to modernity. Mumbai and Bangalore might well be paragons of late modernity, akin to New York and London, but the fact of seven separate Maoist insurgencies going on in the Indian provinces should warn us that perhaps equilibrium is in the eye of the beholder rather than a given set of circumstances.[4]

Turning to assumption two, linear causation might be a common phenomenon in the natural world, but it is far from clear that causes in the social world lead to unambiguous and time-invariant effects.[5] Consider the Great Depression as a cause. Here, one phenomenon (deflation) affected, in some cases, very similar states yet produced extremely different effects, ranging from Swedish social democracy to German fascism and Japanese imperialism (Blyth 2007). One could posit a series of intervening variables to explain this variance, but one could just as easily, and perhaps more plausibly, acknowledge the "Galton's problem" dynamics at work and accept causation in such a world to be emergent rather than linear.[6]

Our third assumption, of exogenous sources of change, fares little better when one really examines the issue. First of all, what is actually exogenous and what is endogenous to the social world is oftentimes analytically, not empirically, adjudicated. As noted above, World War II is often invoked as an exogenous shock that punctuated the institutional order of the 1930s and made possible the construction of the West and the Rest thereafter. But to what extent is World War II exogenous in any empirical sense? Presumably, the Great Depression in some sense caused the war insofar as those prior events empowered agents within that context to make war, which then, in the manner of Charles Tilly, (re)made the state (Tilly 1993). What, then, does the word *exogenous*, like *equilibrium*, actually refer to here beyond a convenient artifice?[7]

Fourth, and perhaps most critical, outcomes in the social, political, and economic world are hardly ever normally distributed. Consider a few simple examples. Half of the decline in the dollar vis-à-vis the yen between 1986 and 2003 happened during ten days, or on just 0.21 percent of the trading time, and twenty stocks explain 40 percent of the performance of the S&P 500 (Taleb 2005). Recall that Suharto's "crony capitalism" and other East Asian regimes were being lauded by international financial institutions such as the World Bank for fostering economic development weeks before the entire East Asian political economy collapsed (Stiglitz 1998). Finally, one need only note how the multi-trillion meltdown in global financial markets that occurred in 2008 and 2009 was nowhere to be seen in the prior data and constituted yet another "ten sigma" (three times in the life of the universe) deviation just ten years after the last one (the East Asian financial crisis).

In sum, what if we live in a world that is actually disequilibrial and dynamic, where causes are endogenous and nonlinear, and where outcomes of interest are *not* normally distributed? The consequence is that our nonideational theories should lead us astray a lot of the time, which they do (Blyth 2006; Tetlock 2005). Why this is the case has an unobvious answer: a priori knowledge of probability distributions. Specifically, how the assumption of normality in outcomes, primus inter pares with our other three taken-for-granted assumptions, leads us both to misunderstand the way the world is put together and to see the world as being inherently stable, causally linear, and punctuated by exogenous causes. In doing so, we obscure the vital role of ideas in actually producing the taken-for-granted stability that we see around us and thereby assume the world to be much more stable than it actually is. Once this is appreciated, the role of ideas as critical components of social explanations becomes clear.

WHY THE "NORMAL" WORLD MISLEADS US

The Risk of Normal Distributions

These four assumptions—equilibrium, linearity, exogeneity, and normality (ELEN)—serve as the foundation of what we might call the standard model of the social world. Assuming that the world works according to ELEN-type rules gives us a fixity that allows us to posit, for example, autonomous agents with interests, with those agents' interests generated by the material context they find themselves in, as reflected in their preferences. We can then make the further assumption that such preferences are not subject to random reversals and further suppose that realizing those interests depends on resources, collective action limitations, and information. In doing so, we view the world that agents face under ELEN-type assumptions as a world of risk with incom-

plete information. With the "right" theory, one that has ELEN-type assumptions built into it, outcomes in such a world can be predicted within defined tolerances.[8] Specifically, with the assumption of "normality," we can assume that most agents, given their strategies, will be neither completely successful nor completely unsuccessful in their endeavors and that most of the action will lie in the middle of the distribution, which, in turn, explains why things are "stable" most of the time. Thinking of the world in this way has one great advantage: it makes this world a world of risk and therefore (probabilistically speaking) estimable. There are, however, very good reasons to doubt that the world is, in fact, "risky" a lot of the time.

Consider an example. A six-sided die might exhibit randomness in that it is possible (but most unlikely) that one could throw six threes in a row, but it would not take very long to figure out the expectation of 3.5 and thus the parameters of the possible given the possible values the generator (the die) can take. With more complex generators such as roulette wheels and lotteries, the problem is more computationally challenging but is essentially similar. There are only so many combinations of black and red, even and odd, or permutations of a seven-digit number, and with an adequate sample of past events, one could predict the mean, the median, and the higher moments of the distribution. It is, however, far from certain if rolling a die or spinning a wheel tells us much about how the social world operates, and yet ELEN-type theories assume that a die roll and the social world are analogous systems. But what if they are not?

Someone who thought such an analogy problematic was John Maynard Keynes. As he put it regarding outcomes in social systems: "We have, as a rule, only the vaguest idea of any but the most direct consequences of our acts . . . the fact that our knowledge of the future is fluctuating, vague, and uncertain, renders wealth a peculiarly unsuitable topic for the methods of classical economic theory. . . . [A]bout these matters there is no scientific basis on which to form any calculable probability whatever. We simply do not know" (Keynes 1937, 213–214).

Keynes suggests that the world of the die roll, the world of physical systems, and the world of human action might be more ontologically different than the case appears at first blush. While those parts of the physical world constituted by observable fixed value generators and constant causes might be predictable to a large degree, the social world more generally might be characterized by *uncertainty* rather than probabilistic risk. Uncertainty and risk are qualitatively different situations in which an infinite set of past throws of the die does not *necessarily* tell us what number will (probably) come up next. In such a world, past events and strategies drawn from them might not, as the recent global financial crisis has shown all too well, be a good guide to the future. With uncertainty rather than risk in the mix, the world and the outcomes it generates become a far less certain affair, even in a probabilistic sense.

Admitting that the world is deeply uncertain, rather than risky, is, however, problematic for any nonideational social science. If uncertainty rather than risk is accepted, then finite variance in outcomes cannot be assumed. Consequently, parameter estimates, the central limit theorem, probability calculus, ordinary least squares, and even linear efficient causation are all called into question. Given such upsetting conclusions, the key question becomes whether we live in a world of risk or uncertainty. For if we can establish that we do sometimes live in a world of uncertainty rather than risk, then it is a short step to show not only how ideas matter but, further, that ideas are *fundamental* to any "social" science.

The Problem of Hidden Generators

The basic problem that social actors face is that we do not actually see the generators of reality, but we see only their outcomes. Those outcomes are always mediated by human agents, which invites variation and uncertainty into the mix. Recognizing this does not mean that we have to draw a hard and fast distinction between the physical and social worlds, however. Indeed, much recent work in the physical sciences in fields as disparate as chemistry (Prigogine 1997), neuroscience (Rizzolatti 2008), and evolutionary biology (Kauffman 2008) has described a world in which change and uncertainty, not constancy and equilibrium, are taken as fundamental. I stress a continuum of possibilities rather than a hard and fast distinction between the natural and the social, defined by the degree of the observability of causal generators. What is important, then, is that mainstream social scientific understandings of the world have singularly failed to recognize this embrace of the contingent and the emergent by natural science, sticking to an eighteenth-century notion of causation married to a nineteenth-century understanding of mechanics.

To see why this is the case and why it is hugely important, it is helpful to play an imaginary game of Russian roulette. Normally, and quite reasonably, most people do not want to play Russian roulette, because they think that the risk/reward ratio is too high (one bullet and six chambers). This is correct, but it presumes that one knows how many chambers the gun has. This is the problem that Nassim Taleb and Avitel Pilpel have called "the problem of the non-observability of probability generators," and it cuts to the heart of why ideas are far more than simply another variable to be added to the usual suspects in a world governed by ELEN's assumptions (Taleb and Pilpel 2003).

Assume that I have been asked to play Russian roulette, and I am handed a gun whose chambers I cannot see, but I have reason to suspect that they number in the millions, not the usual six. I do know, however, that there is only one bullet in the gun. I am also told that each time I pull the trigger, I will receive $10,000. Being a good probabilist and needing some cash,

I decide to play. I click once and am $10,000 richer, so I click some more. By lunchtime, I am a millionaire, and I grow confident. Technically, each trigger pull is equivalent to a piece of information about the likely probability distribution I face. As I sample more and grow richer, assuming that more information is better than less, I should be able to make a more accurate prediction of where the bullet is. Unfortunately, with the next click, I blow my head off.

The problem here is that sampling on outcomes in the absence of direct observation of the generator suggests that an agent can compute risk, and in some environments, this assumption might be justified (we might live in a world of risk/see the generator, some of the time). However, accurate sampling assumes both a normal distribution of outcomes (or a priori knowledge of the actual distribution one is in and thus its parameters) and, crucially, direct observation of the generator of outcomes. To return to our example, if you know that you are facing a six-chamber revolver, you don't play. But if you think that there are a million chambers, you might play, and yet without direct observation of the generator (six or a million), no number of trigger pulls will tell you where in the distribution the bullet lies.

The problem is this: to estimate risk, one has to assume an adequate sample of past events, but how much is enough? How many trigger pulls will tell you where the bullet is if you cannot see the generator? The answer is none, as Bear Stearns and Lehman Brothers found out recently. This problem is endemic in the social world, since we do not normally see the generator of outcomes directly and thus can neither see nor set the parameters of the distribution apart from our sample of the distribution itself. To see why this is the case and why it tells us a lot about the role of ideas in the social world, imagine three possible worlds of human action governed by three different generators, and then decide which world we actually study. One of them does not need to admit ideas as part of a satisfactory explanation; the other two do.

How Normal Is the World?

Our first world is the familiar world of observable generators and computable probabilities. Here, we live in a world of risk. To return to the example of the die, we can see the generator directly (the die) and know that it has six possible outcomes. Given a few dozen throws of the die, the expected and actual means converge rapidly (via sampling), and this is sufficient to derive the higher moments of the distribution (Taleb and Pilpel 2003, 10). This distribution, given the fixed and known values of its generator, is reliably "normal," and sampling the past is a good guide to the future. One cannot throw a 317 with a six-sided die and skew the distribution. This world (type one) is reliably Gaussian and is, within a few standard deviations, predictable.

Our second world is a "fat-tailed" distribution (a Gaussian plus Poisson distribution), where uncertainty rather than risk prevails. Here, an example of the generator would be a complex lottery such as a stock market or a general election with multiple parties. In such an environment, although one can sample past data exhaustively, agents do not observe the generator of reality directly. One sees actors' decisions, not what is driving them; for example, adherence to a monetary theory of inflation or sentiment about "the new economy." As our game of Russian roulette shows, there is the possibility that large events (the bullet) not seen in the sample (the prior trigger pulls) might skew the results and become known only after the fact.

In the recent financial crisis, for example, no analysis of options sold or "chartist" analysis of stock prices over the past *N* period would have told us that the crisis was coming. For example, market returns might seem normal through sampling, but a "Russian default" or a "mortgage crisis" (financial bullets) will radically alter the distribution in ways that agents cannot calculate before the fact by sampling the past. This is why this world (type two) is governed by uncertainty as much as it is governed by risk. Agents simply cannot know what might hit them, although they might be (over)confident that the probability of being hit is small.

Our third world is even more problematic. Thus far, we have assumed some form of "normality" in the world, even allowing for fat tails in the distribution. However, imagine a generator such as the global economy. In this case, not only can one not see the generator directly, but agents also can sample the past till doomsday and actually become steadily *more wrong* about the future in doing so. As Taleb and Pilpel put it, with such complex generators, "it is not that it takes time for the experimental moments . . . to converge to the 'true' [moments]. In this case, these moments simply do not exist. This means . . . that no amount of observation whatsoever will give us E(Xn) [expected mean], Var(Xn) [expected variance], or higher-level moments that are close to the 'true' values . . . since no true values exist" (Taleb and Pilpel 2003, 14).

To see what this means in practice, consider an example from the literature on currency crises. There have been three generations of currency-crisis models during the past two and a half decades, driven by three distinct waves of currency crises that have happened out there in the world (Krugman 1979; Obstfeld 1994; Krugman 1996). The first set of models, which were derived from a set of crises in Latin America in the 1970s, proved unable to explain the second crisis, which developed in Europe and Mexico in the 1990s. In turn, these second-generation models failed to explain the next set of crises in East Asia in the late 1990s, and third-generation models were born. Given that the goal of scientific theory is to explain both old and new phenomena within the same framework, such generations of models would seem to be both post hoc and ad hoc. But perhaps there is another, more positive lesson here.

Rather than try to shoehorn the data to fit the prior models, these second- and third-generation models accepted that the causes that gave rise to the second and third rounds of crises were fundamentally different from those that gave rise to the first round. If this is the case, then filtering data through the first-generation models will necessarily be looking for a set of causes in the world that no longer exist; the second- and third-generation events become surprises or inexplicable. This example strongly suggests that we face a world of inconstant and emergent causes rather than a world of linear causation.[9] In such a world, outcomes are truly uncertain rather than risky, since the causes of phenomena in one period are not the same causes in a later period.[10]

Given all of this, when we assume that the generator of reality produces outcomes via constant causes that conform to a Gaussian distribution, we make too strong a claim about the world. Any sample of past events can be fitted to confirm the past, but it cannot be projected into the future with the confidence we usually assume. Nor, to return to an earlier example, can any sampling and taxonomy of racial violence in the U.S. South from Reconstruction to World War II tell you that someone, somewhere soon, is going to refuse to sit at the back of the bus and thereby (indirectly) start the civil rights revolution. Sampling the past to predict the future is not merely uncertain; it becomes a dangerous exercise, since such dynamics "invalidate our ability to conclude much from…past behavior to…future behavior; in particular, it makes it impossible for us to assign any specific probability to future outcomes, which makes the situation one of uncertainty" (Taleb and Pilpel 2003, 16).

Further Complications: Interdependence and Linear Causation

These problems are further complicated by a feature endemic to complex social systems with hidden generators: the problem of interdependence of subject and object. While distributional normality cannot be assumed because of hidden generators and inconstant causes, the interdependence problem similarly undermines claims for linear causation. To see why, imagine two rival views of why the stars move as they do: physics and astrology.

Physicists make no claim that the stars affect our lives, whereas astrologists do. Indeed, proponents of astrology structure their lives around their readings of the stars. What is interesting here is that despite these fundamental differences between the two groups, regardless of which view one adheres to, whatever either physicists or astrologers think about the stars has no impact on the movement of the stars themselves. There is in this case, as there is in the natural world in general, an independence of subject and object. Notice, however, that for astrologers, the movements of the stars, given their ideas about those movements, affect the social system itself. In adhering to astrological beliefs about the stars, the world these agents operates in is altered, since their behavior is governed by their

ideas about what controls behavior: the stars. This type of "recursive looping" undermines claims about linearity in the social world from the get-go.[11]

Think, for example, about an environment with complex hidden generators that became all too visible when least expected; the recent global financial crisis. The financial equivalent of astrology is called technical analysis or chartism, in which millions of data points are fitted to curves in order to predict the future, given prior curves drawn from other data. As we have noted above, however, complex systems such as internationalized economies are replete with interdependence effects, emergent causes, and Galton dynamics. For example, if large numbers of financiers believe in technical analysis and the models they share that are built on those beliefs point to an inflection point coming up in the curve, then we can expect those actors using those models to alter their portfolio allocations in light of these beliefs, which would then alter the behavior and outcomes of the system itself (Mackenzie 2006). Such mechanisms in part underpin bubble dynamics (Minsky 1986).

Another example comes from the world of risk management, where banks use variants of what are called value at risk (VaR) models to calculate how much an institution has at risk at the end of the day when margined to the ninety-fifth or even ninety-ninth percentile. However, while it is quite rational for each individual bank to hedge itself across its portfolio in light of its VaR analysis, if all banks do it, the result is collectively disastrous. If all players chase the same asset classes and benchmark similar returns as targets, then while any single bank might be hedged, the super-portfolio of all banks' risk becomes dangerously correlated as everyone hedges the same way. This makes the system as a whole undiversified and prone to exaggerated-tail risks. As such, out of individually rational strategies to avoid risk, uncertainty emerges that makes the system as a whole, as we just saw in 2008 and 2009, prone to endogenously generated systemic crisis (Blyth 2009). These examples clearly show how emergence and interdependence are normal dynamics of complex systems, while simple causal linearity is the exception in such environments (Cartwright 2007).

In this case and in many other cases in the social and political world, particularly at higher levels of aggregation, subject and object are not independent. Rather, they are interdependent, since actions taken in light of beliefs alter the nature of the system itself. Admitting the problem of interdependence as an endemic feature of social systems means that linear causation becomes far more contingent than merely "necessary and sufficient." For if many causes have their roots in the reciprocal relations of ideas, agents, and objects (financial theorists, financial analysts, and financial systems), then nonlinearity resulting from interdependence must be seen as an endemic feature of social reality rather than simply an added complication to be more or less ignored in the name of parsimony. Viewed either way, two of our foundational assumptions, normality and linearity, might not be as foundational as we like to think.

If uncertainty, nonnormality, interdependence, and nonlinearity are then equi-plausible conditions of human action, which of our three worlds is the one most like the one we wish to understand through social science? Our first hypothetical world can be ruled out, since if the world were so predictable (and directly observable), risk would be our only issue, and as a consequence, our theories should be able to predict accurately. Given how rapidly the world changes around us and our (inter)disciplinary track record in apprehending such changes, it is probably safe to conclude that the world we occupy is not this world (Blyth 2006). Our second world seems suspiciously normal most of the time but disconfirms our theories much more than it should, since we cannot see the generator of outcomes. Financial meltdowns, wars, civil strife, and so on, all occur far more often than our theories (based on the normal distribution of outcomes and linear causes) would predict. Our third world is, however, even more worrying, since it implies that the more one samples the past, the less one knows, and the more blind one becomes to the outcome just around the corner. Here, uncertainty is so high that agents would have a tough time living in this world. Life would be a series of constant surprises, and nothing would be predictable. In this world, society, and hence government, would be impossible, which suggests that we, or at least some of us, do not live in such a world. But maybe thinking of the world in this way finally allows us to figure out which world we occupy and why ideas need to be central to our social scientific endeavors.

ELEMENTS OF AN IDEATIONAL SOCIAL SCIENCE

Ideas as the Basis of "Contingent Stability"

Having brought normality and linearity into doubt, let us go back to ELEN for a moment and reconsider the first assumption of equilibrium. Our analysis suggests so far that the hidden generators of the social world cannot be assumed to produce "normality" in the distribution of outcomes. Interdependence effects further limit our ability to model the world as one of risk, given emergent, nonlinear causation. If these two factors are admitted, then, unfortunately, our first assumption of equilibrium cannot be taken for granted, either. While the modern world and all of its works seem very stable, especially for those of us lucky enough to live in the developed West, playing with Macs and sipping coffee, we also know from examples as varied as Tito's Yugoslavia and the Inca Empire that such stabilities can prove fleeting indeed. However, rather than deny this, if we view social equilibria as partial, fragile, and contingent, rather than a firm foundation for indubitable theory, then the centrality of ideas to both human action and explanations of cause in the social world comes one step closer.

To see why, consider the possibility that the default condition of most human societies is our very uncertain third world. In such a highly uncertain environment, all bets are off regarding what the future might bring. Humans do not, however, deal particularly well with such uncertainty and try to insulate themselves from it. On this point, contemporary rationalists, historical institutionalists, and constructivists all agree. For most theorists, at least, what holds the social world together is not simply given by the material environment. Rather, it is human agency that produces stability. Where such theories fundamentally differ, however, is in what agency is/does and how stability is actually produced.

For contemporary rational choice theorists, although the importance of norms and ideas is increasingly accepted, they still appear, at base, as a functional filler for incomplete information (Greif 2006). This view of the world, as noted above, takes for granted that the world is in principle computable. That is, there is a knowable and fixed probability space and a computable search algorithm that can exhaustively search that space. It also suggests, then, by derivation, that ideas act as a filter of incomplete information, thus decoding the environment to some version of its true (and hence risky and computable) nature. This, in turn, implies that if it were not for informational imperfections, agents could see the world as it "truly" is. That is, they could see directly the generators of outcomes. Agents in such a world would be able to edge toward what they think is a correspondence theory of the world. Given this, agency is then reducible to coping with risk in a materially given environment that conforms to ELEN-type rules. Equilibrium might be built by agents seeking gains in trade or distributional advantages given their interests (North 1990; Knight 1992), but once it is, given Nash-type assumptions and ELEN-type rules, the world is seen as linear and normally distributed. Ideas do not cause nonlinear interdependence effects, and hidden generators do not radically skew outcomes.

For historical institutionalists, at least those of this school who do pay attention to ideas, ideas are much more than filters or fillers. They are variously norms, conventions, schemas, and ideologies, collective products that make the world hang together (see Schmidt, Berman, and Lieberman, chapters 2, 5, and 10 in this book). In this, they implicitly see the world as being more uncertain than risky—but only implicitly. In this world, equilibrium might be a contingent property of institutions that no one really "designed," and some causes might even be nonlinear, but such dynamics are usually seen to reinforce rather than undermine the equilibrium over time (Pierson 2004). These theories still tend to operate within a variant of the standard model, even if their approach pushes them to explore its limits.

Contemporary constructivists, increasingly common throughout the social sciences, come closest of all to the position espoused here. Although such

scholars arrive at their distinct theoretical position from a variety of routes—through the analysis of processes of socialization, manipulation, persuasion, and mimesis, for example—what they argue is consonant with the position developed here (Hay and Parsons, chapters 3 and 6 in this book; Widmaier, Blyth, and Seabrooke 2007). That is, rather than stability being a property of individual agency in a world of risk (world one) or the result of institutional path-dependence in an uncertain/risky world (world two), constructivists see stability in social systems as being a social product derived from agency as agents attempt to move from our highly uncertain world three to a less uncertain (risk plus uncertainty) world two. Whether through the promulgation of social norms and conventions, the construction of institutions, or the evolution of governing schemas and ideologies, the result is the same. For constructivists, human agents create the stability that they take for granted.[12]

Indeed, constructivists go farther than either rationalists or institutionalists and argue that there is no stability apart from agency and that agency cannot be assumed to be a derivative consequence of the material environment plus collective action limitations. The assumption of equilibrium therefore disappears as a given condition and reappears as a contingent condition, which is entropic and therefore difficult to maintain. Since humans cannot live, or at least cannot develop complex societies, in a highly uncertain world, human societies develop norms, institutions, cultures, and even civilizations. But what is driving this process at base is the uncertain nature of the world and their efforts to tame that uncertainty.

Ideas, whether in the form of free trade doctrines, religious worldviews, schools of legal pedagogy, or laws of the road, are the basis of all such constructions. But when agents are successful at building stability, researchers (and agents) draw the inference that we live in a world of predictability and develop theories to navigate such a world. Nonideational theories imagine and model a world where ELEN reigns supreme (see Schmidt, chapter 2 in this book). Unfortunately, we actually have succeeded only in constructing a world of fat tails, where risk and uncertainty live side-by-side. We therefore think and model the world as a world of risk while living in a world of uncertainty; where contingency reigns, we see necessity; and where stability is constantly reconstructed and renegotiated by agents, researchers look for equilibria as the norm.[13]

If the above is plausible, then ideas must be seen as more than simply add-ons to existing frameworks. They are, instead, the fundamental media through which agents interpret the world and construct stability in it. And if the world is more uncertain, more nonlinear, and less normal than is commonly assumed, then a helpful way to view the environment is not to view it as a set of constant causes and invariant rules like a mechanical system. Rather, it suggests placing ideas in an uncertain world in an evolutionary perspective, in which ideas, agents, and an uncertain environment codetermine one another. Far from making social

scientific theory mushy and less deterministic, as I shall demonstrate below, such a perspective actually allows us to improve our theories by attending to what agents think they are doing and thereby attend to the unintended consequences of action in their environments. Key to such an understanding is to appreciate that social institutions do evolve but that they are populated by learning subjects who can "act back" upon their environment in purposive ways.

Ideas, Evolution, and Intelligence

When thinking about how, or even if, evolution occurs in the social world, especially from a constructivist standpoint, two things should be clarified at the outset. First of all, the core idea of evolution is not reducible to the notion of survival of the fittest. Indeed, modern evolutionary theory is decidedly nonreductionist (Lewontin 2000). Evolution does not imply that the most fleet of foot or supple of mind are fitter than the rest of the population and thus have a better chance of survival. Rather, what evolutionary theory actually argues is that the coevolution of organisms and environment, how they (re)act back upon one another (recursive looping, another form of interdependence), shows that which organism is fittest is chronically underdetermined. Since coevolution is therefore a contingent process, it is impossible to say which organism from a set of organisms, all adapted to their environments at a particular moment, will be the survivors of the future. The sum total of organisms acting upon their common environment alters that environment, thereby changing the future conditions of survival in unpredictable ways. How does this apply to human societies? One need only think of industrialism, greenhouse gases, and global warming, or even risk-management techniques, epistemic arrogance, and financial meltdowns, to get the point.

A second necessary clarification when applying evolutionary arguments to social systems is that human beings are peculiar evolutionary subjects in that they have intelligence. That intelligence allows for language, purposive action, and, most important, instrumentality. Human actions are consciously directed toward specific goals. To what extent, then, are humans affected by evolutionary pressures, given that they consciously design the environment and can anticipate effects? The answer to this is much more than we think.

First of all, as noted above, a social whole is seldom the sum of its human parts and, as such, has causal effects not reducible to those parts. As Lewis and Steinmo argue, given emergent rather than efficient causation arising from the contingent nature of coevolution, "a series of unguided interaction at the micro-level creates emergent properties at the higher levels of analysis" (Lewis and Steinmo 2007, 8). No industrial society sets out to cause global warming. No price-cutting businessman is out to start a deflation. No fast-food establishment (probably) is purposively out to cause obesity. And no bank sets out to start a run on the bank. But

it happens. In short, a social whole is seldom a simple aggregation of the strategies of its individual parts. Social systems are most definitely complex adaptive systems replete with feedback loops, unintended consequences, and nonlinear dynamics. They are also decidedly unergodic and nonprogrammable (Kauffman 2008). Layer this complex evolutionary view of macro-outcomes onto our micro-level of uncertainty and hidden generators, and it seems that while we may design the world around us, our control of the consequences of those designs might be, despite our intelligence, much less than we think. It also shows that how we think about the world affects the strategies for building contingent stability in that world.

Two examples show what is gained by adopting an evolutionary rather than an equilibrium perspective. Consider the much-admired "varieties of capitalism" literature that dominates discussions in contemporary comparative political economy (Hall and Soskice 2001). After going through various permutations, the literature has settled on a dual equilibrium of liberal market economies (LMEs) such as the United States and the United Kingdom, contrasted with coordinated market economies (CMEs) such as Germany. Either equilibrium is seen to be functional under conditions of globalization, since both LMEs and CMEs have homeostatic institutions that are particularly suited (selected) to the current environment. Both provide growth and employment, albeit with different inequality/efficiency trade-offs.

From the point of view of our first evolutionary caveat, that of coevolution, such an equilibrium view of the word runs into the problem that the very institutional advantages that make the LME and CME adaptive today might, over time, change the environment in such a way that they become less adapted to the environment. Increasing returns are not guaranteed, nor are they guaranteed to reinforce. Moreover, there are many other capitalisms (organisms) in the system that are neither LME nor CME, and their actions are also likely to alter the environment (China springs to mind). Given these evolutionary dynamics, present adaptability, as our examples noted above, is no guide to future success. The future is underdetermining, while our theories overdetermine what currently works.

Our second caveat, that of intelligent agents and evolution, speaks to the issues of unintended consequences and emergent causes as important parts of any social scientific explanation. Sheri Berman's analysis of the collapse of the German Social Democratic Party (SPD) in the interwar period is an excellent example of exactly this type of dynamic (Berman 2006; chapter 5 in this book). Berman notes that the German SPD members viewed themselves as the heirs of Karl Marx and rigidly held on to a view of the world governed by the idea of historical materialism, complete with its usual economistic, linear, and teleological components. Given such ideas, when Adolf Hitler came to prominence, the SPD could only see the Nazis as a "bourgeois deviation" of no consequence.

This "cognitive locking" (Blyth 2001; Berman in this book), combined with the fact that as an antiparliamentary party in charge of the parliament, the SPD was simultaneously trying to be the handmaiden and assassin of capitalism, ensured the demise of the party and the rise of another, much weaker one: the Nazi Party. In evolutionary terms, the SPD's ideas (particular intelligence) caused a lack of fit with the environment (its governing responsibilities) that allowed a competitor to flourish. Since social ideas are not correspondence theories of the world (we do not see it as it "really" is, given hidden generators, etc.), ideas and environment can therefore combine to produce outcomes that no one expects. These dynamics, plus the micro-level uncertainties discussed above, combine to make the social world an evolutionary and uncertain world in which ideas play a critical role as both the medium and media of stability and change.

CONCLUSION

This chapter has made a fundamental rather than empirical argument about how the social world is constituted, what type of social scientific enterprise this invites, and how ideas might play a rather central role in all of this. I have sought to make a maximalist case, one that some of the authors in this book will no doubt feel uncomfortable with. For while the case made here is that ideas matter all the time, since they are, at base, what enables both stability and change in social systems, this claim is contingent upon these type-two and type-three dynamics being active in the world. It is not readily apparent to those of us lucky enough to live in the wireless, connected, globalized, and rich part of the world that this is indeed the case, but that case has been made nonetheless.

The first substantive claim made in the chapter was that there were four unspoken, taken-for-granted assumptions that underlie most nonideationalist theories: equilibrium, linearity, exogeneity, and normality (ELEN). The first claim, regarding equilibrium being the normal state of the world, was challenged on the grounds that to the extent that social equilibria exist, they are probably not functional homeostatic entities that automatically reconstitute themselves. Equilibria should be seen as contingent and dynamic constructs in which agents constantly negotiate their environments, rather than steady states in which once they are established, agents have no reason to change them. Institutions stabilize the world only because of the constant struggles of agents to impose and contest the ideas that make them possible in the first place (Blyth 2002). Institutions are the result of agents' attempts to tame uncertainty and create stability, and they are never quite as equilibrating as our theories imagine.

The second taken-for-granted' assumption, linearity, was criticized on the grounds that outcomes in the social world occur from the interaction of complex hidden generators, inconstant causes, and evolutionary dynamics. Causation should be seen as emergent and contingent rather than linear and efficient.

The third assumption critiqued was that of exogenous causes as sources of change in otherwise equilibrium systems. The problem here was that declaring a cause to be exogenous often rests on an unacknowledged analytical nominalism, since what is truly exogenous to the social world can only be nonsocial causes. Thus, meteor strikes are truly exogenous in a way that wars or declarations that some event is a "crisis" are not (Hay 1999).

Finally, normality in outcomes was taken to task for the simple reason that it is simply not true empirically much of the time.

The other reasons discussed involved interdependence and evolution. The interdependence of subject and object in social systems is an absolutely fundamental aspect of their existence. Not addressing this and instead speaking of idealized "independent" and "dependent" variables (subjects and objects) ignore their mutual constitution and imbrications over time and the evolutionary nature of social systems. As Lewis and Steinmo put it, "the idea of isolating factors as independent variables may be an ontological fallacy" (2007, 10) that relies on reducing the world to a game of dice. Unfortunately, from the point of view of predictive social theory, at least, we probably do not live in this world. Unlike ideas such as the "laws" of probability that concern fixed and isolated known-value generators such as dice, ideas about the workings of the social world are almost never correspondence theories of the world as it really is, since that world is always evolving. In the language of philosophy, it is always becoming, never being. The struggle to define the world and thereby delineating what is worth bothering about in the first place—inflation or unemployment, Iran or China, *American Idol* or Americans being idle—is a political struggle that is fundamentally a contest over ideas.

At base, the standard model takes as given things that need to be investigated in their own right rather than taken for granted. Such assumptions are more appropriately thought of as conditions for our theories to operate under, rather than as constitutive elements of those theories, and those conditions vary for the reasons discussed above and seldom conform to the ideal typical case. Perhaps if we recognize this, if we take these ideas about ideas to heart, then, rather than face another century of failed predictions, empirical surprises, and contradictory results, we might finally, and rather against our nonideational instincts, actually develop *social* science by paying attention to the adjective as well as the noun that constitutes our collective enterprise.

NOTES

1. This is not to argue for a particular conception of ontology (e.g., that the world is always and everywhere composed of classes) as correct. Rather, it is to draw attention to the fact that social ontologies, more than natural ones, are worlds of becoming rather than being. I thank William Connolly for this formulation.

2. Some scholars, most notably Paul Pierson (2004), have made the case that nonlinear dynamics lie at the heart of path-dependence in social systems. I do not differ in this regard. Rather, I simply point out that most theorists seem to see path-dependence as a linear process occurring as a result of increasing costs of revision and socialization.

3. A common response to the position taken here is that theories cannot be tested by their assumptions, only by their predictive capacity (Friedman 1953). However, if one does invoke this standard, one also has to acknowledge that social scientific theories' predictive capacity is so low that the claim to "a different standard" collapses under its own logic; see Blyth (2006).

4. See http://www.satp.org/satporgtp/sair/Archives/3_24.htm, accessed April 2, 2007; and http://news.bbc.co.uk/2/hi/south_asia/8275249.stm, accessed September 25, 2009.

5. Even in the natural world, it is not always clear that linear causation operates. See Prigogine (1984).

6. That is, the whole is not reducible to its parts, and properties of the whole cannot be predicted from the parts. For examples, see Prigogine (1984) on chemistry; Lewontin (2000) and Kauffman (2008) on biology; and Connolly (2002) on neuroscience.

7. Perhaps realizing this, many institutionalist theorists have attempted to identify endogenous mechanisms of causation, such as institutional layering, conversion, and drift. Note that in doing so, however, such theories rob institutions of much of their stabilizing power and instead portray, usually implicitly, a world of disequilibria and dynamics. See Streeck and Thelen (2005).

8. Technically, one can think of this world as a Monte Carlo simulation in which millions of iterations of individual event histories converge through ergodicity to a sample mean with a statistically normal distribution. This world is Gaussian, and outcomes are normally distributed.

9. Such an approach sits squarely within work on complexity and biology by authors such as Goodwin (2001) and, more recently, Kauffman (2008), who engage emergent cause directly in their work on complex biological systems. Kauffman's notions of ontological emergence and "emergent possible" spaces where similar conditions produce dissimilar outcomes are particularly useful concepts for ideational scholars.

10. As Karl Popper (1990) (among others) has argued, these same dynamics also occur in natural systems. To take one example, in biosynthetic processes, the introduction of a new compound changes the parameters of the possible for any other compounds that can emerge from that point on. What occurs is thus a propensity rather than a probability.

11. This is not to say, however, that any and all social actions always have perverse outcomes, pace Hirschman (1991).

12. When I leave the house in the morning, I do not say to my wife, "See you later, honey, I'm off to replicate the structures of late capitalism," even if my going to work does precisely that.

13. One could almost say that thinking, like ELEN, degenerates.

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Part II

ANALYSIS

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5

Ideology, History, and Politics

Sheri Berman

As the work in this book indicates, some social scientists have once again begun to focus on the role of ideas in political life. Ideologies in particular have garnered much attention of late, partially in reaction, perhaps, to the premature prediction of their demise at the end of the Cold War. Indeed, once again, ideologies seem to have become the cause of major political upheavals. But although it is difficult to discuss the world we live in without reference to ideology, ideas-based explanations in general are still met with much skepticism, if not outright hostility, by other members of the social scientific community. To some degree, this is a result of intellectual biases. As the introduction to this book points out, for example, Marxists, many rational choice scholars, and realists tend to view ideas and ideology as mere epiphenomena, rising and falling thanks to changes in underlying economic interests or material conditions and not exerting a significant independent impact on political life. They have not, therefore been considered the subject worthy of much extended discussion. This is a mistake, because even a cursory glance at a newspaper or a simple reading of history shows that ideologies have played an important role in driving events down paths they would otherwise not have taken. Ideologies are a subset of ideas, broad "worldviews" that provide coherent interpretations of the world and guidelines for dealing with it (Knight 2006). Ideologies link people who would not otherwise be linked and create political communities that simply would not have existed in the ideologies' absence. And these communities are driven to achieve certain political ends rather than others, ends that would not necessarily have been pursued in the ideologies' absence.

But for other social scientists, skepticism is more methodological than philosophical: it is the amorphousness (and unquantifiable nature) of ideologies and lacunae in and occasional sloppiness of some of the existing literature on ideologies that leave them wary. One problem here is that much of the previous work on the subject has been done by intellectual historians, who often produce rich and fascinating accounts of the content and advocates of ideologies

but often ignore important questions concerning where ideologies come from or how they are shaped by the wider social, political, and economic contexts out of which they spring (but see Freeden 1998; Freeden 2001). Insofar as they do address such issues, the changing fortunes of ideologies are most often explained with reference to the actions of individual political figures or the intrinsic structure and nature of the ideologies themselves. Such accounts do not lend themselves to the generalizations, predictions, and, increasingly, modeling that are the bread and butter of social science.

Explanations produced by other social scientists sympathetic to the study of ideas, on the other hand, have their own problems. As noted in the introduction and in chapters 1 and 2 by Jal Mehta and Vivien Schmidt, a growing body of literature on ideational variables—ideology, ideas, norms, culture—has emerged in recent years. After years of neglect, scholars have once again begun studying the role played by things such as ideas, norms, and culture in political life (Berman 2001). While this literature has advanced our understanding of political actors and outcomes in critical ways, it is also plagued by a number of problems, one of which I will focus on here: its status quo bias. Ideational variables are often presented as preexisting parts of the landscape, and the focus is on how they produce stable patterns of behavior over time (Eckstein 1988; Blyth 1997). This is certainly a critical task, and those who undertake it have been successful in helping us understand why political actors often remain stubbornly attached to traditional behavioral or decision-making patterns even when confronted with powerful incentives to change course. Yet political life is never totally stable, and ideational scholars must be able to account for both continuity and change. If not, they will be open to the charge that one must look to other kinds of variables for the answers to crucial questions (Eckstein 1988; Pontusson 1995; Sewell 1992). In addition, from a methodological viewpoint, if we can show how different ideational variables emerge and develop over time, we will be in a much better position to trace out their independent impact on political life. Students of ideology (as well as ideational scholars more generally) need, in short, to spend more time exploring why individuals and groups jettison old ideologies and how new ideologies develop and are adopted. This is consistent with Jal Mehta's stress on the need to understand how ideas shift over time (chapter 1 in this book) and with Mark Blyth's emphasis on the need to recognize and explain periods of disequilibrium and equilibrium (chapter 4). This chapter will do just that. It will show that the best way to do this, furthermore, is through careful empirical analysis of the contexts within which new ideologies appear. Such analyses should additionally recognize that ideologies face, Janus-like, in two directions at once: toward theory and practice, toward abstract ideas and everyday political realities. They achieve their greatest power and hegemony when they seamlessly relate one to another, offering adherents both a satisfying explanation of the world and a

guide for mastering it. Although harmony can emerge between an ideology and its environment, it rarely lasts forever. The political, social, or economic landscape changes, and then the ideology becomes less useful than it was. Sometimes it can be tinkered with or updated to reflect and suit the new conditions; sometimes it just stagnates. If an alternative approach can seize the day, it might vault into prominence and power, and the cycle will begin again. The story of each period of ideological hegemony, therefore, truly begins with the decline of its predecessor.

Ideologies, in other words, rise and fall through a two-stage process. In the first stage, existing ideologies are questioned and tarnished, opening up a political space that competitors aspire to fill (Gourevitch 1984; Haas 1990; Krasner 1984; Rothstein 1996). In this phase, in other words, the perceived failures or inadequacies of the reigning intellectual paradigm(s) create a *demand* for new ideologies. Once a political space has begun to open, the second stage of the process begins, as some political actors start to develop and embrace alternative approaches. In this phase, a *supply* of new ideologies begins to appear, with contenders competing for mind share and political power. The ones that seem to offer the best solutions to contemporary problems, in turn, win out over their "competitors" (see also Mehta's emphasis on the need to "discern what differentiates victorious ideas from their rivals," in chapter 1 of this book). This chapter will trace such a pattern, using the rise of social democracy in late-nineteenth-century and early-twentieth-century western Europe as a case study (Berman 2003; Berman 2006). During this period, western European nations underwent massive change. New social groups increased in size and power; old political patterns and forms of social organization began to crumble; economies were transformed. These developments led many across the continent to question existing political ideologies and search for new ways of understanding and responding to the rapidly evolving world around them. The crises that buffeted the continent during the 1910s through the 1940s accelerated the process of reconsideration. Two world wars and a massive depression discredited many of the institutions, organizations, and approaches that had long dominated European politics, giving added impetus to the ideological reexamination and reformulation that was already under way.

In response to such massive changes and external shocks, political actors in all European societies not only began questioning existing ideologies, but they also began developing a variety of powerful alternatives to the ideological status quo. That the questioning took place across western Europe at around the same time and in a broadly similar way indicates that something beyond the borders of each country was at work. (Or to put it in Mark Blyth's terms, the disequilibrium injected into the system seemed to emanate from factors exogenous to each national case.) But the fact that the precise nature of political battles and their outcomes varied greatly from country to country

shows that one needs to go beyond broad structural changes and cross-national exogenous shocks and examine local political contexts and local political actors to get the full story. In short, from a methodological standpoint, fully understanding the origins of ideologies requires examining different variables and contexts.

THE ORIGINS OF SOCIAL DEMOCRACY: A DEMAND FOR NEW IDEOLOGIES APPEARS

By the last decades of the nineteenth century, a scientific and deterministic version of Marxism (which was largely codified by Friedrich Engels and popularized by Karl Kautsky) had established itself as the official ideology of much of the international socialist movement. The most distinctive features of this doctrine were historical materialism and class struggle, which together argued that history was propelled forward not by changes in human consciousness or behavior but rather by economic development and the resulting shifts in social relationships. As Engels put it: "The materialist conception of history starts from the proposition that…the final causes of all social changes and political revolutions are to be sought, not in men's brains, not in man's better insight into eternal truth and justice, but in changes in the modes of production and exchange. They are to be sought, not in the *philosophy* but in the *economics* of each particular epoch" (1962, 365–366).

As one observer noted, what historical materialism offered was an "obstetric" view of history: since capitalism had within it the seeds of the future socialist society, socialists had only to wait for economic development to push the system's internal contradictions to the point where the emergence of the new order would require little more than some midwifery (Cohen 1999). And in this drama, the role of midwife was played by class struggle and in particular by the proletariat. As Kautsky put it, "economic evolution inevitably brings on conditions that will compel the exploited classes to rise against this system of private ownership" (1910, 90). With each passing day, wrote Kautsky, ever larger would grow the group of "propertyless workers for whom the existing system [would become] unbearable; who have nothing to lose by its downfall but everything to gain" (1910, 119).

The orthodox Marxism promulgated by Engels and Kautsky was in many respects an extremely successful ideology. Within two decades, it had become the dominant socialist doctrine within the Second International, putting other strands of leftist thought on the defensive across much of the continent. Among the main reasons for this is that it provided a simple, accessible, and optimistic catechism suitable for mass proselytizing, offering a straightforward and powerful vision capable of winning converts across the globe. Furthermore, since

the 1870s and 1880s were difficult times for socialists across Europe, with economic depression and repressive legislation taking a heavy toll on workers' movements, a doctrine that stressed the misery, inefficiency, and imminent collapse of capitalism helped many socialists keep their faith during hard times. Still, as the turn of the century neared, the situation confronting European socialists was changing once again, and the new conditions threw orthodox Marxism's weaknesses into unflattering light.

The last years of the nineteenth century, like those of the twentieth, were marked by rapid and disorienting change. Then, as now, a wave of globalization swept the globe, engulfing much of the European periphery and bringing new world products to Europe's shores. The structure of private capitalism was also changing, as business engaged in a frenzy of mergers, acquisitions, and cartelization, while union organizing continued apace. By the end of the nineteenth century, observers such as Hobson, Lenin, and Hilferding were proclaiming the dawn of a new capitalist era. Alongside these economic shifts, social and political ones were also transforming Europe. Between 1870 and 1900, population grew by more than 30 percent; since agricultural and rural areas continued to decline, this left the region more urbanized and secularized than ever. This, in turn, weakened traditional elites and socioeconomic relationships and contributed to growing levels of class and political conflict.

These economic, social, and political changes created a number of problems for orthodox Marxism. One stemmed from the fact that many orthodox Marxist predictions had not come true. The proletariat was not growing more miserable, small farming and business were not disappearing, the middle classes were expanding and becoming more differentiated, and economic collapse seemed increasingly remote. By the 1890s, European capitalism was exhibiting renewed vigor, and the bourgeois state was undertaking important political, economic, and social reforms.

A second problem stemmed from the fact that orthodox Marxism had real failings, not merely as a guide to historical development but also as a guide to constructive political action. By the end of the nineteenth century, socialist parties were becoming powerful actors in a number of European countries, yet orthodox Marxism could not furnish them with a strategy for using their power to achieve their ultimate goals. Indeed, as noted above, orthodox Marxism had little in general to say about the long-term role of political organizations in the transition to socialism. For the most part, it offered only a counsel of passivity, of waiting for the contradictions within capitalism to emerge and bring the system down, and many found this highly unlikely, as well as increasingly unpalatable.

A third problem stemmed from the increasing gulf between orthodox Marxism's passive laissez-faire economism and the psychopolitical needs of mass populations under economic and social stress. This failing it shared with its liberal cousin, and as a result, both found themselves under attack during

the fin de siècle. Then, as now, a growing number of voices were heard bemoaning the erosion of traditional values and communities and the rise of social dislocation, atomization, and fragmentation. As a result, the era witnessed a surge in communitarian and nationalist thought and a renewed emphasis on the import of morality, ideals, and faith as Europeans groped for ways to reintegrate their societies and restore a sense of purpose to the corrupt, amoral, and "disenchanted" bourgeois world (Hughes 1977; Kohn 1949; Nisbet 1953; Nisbet 1966).

If socialism was not going to come about simply because it was inevitable (as Marx, Engels, and many of their influential followers believed), then it would have to be achieved as the result of human action. Some, such as Lenin, felt it could be imposed and set out to spur history along through the politico-military efforts of a revolutionary vanguard. In Lenin's revision of Marxism, historical materialism was replaced by the belief that individuals could be the prime movers of history. Indeed, as François Furet noted, Lenin's revolution "symbolize[d] above all the role of volition in politics and was the proof and even the guarantee that people can tear themselves away from their past in order to invent and construct a new society. It was the opposite of necessity" (1999, 31–32).

Other revisionists, however, rejected violence and elitism and chose instead to revamp the socialist program so as to attract the support of a majority of society. They felt that if the triumph of socialism was not going to be inevitable, it could be made desirable and emerge through the active, collective choices of human beings seeking a better, higher good. These democratic revisionists rejected the pseudoscientific and materialist justifications of socialism proffered by orthodox Marxists; they called for a rediscovery of socialism's moral roots and an emphasis on the ideals and spirit underlying the original Marxist project. (As some contemporary observers noted, they wanted to exchange Hegel for Kant.) Although their thoughts and actions often emerged independently and differed according to the local context, the democratic revisionists shared an emphasis on the desirability rather than the necessity of socialism and on human will and cross-class cooperation rather than irresistible economic forces and inevitable class conflict. By the early twentieth century, the gulf between democratic revisionists and orthodox Marxists made the international socialist movement, like many of its constituent parties, a house divided against itself.

THE ORIGINS OF SOCIAL DEMOCRACY: A SUPPLY OF NEW IDEOLOGIES EMERGES

The first practical challenges to orthodoxy arose in France, where the early arrival of democracy and the importance of the peasantry created strong

incentives for socialists to abandon some of Marxism's central tenets early on. France's first truly Marxist party, the Parti Ouvrier Français (POF), was born in 1879 and championed a crude and doctrinaire version of Marxism. The result was that already by 1882, some socialists had become so frustrated by the POF's insistence on the primacy of economic forces and the inevitability of class conflict that they split off to found a new party, the Possibilists (so called because of its faith in the possibilities offered by the Republic). Although the POF ruthlessly attacked this new party, during the coming years, it was forced to accept many of its stances to avoid electoral decline. For example, the POF increasingly became involved in developing and managing local welfare policies and institutions, practices for which its leaders had vociferously attacked the Possibilists only a few years earlier. In fact, in the municipalities in which it ruled, the POF devoted itself wholeheartedly to providing libraries, soup kitchens, school lunches, and old-age and sickness pensions—the whole range of policies and services associated with the welfare state (Baker 1967).The reality of French politics also led the POF to move away from orthodox views of societal development and relations, particularly with regard to the peasantry. By the early 1890s, it had become increasingly clear that, contrary to Marx's predictions, the peasantry in France was not declining and therefore represented a major source of potential votes. At the POF's 1892 congress, accordingly, the party put forward a program of agrarian reforms that, among other things, promised small peasants that their land would not be collectivized. At the party's 1894 congress, the POF went even further, pledging to expand its efforts on behalf of the peasantry (Jonas 1985; Landauer 1961; Lehmann 1970). Although the party's leaders tried to claim that this new agrarian program did not represent any fundamental deviation from orthodoxy, few were convinced by such protestations, and leading orthodox figures in the Second International criticized the POF accordingly. Engels, for example, characterized the POF's program as "inconsistent, futile, and opportunistic." "Bluntly speaking," he wrote, "in view of the small farmer's economic position . . . we can win him now or in the near future only by giving him promises which we know we cannot keep" (1972, 501). Yet despite such (correct) criticisms of the party's program as representing a dramatic break with orthodoxy, the POF refused to change course, and indeed, many now found themsleves criticizing the defenders of orthodoxy for being out of touch with the actual needs of the party.

But if the reality of electoral politics and the nature of French society caused the POF to abandon some hard-line positions, the Dreyfus affair caused an even more extensive and open questioning of many of the key components of orthodoxy. At the height of the controversy, the battle lines in French society—between monarchists and Republicans, the Church and secularists, and anti-Semites and humanists—were drawn more sharply than ever. What was at

issue in the Dreyfus affair was less the guilt or innocence of a particular individual than the desirability and future of the Republic. The controversy thus forced socialists to confront head-on their views about democracy. Should they defend the Republic because democracy was critical to the achievement of socialism or stay on the sidelines because the fate of the bourgeois state did not really concern them and had little to do with socialism's ultimate victory in any case? On one side stood the most prominent leaders of the POF such as Jules Guesde and Paul Lafargue, who simply could not bring themselves to defend the bourgeois Republic and accept the open modification of their understanding of socialism that such a defense would entail. On the other side stood those for whom the institutions and ideals of the Republic were a critical component of their socialist vision. The undisputed leader of this camp was Jean Jaurès, who argued that "the democratic Republic is not, as our self-styled doctrinaires of Marxism so often say, a purely bourgeois form . . . it heralds Socialism, it prepares for it, contains it implicitly to some extent, because only Rebublicanism can lead to Socialism by legal evolution without break of continuity" (in Thomson 1946, 49–50).

Jaurès's defense of the Republic during the Dreyfus affair brought him into the political spotlight. Indeed, from then until his assassination in 1914, he became one of the most important and beloved figures in the French and international socialist movements—and a champion of a vision of socialism that, although he himself often denied it, diverged fundamentally from orthodox Marxism.

Jaurès argued that socialism should be viewed not as the consequence of inevitable economic development and class conflict but rather as a possible result of the work of socialists themselves, who, motivated by their belief in a higher good, would use the democratic system to attract adherents and begin changing society from within. Jaurès believed that it was "contrary to common sense to suppose that the socialist idea could assert itself automatically, without the aid of human faith and enthusiasm. . . . Socialism would not exist without the forces set in motion by capitalism . . . but it would also not exist if it were not for the conscious will of humanity, a thirst for freedom and justice and inspired by the energy to transform the opportunities offered by capitalism into reality" (in Kolakowski 1978, 127). Jaurès also argued that in the struggle for socialism, democracy was the movement's most valuable instrument, since it could be used to effect a peaceful, gradual transformation of the existing order—a process that Jaurès referred to "revolutionary evolution." To set this process in motion, socialists needed to focus their attention on winning the support of the great mass of French citizens. "The great social changes that are called revolutions," Jaurès noted, "cannot, or rather can no longer, be accomplished by a minority. A revolutionary minority, no matter how intelligent and energetic, is not enough in modern times to bring about a revolution.

The co-operation and adhesion of a majority, and an immense majority, is needed" (Jaurès 1906, 51).

Jaurès's conviction that socialism could and should have broad appeal led him to reject orthodoxy's vision of class struggle. Rightly understood, socialism should appeal "not to a narrow faction" of society but to humanity itself, and its achievement should be the consequence not "of a violent and exclusive agitation of a social fraction, but . . . of a national movement" (Weinstein 1936, 54). Jaurès thus urged socialists to focus their attention not merely on workers but also on the numerous groups suffering most directly from the injustices of capitalism (such as small producers and peasants), and he believed that socialists should work with almost any group genuinely committed to positive change—including the bourgeoisie, who he argued had often been a force for progress.

Ultimately, the obvious attractions of the Republic won out, and Jaurès was able to convince a majority of French socialists that their overriding goal should be protecting it from anti-Dreyfus forces. With socialist support, a new pro-Republic government was elected in 1899 under Renè Waldeck-Rousseau, who then paid back the favor by asking a socialist named Alexandre Millerand to join his cabinet. This request, however, triggered a full-fledged crisis within the socialist movement, since the cross-class cooperation and acceptance of the bourgeois state that this would entail challenged orthodoxy head-on.

Millerand was a protégé of Jaurès and had long advocated a dramatic revision of socialism. He rejected orthodox Marxism's emphasis on economic developments and class struggle, arguing instead that the achievement of socialism required "the Socialist party endeavor[ing] to capture the government through universal suffrage." He believed that those who recoiled from full engagement with the institutions of the "bourgeois" state ran the risk of dooming socialism to insignificance, stating: "To put the people off to the mysterious date when a sudden miracle will change the face of the world, or day by day, reform by reform, by a patient or stubborn effort to win step by step all progress—those are the two methods we must choose between" (in Ensor 1904, 64). Joining a nonsocialist government was the natural consequence of such views, and so he accepted Waldeck-Rousseau's offer, thereby becoming the first European socialist ever to join a bourgeois government. This move outraged Guesde, Lafargue, and many others. Since ministerialism—the policy of socialists joining a bourgeois government—implied the viability of a political path to socialism and the desirability, if not necessity, of alliances with nonproletariat groups, it constituted, as one opponent noted, "an egregious violation of the fundamental doctrine of the class struggle" and an implicit rejection of the view that socialism would come about only as a result of the working of ineluctable economic forces (Edouard Vaillant, in Noland 1956, 261–262).

The seriousness of Millerand's move was reflected in the convocation in 1899 of France's first all-socialist congress since the split between the POF and

the Possibilists back in 1882. Not surprisingly, at the congress, Jaurès emerged as the strongest defender of Millerand and his larger strategy, reminding his audience that Guesde had been so convinced of the righteousness of orthodoxy and the inevitability of capitalism's collapse that he had predicted that socialism would arrive by 1900. "If we can't predict exactly when and how capitalism will collapse," Jaurès argued, we need to "work for those reforms which . . . will prepare the way" for socialism (in Goldberg 1962, 261–262). Guesde and other opponents of Millerand needed to take account of changing times and update their views accordingly.

The outcome of the congress was ambiguous. Two resolutions were adopted, the first declaring the participation of socialists in a bourgeois government incompatible with the principle of class struggle and the second saying that under "exceptional circumstances," such a tactic might be permitted. The controversy thus continued to simmer and, in fact, spread from France across the European continent, since it crystallized in practice the theoretical challenge to orthodox Marxism that Eduard Bernstein was simultaneously raising in Germany.

Bernstein was no ordinary socialist. He was one of the SPD's most important leaders and intellectual figures, a trusted and early colleague of Marx and Engels, and a good friend of Kautsky's. As such, his views had to be taken seriously. As one observer noted, "when Bernstein challenged the accuracy of Marxian prophecy it was as if the pope declared there would be no Second Coming" (Muravchik 2002, 95). In addition to his background, the fact that Bernstein's home base was the continent's most powerful socialist party and one that viewed its identity as being tied up with orthodox Marxism also gave his theoretical critique added weight.

Bernstein's apostasy originated in his assessment of the implications of the changing conditions of his day, in particular in his growing conviction that historical materialism could no longer provide a good explanation for the dynamics either of contemporary capitalism or of the transition to socialism: "No one will deny that the most important part in the foundation of Marxism, the basic law so to speak, that penetrates the whole system, is the particular *theory of history* known as the materialist conception of history. In principle, Marxism stands or falls with this theory; and insofar as it suffers modification, the relationship of the other parts to each other will be affected. Any investigation into the correctness of Marxism must therefore start with the question of whether or how far this theory is valid" (Bernstein 1993, 12).

Observing the world around him, Bernstein recognized that many of Marx's predictions were not being fulfilled. He argued, for example, that capitalism was not leading to an increasing concentration of wealth and the immiseration of society: "That the number of property owners increases rather than diminishes is not an invention of bourgeois 'harmony economists' but a fact which . . . can

now no longer be disputed. But what does this fact signify for the victory of socialism? Why should the achievement of socialism depend on its denial? Well, simply because . . . a plank threatens to break away from the scaffolding if one admits that the social surplus product is appropriated by an increasing instead of a decreasing number of property-owners" (1993, 200–201).

Indeed, during the late nineteenth century, Bernstein argued that capitalism had become increasingly complex and adaptable: "We impute to business relationships a rigidity and narrowness which might pertain to the age of manufacture or the beginning of the machine age . . . but which are blatantly at odds with the characteristic features of modern industrial life. [In fact, what we have witnessed over the past years is a] steadily growing number of different kinds of business . . . and [a] growing adaptability and flexibility [in] the contemporary business world (in Tudor and Tudor 1988, 164–165).

These observations reinforced Bernstein's opposition to the view that socialism could emerge only after capitalism collapsed: "I . . . oppose . . . the view that we stand at the threshold of an imminent collapse of bourgeois society, and that Social Democracy should allow its tactics to be determined by, or made dependent upon, the prospect of any forthcoming major catastrophe" (1993, 1). Indeed, he urged socialists to reject more generally arguments based on the view that "the victory of socialism [was] depend[ent] on . . . 'imminent economic necessity'"; socialists should recognize, he believed, that it was "neither possible nor necessary to give the victory of socialism a purely materialistic basis" (1993, 200).

Bernstein's loss of faith in the ability of economic developments to deliver the desired socialist outcome led him to an appreciation, like Lenin's, of the potential of human will and political action. Unlike Lenin, however, Bernstein made ethics, rather than violence, the motor of his revolution. If socialism was not something that *had* to be, then it should be "something that *ought* to be" (in Steger 1996, 95). He viewed orthodox Marxists as "Calvinists without God" (1993, 13) and argued that their faith in the inevitability of socialism bred a dangerous political passivity that would cost them the enthusiasm of the masses. Over the long term, he felt, individuals were motivated by their beliefs in ideals and by a vision of a better world; they could not be convinced to struggle or sacrifice for socialism if it was presented merely as the historically inevitable result of economic laws. This was why Bernstein criticized the tendency of the founding fathers to denigrate or dismiss the role of morals and ethics in history. Instead, he argued that there was only "one specific 'socialist' element in socialist theory: its all-pervasive *ethics* and its conception of justice. . . . [T]hese can never be science" (in Steger 1996, 117).

Socialism, in Bernstein's view, thus had to emerge from a conscious struggle for a better world. The challenge facing the working class was evolutionary: socialists had to come up with "positive suggestions for reform capable of

spurring fundamental change" (1993, 61). The socialist program, moreover, should aim "not at the decrease but the increase in social wealth," at improving the living conditions of the great masses of society. "With regard to reforms, we ask, not whether they will hasten the catastrophe which could bring us to power, but whether they further the development of the working class, whether they contribute to general progress" (in Tudor and Tudor 1988, 222).

Bernstein's revisionism replaced historical materialism with a belief in the primacy of politics, a conviction that individuals, motivated by their ideals and by a vision of a better world, could band together and use the power of the democratic state to gradually reshape the world around them. But Bernstein did not stop with a critique of historical materialism; instead, he went on to attack the second pillar of orthodox Marxism as well. In his view, the doctrine of inevitable class struggle shared the same fatal flaws as the belief in economic determinism: it was both historically inaccurate and politically debilitating. By the end of the nineteenth century, it was becoming clear that "If the collapse of modern society depends on the disappearance of the middle ranks . . . if it depends on the absorption of these middle ranks by the extremes above and below them" and on a continual increase in the ranks of the proletariat, "then its realization is no nearer in England, France and Germany today than at any earlier time" (Bernstein 1993, 78, 106). Bernstein urged his colleagues to recognize that "the intensification of social relations has not in fact occurred as the Manifesto depicts it. It is not only useless but extremely foolish to conceal this fact from ourselves. . . . Everywhere in the more advanced countries we see the class struggle assuming more moderate forms, and our prospects for the future would hold little hope if this were not the case" (1993, 2, 6).

The flip side of Bernstein's belief in a moderation of the class struggle was his faith in a potential community of interest between workers and the other citizens suffering from the injustices of capitalism. At the turn of the century, many in the middle classes and the peasantry were feeling economically threatened and politically frustrated. Bernstein saw them as potential allies or even recruits and urged socialists to ground their appeals in "the feeling of common humanity [and a] recognition of social interdependence" (in Steger 1996, 152). With his focus on cross-class cooperation and his rejection of the primacy of economic forces in history, Bernstein differentiated himself from his orthodox colleagues and tapped into the idealism, the quest for faith and activism, and the renewed emphasis on national unity that characterized the fin de siècle.

Arguments like Jaurès's and Bernstein's were echoed by a small but growing number of dissident socialists across Europe who shared an emphasis on a political path to socialism rather than its necessity and on cross-class cooperation rather than class conflicts. During the first years of the twentieth century, this challenge continued to spread as the reality of European economic and political life caused more and more socialists to question orthodox Marxism.

The result was that although orthodox Marxism remained the official ideology of most western European socialist parties and the Socialist International, it became increasingly beleaguered in the years leading up to World War I. The consequent tension and confusion left the international socialist movement, like many of its constituent parties, a house divided against itself. World War I and its aftermath brought the house down.

The immense changes wrought by World War I pushed increasing numbers of people on the left to openly reject the twin pillars of orthodox Marxism—class struggle and historical materialism—and to embrace their antitheses: cross-class cooperation and the primacy of politics. The first pillar suffered a critical blow with the outbreak of the war. Socialist parties across the continent abandoned their suspicion of bourgeois parties and institutions and threw their support behind the states they had hitherto pledged to destroy. Even the German SPD, the International's largest party and the standard-bearer of Marxist orthodoxy, pledged itself to the defense of the *Vaterland* and quickly voted to authorize war credits. In France, the socialists not only joined with other groups in a *union sacrèe* to defend the *patrie* but, putting aside years of controversy, also sent two of their most prominent members—Jules Guesde and Marcel Sembet—to join the government.

The doctrine of class struggle came under even more pressure in the postwar era, as the democratic wave that spread across much of Europe confronted socialists with unprecedented opportunities for participation in bourgeois governments. Given a chance to help form or even lead democratic administrations, many were forced to recognize the uncomfortable truth that workers alone could never deliver an electoral majority and that cooperation with nonproletarians was the price of political power. The war also revealed the immense mobilizing power of nationalism and bred a generation that valued community, solidarity, and struggle. Populist right-wing movements across the continent were riding these trends, and many socialists worried that clinging to orthodox Marxism's emphasis on class conflict would prevent them from responding to the needs of ordinary citizens and thus cause them to lose ground to competitors.

The second pillar, historical materialism, was also dealt a critical blow by the war and its aftermath. The pivotal position occupied by socialist parties during the interwar years made it increasingly difficult to avoid the question of how political power could contribute to socialist transformation, and the subsequent onset of the Great Depression made preaching submission to economic forces tantamount to political suicide. Indeed, by the early 1930s, the protest against liberalism and capitalism that had been growing since the end of the nineteenth century reached fever pitch, with the legions of the disaffected ready to be claimed by any political movement willing to press for change. Because of their emphasis on letting economic forces be the drivers of

history, orthodox Marxist parties here, too, ceded ground to activist groups on the right. Revisionist socialists watched these developments with concern and argued that clinging to orthodox Marxism would doom the democratic left to political oblivion. What was needed, they said, was a new program that would respond to the demands of the disoriented and discontented masses, and to develop such a program—an agenda for what would later become a truly distinctive "social democracy"—they returned to the themes and critiques offered by revisionists a generation before: the primacy of politics and the value of cross-class cooperation.

In the context of the interwar years and the Great Depression, this meant, first and foremost, using political forces to control economic ones. This led to sustained conflict with orthodox Marxists, since taming capitalism was something they did not believe possible. The conflict played itself out most dramatically in Germany, as the SPD leadership backed conservative economic policies and a conservative government during the early 1930s, while their supporters and the rest of German society clamored for a more activist response to the catastrophe befalling them (Berman 1997). Many, however, were frustrated with this course, and at the party's 1931 congress, union leader Fritz Tarnow summed up the dilemmas emanating from the SPD's policies as follows: "Are we standing at the sickbed of capitalism not only as doctors who want to heal the patient, but also as prospective heirs who can't wait for the end and would gladly help the process along with a little poison? . . . We are damned, I think, to be doctors who seriously want to cure, and yet we have to maintain the feeling that we are heirs who wish to receive the entire legacy of the capitalist system today rather than tomorrow. This double role, doctor and heir, is a damned difficult task" (SPD 1974, 45–46).

Despite the growing recognition that continued inaction was crippling the party as well as the Weimar Republic, the SPD's most important economic theorist, Rudolf Hilferding, along with most of its top leadership, refused to believe that they could truly make things better. Hilferding claimed that the only solution to the economic crisis was to wait for the business cycle to run its course. In his view, an "offensive economic policy" had no place, because the ultimate arbiter of developments was the "logic of capitalism." Not believing that politicians could resolve the Depression on their own, he fought all attempts at an activist, neo-Keynesian course shift (Berman 1997; Schneider 1975). (Since neoclassical liberalism and orthodox Marxism put their faith in ineluctable economic mechanisms and denigrated political interference in the economy, orthodox Marxists, Hilferding once noted, were the last and best of the classical economists; Gates 1970, 78).

The true social democrats were unwilling to accept such passivity and fought for programs that would use the power of the state to tame the capitalist system. Neither hoping for capitalism's demise nor worshipping the market

uncritically, they argued that the market's anarchic and destructive powers could and should be fettered at the same time that its ability to produce unprecedented material bounty was exploited. They thus came to champion a real "third way" between classical liberalism and Soviet communism based on a belief that political forces must be able to triumph over economic ones.

A key figure in these developments—and a crucial link between the revisionism of Bernstein and the mature social democracy of the post-'45 era—was the Belgian activist Hendrik de Man (Dodge 1979, 1966). Wartime experiences played a critical role in de Man's ideological and political evolution; by highlighting the tenacity of national feeling, the potential for state control of the economy, and the immense motivating power of morality and idealism, the war, in de Man's own words, "shook my Marxist faith to its foundations" (de Man 1928, 12). Becoming convinced that saving socialism required a fundamental break with prewar orthodoxy, de Man put forth a new vision of socialism in his writings, especially in *The Psychology of Socialism* and in his depression-fighting *Plan du travail*, which combined short-term policies designed to increase demand and credit flows with a long-term scheme for the transformation of the economy. In addition to providing an activist strategy for fighting the Depression, the *Plan* provided a generation of socialists (often referred to as *planistes*) with a new way of conceptualizing their role in society and the relationship between capitalism and socialism. As with Bernstein, de Man neither believed in nor hoped for capitalism's immediate collapse; instead, he argued that a strategy of evolutionary reforms could transform it. In order to begin this transformative process, de Man urged socialists to recognize that "the essential thing [was] not the taking over of . . . ownership but of control" (1928, 51). By capturing political power and the state, social democrats could direct and tame capitalism and insulate citizens from the destructiveness of the market without having to resort to Soviet-style nationalization. To prepare the left for the "struggle" for control, de Man, once again like Bernstein before him, turned to the great motivating power of morality and idealism. "The thoughtful members of a younger generation," de Man argued, "are yearning for . . . faith" that orthodox Marxism could not provide.

De Man and his *Plan* proved an inspiration to socialists across Europe. In France, *planisme* found its champions in Marcel Déat and a group of neosocialists who broke with the mainstream French Section of the Workers' International over the latter's orthodoxy and immobilism. Like de Man, Déat recognized that "in the present historical phase forms of society appear possible which are not yet socialist but which are no longer capitalist." The question socialists had to answer, therefore, was, "Are we going to allow these experiments to be carried out without us?" (White 1992, 87). Déat's answer was no, and he urged his colleagues to focus on gaining political power so that they could begin "transforming the world in which they lived" (Goodman 1973). In

Germany, *planistes* could be found among the generation of young socialists who came of age during World War I and saw in state intervention the basis for a long-called-for offensive strategy. One of the most prominent members of this group, for example, Carlo Mierendorff, argued that such policies would finally provide the SPD with a "concrete socialist vision," a way of showing German citizens that it could actively work to improve their lives (White 1992; Wohl 1997).

Planisme wasn't the only activist economic strategy championed by social democrats during the interwar years. Some found in proto-Keynesian policies the perfect tool for combating the depression and starting the process of social transformation. The best-known example of this occurred in Sweden, where the Swedish Social Democrat Party initiated the single most ambitious example to reshape capitalism from within, but such strategies found champions in a number of other European countries as well. For example, in Germany, it was not Mierendorff's *planisme* but actually the proto-Keynesian Woytinsky-Tarnow-Baade (WTB) plan (championed by Wladimir S. Woytinsky, a Russian émigré who became head of the main labor union's statistical bureau) that emerged as the main alternative to the reigning orthodoxy. Woytinksy argued that the time had come for the SPD to surrender its faith in historical development, "to stop lulling the masses with *sozialistische Zukunftsmusik*" (socialist future music) and the "mystical powers of the market" (Woytinsky 1931, 439). By using the levers of political power to help improve the lives of the masses, by helping to tame the anarchy of the market, and by showing the way to a more organized and just economy, the WTB plan could finally provide the labor movement with a concrete foundation upon which to build a new economic and social order (Woytinsky 1932).

Regardless of the specific policies they advocated, one thing that joined all of the budding interwar social democrats was a rejection of the passivity and economic determinism of orthodox Marxism and a belief in the need to use state power to tame the capitalist system. In order to do this, however—and finally relegate historical materialism to the dustbin of history—they had to win majority support for their programs. During the interwar years, then, many returned to the themes of cross-class cooperation that Bernstein and other revisionists had preached a generation earlier. Furthermore, in an era of dislocation and disorientation, these social democrats realized that appeals to the "people," "the community," and the common good were much more attractive than the class-struggle perspective of orthodox Marxism or the individualism of classical liberalism, and so they often embraced a communitarian, corporatist, and even nationalist approach.

Here again, de Man was a key figure. With Bernstein and others, he believed that the class-struggle perspective of Marx and Engels had been invalidated by changing economic and social conditions, and by the 1920s, he saw that a

purely proletarian focus would doom social democrats to minority status. Furthermore, de Man did not believe in inevitable conflicts of interest. Especially since the war, he argued, there had arisen "a national community of interest between certain groups of workers and employers, or between the working class and the employing class as a whole," as well as between workers and intellectuals (de Man 1928, 303). As a result, the time had come to replace existing conflictual relationships and institutions with cooperative and corporatist ones. In order to do this, however, socialists needed to design their policies and appeals with cross-class alliances and solidarity in mind. His *Plan du travail*, for example, was explicitly formulated to "appeal . . . not only to the working class but also to all classes of the population suffering from the present economic distress and to all men of good will" (in Dodge 1979, 291).

Comparable visions gained adherents across the continent. In France, Déat drew on the legacy of both Bernstein and Jaurès in developing his cross-class, corporatist, and *patrie*-centered *socialisme communautaire* (Goodman 1973, 172; White 1992, 172). In Germany, the *planistes* around Mierendorff also supported cross-class and corporatist appeals, while many of the supporters of the WTB plan became vigorous advocates of a *Volkspartei* strategy. It was in Scandinavia, however, and particularly in Sweden, that the new approach was embraced wholeheartedly by a unified party. This is why one must turn to Sweden to observe the full dimensions, and potential, of the new and truly social democratic alternative.

During the interwar years, the Swedish Social Democratic Party (SAP) developed a new view of its role and position in society, as well as of the relationship between politics and economics. Especially after World War I, an increasing number of party members began arguing that the market's productive powers could be harnessed while its destructive potential was controlled. The key figure in this development, Nils Karleby, insisted that "improvements in the efficiency of economic activity have always been, and should continue to be, the only means . . . of improving society's welfare" (Karleby 1926, 145). He argued further that capitalism or bourgeois property relations should be viewed as a bundle of rights. If ownership was only the conglomeration of a number of individual rights, then the rights could be separated from one another and gradually made subject to societal influence. Hence "[a]ll social reforms . . . resulting in an increase of societal and a decrease in private control over property [represent a stage] in social transformation. [Furthermore] social policies are, in fact, an overstepping of the boundaries of capitalism . . . an actual shift in the position of workers in society and the production process. *This is the original (and uniquely) social democratic view*" (Karleby 1926, 85, 83).

Such views laid the groundwork for the SAP's championing of a proto-Keynesian program during the Depression and selling this program to the electorate by stressing its activism and commitment to the common good. For

example, during the 1932 election campaign, a leading party paper proclaimed: "Humanity carries its destiny in its own hands. . . . Where the bourgeoisie preach laxity and submission to . . . fate, we appeal to people's desire for creativity . . . conscious that we both can and will succeed in shaping a social system in which the fruits of labor will go to the benefit of those who are willing to . . . participate in the common task" (SAP 1932). Similarly, at the party's 1932 congress, Rickard Sandler, a leading party activist, urged his colleagues to recognize that socialists had to "abandon the view that we or our children will enjoy some kind of 'freebie' socialism that . . . 'developments' will place in our hands." The only way society would move toward socialism, he argued, was if the party used all of its power to push it along this path (Karleby, in SAP 1932, 429).

While the SAP was trumpeting its willingness to use political and, in particular, state power to shape market developments, its leader, Per Albin Hansson, was popularizing his theme of Sweden as the *Folkhemmet*, or "people's home." He declared that "the basis of the home is community and togetherness" and stressed that social democracy sough to "break down the barriers that . . . separate citizens" (Berkling 1982, 227–230). The confluence of the party's activist economic strategy and its cross-class appeal came through clearly in its 1932 election manifesto: "We [see] a crisis developing which claims victims in all sectors of society. . . . In the middle of abundance . . . misery and unemployment prevails. [The SAP] does not question . . . whether those who have become capitalism's victims . . . are industrial workers, farmers, agricultural laborers . . . civil servants or intellectuals" (1959). As a result of these policies and appeals, while in countries such as Germany and Italy, it was the populist right that appeared politically dynamic and championed communal solidarity, in Sweden, it was the social democrats who became known as the party with exciting plans for helping the "little people" and as being "one with the nation."

By the mid-1930s, therefore, the democratic strand of revisionism had blossomed into a powerful and creative political movement all its own. In contrast to orthodox Marxism's and classical liberalism's laissez-faire approach, social democracy proclaimed the primacy of politics and a willingness to use the state to control, or at least direct, the market. In addition, in contrast with orthodox Marxism's focus on the proletariat and classical liberalism's focus on the individual, social democracy appealed to the ordinary or "little" people, the "community," and the collective good. These principles found expression in the advocacy of Keynesianism and state intervention in the economy, cross-class alliances and corporatism, and the welfare state. The former gave social democrats a theoretical rationale for using political power to reshape and redirect economic developments, while the latter embodied the norms of national solidarity and cooperation that lay at the heart of their worldview. By the end of the 1930s, all of the components of what would come to be known as the

postwar social democratic compromise had already been developed, although it was only in Sweden that they were fully implemented.

During the postwar era, this social democratic compromise provided the basis for a period of unprecedented political and economic success in western Europe. Indeed, the transformation of the political landscape in the decades after 1945 was nothing less than amazing, as parties across the political spectrum advocated, or at least accepted, some version of the policies and appeals championed by the revisionist and social democratic reformers of the early twentieth century. As a result, while some have referred to the system that reigned in the decades after World War II as "embedded liberalism," it would be much more correct to refer to it as "social democratic." Not only did the policies themselves flow naturally from principles that had been championed by democratic revisionists and social democrats (rather than liberals) for decades, they were also precisely the ones developed by activists such as de Man, Woytinsky, and the Swedes during the interwar years.

CONCLUSION

Ideology is one of the most important yet least understood political phenomena. In particular, social scientists have not spent enough time exploring why individuals and groups jettison old ideologies and how new ideologies develop and are adopted. This chapter has argued that ideologies often rise and fall through a two-stage process. In the first phase, existing ideologies come under question or even attack, and a political space is opened up that new ideologies aim to fill. In this phase, in other words, a demand for new ideologies emerges. Once such demand exists, the second phase of the process begins as political actors advance and adopt alternative approaches. In this phase, in other words, a supply of new ideologies begins to appear, with the new alternatives competing for adherents and power. In contrast with the approach taken by many previous analysts, this chapter has shown that in this two-stage process, both agency and structure play key roles.

Intellectual historians, who have produced many of the most influential studies of ideologies, have tended to focus on the role played by key individuals and their local contexts, giving the misleading impression that ideologies emerge largely from the internal debates and efforts of particular thinkers, writers, and activists. Those political scientists, on the other hand, who study ideology tend to focus on the influence of broad environmental and structural factors, giving the equally misleading impression that ideologies emerge merely in response to new external circumstances. In practice, of course, and as this chapter has shown, structure and agency work together to shape ideologies, and therefore, a true understanding of their origins and development must take both into account.

The study of social democracy is a case in point. Social democracy, as we have seen, had its origins in several structural and environmental changes that came together during the late nineteenth century to create a *demand* for new ideologies by undermining the main tenets of orthodox Marxism. Capitalism simply wasn't collapsing; small businesses and agriculture weren't disappearing; society wasn't becoming divided into two implacably opposed groups. Socialist parties, moreover, were gaining strength in various countries and found themselves confronting dilemmas about whether and how to use their new-found power to press for change—a topic about which orthodox Marxism had little to say. And the immense social dislocation caused by capitalism had generated a rise in communitarian and nationalist sentiment across the continent, a hunger for belonging that the chilly strictures of Marxist orthodoxy could do little to satisfy. The result, as we saw, was that challenges to orthodox Marxism arose in all western European countries during the fin –de siècle. The similarity of these challenges and their cross-national nature indicate that understanding their origin and development require looking at factors outside the borders of any particular country. At this stage, in other words, broad macro-historical analysis was necessary to understand how and why a demand for new ideologies appeared.

But if the demand for alternatives to orthodox Marxism was driven largely by environmental and structural changes, the *supply* side of the equation was not. To understand why particular challenges emerged and fared differently in each country, one must focus on individuals and the precise contexts within which they operated. At this stage, in other words, more fine-grained micro-analysis of national contexts and the actions of key political actors was necessary to understand why particular ideological alternatives won out in each country. Understanding the origins of ideology, in short, requires "careful historical reconstruction and process tracing" (Mehta, chapter 1 in this book) of both the macro and micro varieties.

In France, for example, the early arrival of a democratic political system and the continued importance of the peasantry created strong incentives to challenge orthodoxy early on. The notions that democracy was no better than its alternatives, that socialists should remain aloof from political alliances and cross-class cooperation, and that small farmers should be either ignored or encouraged to recognize that their days were numbered were particularly ludicrous in the French context. Such factors help to explain why the first practical challenges to orthodoxy arose in France. Germany, on the other hand, was the home of the most important theoretical challenge to orthodoxy, and to understand this, we must also turn to a study of individuals and country-level variables, in particular Eduard Bernstein and his milieu. No ordinary socialist, Bernstein was the executor of Engels's will, a close friend of Kautsky, and a major figure within both the international socialist movement and its most

important national party. It was his personal stature, along with the force and comprehensiveness of his theoretical critiques and the critical importance of the German SPD as the standard-bearer of Marxist ideology, that gave his challenge to orthodoxy such power and bite.

What was true of the origins and early development of revisionism was true of the transition to social democracy as well. Although attacks on orthodoxy grew increasingly powerful during the late nineteenth and early twentieth centuries, it took the immense structural and environmental changes wrought by World War I to turn revisionism into social democracy. The internationalist and revolutionary credentials of socialist parties across the continent were permanently tarnished by their decisions to support their national war efforts and even in some cases to join national unity governments, and many were overwhelmed by the nationalist upsurge accompanying the war. Many socialists' faith in the inevitability or even the desirability of class struggle never recovered from these blows. After the war, trends continued to push many socialists away from orthodoxy. Newly established democratic political systems, for example, provided socialist parties with new opportunities to share governing power, a development that led many to recognize that the orthodox insistence on the inevitability of class conflict and the undesirability of cross-class cooperation was a recipe for political irrelevance. The onset of the Great Depression put the final nail in the coffin of historical materialism and class struggle, as it became clear to many that orthodoxy's insistence that economic forces could not and should not be tampered with and that the needs and fears of groups outside the proletariat were of no concern to socialists was tantamount to political suicide.

Yet if to understand why social democracy emerged during the interwar period, one must look at cross-national structural and environmental trends, to understand the precise form that these new movements took and their varying degrees of success, once again, one must look within individual countries themselves. In Germany, the story of social democracy was one of abject failure. The unwillingness of the SPD to heed calls for a shift to a "people's party" strategy and its rejection of an activist, Keynesian-style response to the Depression were consequences of the actions of key individuals (such as Rudolf Hilferding), as well as a widespread belief that a social democratic course shift was simply too great a break with the past. In Sweden, on the other hand, precisely the opposite outcome emerged: the victory of social democracy and the adoption of a "people's party" appeal and a new economic strategy, thanks to the actions of key individuals and peculiarities of the Swedish situation. The SAP's leader during this period, Per Albin Hansson, was savvy and charismatic and worked with others to co-opt themes and appeals from the right—in particular, the idea of Sweden as the "people's home"—and to position his party as the champion not

merely of the proletariat but also of the "little people" more generally, the common good, and even the nation. Similarly, the SAP's adoption of "Keynesianism before Keynes" depended on the actions of key individuals such as Nils Karleby and Ernst Wigforss, who worked throughout the interwar years to build an acceptance of, and practical strategies for, using the state to control economic developments. The relative ease with which the final transition to social democracy was made in Sweden depended on the party's prewar acceptance of revisionism: By the 1930s, the intellectual and practical groundwork for a course shift had long been in place. And finally, unlike the SPD, which both felt itself and was considered by others to be the standard-bearer of orthodoxy, the Swedes, on the periphery of Europe, could move increasingly away from Marxism without having to worry very much aboutthe reactions of others.

This reexamination of social democracy has much to teach us about ideologies and ideational analysis more generally. In particular, it reminds us of the need to carefully analyze the role played by both structure and agency and the need for different types of historical analysis and process tracing in order to understand the two-stage process by which ideologies rise and fall. A better understanding of how and why ideologies develop, in turn, can critically contribute to the ideational "wave" currently sweeping through the social sciences.

6

Ideas, Position, and Supranationality

Craig Parsons

As the introduction and other chapters (such as Jal Mehta's chapter 1) have made clear, this book is about how ideas might matter in politics and how we would show how much they do. Other chapters in part II explore empirical issues ranging from the rise of social democratic ideology to the crystallization of knowledge regimes and the relationship between ideas and institutions in American race politics. In a similar vein, this chapter argues that the history of the European Union provides an unusually clear display of the impact of ideas in major political outcomes, and that this unusually clear case provides some unusually clear lessons for broader methodological and theoretical issues in comparative and international politics.

The impact of ideas in EU history, as causes irreducible to other factors, can be demonstrated with unusual clarity for four reasons. First, the EU's "supranational" institutions marked an explicit departure from previous organizing principles in modern politics. The EU project was very openly about action based on what people generally perceived as new and radical ideas. Second, the EU project came very far in a fairly short period of time. During less than fifty years, we can trace how new ideas emerged as active options and were built into new patterns of authority and resource allocation. Third, EU institution building has been a very elite-focused exercise—a process of "enlightened despotism," according to one French foreign minister (Védrine 1996)—so its core dynamics involve a manageable number of actors. Fourth and most important, a close look at elite patterns of support for the EU project turns up fortuitously clear foundations for an argument about the autonomy of ideas. Political debates over supranationality have cross-cut the main organizing lines of European politics to a striking degree. This pattern undercuts attempts to explain the EU project as any sort of straightforward, rational responses to salient features of an objective environment. Whether we consider how Europeans were collectively positioned in states, classes, parties, bureaucracies, sectors, or regions, we find that people in the same positions tended to disagree as much as they agreed about how supranational institution building would harm or benefit them. They have

fought a battle of ideas that their objective "interests"—by which nonideational scholars mean their position in some sort of unambiguous obstacle course (Parsons 2007)—were unable to decide for them.

The unusual clarity of the role of ideas in EU history carries implications for ideational scholarship more broadly and also some methodological lessons for nonideational scholars. It highlights how attention to fairly concrete positional patterns of political action (or the lack thereof) can complement more common ideational methods of process tracing and interpretation, even in situations without such dramatically cross-cutting constellations. It also showcases rather starkly some problems with nonideational scholarship that extend well beyond EU studies. In a case where divided elites perennially shouted at one another about explicitly new ideas, nonideational social scientists did their best to boil the politics down to straightforward reactions to positions in an evolving obstacle course. They managed to hold to such accounts by relying mainly on fairly narrow and selective process tracing of events rather than systematic research on the positional patterns implied by their own theories. I suggest that these errors were facilitated by a pursuit of nonideational hypotheses without serious consideration of ideational alternatives. In EU history, as in many other settings, nonideational theorists have held a debate only between positional arguments. They asked only what kind of positional pattern lay behind political outcomes, seeing little reason to look for debates or mobilization that cross-cut material or organizational positioning. Ideational explanation, by contrast, is built on the notion that action can vary independently from "objective" positioning. Its logic instructs us to investigate closely the relationship between positioning, ideas, and action, since there might well be multiple viable ways to interpret any position. This is certainly not to say that ideas always strongly cross-cut objective positioning. But this case where they did displays dramatically why nonideational theorists, too, must entertain the possibility of idea-caused variation to test their own claims seriously. Moreover, it suggests that if traditional theorists overlooked the role of ideas here, where it is unusually easy to see, they have probably missed ideational causes in many other places as well.

I present book-length empirical evidence for my account of EU history elsewhere (Parsons 2003) and only quickly summarize it here to offer bases for the methodological discussion and criticisms that follow.[1] The chapter's focus is on the role of material and organizational positioning in debates between ideational and nonideational explanatory claims.

SHOWING IDEAS AS CAUSES IN EU HISTORY

This section summarizes an argument based on archival and interview research across the treaty deals that constructed the EU: the European Coal and Steel Community (ECSC) in 1952, the European Economic Community (EEC) in

1957, the European Monetary System (EMS) in 1979, the Single European Act (SEA) in 1986, and the Maastricht Treaty and Economic and Monetary Union (EMU) in 1991. The best-known explanations of these deals (and the institutions they created) give short shrift to ideas. Structuralist scholars such as Andrew Moravcsik or Alan Milward explain the EU as the straightforward response to objective economic interdependence. Far from showcasing new ideas, they argue, the EU emerged from the "normal politics" of social demand and government response (Moravcsik 1998; Milward 1992). Institutionalists in the tradition of Ernst Haas explain the EU as an incrementally constructed, partly unintended outcome of "path-dependent" choices. They see EU institutions as less fully dictated by the material landscape, but argue that the gradual construction of a man-made obstacle course over it narrowed Europeans' options. Initial steps in institution building led to further steps through the interaction of self-interested supranational agents, social demands, and unintended organizational consequences (Haas 1958; Sandholtz and Stone-Sweet 1998; Pierson 1996; Fligstein and Mara-Drita 1996).

As we will see in a bit more detail below, these accounts suffer systematically from two failures to test their own logic. One is a focus mainly on process-tracing evidence that certain key actors confronted structural or institutional imperatives, with less systematic attention to patterns of support and opposition for various EU proposals. Structuralists try to trace leaders' endorsement of certain outcomes back to clear environmental pressures, downplaying that integrative projects were conceived by certain members of governments, parties, or interest groups and often advanced past widespread reluctance or outright hostility in the same groups. Institutionalists see supranational agents rallying ministers, party leaders, and interest groups to each outcome, overlooking that equally prominent ministers, party leaders, and interest-group members rejected the agents' proposals. The second (related) failure is inadequate attention to historical alternatives. Less focus on broad patterns of support and opposition means less focus on what opposed actors preferred. Rarely do structuralists or institutionalists spell out the range of possible outcomes from which their hypothesized causes selected.

Addressing these methodological oversights leads us to the causal significance of ideas in EU history. A more complete look at who sought or fought integrative projects shows that the pattern follows ideational lines that were distinct from structural or institutional positions. Reconstruction of the alternatives elites perceived at each step in European cooperation turns up strong support and viable "bargaining space" for Europes radically different from the one we have. The result is my argument: several different kinds of domestic coalitions and international institutional bargains were available in postwar Europe. Since debates over Europe cross-cut parties and coalitions and so separated European policy from electoral considerations, top leaders had the autonomy to assemble support for their personal European ideas. Leaders'

ideas, as a causal factor irreducible to others, selected the form and extent of international cooperation in postwar Europe across a wide range of active options.

Foundations

Three observations lay foundations for this argument. The first is methodological. Ideas' autonomous effects are most visible when they cross-cut structural and institutional positions. Where most members of a government, party, ministry, sector, or interest group endorse certain strategies, it is tempting to see those strategies as the fairly obvious, rational response to the shared position that the group occupies in an unambiguous landscape (as the "interests" of the group in question). As I elaborate below, in such cases, ideational claims about coherent group interests are forced to rely on process tracing and counterfactuals to argue for the effects of ideas. But where organizations and groups are strongly divided—and if those divisions do not trace to some demonstrable pattern of different incentives and constraints within the group—we know objective signals at the level of the group are not dictating clear strategies. If divided peers also *say* or *write* that they analyze the situation differently, we can conclude that different ideas are directing similarly positioned individuals to these divergent choices. A pattern of mobilization that strongly cross-cuts group lines at many levels, when combined with rhetorical (or "interpretive") evidence, offers the strongest possible demonstration of the autonomous effects of ideas on action.

Even given the confinement of EU deal making to high-level political elites, tracing its individual-level patterns across the history and member states of the EU is a gargantuan task. My second observation is that we obtain considerable leverage over the EU story by focusing on one country: France. Scholars of all theoretical persuasions agree that European cooperation took the shape it did in the 1950s—the institutionally strong, geographically limited EEC—above all because the French government demanded it. The preferences of the other main actors (Germany, Britain, Benelux) added up to favor broader and weaker institutional options. In the 1980s, the French led the charge to strengthen the EEC institutions over British opposition and German hesitation. From the 1970s to the 1990s, the French championed progressively stronger delegations of monetary sovereignty, again over British and German reluctance. At each step, other institutional deals (or the status quo) were clearly available had the French chosen less supranational strategies. Thus, certain French strategies were necessary causes of a wide range of European outcomes. If cross-cutting debates and interpretive evidence show that leaders' ideas selected among several viable French strategies, then variation in those leaders' ideas alone would have led to very different Europes.

My third observation—and the immediate foundation to my argument—is that just such a cross-cutting debate emerged in France in the late 1940s. Initial French postwar plans called for rebuilding France while blocking a German revival through occupation and anti-German alliances. By 1948, however, the emergence of the Cold War undercut these plans. The Americans were intent on rebuilding West Germany into an ally against the Soviets. In addition to simply forcing this on the French, they offered the incentive of Marshall Plan funds. In response, French parties, ministries, regions, and sectors fragmented in a debate around three potential strategies. They largely agreed that extensive European cooperation was unavoidable, but favored different formats. Adherents to a "traditional" model called for a minor modification of earlier plans. Some direct controls on Germany could still be salvaged; military and economic alliances with other powers could still be sought; if necessary, bilateral deals could even be struck with the Germans themselves. These steps would uphold a balance of power and support technical cooperation in standard bilateralism without risking French sovereignty in uncontrollable international organizations. Advocates of a "confederal" strategy shared this concern for sovereignty but felt that Franco-British direction of a multilateral Europe would best prevent German dominance and foster technical cooperation. Broad, weak organizations could ensure stability and economic coordination without requiring formal losses of sovereignty. Proponents of a "community" strategy, lastly, argued that only a new sort of strong, "supranational" European institution could control the Germans and bring prosperity on an American scale. Since most British abhorred supranationality, this meant forsaking the Franco-British counterweight to Germany. But the result would be real "integration," perhaps leading to a powerful "United States of Europe."

The Historical Argument

I build an argument on these foundations in four steps. First, international bargains and French domestic support for all three strategies were available—not just in the 1950s but across EU history. Let me sketch two examples: the approach to the EEC deal toward the beginning of the story and the approach to the EMU deal toward the end. In the mid-1950s, almost all French elites saw rising structural incentives to more European cooperation in industrial trade, agricultural trade, and the new realm of atomic energy. Internationally, France's partners were not pushing it into a community format. In industrial trade, the British, the Germans, and most Benelux elites favored extending the currently dominant format for cooperation: the confederal, Franco-British-dominated Organization for European Economic Cooperation (OEEC). In agriculture, all but the Dutch preferred the traditional status quo of bilateral export contracts. In atomic energy, the Germans, Benelux, and Italy prioritized

ties with the advanced British, all favoring a confederal OEEC plan. Domestically—contrary to most accounts—French business preferred bilateral or confederal OEEC deals to a binding "community" framework.[2] French agriculture was also "suspiciously antagonistic of anything more complicated" than bilateral contracts (Milward 1992, 293). Most French nuclear officials favored either OEEC cooperation or independence (Scheinman 1965). Most support for a supranational "community" deal came from politicians—though even here, fairly even thirds of parliamentarians scattered across right, left, and center favored traditional, confederal, and community formats.

Consider next the approach to EMU. In the late 1980s, almost all French elites saw rising structural and institutional pressures for monetary cooperation. Increasing capital mobility and the asymmetric constraints of earlier monetary deals in the EMS (which forced the French to shadow German interest rates) made some change desirable. Unlike in 1955, there was no question about the basic framework for cooperation: the EC and its EMS appendage were widely accepted. But within this partly institutionalized arena, further steps could still take several formats. Internationally, again, France's partners were not pushing the strongly community-style option of wholesale delegation of power to a supranational central bank (full EMU). If German Chancellor Helmut Kohl was open to full EMU, broader German reluctance meant that at most, Kohl could accept, not demand, this deal (Heisenberg 1999; Kaltenthaler 1998). The British favored the status quo but would accept either stronger links among national banks without new institutions (a traditional-style solution) or possibly an intermediate "common currency" plan (along confederal lines).[3] Domestically, there was "no evidence of strong private sector preferences in favor of or against EMU" in France (de Boissieu and Pisani-Ferry 1998, 82). Finance officials mostly favored either more national-bank coordination or the common currency. Successive finance ministers Édouard Balladur (Gaullist) and Pierre Bérégovoy (Socialist) strongly favored the confederal-style common currency—not just in initial discussions but also through the EMU negotiations (Aeschimann and Riché 1996; Bauchard 1994).[4] Again, parties on right and left were divided.

In both the EEC and EMU cases, then, alternative international deals and domestic coalitions were available. Why did the French government demand "community" deals? My second step is to claim that top French leaders[5] have consistently been elected on cleavages that cross-cut these European debates. This gave them the autonomy to pursue their personal preferences over European strategies. Each deal we see with hindsight as leading to the EU emerged when "pro-community" leaders reached power on other issues. After engaging France in treaty deals, they used payoffs on other issues and party and coalitional discipline to assemble one of several potential majorities for ratification. EEC negotiator Guy Mollet (Socialist) became premier in 1956 at the

head of a coalition built on social policies and led electorally by anti-EEC centrist Pierre Mendès France.[6] Rather than being lobbied by interest groups or parties to pursue EEC, Mollet earnestly lobbied *them* to accept it. Support for his unrelated policies in Algeria helped deliver ratification. EMU champion François Mitterrand surprised his own advisors in demanding the full, dated EMU commitment at Maastricht. He had no electoral mandate to do so; not only did his referendum to ratify win by the slimmest of margins (50.9 percent), but the victory was "built on the votes of his opponents in the [French right]" (Appleton 1992, 14). A clear majority of Mitterrand's 1988 voters opposed ratification. A majority of his Socialist parliamentarians voted to ratify "without really knowing why," under presidential and party pressure (Criddle 1993, 238). France chose these community deals, over radically different alternatives, because leaders enjoyed the autonomy to assemble support behind their personal ideas in a fragmented, "multidimensional issue space."

My third step elaborates the institutional consequences of these moves. While elections on other cleavages cycled power erratically among community, confederal, and traditional leaders, France's European policies displayed decreasing variation over time. Each community-style deal erected a further set of explicit constraints on subsequent policies (which is, of course, what institution-building treaties are for). The original ECSC deal did so the least; it was a narrow arrangement in sectors of decreasing importance. But the EEC expanded constraints across all of the foreign economic relations of France and its partners. The EMS added monetary constraints; the SEA brought changes across the domestic economy; EMU extended these steps and set them in a more supranational framework. After each deal, it became more difficult for anticommunity leaders to shift French strategies and European bargains back to the alternatives they had preferred. This did not mean that traditionalists or confederalists were "converted" to community zeal. At each step, the debate reappeared, and the community project only advanced when procommunity leaders reacquired control of policy making. But each round of debate concerned a narrower range of options. By the late 1990s, the debate was so narrow that extremists on right and left denounced *la pensée unique* ("the uniform thinking") of mainstream politicians. France and Europe were bound into the community architecture. Over the course of the 1980s and especially the 1990s, longtime EU skeptics (such as Jacques Chirac, who assumed the French presidency in 1995) forgot about their opposition, reluctantly rationalizing the supranational EU as being "in French interests."

My fourth step addresses a final obvious question. Why did community ideas gradually win this battle? Why didn't confederal or traditional leaders better institutionalize their strategies when in power? Although I believe the answer must recognize contingency—nothing made the community victory inevitable—two factors favored this outcome. One lay in the nature of community

ideas themselves. The community model emphasized that strong, binding, elaborate new institutions would solve a whole host of European problems. Competing confederal or traditional ideas centered on *avoiding* binding institutional deals. Thus, each procommunity leader left a legacy of new institutional constraints to his successor, whereas confederal and traditional leaders did not. This was not a result of passivity; Europe is littered with the weak husks of confederal and traditional projects (OECD, Council of Europe, WEU, Franco-German Treaty, etc.). But in a world where institutions matter, ideas that change them matter more than those that do not. The second factor was that community ideas connected fortuitously to other powerful European norms. The clearest such connection was law: enshrining community projects in elaborate legal arrangements (which confederal or traditional projects avoided) made them especially difficult to undo. In no way did law per se require supranational institutions, but basing such institutions in law gave them a long-term advantage (Weiler 1991; Burley and Mattli 1993; Alter 1997). Later community projects (EMS, EMU) connected to the newer norm of monetarism. As many economists pointed out, monetarism did not dictate EMU, but the broad legitimacy it extended to independent central banks gave power to simplistic arguments for EMU in public debates. The institutional activism of community ideas, together with connections to broader norms, helped them crowd out their competitors.

Overall, cross-cutting patterns of mobilization and rhetoric show that French leaders' ideas were distinct necessary causes of European outcomes across an immense range of possibilities. The difference between today's EU and the likely result of French traditional strategies was the whole range of outcomes considered by the EU literature, from the most authoritative international institutions ever built to a loosely organized diplomatic space much like what prevailed everywhere else in the twentieth century.[7] Why debates over supranationality came to cross-cut previous lines of political organization—the prior question to my argument—is hard to explain. It appears to have no systematic basis, with individuals in similar positions gravitating to different European ideas in the new debate for idiosyncratic reasons.[8] Once the cleavage emerged, however, action took on enduring patterns that did not trace to structural or institutional positioning and did trace to actors' consistent European rhetoric in private and in public.

SO WHAT? LESSONS FOR IDEATIONAL AND NONIDEATIONAL ARGUMENTS

Most empirical scholarship in the "ideational turn" codified in this book takes process tracing as its key method. The scholar traces the environmental pressures impinging on certain decisions, allowing that at least some of them

were relatively unambiguous ("intersubjectively present" or, in more positivist language, "objective"), and concludes that they did not fully determine a choice of action. The implication is that we need to interpret the ideas or beliefs or norms or practices that did. The author then traces out the logic by which certain ideas connected indeterminate environmental constraints and incentives to a specific strategy or action. Counterfactual analysis often sketches how the actor might have done something different given different ideas.

Process tracing of mechanisms of thinking and action is an unavoidable method for ideational argument. When employed alone, however, its main weakness is a lack of factual leverage on a range of causal effects of ideas (or anything else) relative to other causes. Skeptics can always question the range or degree assigned to certain pressures or incentives in the process, suggesting, for example, that the objective economic pressures toward a strategy have been underestimated—meaning that ideas caused it less than has been claimed. This is an important weakness, because "how much" questions form the entire terrain of debate with nonideational theorizing. Reasonable nonideational scholarship does not argue that people have no ideas in their heads (which would be an absurd view for academics). It simply suggests that the constructs people carry in their heads are fairly straightforward, rational, adaptable responses to their unambiguous environment. Usually, nonideational theorists are willing to allow some small residual autonomy for ideas, but they present it as decorative flourishes or random noise alongside the core dynamics of action. The problem with founding ideational claims solely on process tracing is that this draws no sharp lines around this residual area. If skeptics are allowed to imagine that a variety of underestimated or unnoticed structural or institutional conditions account for some of the variance claimed for ideas, they can always remain dismissive.

On one level, this problem is an irresolvable one confronted by all explanations. Stubborn skeptics can always speculate about unspecified causes we might have missed, or can vaguely contest our measurements of degree or range. But some ideational arguments offer ways to beat back the creep of skeptical "residualness." Most notable is the use of cross-case comparisons to support process tracing. In contexts ranging from the early industrial revolution to socialist party formation to interwar military strategizing, scholars have shown that actors in objectively similar situations adopted different strategies as a result of different ideas (Biernacki 1995; Kier 1997; Berman 1998). Other studies show the need for interpretation in the proliferation of similar policies across environmentally different cases (Finnemore 1996). This bolsters process tracing by suggesting in real cases (not just counterfactuals) that people acted differently given different ideas (or similarly given similar ideas but different structural and institutional contexts).

The weakness of this method is that cross-case (and especially cross-national) comparisons are rarely similar enough to ascribe very concrete variation to ideas. Critics might still suspect that unnoticed differences in structures or institutions account for part of the divergence (Berman 1998, 11).[9] My reliance on cross-cutting patterns of action in EU history shows a way to deal partly with this problem, at least in some cases. It turns the logic of cross-case comparison inside out. If we are trying to contest that a party, for example, faced unambiguous, nonideationally informed electoral incentives to adopt a certain strategy, even more convincing than showing that similar parties adopted different strategies is to show that a substantial number of members of the party in question wanted to adopt a different strategy at the time. This amounts to an individual-level comparison, contrasting how people in similar positions in the same landscape interpreted shared constraints and incentives. If we find such cross-cutting dissent, we gain the clearest and strongest possible bases for ideational claims. It allows us to specify a range of ideational causality that flows from the actors, not from our own interpretations as observers. If two people in the same position in the world argue respectively for actions *a* and *b*, then we know that environmental constraints at the level of their shared position allow for interpretations at least as different as *a* and *b*.

This method is just an extrapolation of the basic logic of nonideational explanation. As I have suggested, nonideational explanations address what people do as a function of their position in some sort of external landscape (with the exception of psychological explanations; see Parsons 2007). If we show that two people in the same position did not interpret that landscape similarly, we have strong bases for arguing that interpretations made a difference. Of course, the reverse is not true: where groups of people in similar positions interpret the landscape in similar ways, this does not imply that ideas are absent. In fact, the most important ideas in the world are probably those that spread across large, powerful groups of people in fairly consensual ways. But where ideas cross-cut positioning in structural and institutional obstacle courses, we gain a powerful method for demonstrating a range of distinct ideational causality.[10]

While strongly cross-cutting patterns such as those in French ideas about Europe might be fairly rare, even less complete cross-cutting patterns can greatly strengthen ideational claims. To stick with party examples, consider a case in which 20 percent of a party's executive committee argues strongly against a certain shift in strategy. They loudly assert that the majority is misinterpreting the electoral incentives or is in hock to some unworkable doctrine and consistently advance a different understanding of the situation in speech and writing. Unless we can find another demonstrable set of incentives at some other level that targets this 20 percent but does not apply to the rest of the committee—perhaps they are young bucks who see openings for a challenge to leadership—the most reasonable conclusion is that they really do hold

different beliefs about the party's best strategy. We can then attempt to document this in other statements and actions. Given this combination of patterns of argument and mobilization with rhetorical evidence of certain ideas, we can then infer that certain ideas are distinct causes of party strategies across something close to this range of debate.

The importance of the EU case for ideational methodology, then, is that it presents an unusually clear example of a method that can strengthen many ideational claims. Again, the point is not that our world is riddled with ideas that strongly cross-cut all kinds of structural and institutional positioning. Most ideas run mostly parallel to some structural and institutional lines, for the very good reason that mostly rational human beings do tend to form their ideas with some regard to salient aspects of intersubjectively present structural and institutional obstacle courses. But substantial dissent in organizations and groups is not exactly a rare phenomenon, especially in complex policy decisions or other major political strategies. Ideational arguments should always begin by looking for it. No matter how clever we are at process tracing or cross-case comparisons, the hardest-to-reject evidence for the influence of interpretation is if real actors interpreted the same conditions differently.

WHY NONIDEATIONAL CLAIMS HAVEN'T PAID MUCH ATTENTION TO POSITIONING

Now I turn to a more deconstructionist exercise. My research suggests that the nonideational approaches to EU history have leaped over some fairly clear empirical gaps in their arguments. Of course, proponents of those nonideational approaches will contest just how clear my empirical claims are, and I welcome that empirically focused debate. Again, however, I cannot detail my side of it in a short chapter. But if we take as granted for the moment that my claims hold up respectably in longer exposition, we can pose some other questions. What might have led theorists of positional explanations to overlook the patterns described above? What theoretical leaps allowed them to defend explanations even without careful research on these patterns?

Let me be clear: my research does not just suggest that traditional accounts overlook the rhetoric of integration or its precise process. Even if we discount my claims a bit to allow for subjective interpretation of evidence, they suggest that the standard accounts missed major concrete patterns of support and opposition for the EU project. Consider the EEC deal again. Ernst Haas famously argued that the French and other governments were carried along to the EEC outcome by institutional path-dependence from the earlier ECSC deal (Haas 1958). Moravcsik presented Guy Mollet's pursuit of the EEC deal as reflecting clear structural "French interests" in the late 1950s (Moravcsik

1998). But easily available historical sources show that toward the end of the EEC negotiations, a clear majority of Mollet's cabinet ministers, most of his bureaucratic experts, most of his party, and his main interest groups viewed the treaty with either hostility or reluctance. Most French elites simply did not perceive the institutional pressures or structural imperatives that scholars have drawn around them. What kind of methodological choices could lead to such tortured conclusions?

In my view, these oversights follow partly from the long-running exclusion of ideational theorizing from debates over the EU. Early on, scholarship on European integration became a debate between positional claims that did not confront ideational competitors. After some early atheoretical accounts stressed the role of Europeanist ideology in the EU project (Aron and Lerner 1957; Brugmans 1965; Lipgens 1977), ideas basically fell out of the discussion along with the general dismissal of ideational explanation described by our editors. As we have already seen, one major side of these debates was materialist and structural. Historians debated whether it was mainly economic or geopolitical positioning that dictated the EU outcome (Milward 1992; Hitchcock 1998). Moravcsik confronted similar debates in political science between international relations realists and neoliberals and combined them by boiling down the EU story to a clever combination of interest groups reacting to economic positions and states reacting to positions in a distribution of power.[11] The other major side of these debates was institutionalist. Proponents of this approach focused on showing that the structuralists' nationally framed aggregations of positions failed to capture what happened when early institution-building steps altered the shape of the positional obstacle course. They argued that the aggregation of positions in European societies was gradually channeled in pro-integration directions by the creation of supranational rules and institutional actors (with the latter acting as entrepreneurial leaders, given their own organizational interests in more supranational power; Haas 1958, 1964; Sandholtz and Stone-Sweet 1998). But their basic logic was equally positional: they reasoned that the overlay of a new institutional obstacle course on top of the material landscape shifted the way in which societal interests were translated into political outcomes.

Several scholars in an institutionalist vein admittedly began to shade into ideational territory in the 1990s, although they held to narratives in which European Commission officials acted largely out of organizational power maximization and affected developments mainly by cleverly "trading off the interests of important state and corporate actors" (Fligstein and Mara-Drita 1996; Jabko 1999). The literature also placed increasing emphasis on the complexity and unforeseen consequences of the emerging EU policy (Marks, Hooghe, and Blank 1996; Pierson 1996). Yet even this thrust was largely about the messiness and unpredictability of the overall EU process, not the notion that any

given group or organization might confront substantial ambiguity about how a certain position in the world dictated preferences or strategies in European institution building.

Since all major EU scholars worked from theoretical views that grounded action in objective positioning of some sort, it might not be surprising that no one thought to look for patterns of mobilization that cross-cut positions. Moreover, the dominant Moravcsikian and Haasian approaches hypothesized very similar patterns of position-based mobilization outside of small circles of state leaders and EU agents, sharing foundations in an economic-liberal "interest group theory of politics" (Parsons 2000). Perhaps because of this basic consensus, they did not even pay much attention to documenting their own broad positional patterns in European societies. Instead, they focused most of their empirical research on process tracing of elite decision making. In the course of glossing past the foundations of their own arguments, they overlooked cross-cutting patterns of mobilization that contradicted them.

This might seem to be an inflammatory claim, but consider some examples. Although Moravcsik's 1998 book looks like the closest thing to an exception to my characterization—making an attempt to research patterns of economic interest across societies and governments—he, too, ends up resting his claims substantially on process tracing. Perhaps the clearest example comes from the two-part, 106-page article on the European policies of Charles de Gaulle that he expanded from one of the book's chapters (Moravcsik 2000a; Moravcsik 2000b). Only six pages even mention any evidence about patterns of demands or concerns from French interest groups, parties, bureaucracies, or de Gaulle's advisors.[12] The overwhelming focus is on exegesis of de Gaulle's personal thinking to show that he was focused on economic concerns rather than geopolitics or nationalist ideology. Such evidence is not irrelevant to Moravcsik's argument, but it gives us little leverage for or against a claim that "French economic interests" drove specific policy decisions. To know how much de Gaulle's thinking connects to rationally perceived objective interests in a concrete landscape, we need to know how much other people in similar positions agreed with him. If de Gaulle, in fact, disagreed with people in similar positions—farmers, relevant policy makers, connected politicians, and others, depending on the various steps in the argument—then no amount of rhetorical evidence that the president was concerned with economics can support the conclusion that clear, rationally perceived "French interests" selected these policies. De Gaulle's thinking is evidence of de Gaulle's understanding of his interests, not of French interests. Moravcsik's theoretical logic is positional, but most of his evidence is not.

Institutionalists often showcase a similar misfit of theory and evidence. Consider the most prominent episode for most recent EU-focused institutionalism: the role of Commission President Jacques Delors in the SEA. Even

Moravcsik allows that this story features some institutional path-dependence (Moravcsik 1999). Earlier delegations of power to the Commission created the opening for Delors's supranational entrepreneurship in crafting the SEA deal, and his action altered national interests and bargaining. To show that this logic captured a major part of the causality behind the SEA, we would want to document several patterns. First, we would show that national-government strategies at some earlier point were not close to an SEA-like deal in some sense. Next, we would show that it was exposure to activity from Delors and the Commission, not other factors, that altered these strategies. To make this an argument about the European Commission affecting the "interests" of national governments as collective actors, we would presumably check to see that officials in any given national government received the Commission proposals in similar ways. If members of a national government did not—with some finding Delors's proposals persuasive and others rejecting them—we could hardly argue that Commission proposals were clearly altering "national government interests." Instead, we would have to look for some other factors that explained the differential appeal of Delors's action. Yet this step is basically absent from the large institutionalist literature on the SEA (Corbett 1987; Sandholtz and Zysman 1989; Cameron 1992; Fligstein and Mara-Drita 1996; even Jabko 2006). Rather than looking at the positional patterns suggested by their theory, they process-trace a dynamic of entrepreneurship and persuasion through a small set of actors. They focus their main attention on Delors being innovative and persuasive, with some attention to the reception of his ideas by a few top leaders. But no institutionalist account looks systematically inside national governments to see how these ideas were received more broadly. Thus, they miss that in France, for example, most officials in the Foreign Ministry, the relevant technical ministries, and even several members of the French SEA negotiating team opposed the SEA's institutional reforms that Delors proposed (Parsons 2003, 193–194). This does not entirely undercut the notion that the Commission's institutional position gave Delors some influence—it gave him access to key top leaders—but it does strongly cut back how much we see this story as one in which institutional channeling per se shaped widely perceived "interests." To put it simply, Delors and the Commission simply did not alter how many national officials and politicians saw their interests.

In light of common criticisms that ideational scholarship is weak partly for relying so strongly on process-tracing evidence, this criticism carries some irony. The leading nonideational, positional explanations of the EU look past positional patterns to focus on fairly thin tracing of the history of successful proposals. Tied into this oversight is erratic attention to historical alternatives. Much of the literature mentions some alternatives to the major EU deals, but I know of no work in these approaches that gives systematic attention to historical alternatives and why they were excluded. Instead, unsuccessful proposals are generally

mentioned as abstract contrasts or as brief vetting of ultimately unlikely trial balloons. (The major instance of a failed proposal that has garnered substantial attention from historians—the EDC disaster of the mid-1950s—is entirely set aside by these two schools.) Moravcsik has called explicitly for counterfactual argument in historical explanation (Moravcsik 1995, 616), but his empirical work is often confusing about what the historical alternatives were. On the EEC, for example, he notes that it was "only one of at least three broad alternatives considered at the time" but oddly characterizes the other two as an OEEC Free Trade Area and "liberalization through the sixteen member OEEC"—the same thing (Moravcsik 1998, 86). He then argues that the clear constellation of interests meant that the OEEC framework was not a real alternative, anyway.[13] Moravcsik also notes correctly (if ironically) that institutionalist work tends not to consider clear counterfactuals. It does not explicitly address how much supranational entrepreneurs recrafted interest-group coalitions or altered otherwise likely national bargains (Moravcsik 1999). The closest thing to an exception is Wayne Sandholtz's work on EMU, although his attention to alternative monetary arrangements mentions them as abstract possibilities rather than discussing who exactly supported them (Sandholtz 1993). Once again, this sketchy treatment of alternatives is clearly related to passing over broad patterns of support or opposition. The literature has tried to process-trace the threads of hypothetical structural or institutional imperatives backward from observed outcomes, not to reconstruct patterns of constraints, patterns of action, historical possibilities, and selection mechanisms in a forward-looking way.

CONCLUSION

Even more than in this book's other contributors' compelling arguments about social democratic parties, race in America, knowledge regimes, and other contexts, the impact of ideas is especially easy to see in the cross-cutting fights behind the construction of the EU. Unfortunately, the absence of ideational thinking from debates over EU history long allowed scholars to overlook these cross-cutting patterns. Quite simply, it seems that no one conceived that action might have some autonomy from objective positioning. Once we allow for this possibility—if we merely admit that it is plausible that groups and organizations might be unsure or conflicted about their positional interests—we are driven to do careful research on patterns of mobilization and rhetoric to check. Of course, if we find in other contexts that certain patterns of action and rhetoric debates do follow fairly clear material or organizational lines, this alone would not confirm positional theories and rule out interpretive variation. We would then need to do close process tracing and consideration of counterfactual historical alternatives to interpret how tightly a course of action followed from salient and

consciously understood goals, costs, and incentives. But our first steps must include a questioning of traditional assumptions and research on how action and rhetoric map onto positioning. Not only can this unearth the clearest evidence for ideational claims, but it is also the route to the clearest foundations for nonideational claims.

NOTES

1. See the book version for more extensive citations, which I have limited here to keep them from overwhelming the condensed text.

2. Thus, for example, business representatives in the French Economic and Social Council voted unanimously in July 1956 to relocate the early EEC talks to the OEEC (Szokolóczy-Syllaba 1965; Mahant 1969).

3. The common currency would be emitted by a European bank alongside national currencies, rather than in place of them in full EMU. My larger study shows that the common-currency plan shared the same justifications as earlier confederal projects and was supported by many of the same people in France.

4. Given German skepticism about the common currency, it probably was not a potential outcome. Had the French pushed for it and refused full EMU (as Balladur and Bérégovoy wanted), the outcome would have defaulted to traditional-style national-bank coordination (which both Balladur and Bérégovoy preferred to full EMU).

5. Prime ministers under the Fourth Republic, presidents under the Fifth.

6. Everyone expected Mendès France to become premier; conservative President René Coty chose Mollet instead, partly because Mollet promised a tougher policy against Algerian independence.

7. The book version also argues that the major substantive policy activities of today's EU (the CAP, the single market, and the single currency) were consequences of the community institutional format. No policy coordination on a similar level would have arisen through confederal or traditional projects.

8. Besides the party, regional, bureaucratic, and sectoral-linkage patterns mentioned earlier, I have researched French elites' educational trajectories and wartime experiences and found no notable correlation to stances in European debates.

9. For the best effort to eliminate such skepticism from a cross-national comparison, see Biernacki 1995.

10. On how this method is also extremely conservative, see Parsons (2003, 14, 232).

11. Moravcsik (1998) does set up what he calls a "geopolitical ideology" alternative to this theory. But this strange conflation of realist-style foreign-policy logic and unspecified ideological commitments does not amount to a serious consideration of an ideational alternative.

12. I also think that Moravcsik's mentions of these patterns are poorly researched, but that is a separate issue. See Parsons (2003, 28–29, 98–107, 135–138).

13. On de Gaulle, Moravcsik's one counterfactual is the assertion that because of economic interests, "Any other French government of the period would have sought the same objectives"—a crudely deterministic claim that contradicts the more nuanced caveats he allows about his "essentially multicausal" factual argument (Moravcsik 2000b; Moravcsik 2000c).

7

Ideas, Policy Change, and the Welfare State

Daniel Wincott

The fate of the Welfare State has been the focus of important research on the role of ideas in processes of continuity and change for institutions and policies (Cox 2001; Cox 2004; Béland 2005; Béland and Hacker 2004). But ambiguities and silences over the treatment of ideas in institutionally-oriented Welfare State scholarship (Esping-Andersen 1992; Esping-Andersen 1999; Pierson 1994; 1998; Hacker 2005) are equally striking. Moreover, a lack of clarity over the role of ideas also plagues gradualist varieties of institutionalism that repudiate the standard account of Welfare State resilience (Streeck and Thelen 2005). And while landmark studies by institutionalists interested in ideas—such as Blyth's (2002) *Great Transformations*, Berman's analyses of social democracy (1997; 2006) and McNamara's account of economic and monetary union in Europe (1998)—typically have significant *implications* for the Welfare State, as a concept the idea itself remains fuzzy perhaps because it is not the explicit focus of analysis.

In fact, much of the literature operates with an ambiguous idea of the Welfare State (Veit-Wilson 2000; Wincott 2001). The concept can be understood in ontological terms, as a "redefinition of what the state is all about". Typically, this approach involves acknowledging that few states have ever matched the terms of the definition (Esping-Andersen 1990, 23). The idea can also be deployed in a programmatic or sectoral manner. This approach is widely used to accommodate many more cases in the welfare state family. To some extent, of course, these two approaches lean on each other: a state that becomes a welfare state is likely to have a broad portfolio of effective social policies, while we also require some criteria to define a particular policy sector or program as having 'social' or 'welfare' character. In fact, mainstream theory focuses mainly on the large-scale entitlement programs such as benefits for the sick, the elderly, and the unemployed, seen as the welfare states' defining core (Esping-Andersen 1990). This tendency to dig in the same ditch

deepens our understanding of these policy areas, but the lessons do not necessarily transfer to other policies and programs that make up the broader welfare state. Important heterodox welfare state research—including pathbreaking feminist analyses—*has* addressed a wider set of policies. Crucially, however, relatively little of the literature recognizes the distinction between ontological and sectoral definitions of the welfare state. Where scholarship oscillates between demanding ontological definitions and comparative analysis based on a looser programmatic approach, a misleading impression can be given of the character and scope of the welfare state in western democracies since 1945 (see Wincott 2001).

The analysis developed here takes a different approach. It concentrates on a single policy sector—Early Childhood Education and Care (ECEC)—that is not one of the usual suspects of mainstream research and looks in detail at three countries that are usually regarded as social policy laggards—the United States, the United Kingdom and Australia. Not only do these policies feature significantly in some feminist arguments about social citizenship, over recent years political analysts and advocates have hinted that ECEC could provide a cornerstone for a new welfare state settlement (Pearce and Paxton 2005, xxi; Esping-Andersen et al. 2002; Hacker 2005, 54). Viewing mainstream theory through the prism of these cases and this policy sector casts it in an unusual light. It creates space for three sets of observations about and interventions in debates about ideas, institutions and the welfare state.

First, the focus on a particular social policy sector highlights a strange case of mutual neglect between various cognate scholarly literatures, all of which are concerned with—or at least touch on—the role of ideas in institutional and policy change: institutional analysis, policy theory and studies in American political development (APD). Indeed, we shall see that even within the family of institutional analysis there are striking examples of an apparent lack of dialogue and debate. Secondly, ideas are deployed in a variety of guises in institutional and policy analysis. In particular, these literatures typically suggest that ideas can operate a different 'levels'—contrasting overarching ideational frameworks with much more specific policy ideas (problems and solutions) and even with ideas about the settings of policies. Yet there has been surprisingly little focused debate about the different ways in which overarching ideas are conceived and defined – whether as paradigms, the terms of political discourse, the 'national mood', political traditions, the *zeitgeist*, or public philosophies. In relation to these two points, by drawing together APD and policy analysis with institutionalism, the present volume marks an unusually ecumenical effort to bridge these divides. Thirdly, focusing on ECEC raises questions about the standard historiography or periodization of the welfare state—particularly the notion that it enjoyed a "Golden Age" roughly from 1945 to 1975. We shall see that disparate and apparently mutually hostile perspectives on the welfare state

share this "epochalist" historiography. But whether or not it accurately encompasses the trajectory of the mature income transfer policies on which mainstream welfare research focuses, ECEC policy development has moved to a different temporal rhythm. I will deal with these three issues in turn, before presenting my historical analysis of ECEC policies in the United States, Australia and the United Kingdom.

IDEAS IN INSTITUTIONAL AND POLICY RESEARCH: EXAMPLES OF STRANGE MUTUAL NEGLECT?

Over recent years, institutionalist research on the welfare state has been heavily focused on the issue of retrenchment. While they differ over its extent, influential institutionalists seem to share an equivocal approach to the role of ideas in retrenchment. Paul Pierson's argument for the relative resilience of the welfare state in the face of permanent austerity exemplifies this ambivalence, in his argument that it is not clear whether "policy learning is central to the formation of government agendas or to the final choices between alternative policies" (1994, 42). While trenchant in their critique of the welfare resilience argument Streeck and Thelen (2005, 6) appear equivocal about ideas. Their argument that liberalization has *gradually* undermined the welfare state and related social and economic institutions is made without considering the the literature on "Great Transformations" in twentieth century ideas and institutions (notably Blyth 2002). Nor do they provide their own systematic treatment of these themes. Nevertheless, we shall see that ideas break through the surface of *Beyond Continuity* (Streeck and Thelen 2005) in several ways.

Theories of welfare-state development and retrenchment might not fully explain instances of welfare expansion after the onset of hard times, but other traditions—particularly policy analysis and American political development (APD)—offer helpful orienting perspectives and theoretical tools. Policy analysis has a long record of focusing on the politics of ideas (Heclo 1974; Kingdon 1984; Majone 1989; Sabatier 1993; Fischer 2003). Even where they share common interest in ideas (for example, both draw on Heclo's seminal [1974] analysis of social and policy learning), there has been surprisingly little interaction between policy analysis and comparative institutionalism,at least until recently (Schmidt 2002a; Fischer 2003; Peters, Pierre, and King 2005). This is a strange case of mutual neglect.

For example, Peter Hall's influential analysis of ideas and social learning makes only one passing allusion (1993, 295–296, n. 59) to Kingdon, who does not feature in the canon for comparative institutionalists (Berman 1998; Berman 2006; McNamara 1998; Blyth 2002). Equally, Sabatier (1993, 36–7) engages (critically) with state-centered theory but not with comparative

institutionalism. Even in the second edition (first published in 1995), Kingdon makes only passing reference to institutionalist theory, despite his extensive use of garbage-can decision making, which was initially developed by key institutionalists (he does not refer to P. Hall [1993] at all).

Mutual neglect might reflect real differences between policy and institutional research. First, policy analysts tend to have a have a narrower and more specific focus than institutionalists, concentrating on the analysis of particular programs and policies: what Paul Sabatier (1993) has called "policy sub-systems." Both the system and other subsystems appear exogenous or parametric. Second, their time frame tends to be shorter (even when stretched by Sabatier [1993] to a decade or more). A partial exception is Kingdon's notion of "the national mood," which touches on the broad political context (2003, 146–149; the theme is referred to by P. Hall [1993, n. 59]). While clearly ideational in character, this mood is a rather "vague phenomenon" (as Kingdon has himself acknowledged [2003, 18]); whether it is rooted in elite ideas or one driven by constituents' views also remains unclear (compare 2003, 148, 149). The national mood is also analytically restrictive in a less widely noticed way: it focuses attention on a single country, usually the United States.

By contrast, much ideationally focused institutionalism is explicitly comparative (Berman 1998; Schmidt 2002a; King 1995; King 1999; Blyth 2002; or, with the European Union, McNamara 1998; Parsons 2003). That is, the ideas addressed by comparative institutionalists cross the boundaries of states and nations. Some analysts have attempted to extend the comparative reach of policy theory through the concept of policy transfer (e.g., Dolowitz and Marsh 2000). However, the concept of policy used here is too narrow to do justice to the complex process of ECEC ideational (cross-)fertilization. Moreover, the transfer literature depicts the process as a recent phenomena, often linked to globalization. As we shall see, evidence from ECEC suggests that ideational spillover across national boundaries have a longer history—and deeper roots—than the transfer literature suggests.

If there is surprisingly little direct interaction between policy analysis and historical institutionalism, APD does draw on both of these bodies of scholarship. APD also lays emphasis on the ideationally and institutionally polymorphous character of U.S. policy and politics in debates on multiple traditions or multiple orders (Smith 1997; King and Smith 2005; Lieberman 2002; Lieberman, chapter 10 in this book). These scholars expect distinct, often mutually incompatible ideas and institutions to coexist within a single polity. In a different way, APD might serve also as a bridge to Streeck and Thelen's critique of institutionalism's inertial bias. These scholars pursue a similar theme—of institutional ambiguity, contestation, and contradiction—albeit without devoting equally explicit or sustained attention to ideas.

Various contributors to *Beyond Continuity* (Streeck and Thelen 2005) depict institutional ambiguity as opening spaces within which change-generating action can occur (see Hacker 2005; Crouch and Kuene 2005; Palier 2005; Jackson 2005; Quack and Djelic 2005). But neither the opening of action spaces nor the action itself will take place automatically. Using ambiguity or diversity (Crouch and Keune 2005) to motivate institutional change is a creative process, during which ideas matter. Jackson explicitly invokes shifts in shared and private beliefs (2005, 233), while Quack and Djelic nod toward epistemic communities as the source of new ideas (2005).

Streeck and Thelen offer a fluent and insightful discussion of similar issues in different language. They assert that "the *enactment of a social rule is never perfect* and that there always is a gap between the *ideal pattern* of a rule and the *real pattern* of life under it" (2005, 14). The "*meaning* of a rule is never self-evident and always subject to and in need of interpretation," they go on to explain: the " honest application in good will of a rule to empirical conditions may cause unanticipated results," and "rule takers do not just implement the rules made for them, but also try to revise them in the process of implementation, making use of their inherent openness and under-definition" (2005, 14–15; emphasis added).

Ideas also appear in another way—fleetingly but significantly—in Jacob Hacker's influential contribution to *Beyond Continuity*. In it, he concludes that "formal welfare state policies may turn out to be more resilient than the ideals embodied in them" (2005, 76). But Hacker only considers how gradual, low-key forms of policy change—drift, layering, and conversion—operate to undermine welfare ideals. Daniel Béland has argued compellingly (2007) that the ideological direction of hidden policy change is not preordained; in principle, it can extend welfare commitments. More important, there might be nothing eternal about these welfare ideals themselves (except perhaps at the most abstract level, say, of equality or solidarity but not at the more concrete levels of particular equalities or solidarities). Historical analysis of ECEC across the Western world reveals episodes of policy expansion or extension in many countries since the 1960s, which has involved similar expansive change in welfare ideals (sitting alongside a diminishing ability to meet other long-standing welfare objectives in the manner Hacker describes).

Hacker's intriguing hints about welfare ideals seems to suggest that they operate at an overarching level, above the particular institutions and ideas that make up individual programs. As I seek to draw together insights from cognate literatures concerned with ideas in processes of institutional and policy change, it is important to consider how they conceive of these broad frameworks of ideas—such as paradigms, the national mood, a public philosophy, or the zeitgeist—and how different aspects (or levels; Mehta, chapter 1 in this book) of the politics of ideas fit together.

LEVELS AND FRAMEWORKS OF IDEAS

Kingdon's analysis of policy agendas famously distinguishes between policy, problem, and political streams. He does not create a clear hierarchy between these elements, suggesting that all need to link up before policy change can occur. However, he does distinguish between "problem" and "political windows of opportunity" for change and implies that each tends to link with a policy idea (Kingdon 2003, 173–174). His distinction between "problem windows" and "political windows" does suggest two alternative sequences in the development of a new policy. Equally, he locates his notion of the overarching national mood largely within the political stream, which might pitch that stream at a higher level of generality. Nevertheless, weakness of the hierarchy among these streams, together with the complexity of the processes Kingdon describes, translates into a policy process marked by a considerable degree of contingency.

By contrast, Jal Mehta (chapter 1 in this book) and Peter Hall (1993) view policy change in more clearly hierarchical terms. As Mehta's analysis is easily accessible here, I will deal with it rapidly. Although his discussion of policy solutions and problem definitions partly follows Kingdon, Mehta describes the former as operating at a lower level of generality than the latter. Mehta devotes much more attention to public philosophies and the zeitgeist than Kingdon does to the national mood, not least by distinguishing between these two forms of overarching ideas, while explicitly identifying both as operating at a high level of generality. Mehta also displays a particular interest in 'upward flow': how a particular solution can reshape relevant ideas at higher levels of generality. Finally, in contrast to his notion of the zeitgeist, Mehta's concept of public philosophies makes space for the coexistence of several potentially mutually inconsistent traditions, potentially forging a link to the APD conception of multiple orders.

Hall's analysis of economic policy change in the United Kingdom (1993) explicitly distinguishes a hierarchy of first-order (instrument settings), second-order (instruments themselves), and third-order change (also covering the hierarchy of goals behind policy). In this usage, the paradigm appears as an internally coherent and encompassing ideational system. For example, it stands in contrast to the ambiguity and diversity that Crouch and Kuene detect in British macroeconomic institutions during the postwar period (2005, 88–90). For Hall, the third-order shift from Keynesian macroeconomic management exemplifies paradigmatic change. Two linked features of Hall's analysis are arguably less widely recognized. First, in addition to paradigmatic change, he also discusses "the terms of political discourse that are current in the nation at a given time" (1993, 289). The precise relationship between the paradigm and political discourse concepts is not entirely clear (Blyth 2002, 22), although the

latter notion is wider and looser—Hall describes political discourse as overarching. But, in places, Hall associates "radical changes in the…terms of political discourse" "with a paradigm 'shift'" (1993, 279, 284), which seems to suggest that the two notions largely overlap. Yet—and this is the second point—Hall's paradigm concept is closely linked to a particular policy sector. Of course, his choice of sector—macroeconomic policy—is one with a broad significance for a state's institutional or policy regime. Nevertheless, Hall's conception of the Keynesian paradigm is relatively restricted and technical. Subsequently, he has indicated that not all policy sectors have the characteristics necessary to achieve paradigmatic status: "less technical" policy domains such as social policy seem to be excluded (see the discussion in Palier 2005, 142). We might, then, expect the general terms of political discourse to range widely beyond this technical conception of the policy paradigm.

More generally, there is surprisingly little agreement on the precise meaning of the term *paradigm* in social science research. It is often used without any sustained attempt at definition, sometimes as a summary representation of historical periods. Keynesianism and the Keynesian paradigm sometimes stand quasi-metonymically for the policy-institutional character of the postwar era (Jenson 2006, 32). Used in this way, paradigms suggest that a coherent set of ideas holds general sway over a society for a period of time. Sometimes the paradigm concept self-consciously refers to contradictory but relatively enduring normative patterns and institutional settlements; Jenson's (1989) notion of societal paradigm is deployed in this way. All uses of the concept share a predisposition to conceive of change as discontinuous, with long periods of relative stasis or equilibrium punctuated by episodes of rapid change, precisely the vision criticized by Streeck and Thelen (2005). Paradigm analysis treats punctuations as marking fundamental and wholesale changes of condition or state. The possibility of continuities across moments of punctuation is downplayed, if not wholly excluded.

IDEAS AND PERIODIZATION IN SOCIAL SCIENCE RESEARCH

A powerful conventional wisdom—the idea that the welfare state enjoyed a thirty-year golden age after 1945—shapes much social science research. This standard "epochalist" view is often repeated but seldom analyzed and very rarely explicitly challenged (for one exception, see Braithwaite 2008). To be clear, I do not wish to claim that the golden age narrative is wholly meretricious: it certainly points toward important aspects of postwar political economy. But it is widely taken for granted and, in some respects, may be misleading. We might expect scholars of welfare state resilience (Pierson 1994) and proponents of gradualist perspectives on institutional change (Streeck and Thelen 2005) to be

skeptical of epochalism; in fact, despite their seemingly sharp disagreements on other matters, they share in the conventional periodization. Landmark statements of both these positions contrast a more recent era of austerity and retrenchment from the preceding period of postwar capitalism and welfare expansion. They use strikingly similar descriptions for the periodization: the "*Golden Era*" (Pierson 1994, 2–4) or "*Golden Age*" (Streeck and Thelen 2005, 3).

The implications of this "epochal" framework for these two forms of institutionalism merit consideration. It seems puzzling that analysis of the difficulties and limits of welfare retrenchment displays epochalist features. Since Pierson's analysis of the Thatcher and Reagan administrations (1994), the welfare state has come to be seen as the archetypically conservative and inert institution (not least by Streeck and Thelen 2005, 6). But there are dangers in exaggerating this argument. Pierson focused on the *relative* stability of social policies (1994, 5), compared to the claims of the New Right. And other aspects of the institutional political economy were subject to more fundamental change. The ambiguity in the meaning of the welfare state concept (Wincott 2001) may have contributed to this confusion. Pierson's basic conceptualization of the welfare state is aggregative: he identifies it as a series of social policies and programs. He issues the explicit warning that "the extent of programmatic variation deserves emphasis" and notes the "dangers in generalizing about "the welfare state"" (1994, 5), but it remains unclear whether his argument about the persistence of some welfare programs amounts to an ontological claim about the nature of the State. Equally, for all their valuable emphasis on gradual change and repudiation of models of punctuated equilibrium, Streeck and Thelen (2005) are also ultimately concerned with the overall transformation—in the form of liberalization—of advanced political economies, which makes their non-engagement with institutionalist scholarship on ideas and "Great Transformations" (Blyth 2002) particularly odd.

Periodizations play a substantial, but often hidden and undertheorized, role in social science research. When, without questioning them, we work within conventional periodizations, we often imbibe theoretical propositions without recognizing it. The attention paid to the detail of processes of institutional and policy change that may be stymied, gradual or hidden is a major strength of both Pierson (1994) and Streeck and Thelen (2005), perspectives that share more than we have been led to believe. But by situating analysis within an already established 'after the "Golden Age"' narrative, both they risk pre-judging the nature of the liberalization processes with which they are concerned. We should remember that the division of history into periods is neither given naturally nor theoretically innocent: such periods can be fruitfully viewed as *ideas* about history.

Historical periods are generally symbolized and summed up by particular features or facets that are deemed as their defining characteristics – although these

may not be stable or uniform. Thus, for example, as well as being identified with the welfare state, the postwar "Golden Age" is variously understood or labeled as an epoch of postwar capitalism, economic growth, full employment, Keynesianism and the Keynesian welfare state. While it is an almost inevitable feature of a periodic approach to history, the metonymic representation of historical epochs can mean that other social and political phenomena are subsumed under, or assimilated to, the purportedly dominant facets of a particular period.

EMPIRICAL ANALYSES

Head Start

Project Head Start, the breakthrough ECEC program in the United States, was a stepchild of President Lyndon Johnson's War on Poverty; it became the Great Society's most robust and long-lived offspring. During the late 1950s and very early 1960s a few local early childhood education projects had sprung up and some discussion of child care had percolated into Washington politics (Michel 1999), but the Great Society transformed the context for ECEC. Although this is not the place to pursue a detailed analysis of the Great Society, analysts typically point first to the impact of the civil rights movement and the "tumult of…disorderly politics of the 1960s," especially "the powerful and unpredictable insertion of race into the core of American political life" (Katznelson 1989, 188; Zigler and Andersen 1979, 3), second, to the role of experts ("social science and policy analysis ascendant"; Katznelson 1989, 188) but also of social commentators (Harrington 1962, MacDonald 1963) and, third, to the reformist tradition of the Democratic Party, particularly in the aftermath of John F. Kennedy's assassination.

The Economic Opportunity Act (EOA) of 1964 was, in effect, a framework for working out various Great Society policies and programs. It identified overall objectives, general themes, and a reference list of techniques for achieving them but lacked detail (Johnson 1979, 44–49). The job of filling in the detail was largely left to the experts recruited to the Office of Economic Opportunity (OEO), supported by a significant budget. Partly reflecting this lack of detail, some big EOA initiatives began slowly: halfway through fiscal year 1965, only about a sixth of the $300 million appropriated for Community Action Programs (CAPs) had been allocated (Zigler and Muenchow 1994, 3–8). Slow progress also reflected the CAPs' contentious quality, heavily laced with the politics of "race" (Quadagno 1994, 43) and "the maximum feasible participation of residents of the areas and members of the groups served" (EOA, August 20, 1964, 78, Stat. 508). The slow development of CAPs created a policy gap that could be filled by new proposals.

Head Start provides a particularly striking example of the EOA's fluidity: searching for substantive initiatives, the OEO seized on its single line that authorized (but did not mandate) support for preschools. Head Start had organizational advantages. As a six-to-eight-week summer program, it was relatively easy to organize and could draw on the expertise of teachers, providing them with additional work and income during part of their summer vacation. Head Start adopted a CAP style, bypassing Southern segregationists embedded in school boards as well as state and local government. As an education program for disadvantaged young children, it was less politically contentious than most CAPs.

Once approved, Head Start grew with astonishing speed: in its first year the planning committee expected "approximately 100,000 children…instead the first summer of Head Start saw 500,000 children entered into the program" (Zigler 1979 [1976], 374). It continued to grow rapidly in subsequent years. The program gained a high profile and attracted top-level political support. President Johnson and Lady Bird Johnson were attracted by the idea that Head Start could compensate poor children for their disadvantaged background, which also captured the imaginations of OEO director Sargent Shriver and his staff. Although it had not been designed as an IQ raising program (its mission to counteract the adverse impact of deprivation was more wide-ranging), Head Start rapidly came to be seen in these terms, not least by the president and first lady. The IQ based argument drew on academic research into human development, which took an environmentalist turn from the late 1950s. Scholars emphasized nurture over nature, displacing the earlier view that life chances were largely determined by inherited characteristics. Experts argued that the first five years were a critical period for intellectual growth (Bloom 1964; Hunt 1961), Although the new scholarly perspectives did not endorse the program directly, they did bolster the confidence of politicians and policy makers in Head Start. The program became an attractive means of filling the OEO's policy gap: "we actually thought that we could compensate for the effects of several years of impoverishment as well as inoculate the child against the future ravages of such impoverishment, all by providing a six- or eight-week summer Head Start experience" (Zigler 1979 [1976], 369).

Some in the planning group were uneasy about both the rapid expansion of Head Start, and the extravagant claims made for its impact on IQ, (which also distracted attention from other program objectives). But the IQ-based argument for Head Start contributed to a dynamic of ideas and policy around the program that became self-reinforcing and ran out of control. Even before Head Start was implemented (Zigler and Muenchow 1994, 24–27), President Johnson's advocacy was largely couched in terms of IQ gains. The program was hailed as a success before its first cycle had been completed or assessed. In this case, an *inaccurate* idea—the notion that a single summer of preschool could

permanently raise the IQs of deprived children—sparked Head Start's rapid expansion and institutionalization.

But if the emergence of a conventional wisdom naïvely focused on nurture galvanized Head Start during its early years, it also created an opportunity for the program's critics and opponents. Extravagant claims about Head Start's impact on IQ initially garnered resources for the program, but they also abstracted one element from a complex range of program objectives. When, around the end of the 1960s and the start of the 1970s, longer-term evaluations began to become available, the early overoptimism of some Head Start advocates became a source of vulnerability for the program. Analyses such as the 1969 Westinghouse Study, indicated that IQ increases were not sustained (Cohen 2001, 44–45). They had much more impact than other, more broadly based and supportive evaluations (e.g., the Kirschner Report; see Zigler 1979 [1976], 371, for a plaintive comparison).

While theories of policy change provide useful orienting perspectives for the analysis of the Great Society in general and Head Start in particular, neither experience closely matches the expectations they generate. For example, while the Great Society looks like an obvious case of Hall's third-order change, there is little sense of the earlier federal government experimentation with policy settings or instruments or of the accumulation of policy puzzles and paradoxes. Although ideas from specialized and technical areas of social research—particularly psychology and child development—had an important impact, these ideas are not closely analogous to those Hall emphasized in his account of U.K. economic policy change. In Mehta's terms, the Great Society appears as the assertion of a new, somewhat inchoate public philosophy linked to a series of general policy problems or challenges. "Upward flow" (Mehta, chapter 1 in this book) from policy solutions to problem definitions and public philosophies and the zeitgeist only occurred once the War on Poverty was under way; the political focus on Head Start as an IQ-raising program helped to raise the environmental perspective on child development to the level of the zeitgeist (Zigler and Anderson 1979, 7).

In contrast with Kingdon's expectation that policy agenda change is usually triggered by a solution becoming coupled initially with either a problem or a political opportunity (2003,172–175), the Great Society emerged from the coupling of a political opportunity with a number of rather general and intertwined problems or challenges Johnson faced. His landslide election victory in 1964 following the Kennedy assassination was an important element of the political stream. Equally, the civil rights movement both posed policy problems and influenced the political climate (Kingdon 2003, 148–149, argues that social movements influence the national mood; this must be a seminal example). Even the initial impetus from social commentators—in the form of the "rediscovery of poverty" (Harrington 1962; Macdonald 1963)—created a

powerful sense that something needed to be done. It helped to alter the national mood rather than offering either a clear-cut solution or even a particularly precise definition of the policy problem. This was matched by remarkable (public and expert) confidence in the ability of experts to resolve social problems, loosely rooted in the increasingly prevalent view that individuals' abilities were significantly shaped by the environment, rather than being largely determined by their inherited genetic makeup. Neither the Great Society nor Head Start began with policy ideas or specific problem definitions; as a result, they look like particularly chaotic examples of garbage-can policy making. Nevertheless, in 1971, some seven years after the launch of Head Start, the U.S. Congress passed a further major extension of ECEC in the form of the Comprehensive Child Development Act (CDA), to which we shall return below.

Britain and Australia in the 1960s and Early 1970s: The International Impact of Head Start

As in the United States, ECEC was the subject of limited policy discussion in the United Kingdom and Australia during the early 1960s, largely focused on day care for the children of working women. As early as 1964, the Australian Department of Labor and National Service established a Women's Policy Section, which grew to become the Women's Bureau in 1967. Once created, this unit sought to strengthen its position by cultivating links with the voluntary sector and civil society (Brennan 1998, 60–64). Together with the U.K. Plowden Report (see below), Head Start contributed to the "unprecedented attention" given in mid-1960s Australia to the contribution of preschool to subsequent "educational development" of children (Brennan 1998, 56). It would, however, be inaccurate to describe these factors as giving rise to strong social pressure for ECEC. Instead, quiet but assiduous bureaucratic maneuvering in the Commonwealth (Federal) Department of Labor and National Service led by Women's Bureau director Lenore Cox—a (proto-)"femocrat"—generated the 1972 Commonwealth Childcare Act (CCA). Although inspired by ideas about the changing position of women, Cox operated in a delicate context: legislators were anxious about the promotion of work for women, so the legislation "reflected the assumptions and approaches to day care adopted by the traditional voluntary agencies" (Brennan 1998, 67–68). Although not greeted as a turning point, the CCA did establish an unusual commonwealth-level competence: social and educational policies typically fall under the remit of the states and territories. Heclo's (1974) concept of public servants puzzling on society's behalf best captures these Australian policy developments, if we add influence from other countries to it.

In the United Kingdom, early1960s discussion of ECEC was largely focused on day care and confined within the civil service. The debate within Whitehall

involved departments successfully seeking to avoid this policy responsibility (Randall 2000, 56–59) rather than puzzling out new initiatives on society's behalf (Heclo 1974). Head Start's influence was channeled through a different route. The "rediscovery of poverty" by politically engaged British social scientists—often with links to the Department of Social Administration at the London School of Economics—predated and influenced the equivalent process in the United States (according to Silver and Silver 1991). But before Head Start British anti-poverty experts had paid little attention to education. In 1963, when the Conservative government commissioned Lady Plowden to chair a Central Advisory Council for Education (CACE) investigation into *Children and Their Primary Schools*, the topic was widely regarded as mundane (Silver and Silver 1991, 219). Yet this CACE investigation—informally known as the Plowden Committee—was galvanized by the launch of the War on Poverty the following year, and brought educational issues firmly into the British debates. Poverty experts on the committee, including sociologists Michael Young and David Donnison, were particularly drawn to the U.S. example of Head Start (Banting 1979, 115). The committee also took up the idea of targeting education investment in deprived areas from the United States, embodied in British Education Priority Area (EPA) policy.

If only one line of the EOA mentioned preschools, they were wholly absent from the Plowden Committee's original brief. A full chapter of the final report developed a strong case for expansion of nursery education, "not only on educational grounds, but also for social, health and welfare considerations," particularly where "nursery education can compensate for social deprivation and special handicaps" (Plowden Report 1967, 117, 119). "[E]ducational stimulus for young children is of great importance, particularly for the deprived," it argued, recommending universally available part-day preschool, with initial investments targeted at EPAs. Silver and Silver argue that U.S. influence on the committee's work was systematically and deliberately written out of the final report (1991, 231, 232–235, 241–242), but the impact of Head Start—the subject of a study visit to the United States—remains clear: in "the U.S.A. at the present time federal and other authorities, and private foundations, are providing large sums of money for programs of nursery educations, to counter the effects of extreme deprivation" (Plowden Report 1967, 120).

The Plowden Report strongly opposed allowing general child-care support for mothers' full-time paid employment, noting that "some mothers who are not obliged to work may work full-time, regardless of their children's welfare." It was "no business of the educational service," the report went on, "to encourage mothers to do so. It is true, unfortunately, that the refusal of full-time nursery places for their children may prompt some of them to make unsuitable arrangements for their children's care during working hours. All the same we consider that mothers who cannot satisfy the authorities that they have exceptionally

good reasons for working should have low priority for full-time nursery for their children" (Plowden Report 1967, 127–128, 132). Arguing that full-day care could be justified only as the lesser of two evils, the Plowden Report saw such provision as appropriate where mothers are "unable to care effectively for their children" and/or "home circumstances are very poor" (1967, 127).

If the Plowden Committee put preschool provision on the political agenda, initially it failed to divert resources to this sector. However, the year after its publication Conservative MP Enoch Powell's notorious "rivers of blood" speeches catapulted race to the forefront of political debate. In response, the Labour government quickly drew existing proposals, including Plowden's proposals for nursery provision in EPAs, into an "Urban Programme" (Banting 1979, 133). This initiative helped create some 18,000 EPA nursery places during the following four years, albeit less than two-fifths of the Plowden target for these areas. By projecting preschools into public debate, the Plowden Committee positioned them to benefit from other opportunities for expansion. As in Australia, ideas about ECEC were absorbed into the U.K. bureaucracy. Five years after the Plowden Report these ideas reappeared in a Department of Education and Science (D.E.S.) proposal under the unlikely leadership of Margaret Thatcher. Echoing the ideas of the Plowden Report, D.E.S. policy was to make part-day preschool universally available within ten years (Department for Education and Science 1972, 4–9).

Forty years ago, long before the scholarly preoccupation with globalization or the (related) literature on policy transfer, Head Start was influencing the terms of policy debate in the United Kingdom and Australia. Today, ECEC policy is associated with "*new* social risks": in the early 1970s, the United States Congress passed the CDA, while both the United Kingdom and Australia had developed national ECEC policy commitments, respectively focused on nursery education and day care. In neither case, did U.S. influence take the form of direct transfer or borrowing of policy design or models described in the policy-transfer literature. Instead, U.S. policy developments helped to shape the climate of opinion in Australia and the United Kingdom, providing actors with ideas, opportunities, and prompts. Particularly in the United Kingdom, then, the terms of political discourse were influenced by ideas from beyond the borders of the nation-state. The zeitgeist—or even a public philosophy—was not contained within the borders of nation-states.

The Comprehensive Child Development Act in the United States

In the United States, the new Head Start program placed the accent on early education to improve the life chances of disadvantaged children, but it did not provide child care for working families. By the late 1960s, however, its supporters believed that "Head Start's survival depended on broadening the base

of its constituency. This meant identifying the need for child care services in the larger population" (Marian Wright Edelman, in Morgan 2001a, 227). The growth of female participation in the labor force and second-wave feminism also helped child care to emerge as a significant policy issue during this period. In 1971, the U.S. Congress passed the Comprehensive Child Development Act (CDA). This legislation was, Morgan argues, "born from an effort to safeguard the Head Start program" (2001, 227).

Richard Nixon began his presidency strongly committed to child development programs (Michel 1999, 248; Morgan 2001a, 223, 232; Cohen 2001, 44): the 1970 White House Conference on Children "gave top priority to the expansion of comprehensive child development programs, including… day care" (Zigler and Muenchow 1994, 135; Quadagno 1994, 149–153). Yet Nixon vetoed the CDA. It is tempting to attribute this change of heart to the perceived accumulation of negative evidence about the effects of ECEC programs and a change in the tide of expert opinion away from environmental perspectives on child development. The 1969 Westinghouse Report was particularly damaging for Head Start. However, it was only one report among many, and others were generally more supportive (Zigler 1979 [1976], 371), and Nixon offered significant support for the expansion of child development and child care *after* its publication. Nixon's rapid turn from general support for ECEC to vetoing the CDA requires explanation, not least because in 1972, shortly after the CDA veto, Nixon reauthorized Head Start, the target of the Westinghouse critique.

Some policies create virtuous political cycle, by generating or consolidating a political constituency for the policy. Such policies are made up of programs "that effectively reinforce the commitment of supporters to…policies and goals" (Katznelson 1989, 186, see also Esping-Andersen 1985). CDA supporters believed it had this sort of capacity to forge a new political constituency. But the same qualities that gave ECEC potentially broad appeal were also potential fissures within this imagined constituency. The attempt to situate Head Start within a wider ECEC policy proved difficult. Differences between "middle-class liberationists" and "welfare mothers" weakened support for the act (Morgan 2001a, 226, quoting Edelman), particularly over the balance between early education and child care.

Equally, the threat of a new coalition mobilized conservatives within and beyond the Nixon administration. They launched a major—and often hyperbolic—ideological attack on the act. "Child development" advocates, they claimed, wished to replace the "American family" with "Soviet-style child-rearing" (Cohen 2001, 48; see also Morgan 2001a, 220, 234). Some commentators even claimed that the CDA would take children from their parents and give them to the Department of Health, Education and Welfare (HEW) (Zigler and Muenchow 1994, 148). The veto statement, substantially drafted by Pat

Buchanan, was also extreme: "putting in what the right wing wants to hear" (Zigler and Muenchow 1994, 146; Morgan 2001a). If passed, the CDA might have strengthened political liberalism in the United States; the campaign against it bolstered both right-wing political discourse across the country and the position of conservatives within the Nixon administration. Nixon's veto was not preordained: the CDA could have become law. But the veto did help to set the nation on a new path. As an ideological attack on welfare expansion that occurred before the first oil shock, the CDA veto also casts the golden age welfare-state periodization in a different perspective. Mobilization against the CDA was, in Morgan's persuasive argument, "the first strike of a growing social and political movement—later termed the New Right" (2006, 102).

In contrast with the expansive ambitions for Head Start of the War on Poverty and then the CDA, Nixon removed the program from its radical context. To weaken community action and the OEO, the president moved Head Start to the new Office of Child Development in the Department of Health, Education, and Welfare. By doing so Nixon made it possible for a limited version of the program to survive (Morgan 2001a, 241). Head Start went on to survive sustained criticism through the 1970s, offering a "modest exception" to the general failure of the U.S. government to address "changing social realities" (Hacker 2005, 54).

After nearly thirty often tempestuous years, its supporters would claim that Head Start was America's most successful educational experiment (Zigler and Muenchow 1994). Subsequently, President Bill Clinton built on the program to launch Early Head Start for children from birth through age three, while ECEC featured prominently in Barack Obama's presidential platform. The survival of Head Start through the vagaries of U.S. public policy for decades after the 1960s illustrates the complexity of national policy configurations: a system usually regarded as inhospitable terrain for welfare policies has generated and sustained ECEC programs that have become positive policy models with considerable international influence. In particular, Head Start and related programs (especially the widely cited High/Scope Perry Preschool Program in Ypsilanti, Michigan) have, over the decades, accumulated evidence that long-term advantages accrue to deprived children from high-quality ECEC.

Policy Expansion and Backlash in Australia

During the 1980s and early 1990s, Australia appeared to be moving in a different direction from the United States and the United Kingdom. While Margaret Thatcher and Ronald Reagan dominated British and American politics, the Australian Labor Party (ALP) enjoyed one of its longest periods in office. On coming to power in 1983, Labor placed a policy of wage restraint at the heart of its strategy, sweetened for traditional Labor supporters by trading limited

wage growth against social wage expansion. The scope for such increases was probably greater in Australia than elsewhere because of historically low levels of direct public expenditure. Traditionally, the Australian system protected male breadwinners' family wages through legal regulation and collective bargaining. Male breadwinners earned a sufficiently large and politically mediated family wage to enable them to protect the welfare of their wives and children. The choice to channel a significant part of the new social wage into child care was by no means automatic: it drew on the fragile legislative legacy of the Commonwealth Childcare Act, which had mixed with vibrant local action. Through the late 1970s, community-based child care had become a focus for feminists and (other) New Left activists, spreading out from early initatives in the State of Victoria.

After Labor won the 1983 commonwealth election, its choice of child care as a focus for social wage expansion was influenced by several factors. First, the Commonwealth Childcare Act gave the federal government direct capacity to enhance the social wage in this area by eliciting funding submissions from community-based child-care centers, rather than working through the states and territories. Community-based child care came to be seen as a distinctive "Australian model" (Brennan 1998). Second, through the 1980s, child care was promoted by "femocrats"—civil servants inspired by feminist ideas—within the commonwealth bureaucracy. Third, in contrast with the tone of economic expertise taken up by U.S. and U.K. governments in the 1980s, leading Australian economists—including Bob Gregory—developed a politically influential rationale for public support for ECEC (Anstie et al. 1988), arguing that the economic benefits of maternal employment outweighed the costs of state support for child care. Finally, echoing the strategic objectives of some advocates of the CDA in America, elements of the ALP sought to construct a New Left–inflected coalition around child care, focused on women (and parents).

Equally, traditional elements of the ALP and the Australian Trade Union Congress expressed disquiet about child-care policy as middle-class welfare, arguing that its community-based character required (and reproduced) considerable community capital. These groups pushed for the extension of commonwealth child-care support beyond the community sector, to include private centers. In order to secure expansion funds for child care, supporters of the community-based system (reluctantly) compromised on government funding for private centers, on the basis that a formal accreditation scheme was put in place. This system could have limited the potential damage that "the spread of private social provision" can do to public welfare services (see Béland and Hacker 2004, 43 for this argument). The regulation of the community sector had been largely based on the proportion of well-qualified staff employed and included informal elements, based on parental involvement and the sector's not-for-profit ethos.

The policy to implement these ideas was poorly designed. Crucially, the planned accreditation to determine eligibility was introduced only after funding had been granted to the private centers, which were all initially treated as eligible. Rescinding approval proved extremely difficult. Moreover, centers were subsidized on the basis of registration, not attendance; many generated multiple bookings for their places. For a period in the early 1990s, easily available and poorly controlled public subsidy made child care the best small-/medium-scale investment in Australia. Many private centers sprang up, disproportionately in desirable retirement locations (the Gold Coast, northern coastal New South Wales), not in the poor outer suburbs or other areas where demand was high. Earlier experience with the community sector did not prepare government for the dynamics unleashed by subsidizing the private sector. The child-care budget exploded during the early 1990s (insiders talk of budget "blowout"); for several years, policy makers seemed to assume that the boom reflected pent-up demand and would peak.

The distinctive Australian model of community-based child care was significantly weakened, although not wholly eradicated, by this chaotic expansion of the private-sector. As community-based centers tended not to take multiple registrations, some lost children to new private centers. Later, when the welfare-hostile Howard governments sought to gain control of the child-care budget, the community-based sector was weakened further. This episode weakened Labor, undercutting confidence in its competence and fueling resentment from community-based child care's supporters. These conflicts might also have expanded the ideological space into which Liberal and National opposition figures (particularly those associated with the "Lyons Forum") projected an antiwelfare, anti-child-care, and profamily image.

During the 1980s and 1990s ALP governments expanded the social wage, albeit as a tool to control wage growth and, hence, inflation. If not for economic policy, then, at least as far as social provision is concerned, Australia does not fit neatly into the golden age periodization. For a period in the late 1980s and the early 1990s, Australia was on the verge of building a new welfare settlement with a distinctive Australian child-care model. Here, a political opportunity provided by the election and extended period in office enjoyed by the ALP combined with an institutional and policy legacy of relatively low levels of direct government social provision and a commonwealth (federal) policy competence for child care. In Kingdon's terms, child care provided (part of) a policy solution for the ALP as it searched to control inflation within the Labor tradition. Equally, however, child care remained contested within the ALP, which generated an episode of rapid ECEC expansion that ultimately damaged the distinctive Australian model of community-based child care.

Britain after 1997

Although the Conservatives piloted a nursery voucher scheme in the Major administration's dog days, the Labour election victory in 1997 marked a larger change in ECEC policy and rhetoric. Initially converting Prime Minister John Major's vouchers into a (part-day) early education entitlement for children age four (later extended downward to age three), the new government launched Britain's first National Childcare Strategy (NCS) in 1998, and couching policy in these sweeping strategic terms began to raise expectations of ECEC advocates. But initially, Labour ECEC policy had a chaotic quality. After 1997, major new initiatives with overlapping content were proposed more or less annually for four or five years. From the early education entitlement to the NCS and Early Excellence Centres, through Sure Start and the Neighbourhood Nurseries Initiative to Children's Centres and Extended Schools, ECEC policy was marked by "hyper-innovation" (Wincott 2005; Wincott 2006b).

The fragmented quality of policy development partly reflected initiatives that emerged from different parts of government. For a period, early education and child care were brought together in a single government department—Education and Employment—but were not initially managed by an integrated unit. ECEC advocates—particularly the Early Childhood Education Forum (Quarmby 2003, 50)—influenced the initial moves on early education and Early Excellence Centres within this department. By contrast, Sure Start's origins were bureaucratic, rooted in Treasury preparation for the 1997 Labour victory. Before that election, Norman Glass, a Treasury official visiting Washington (for other reasons), met senior Head Start officials. Although aimed at a different group (children from birth through age three), the initial focus of Sure Start on improving "parenting" (Quarmby 2003, 50–51) and early health interventions was modeled on Head Start. As well as its child-development emphasis, the Treasury borrowed Head Start's community-development administrative ethos, engaging service users in priority setting and management for local programs. Given its Treasury origins, the initial child-focused social investment ethos requires emphasis; Sure Start used early intervention aimed to transform the life chances of deprived children. Initially, not all local programs offered child care. Only later did Sure Start's focus change to "promoting parents' employment"—and in particular the theme of "lifting families out of poverty by getting mothers back to work" (Quarmby 2003, 50–51).

At first, NCS support for the supply of child care was extremely limited, restricted to a tiny number of exemplary providers. Despite the grand rhetoric about national strategy, the NCS mentioned only eleven Early Excellence Centres, with concrete plans for fourteen more (1998). Using tax credits, Labour policy provided significant demand-side support for child care, based

on skepticism toward local government (new Early Years Development and Childcare Partnerships [EYDCPs] took local ECEC planning away from local government in England), a desire to limit headline public spending, and perhaps also optimism that child-care supply would spring up to meet new government-backed demand. Tax credits also linked child care to welfare, especially for solo parents: "assistance with the costs of childcare—for 0–3-year-olds in particular," became "heavily dependent on their parent(s) employment status" (Lewis 2003, 233–234).

Government gradually learned of the limitations of demand-side support: particularly in deprived areas, the spontaneous response of voluntary and private providers was weak. The learning process was not smooth; sustained government commitment and expanded supply-side support gradually led to more comprehensive ambitions for ECEC but also added up to a chaotic, hard-to-manage policy patchwork. By 2002, Labour acknowledged that "there are far too many uncoordinated programs relating to childcare which have their own funding streams, planning and bidding processes and targets. Accountability is unclear as EYDCPs have no legal status or bank account. In addition, they do not have full control of either the means or the mechanisms to deliver the numerous targets set by central government. . . . The existence of similar but differently named and separately branded initiatives (Sure Start, Early Excellence Centres, Neighbourhood Nurseries) only serves to confuse the picture" (Interdepartmental Childcare Review 2002, 13).

After this review, Labour strove for an integrated approach to ECEC. From December 2002 until after the 2005 election, integration occurred under the banner of Sure Start. In December 2004, the Treasury published a ten-year plan for child care, which placed equal emphasis on child-development/equality and labor market arguments (HM Treasury 2004, appendices A and B). It laid the basis for the 2005 election manifesto's commitment to universal child care, based on neighborhood Children's Centres, under the strategic leadership of local authorities. Some Sure Start champions criticized universalism for spreading resources too thinly, undermining that program's distinctive ethos and focus on deprivation (Glass 2005). By contrast, the influential Labour-leaning Institute for Public Policy Research argued that high-quality, publicly regulated and comprehensive ECEC should form the centerpiece of progressive institution building in the early twenty-first century, just as the NHS did in the immediate postwar period (Pearce and Paxton 2005, xxi).

Labour's conception of comprehensive ECEC took the form of progressive universalism, with public resources concentrated on deprived areas and families, within a universal framework. This vision faded after the election; despite some policy roll-out, political emphasis on ECEC weakened. Although apparently compelling to Labour insiders, the idea of progressive universalism remained abstract: the focus on deprived families and communities had

been diluted, but it was unclear what Children's Centres offered to the middle-classes.

Labour entered office in 1997 with a commitment to expand ECEC provision and rapidly published a national strategy. However, they had no clear sense of the policy problem(s) for which ECEC was the intended solution, the overall shape of the ECEC system they wished to create or how it might be constructed. Like the ECEC provisions of the Great Society, a political opportunity—the election of a traditionally left-of-center party—was coupled with a rather diffuse sense that something should be done about ECEC. The result was rapid, reasonably sustained, but chaotic expansion of provision. Between 1997 and 2005, Labour's commitment to ECEC seemed to grow as the government learned about this policy field. As in the United States during the 1960s but in contrast to Kingdon's expectations, programs were conjured up hastily. In fact, Labour's ECEC was even more inchoate than the early years of Head Start. Different parts of the administrative structure developed various aspects of ECEC (sometimes drawing directly on policy ideas from other countries), and a number of actors within the Labour administration promoted their own pet projects. Although policy developed chaotically, Labour has achieved a substantial expansion of ECEC provision. By 2005, policy entrepreneurs close to the government projected ECEC as the key to a new welfare settlement (Pearce and Paxton 2005, xxi), and Labour struck a universalist pose in the general election of that year. After 2005—and even before the credit crunch—ECEC faded from its central position in Labour discourse and public debate.

CONCLUSION

We have found that ideas shape ECEC policy across three liberal welfare states during nearly half a century. From particular policy initiatives to a transnational zeitgeist, ideas play a role at each stage of this history, as actors puzzle over policies and struggle over their meaning and direction. Policies sometimes developed chaotically. The ideas in and behind these policies were rarely clearly defined or precise causal variables, often appearing half-formed, incoherent, or plain wrong. Head Start was carried by, exemplified, and reinforced a particular zeitgeist, marked by, among other things, optimism about the potential impact of education on life chances. But the inaccurate idea that a few weeks of summer preschool could permanently raise the IQ scores of deprived children by ten points played a particular and influential role in the early promotion of Head Start. Equally, a few years later, the successful conservative campaign against the Comprehensive Child Development Act exaggerated and distorted its objectives and scope. The pervasive influence of half-formed, half-baked, or inaccurate ideas provides an off-beat endorsement

of Colin Hay's insistence that social and political action is motivated by perceptions (chapter 3 in this book).

At the start of this chapter, we addressed three broad and interrelated themes: the relationship between policy theory, APD and institutionalism; various ways of conceiving of the overarching ideational framework within these literatures; and the role of periodization welfare research. Returning to these themes, policy theory provides a useful set of orienting perspectives to make sense of ECEC, and has undeservedly neglected by institutionalists. The coming together of various factors—such as Kingdon's policy, problem, and political streams or Mehta's levels of policy ideas, problem definitions, and overarching ideas—is often a characteristic often a feature of ECEC policy development. On several key occasions—such as the origin of Head Start in the 1960s and policy development after 1997 in the United Kingdom—processes of detailed policy development took place on the hoof. Thus, ECEC policy may be even more chaotic and contingent than Kingdon's general expectation that change usually follows from the coupling of a policy solution to either a problem or a political opportunity. Here, policy theory could build on Streeck and Thelen (2005) valuable emphasis on institutional complexity and diversity. Equally, the polymorphous, ambiguous, even contradictory character of institutional and policy legacies that they emphasize, cries out to be elaborated more explicitly in ideational terms. Hacker's (2005) allusion to welfare ideals and Crouch and Kuene's (2005) emphasis on the enduring presence of anti-Keynesian ideas in Britain (within state institutions and market-oriented think tanks) hint at how this might be done. The legacies left by ECEC policies provide elements of what Crouch and Kuene call "institutional diversity" (2005); they change the terrain for subsequent policy development. A broader dialogue between policy analysis and various forms of institutionalism would provide a useful range of theoretical and methodological tools with which to better to address these important themes.

The character of overarching ideational frameworks is important for ECEC policy development. But for ECEC, such frameworks have not generally taken the form of policy paradigms, as Hall (1993) defines them. Instead, in contrast to its economic counterpart, this analysis suggests that social policy has neither the technical character or the degree of integration required for the construction of a Hall-type paradigm. Mehta's concepts of public philosophies and the zeitgeist are better able to encompass the ideational and institutional complexity we have uncovered. But this study also shows that the spirit of the times cannot be confined to a single country; instead it spreads beyond the borders of any single nation, and can motivate similar policy initiatives in different jurisdictions. Equally, rich debates within APD (Smith 1997; King and Smith 2005; Lieberman 2002 and chapter 10 in this book) provide a valuable source for comparative scholars interested in tracing the coexistence of a variety of

traditions or orders within polity marked by ideational and institutional complexity, as well as long-term continuities in these traditions.

Turning to questions of periodization, ECEC policy development fits uneasily into the standard "Golden Age" approach to welfare-state history. We have seen major episodes of ECEC policy development during the 1980s (in Australia) and since 1997 (in the United Kingdom). Equally, the CDA failed under a conservative ideological onslaught in the United States *before* the onset of the golden age crisis. ECEC provision was at the heart of a reshaping of welfare policy in the United States right in the middle of the golden age, in the wake of the rediscovery of poverty in the late 1950s and the early 1960s. Indeed, some comparative analysis of ECEC describes the 1960s as a "critical juncture" (Morgan 2001b). At a minimum, the history of ECEC suggests that specific welfare state programs and policies cannot all be assimilated within the golden age framework. The accuracy of this periodization is questionable even for the income transfer programs at the heart of mainstream welfare research: Esping-Andersen writes of countries moving "towards . . . de-commodification . . . only recently, and, in many cases, with significant exemptions", dating the relevant upgrading of benefits to "the late 1960s and early 1970s" (1990, 23) in other words, *immediately* before the end of the golden age.

Even where apparently divided of the importance of gradual liberalization *versus* resilience, key institutional welfare state analyses share more than we might expect. Hacker (2005) and (Pierson 1994) both display an admirable concern with the specific properties of particular welfare programs. The question that seems to divide these perspectives is how we relate their specific policy histories to the overall fate of the welfare state? How and when, in other words, particular policies and programs within the welfare sector add up to a welfare state, in the ontological sense? Hacker (2005) influentially points to the significance of changes to the welfare policy mix; to processes of policy layering, conversion and drift. Although these concepts provide powerful tools for welfare analysis, they do not provide us with a yardstick against which to judge the overall character of the policy mix: that must come from somewhere else.

In fact, Hacker (2005) provides another important, albeit undeveloped, hint about how to respond to this question: by analyzing the relationship between enduring welfare policies and changing welfare ideals. But just as Béland (2007) argues gradual processes of welfare policy change can motivate expansion as well as retrenchment, welfare ideals or expectations of the welfare state also change in complex ways. By today's standards, the golden age welfare state failed to include the majority of individuals within the terms of social citizenship. For example, the failure of the welfare state to address gender inequality and its exclusion of women are major themes of feminist research (Pateman 1989; Orloff 1993; O'Connor 1993). Issues of race raise similar

questions about how fully the welfare state matched its ideals, as Lieberman and others have shown (Lieberman 2002; Lieberman, chapter 10 in this book; Smith 1997; Smith 2006; King and Smith 2005). This list of groups presenting troubling issues for the welfare state can be extended; among others, it includes the status of indigenous or aboriginal groups, lesbians and gay men, migrants, and people with disabilities. These issues became politically salient at times that fit awkwardly with the golden age periodization. Some became prominent only after 1975, while others began to attract attention during the second half of the golden age, during the 1960s. It would, of course, be misleading to argue that any welfare state has met these claims in full. Nevertheless, most are seen as politically significant issues today; and in several states, substantial steps have been taken toward addressing at least some of them (see, e.g., Lieberman on race in the United States, chapter 10 in this book). Ultimately—and this is the key point—debates about welfare retrenchment, indeed about the existence of the welfare state *per se*, cannot be resolved without recourse to welfare ideals, to ideas, norms, perceptions and expectations.

8

Knowledge Regimes and Comparative Political Economy

John L. Campbell and Ove K. Pedersen

One of the key aspects of this book is the exploration of the relationship between ideas and institutions in politics. For instance, as Vivien Schmidt has reminded us in chapter 2, institutional analysis has made a key contribution to the study of ideas in politics. In this chapter, drawing on this type of analysis, we focus on the concept of knowledge regimes, which stresses the interaction between ideas and institutions in the production of economic and policy knowledge. To understand the importance of the concept of knowledge regimes, we can turn to the comparative political economy literature, which has made a direct contribution to the study of ideas and institutions.

Comparative political economy has been dominated since the 1970s by two waves of research. The first one examined how different types of *policy-making regimes* affect policy making and, in turn, national economic competitiveness (e.g., Katzenstein 1978). The second one studied how different types of *production regimes* affect national competitiveness (e.g., Hall and Soskice 2001). Absent from all of this is much discussion about *knowledge regimes*. Knowledge regimes are sets of actors, organizations, and institutions that produce and disseminate policy ideas that affect how policy-making and production regimes are organized and operate in the first place. Knowledge regimes are important because they contribute data, research, theories, policy recommendations, and other ideas that influence public policy and, thus, national economic competitiveness (Babb 2001; Campbell 1998; Pedersen 2006).

It is surprising that such a blind spot exists. Since the early 1990s, a rich literature has emerged on how ideas, broadly construed, affect policy making (Campbell 2002). Some proponents of the production regime and policy-making regime approaches have contributed to this literature (e.g., P. Hall 1993; Katzenstein 1996b). It is ironic, then, that they have not more systematically connected their work on ideas with their work on policy-making and

production regimes. This chapter does so by showing how knowledge regimes vary across different types of political economies.

We proceed, first, by reviewing the research on ideas and knowledge regimes in order to make the point that virtually no one has tried to situate an analysis of knowledge regimes within an analysis of policy-making and production regimes. Second, we compare the most important factors distinguishing between basic types of policy-making and production regimes. We do so in order to construct a fourfold typology of political economies. Third, for each of these political economic types, we examine representative countries to see how their knowledge regimes are organized. The principle countries in question are the United States, Britain, Germany, and France, although we briefly discuss a few others as well. Fourth, we summarize this empirical discussion by hypothesizing the ideal-typical knowledge regimes that are likely associated with our four types of political economies.

We argue that liberal market economies with decentralized, open states (the United States) tend to have market-oriented knowledge regimes that are highly competitive and often partisan and adversarial. Liberal market economies with centralized, closed states (Britain) also tend to have competitive knowledge regimes, but the level of partisan competition is tempered politically by public funding for knowledge producers in civil society and by the state's own in-house analytic capacities. Coordinated market economies with decentralized, open states (Germany) tend to have relatively more consensus-oriented knowledge regimes as a result of having political economies with strong associational and corporatist institutional arrangements, parliamentary systems that often produce coalition governments, and much public funding for knowledge producers. Finally, coordinated market economies with centralized, closed states (France) tend to have statist-technocratic knowledge regimes where much policy-relevant knowledge is produced in-house by the state. Of course, competition and conflict over ideas exist within all types of knowledge regimes. Our point is that the manner in which this is handled and whether it produces winners and losers or compromise and consensus depend on the institutional configuration of the political economy in question.

This chapter breaks new ground. To our knowledge, it is the first study to combine insights from the diverse literatures on production regimes, policy-making regimes, and ideas to better understand how policy-relevant knowledge is created. In particular, we are not aware of any other studies that analyze how production and policy-making regimes together affect the organization and functioning of knowledge regimes. This is our central concern.

Three caveats are in order. First, our argument is preliminary, based only on secondary literatures, and, therefore, requires elaboration through future research. Second, we focus on how different political economic institutions affect how knowledge regimes are organized and operate. That is, we focus on

the knowledge-production process. We are not concerned in this chapter with either the content of that knowledge per se or the impact that it might have on policy makers or with how these ideas may cause changes in political economic institutions. Although obviously important, these issues cannot be addressed adequately without detailed historical case studies, which are well beyond the scope of this chapter. Third, while we accept that ideas often matter, we are not assuming that once an idea is created, it always has an impact on policy making. We recognize that ideas get selected, modified, or ignored depending on constellations of power.

RESEARCH ON IDEAS AND KNOWLEDGE REGIMES

Three literatures point to the importance of knowledge regimes. First is an extensive literature on how ideas affect the policy-making process (Campbell 2002). It focuses on how different types of ideas, such as policy programs, intellectual paradigms, public sentiments, and frames, affect the policy-making process. However, much of this work focuses on how the structure of different types of ideas constrains policy making. Missing is much discussion of the actors, organizations, and institutions that affect the creation, framing, and transmission of these ideas. That is, the literature privileges ideational structure over agency and, thus, does not clearly specify some of the most important actors and mechanisms whereby knowledge affects states and economies (Campbell 2004, ch. 4; Yee 1996; but see Fourcade-Gourinchas and Babb 2002).

A second, much smaller group of studies takes agency more seriously and investigates how knowledge regimes are organized, how they create policy ideas, what the mechanisms are by which these ideas are disseminated to policy makers, and how all of this varies among countries. Some studies focus on only knowledge regimes in a single country (e.g., P. Hall 1993). Some studies examine how different policy ideas were used politically to shape policy making but pay relatively little attention to how the organization and operation of the knowledge regimes that produced these ideas were determined by surrounding political-economic institutions (e.g., Blyth 2002). Some studies show how policy ideas were diffused across countries and the conditions under which these ideas took root or not (e.g., Hall 1989). Finally, some studies analyze how knowledge regimes were organized and operated in different countries. This work is insightful but often explores countries belonging to the same type of production regime, thus limiting the possibilities for drawing comparisons across production regimes (e.g., Furner and Supple 1990; Nielsen and Pedersen 1991), or knowledge regimes of the late nineteenth and early twentieth centuries, an era during which states, economies, and presumably knowledge

regimes were much different from how they are today (e.g., Rueschemeyer and Skocpol 1996).

Finally, a third literature focuses on think tanks, often defined as nonprofit organizations, formally independent from government, and engaged in the analysis of public policy issues. Think tanks attempt to influence policy in many ways, such as by providing expert analysis or lobbying. They often display a high level of scientific expertise (Stone 1996a). A good example of this literature is chapter 9 in this book by Andrew Rich, which discusses think tanks in the United States. Some if it adopts a cross-national perspective, which is quite useful for our purposes (McGann and Weaver 2000; Stone, Denham, and Garnett 1998). However, it tends not to relate the structure and functioning of knowledge regimes to the production and policy-making regimes in which they are embedded as much as we would like. Overall, then, we utilize the insights of all three of these literatures to develop our analysis of the relationship between knowledge regimes and their surrounding institutional environments.

PRODUCTION AND POLICY-MAKING REGIMES

The first step in our analysis is to differentiate among important types of production and policy-making regimes. Our purpose in reviewing these well-known differences is to enable us to identify in the next section four ideal types of political economies and then to discuss what we believe are representative examples of knowledge regimes associated with each one.

Comparative political economists often distinguish between two types of production regimes (e.g., Hall and Soskice 2001). *Liberal market economies*, such as the United States and Britain, structure economic activity primarily through markets and corporate hierarchies in which corporate managers respond primarily to price signals and make strategic decisions without much consultation with other organizations in their environment. *Coordinated market economies*, such as Germany and France, structure economic activity more through nonmarket relationships, such as informal networks, formal corporatist bargaining, associations, and various forms of state intervention and regulation. In some cases, corporate managers consult regularly with other stakeholders—that is, those with a vested interest in the corporation, such as employees, customers, suppliers, and shareholders—and tend to coordinate their decision making with them. In other cases, the state ensures that coordination occurs. Thus, the process of economic coordination and decision making in liberal market economies is driven by market-based competition, whereas in coordinated market economies, it is also driven by institutionally based cooperation of various sorts. In other words, decision making in

coordinated market economies tends to be multilateral and often more consensus-oriented than it is in liberal market economies, where it tends to be unilateral, typically dominated by corporate managers, and less consensus-oriented.

The distinction between liberal and coordinated market economies has been criticized for ignoring important differences among countries, especially within the coordinated category (Amable 2003; Crouch 2005). For instance, France relies heavily on the state as a means of nonmarket coordination, whereas Germany relies heavily on corporatist bargaining. Even proponents of the liberal/coordinated dichotomy have acknowledged recently the significance of these differences (e.g., Hall and Gingerich 2004).

Regarding policy-making regimes, researchers often distinguish between two institutional types of states (e.g., Katzenstein 1978). In *centralized and closed states*, policy making is located in a few policy-making arenas that tend to be insulated from the external influences of civil society. Policy-making authority is vested primarily in the national government. And the electoral victor often controls both the executive and the legislative branches of the government. Moreover, these states often have an extensive, well-developed, permanent, and professional civil service extending far up the bureaucracy. As a result, few bureaucratic layers are subject to removal after elections. In *decentralized and open states*, policy making is much less insulated from external influences, policy-making authority is often shared or delegated to lower levels of government, as is typical in federalist systems, and the permanent civil service is much less extensive.

POLITICAL ECONOMIES AND KNOWLEDGE REGIMES

How do different types of political economies affect knowledge regimes? In this section, we identify four types of political economies based on the distinctions reviewed in the previous section. For each type, we examine how the knowledge regimes of certain countries are organized and operate. We argue that the institutional configuration of a country's knowledge regime reflects and is largely determined by its surrounding political-economic institutions.

There are several types of knowledge-producing organizations in most knowledge regimes. We focus on four that have received the most attention in the think-tank literature. First are academic-style *scholarly research units*, sometimes referred to as universities without students. These are staffed with scholars, professional researchers, and analysts, often with joint university appointments. They are often dependent on public funding. They produce expert research monographs and journal articles, much like those found in academia. They also tend to be politically and ideologically nonpartisan. Second are *advocacy research units*. They tend to be privately funded and are

politically and ideologically partisan. They are less concerned with conducting scholarly research than with packaging and disseminating the research of others in brief policy papers and through the media in order to influence the ideological climate, public debate, and public policy. Third are *party research units*. These are closely associated with political parties and provide a source of expert advice and analysis for party members. Sometimes they are actually housed within the party apparatus itself. Fourth are *state research units*, either directly affiliated with specific government departments and ministries or created on an ad hoc basis to advise government on a specific matter. Unlike the first three, these are largely apart from civil society. Some people have referred to these types of organizations as think tanks. Because there is much debate—and even confusion—about exactly what a think tank is (e.g., Stone 2004; Rich 2004), we avoid this language entirely.

LIBERAL MARKET ECONOMY WITH A DECENTRALIZED, OPEN STATE

The United States is typically characterized as a liberal market economy with a decentralized, open state. Business associations are not nearly as important in organizing the interests of business in the United States as they are in most European countries. Labor unions are also very weak by comparison. Corporatism is virtually unheard of. And cartels and Japanese- or German-style business networks are largely absent.

Insofar as the political landscape is concerned, when compared with most other advanced capitalist democracies, the two major political parties are rather weak and poorly disciplined, because elections are based on winner-take-all rules, and candidates are funded primarily through private contributions. Furthermore, political power is decentralized as a result of constitutional federalism, there is a clear separation of powers between legislative and executive branches of government, and these branches are frequently controlled by different parties. There are dozens of congressional committees affording outsiders access to the policy-making process. Finally, the permanent civil service is not nearly as well developed or extensive as it is in many other countries. When a new government is elected, many high-level personnel in cabinet bureaucracies and administrative agencies are replaced with new political appointees in what amounts to a political spoils system.

The U.S. knowledge regime reflects these political-economic institutions. There are well more than one thousand research units in the United States today and more than one hundred inside Washington, D.C., alone (Gellner 1995). There is a long history of scholarly research units. The first were

established in the early twentieth century and received most of their financial support from philanthropic organizations and, occasionally, corporations. Examples include the Russell Sage Foundation, the Hoover Institution, and, later, the Brookings Institution. Because their finances were secured by generous endowments, they did not have to cater to the partisan pressures of donors. Their goal was to improve and rationalize the political decision-making process, not influence the political agenda. After World War II, a second generation of scholarly research units such as the RAND Corporation, as well as university-based research institutes, were set up as a result of the federal government's desire to contract out for policy research (Abelson 2004). All of this was consistent with the principles of liberal market economies, which favor private-sector activity whenever possible.

Beginning in the 1970s, a generation of advocacy research units developed. These included the conservative Heritage Foundation, the Cato Institute, and the Manhattan Institute and a few liberal organizations, such as the Institute for Policy Studies. They often resembled interest groups insofar as they pressured decision makers to implement policies compatible with their ideological beliefs and those shared by their generous benefactors (Abelson 1998; Rich, chapter 9 in this book).

There are no party research units per se. But there are some state research units, such as the General Accounting Office, the Congressional Budget Office (CBO), the Congressional Research Service (CRS), and the Office of Management and Budget. There is also the President's Council of Economic Advisors, which appoints experts, often from universities, to conduct various policy-analytic activities. Many congressional committees also have research staffs. And most cabinet-level departments have assistant secretaries directing professional research and evaluation units. The capacity for research that some of these organizations have is greater than most research units in civil society and has grown over the years. Notably, the CBO and the CRS have staffs of about two hundred and nine hundred people, respectively. Given the constitutionally mandated separation of powers in the United States and the fragmented nature of the legislative and executive branches, it is not surprising that there are so many research units inside the state. However, this facilitates much competition among branches and agencies and, therefore, their research units. Again, this has created opportunities for state research units to supply data, analysis, technical advice, and political argument to players in these political contests. Some observers have argued that the proliferation of state research units has diminished the relative influence of other types of research units in the policy-making process (Smith 1989). Nevertheless, compared with most European countries, the U.S. knowledge regime is dominated much more by scholarly and advocacy research units than by party and state research units (Abelson 1992; Gellner 1995).

Political-economic institutions led to the development of this sort of knowledge regime in several ways. First, the phenomenal growth of scholarly and advocacy research units has much to do with the fact that tax law makes it easy to establish a tax-exempt, nonprofit organization. Corporate financing is also readily available. So private resources are available to finance research units to a much greater extent than in many other countries. This is consistent with a liberal market economy insofar as much support is given to corporate initiative and private-sector volunteerism.

Second, the decentralized, open nature of the state afforded scholarly and advocacy research units plenty of opportunities to reach policy makers and their staffs if they wanted to do so. For instance, the proliferation of new government programs and bureaucracies and the related demands of the civil rights and antiwar movements catalyzed the emergence of liberal scholarly research units during the 1960s. In turn, this led to a countermobilization by conservatives, who then formed or expanded the capacities of their own scholarly and advocacy research units, often with corporate financing (Abelson 1992; Fischer 1991; Ricci 1993).

Third, comparatively speaking, the government is dominated by temporary political appointees rather than professional career bureaucrats. This also encourages dependency on outsiders for intelligence, analysis, and policy advice (Abelson 1998; Coleman 1991; Gellner 1995; James 1993). Indeed, the initial growth of research units in civil society after World War II was driven in part by a demand among policy makers for policy expertise, particularly in foreign policy.

Finally, political parties are weak and have not established significant in-house policy-research capacities of their own. Moreover, given the undisciplined nature of political parties, American politicians are less likely to toe the party line than politicians in other countries. Thus, they are more inclined to seek policy advice and expertise from scholarly and advocacy research units (Abelson 1998; Abelson 2000; Abelson 2004).

It follows that the United States is much more a competitive marketplace for ideas than most other countries. Indeed, advocacy research units, but to an increasing extent also the more scholarly research units, engage strategically in that competition by trying to attract the attention of the media and influence public opinion in ways that are comparatively unique (Abelson 1992; Abelson 2004; Feulner 2000). Today, the partisan competitive marketing of ideas has gained ground while scholarly detachment has lost ground (Gellner 1995). In terms of its large number of scholarly and advocacy research units, the generous funding and staffing that they often enjoy, the increasingly partisan nature of their activities, and the intensely competitive nature of policy-relevant knowledge production and dissemination, the U.S. knowledge regime is rather exceptional compared with those in other countries.

LIBERAL MARKET ECONOMY WITH A CENTRALIZED, CLOSED STATE

Britain is a liberal market economy with a centralized, closed state. Despite fleeting experiments with corporatism during the 1960s and 1970s, British business associations are not especially central to the coordination of economic activity, and state regulation is fairly limited. There has been some state ownership in a few infrastructure sectors but considerably less so since privatization during the 1980s under Margaret Thatcher's Conservative government. Labor is better organized than in the United States but not nearly as well organized as in most continental European countries. So markets and corporate hierarchies are the key mechanisms of economic governance.

Regarding the state, there are two major political parties competing for power in a winner-take-all electoral system, as there are in the United States. But in Britain, the parties are well disciplined, so members of parliament generally toe the party line. And as is true in most parliamentary systems, the party in power typically controls both the legislative and the executive branches. Therefore, the government can more or less do what it wants without significant opposition. Moreover, Britain has a highly professional, extensive, and permanent civil service, which remains despite changes in the ruling party. Finally, in contrast to U.S. federalism, state power is centralized at the national level. Policy-making authority is vested in the prime minister's office, the cabinet, and the bureaucracy; it is not diffused into parliamentary committees; and there are fewer points of access to policy makers.

Britain has a much smaller field of research units than the United States. There are some scholarly research units. These were first set up during the interwar period, but more were founded immediately after World War II. They emerged in response to political necessities and the inadequacies of contemporary research facilities. Many are publicly funded. Notably, the Royal Institute of International Affairs and the National Institute of Economic and Social Research were established for these purposes and produce a variety of scholarly documents and reports on a wide range of policy-relevant topics (Day 2000; Denham and Garnett 1998; James 1993).

A few advocacy research units have also been around for a long time. The Fabian Society, for example, was established in 1884 and eventually became loosely affiliated with the Labour Party (Day 2000; Denham and Garnett 2004). And the Mont Pelerin Society was established in the 1940s to advance conservative, free-market ideas (Desai 1994). That said, beginning in the 1970s and in response to the perceived failures of Keynesian policies, more advocacy research units emerged with strong conservative orientations, such as the Center for Policy Studies and the Adam Smith Institute, which was established in 1974 by Sir Keith Joseph and Margaret Thatcher (Denham and Garnett

1998; Denham and Garnett 2004; Stone 1996b). Moreover, financial institutions in London began to develop in-house research and analytic capacities that were important in developing and disseminating monetarist and other neoliberal ideas (P. Hall 1993). In the late 1980s and 1990s, leading figures from academia, business, and the unions set up alternatives, such as the Institute for Public Policy Research (IPPR) and Demos, to reverse the intellectual dominance of the right (Denham and Garnett 1999; Stone 1996b).

All of these advocacy research units had close ties to either the Conservative or the Labour Party, although they were not established by the parties per se and, therefore, should not be considered party research units. However partisan they might be, the major conservative advocacy research units make serious, thoughtful, well-researched contributions, although they also sometimes recycle ideas from elsewhere and use the media to their advantage strategically (Desai 1994; Gaffney 1991). The same is true for IPPR. In any case, advocacy research units now represent a considerably larger proportion of all civil society research units in Britain than in the coordinated market economies of Europe (Day 2000, 128–129). But compared with the United States, British advocacy research units are relatively rare, have smaller staffs, and have less funding (Denham and Garnett 1996; Denham and Garnett 1998; James 1993). Overall, there is a dearth of research units in British civil society.

Britain has a considerable number of state research units. For instance, in 1970, Edward Heath established the Central Policy Review Staff (CPRS), which was made up of civil servants and provided his government with in-house specialist advice across departments (Denham and Garnett 1999). There are also various planning and research units in most government departments. These are staffed by civil servants and enjoy a degree of independence and autonomy from policy makers and administrators. There are also state research units staffed mainly with outside appointees, notably the prime minister's policy unit. The civil service also has semidetached inspectorates that provide it with independent professional opinion on issues such as social services or pollution. And there are public advisory bodies, such as the Social Security Advisory Committee, set up by government but acting and advising independently. Many of these are formed on an ad hoc basis (James 1993).

Why has Britain developed this sort of knowledge regime? In particular, why are there relatively few research units in civil society, especially when compared with the United States? First, most British research units in civil society are established under law as charities and, thus, are required to be educational and nonpartisan, which also prevents them from lobbying and engaging in similar political activities. And because the tax system is less accommodating for charitable giving than it is in the United States—perhaps because the British state has greater capacity for providing services that might otherwise be provided by charities—there are fewer foundations to support these research

units in Britain in the first place (James 1993; Stone 1996a, ch. 3; Stone 1996b). That said, as in other liberal market economies, corporations are another source of financial support for advocacy and scholarly research units (Fieschi and Gaffney 1998).

Second, the British civil service is more extensive, reaching up to the permanent bureaucratic counterpart of a minister. It also has more internal policy-making capacity, is protective of its dominant position as provider of policy advice, and regards itself as an intellectual elite capable of handling any problem (Coleman 1991; Stone 1996a, ch. 3). Indeed, outside experts are rarely invited to policy discussions, because civil service culture assumes that administrative officials are capable of transmitting any specialist material themselves to policy makers and because the constitutional principle of neutrality within Whitehall requires civil servants to keep their distance from external policy institutes (Coleman 1991; Stone 1996b).

Third, the opportunities for research units in civil society, especially advocacy research units, is limited by the fact that the centralization and insulation of political decision making creates only occasional windows of opportunity for them to have input—windows that are controlled significantly by the prime minister. Thus, in 1983, Thatcher abolished Heath's CPRS but was quite open to the Adam Smith Institute. In contrast, John Major shut out virtually all civil society research units. And the relationship between Tony Blair's government and advocacy research units seems to have fallen somewhere in between (Denham and Garnett 2004).The state research units seem to be more influential on a permanent basis. And those in civil society, especially the advocacy research units, sit precariously on the edge of the political process, using publicity to affect public opinion and government thinking as best they can (Gaffney 1991).

Overall, then, Britain has a smaller field of research units in civil society than the United States because funding possibilities and points of access to the state are more limited and uncertain. However, as noted earlier, advocacy research units have increased significantly since the 1970s. In this regard, both Britain and the United States have competitive marketplaces for ideas. This is not surprising insofar as they both also have traditions of very acrimonious politics as a result of their winner-take-all electoral systems, which, for instance, encourage advocacy research units to pursue high public profiles (Thunert 2000). But in Britain, partisan ideational competition is mollified to a greater degree than in the United States by public funding for knowledge producers in civil society, fewer channels of political access, and the state's own in-house analytic capacities.

It is worth mentioning that Australia is another liberal market economy with a relatively centralized, closed state whose knowledge regime resembles Britain's. There are very few research units in civil society in Australia, and

most of them emerged since the 1970s, are small, and operate on a financial shoestring. The nation's tax structure makes it very difficult to establish philanthropic organizations. Business has stepped forward in some cases to provide funding but only when it is assured that the business perspective will be represented. Moreover, Australian political parties are strong, are well disciplined, and have considerable in-house policy research expertise. Thus, they are closed to external policy research and advice. Insofar as the state is concerned, as in Britain, there are few conduits into government for the exchange of information and personnel. And so the efforts of Australian research units in civil society are often trumped by those of state research units whose analytic strengths and resources are far superior (Stone 1998).

COORDINATED MARKET ECONOMY WITH A DECENTRALIZED, OPEN STATE

Germany represents a good example of a coordinated market economy with a relatively decentralized, open state (Katzenstein 1987). It is a country whose economic actors are organized through corporatist institutions, where consensus building is held in high regard among these actors, and where networks of firms, suppliers, and banks typically work together to coordinate economic activity. Because Germany has a federalist political system, much policy-making authority devolves to the regional-level *Länder* governments, which is one reason the national government lacks the same sort of extensive permanent civil service found, for instance, in Britain or France. Moreover, Germany's system of proportional representation tends to ensure further that politics is based on consensus building often absent in winner-take-all systems such as the United States and Britain. This is also facilitated by six major, well-disciplined political parties.

Germany's knowledge regime has relatively few state research units providing in-house expertise and advice. Instead, it is dominated by more than one hundred scholarly research units, including those affiliated with universities, churches, and other nonprofit organizations. Altogether, they constitute more than half of all the research units in Germany. The Max Planck Institute for the Study of Societies and the Social Science Center Berlin (WZB) are notable examples (Thunert 2000; Thunert 2004). Many are also quite large, were created by the government after the war, and receive about half their funding from the federal government and half from the *Länder* governments. In fact, about 75 percent of all German research units receive public funding. This financial arrangement reflects Germany's federal structure as well as the government's desire to encourage competing views on economic policy and economic development (Thunert 2000; Thunert 2004, 71). Chief among the scholarly

research units are the so-called Big Six nonpartisan economic and social research institutes that were created after World War II and funded largely by the federal and *Länder* governments. Policy makers rely on them heavily, particularly when political consensus is fragile. They are considered to be the most important and influential organizations in the field. In general, policy makers depend more on scholarly research units than other types (Day 2000; Gellner 1998).

There are also research units closely associated with the major labor organizations, business associations, and political parties. The Confederation of German Employers Associations, the Federation of German Industry, and the German Federation of Trade Unions have long had their own research units. And each of the major political parties has its own research unit or political foundation, as they are often called; these are more prominent and better funded than their peers in most other countries, in part because state funds support them, too (Thunert 2000; Thunert 2004, 77–78). For instance, the Social Democratic Party sponsors the Friedrich Ebert Stiftung. Except for their party affiliations, the political foundations resemble the full-service scholarly research units found in the United States, such as the Brookings Institution or the American Enterprise Institute. The influence of labor or business research units, of course, varies depending on the party or coalition in power (Day 2000; Gellner 1998).

The existence of many well-funded scholarly and party-based research units has created a situation in which there is little space available for the sort of independent, privately funded advocacy research units that are found more commonly in the United States and Britain. Although their numbers have been growing since 1980, they still constitute only about 30 to 40 percent of all German research units. They have relatively little political influence (Gellner 1998; Weilemann 2000). In fact, most German research units are proud of their scholarly reputations, their research profiles, and the scientific soundness of their work, which they do not want to jeopardize by excessive partisan advocacy (Thunert 2000).

Germany's knowledge regime reflects the strong institutional tendencies within the political economy for corporatist interest mediation, interlocking federalism, negotiation among the political parties, and consensus building in general. Indeed, until the late 1970s, the development of German research units was almost entirely driven by demands of the state, corporatist organizations close to the state, and the political parties. First, given the fact that labor and business peak associations are expected to contribute to consensus-oriented policy discussions and that coalition governments are often in place, the large unions, business associations, and parties have recognized the benefits of having their own reliable sources of policy analysis and ideas and so established their own research units (Thunert 2000).

Second, the state places a higher premium on objective, scientific knowledge than in many other countries. As mentioned above, producing high-quality

knowledge and policy advice is important to virtually all German research units, especially the scholarly ones. Everyone remembers how the Nazis and then, in East Germany, the Communists manipulated scientific knowledge for their own political purposes. Nobody wants to repeat those mistakes. Therefore, the state subsidizes many scholarly research institutes to ensure that their work is of the highest quality. And beginning in the 1990s, most publicly funded scholarly research units have been reviewed by the Science Council, a joint federal-*Länder* advisory board that evaluates these research units on standard academic criteria, such as those used typically to evaluate academic departments and institutions of basic research. Heavy reliance on state funding brings with it certain obligations that have limited the degree to which research units can engage in policy advising, partisanship, or ideational marketing rather than basic research—obligations that have limited the development of advocacy research units (Thunert 2004).

Third, the absence of many privately funded advocacy research units is also a result of the fact that the permanent career civil service, which is perhaps more extensive than in other decentralized, open states, has its own internal policy-making capacities and is traditionally wary of relying too heavily on external advice, particularly that which it cannot oversee. Although Germany's federal structure provides more possible access points to policy makers than do most parliamentary democracies of the Westminster type, this attitude has also helped mitigate the proliferation of advocacy research units (Thunert 2000).

There is a tendency for more consensus-oriented knowledge production in Germany than in many other types of political economies, which seems to reflect the traditions of corporatist bargaining and coalition government. Many well-established research units in civil society are members of institutionalized consulting networks. Notably, the Big Six work together and seek consensus on policy analysis and economic projections. Twice a year, they produce the so-called Common Report, a joint analysis of the government's short- and medium-term performance. This is an analytic exercise, not one designed to offer policy recommendations per se. Such recommendations are left to other research institutes, such as the Stiftung für Wissenschaft und Politik. The intention is for all six institutes to concur on a joint conclusion, although recently this has not always happened (Atkins 2006; Benoit 2006; Thunert 2004).

Of course, German research units compete intensely for funding, prestige, and the attention of policy makers as they do elsewhere. They often represent different political or ideological positions and, as a result, do not always agree on things, as illustrated by the recent dissent among the Big Six. But because they tend to adopt a very scholarly approach in which standards of research are high and because they understand the consensus-oriented nature of German policy making, their policy recommendations are likely to be tempered to a

degree often absent in liberal market economies such as the United States and even Britain. Indeed, policy makers typically solicit input from a mixture of advisors from different backgrounds and a variety of party and scholarly research units, including those affiliated with unions and business (Thunert 2000). Furthermore, another reason the major research units are likely to moderate their tone is that most of their funding comes from the public sector, which is typically run by coalition governments (Weilemann 2000, 173).

Since World War II, the Netherlands has resembled Germany in the sense that it is a political economy based in large part on corporatist negotiation and coalition governments that strive for consensus building. It also lacks the sort of extensive civil service seen, for instance, in centralized, closed states such as Britain and France that have considerable in-house policy research and analytic capacities. All of this spills over into the Dutch knowledge regime.

Research units play an important role in policy making, in part because, beginning in the 1960s, the state apparatus began growing rapidly, policy making could no longer be run from a single political center, and so the government moved to develop external social science expertise for policy making. First, American-style advocacy research units are rare. Second, each of the twelve political parties developed an adjunct party research unit (Day 2000). Third, and more important, there is a group of publicly funded scholarly research units, which provide independent, external advice to the government.

An important example is the Social Economic Council, which includes representatives from labor unions, employer organizations, and crown appointees. Another is the Netherlands Scientific Council for Government Policy, which is considered the country's research unit par excellence and has eleven members appointed by the queen on recommendation of the prime minister. It consists typically of economists, sociologists, legal scholars, international relations specialists, and natural scientists—most of whom are university professors—but also occasionally people from large corporations. Care is taken to ensure that it is not politically partisan or ideologically biased. It focuses on socioeconomic policy and government organizations, and it has about forty scientific and administrative staff. It provides policy advice on many subjects, has independence vis-à-vis the government in terms of setting its agenda and doing its work, publishes reports free of government interference, and seems to have influence on the policy-making process. Yet it is linked to the government in terms of its budget and appointment process.

The point is that the Netherlands' coalition-based, parliamentary, multiparty system, coupled with a state lacking an extensive technocracy and in-house analytic capacities, created space for truly independent, nonpartisan, scholarly research units. And the country's proclivity for corporatist negotiation and political coalition building is reflected in the organization of its knowledge regime (Baehr 1986; Day 2000; Mentzel 1999).

COORDINATED MARKET ECONOMY WITH A CENTRALIZED, CLOSED STATE

France is an example of a coordinated market economy with a centralized, closed state. There is much less corporatism in France than in Germany, but since World War II, the state has engaged in much indicative planning based on formal consultations among labor, business, the Ministry of Finance, and other relevant ministries, often facilitated by the Commissariat du Plan. So economic governance in France is rather statist relative to the other countries discussed here. Furthermore, France has a long history of state-owned enterprise in critical infrastructure sectors. However, the French state's capacity to influence the economy is also considerable because it is very centralized. Policy-making authority is vested largely in the national government, the executive branch tends to hold sway over the legislature, and policy making is closed and insulated formally from many outside pressure groups. Moreover, policy making tends to be technocratic because of the fact that the state has an extensive, permanent, and well-trained civil service. Finally, there are several well-disciplined political parties and an electoral system of proportional representation, often resulting in coalition governments.

Much of France's policy research and analysis are conducted by state research units. In addition to the Commissariat du Plan, which conducts much research and analysis itself, most ministries have similar analytic capacities (Desmoulins 2000). Furthermore, each ministry has its own cabinet, an ad hoc group of experts arranged around a minister, offering him analysis and policy advice. The cabinets complement and often rival the traditional ministerial bureaucracy (Fieschi and Gaffney 2004). By law, each minister has the right to appoint a number of advisors to his or her cabinet. Most of them are civil servants coming from the École Normale d'Administration (ENA), where they are trained to become loyal, nonpartisan, technocratic servants of the state regardless of the particular government in power. The cabinets also include outsiders, notably expert intellectuals from the leading universities. The cabinets have become linchpins connecting political ideas and their application. They have great influence on agenda setting and policy elaboration. They were instrumental, for instance, in helping the Mitterrand government develop new policies after it abandoned its pre-1981 socialist agenda (Desmoulins 2000, 154; Fieschi and Gaffney 1998; Gaffney 1991).

France's knowledge regime also includes an elaborate network of scholarly research units, many of which belong to one of two principal umbrella bodies: the Centre National de la Recherche Scientifique (CNRS) and the Institut National de Statistiques et d'Études Economiques (INSEE). CNRS runs about fifteen hundred research laboratories and research centers around the country, but only a small number of these do policy research (Desmoulins 2000, 144).

INSEE generates social, economic, and political data and analysis, and the Institut National d'Études Demographique (INED) provides demographic data and analysis. Their researchers and managers are drawn mostly from the Grandes Écoles, the elite national universities. Most of the scholarly research units under these umbrella organizations are much smaller in terms of staff and budget than those in the countries discussed earlier. None is comparable in terms of multidisciplinary competence to the most prestigious scholarly research units in the United States, such as Brookings. Most are linked to a particular state administrative organization, receive financing from either it or another state agency, hire civil servants on a part-time basis, and are affiliated with academic institutions. Most also have state representatives sitting on their boards of directors. Many are reviewed annually by the Conseil d'État (State Council) to ensure that they are performing a public-service function (Day 2000; Desmoulins 2000; Fieschi and Gaffney 2004).

The political parties have research units, but these are relatively insignificant. That said, so-called political clubs emerge occasionally, which represent a kind of distant cousin to traditional party research units but more closely resemble advocacy research units. Political clubs are groups of experts, scholars, and other political advisors. They often coalesce around a particular political figure, usually someone who is out of power but seeking high political office. For instance, Club 89 formed around Jacques Chirac, and Démocratie 2000 and Clysthène formed around Jacques Delors. Such clubs often arise when the party to which their political figure is affiliated is viewed as performing badly, so the relationship between clubs and parties is often contentious. When the government's Algerian policies were questioned during the 1950s and 1960s, dozens of political clubs were created and fostered the New Left movement in France. Political clubs often dissolve as soon as the crisis that produced them has passed. As a result, political clubs are a kind of transitory hybrid. They resemble party research units insofar as they are loosely affiliated with a party or at least with an individual or movement that is affiliated with a party. But they are also like advocacy research units to the extent that they are identified mostly through their ideological or political commitments, seek to make an impact on public debate, and only occasionally do a bit of research. They often act more like ideological lobbies than scholarly research units (Desmoulins 2000, 154; Fieschi and Gaffney 2004).

There are some full-fledged advocacy research units. Notably, the French New Right established two very well-known research units: the Groupement de Recherche et d'Étude sur la Civilisation Européenne (GRECE), founded in 1968, and the Club de l'Horloge, founded in 1974. Both sought to influence politics through research and debate and carve out a political discursive space comparable to what the New Left had done (Fieschi and Gaffney 2004). Although advocacy research units have been comparatively unimportant in

France, they have been on the rise since the mid-1990s. Given the intricacies of French electoral politics, this has been a time during which a left-wing government and prime minister have cohabited with a Gaullist president. Both president and prime minister have sought to broaden their powers vis-à-vis each other, and the balance of power between the two heads of the executive has been quite unstable. In this intensely competitive political environment, advocacy research units have been relied on more than usual as sources of information, advice, and policy proposals. Nevertheless, advocacy research units in France are neither large nor long-lived. And they tend not to be affiliated with a particular political party but rather gather together on a temporary basis around single issues (Desmoulins 2000, 149–150).

France's political-economic institutions are largely responsible for the state-centered organization of its knowledge regime. First, the French civil service culture does not provide many opportunities for research units in civil society to participate in policy making. Indeed, most policy makers turn only rarely to these research units for expertise and advice, because they already have their own in-house sources of experts in various disciplines within the civil service (Desmoulins 2000, 149).

Second, given the highly centralized, closed nature of the state, Parliament is not a regular client for independent public-policy expertise. This is much different from the situation in the United States, where dozens of congressional committees and subcommittees often seek outside policy expertise and, therefore, provide many channels of access for research units to engage policy makers. So, with few exceptions, party research units, including political clubs, and traditional advocacy research units tend to be of little use to French policy makers. They might affect public debate, but it is not clear that they affect policy making per se (Desmoulins 2000; Fieschi and Gaffney 1998).

Third, the reluctance to turn to research units in civil society in France also stems from a long-standing distrust of the market in French politics—a distrust that has meant that the French state is seen as a protector of the republic (Desmoulins 2000, 145–146). Similarly, political parties have been historically mistrusted for excessive partisanship, political divisiveness, and being disruptive to the republic, which further hobbles the capacity of party research units to exercise much influence (Fieschi and Gaffney 2004).

Fourth, the French state plays an extensive role in protecting the welfare of its citizens, so there is no well-developed philanthropic sector. The absence of private philanthropy, as well as a lack of much interest among corporations for providing financial support, also contributes to the paucity of many research units in civil society (Desmoulins 2000; Fieschi and Gaffney 2004). This also contributes to the generally small scale of most of these research units. And it is one reason these organizations often have to scramble to make ends meet

financially, pursuing not only state subsidies but also contracts from both public and private actors (Desmoulins 2000).

We have shown that France's knowledge regime is dominated by state research units, as well as scholarly research units closely tied to the state, such as those associated with CNRS. But it is also dominated by a class of people with very similar intellectual and social backgrounds, many of whom are drawn from the prestigious ENA and Institut des Études Politiques. Hence, the French knowledge regime is not only rather statist and technocratic in comparison with other countries, but it is also more elitist. And this intellectual elite, which is heavily concentrated in Paris, provides much informal advice to policy makers. Indeed, France has a long tradition whereby intellectuals influenced policy makers through personal connections, so it is only as a last resort that these experts participate in research units to influence policy (Desmoulins 2000, 153).

Some features of the French knowledge regime can be found in other countries with coordinated market economies and centralized, closed states. For instance, post-Franco Spain inherited a centralized state bureaucracy, a weakly organized civil society, a tendency for much state coordination of the economy, and a lingering distrust by the state of civil society organizations. Moreover, the legal framework since Franco's demise in 1975 has been slow to change, so it has not provided extensive fiscal incentives for the formation of nonprofit organizations. And private-sector organizations have been more concerned with maintaining their special prerogatives than with promoting their broader policy interests. Spanish research units, therefore, are rare, numbering only about one hundred. Of these, the largest minority are state research units. About 40 percent are also scholarly research units. They rely heavily on public financing and tend not to market their findings aggressively or develop a particular political profile, preferring instead to cultivate an image of objectivity and neutrality (Freres, Seabra, and Moraes 2000).

Similarly, Japanese policy making has been conducted historically behind closed doors in a centralized, closed state, albeit in consultation with leaders of large corporations and trade associations. It has also been carried out by governments dominated by one party throughout most of the post–World War II era. Indeed, policy making is largely controlled by government ministries in a technocratic fashion similar to that in France. Neither the law in general nor the tax code in particular allows for an independent, nongovernmental, nonprofit sector. Furthermore, Japanese intellectuals and the media engage in much self-censorship when commenting on government policies. As a result, research units in civil society are rare. There are a few public nonprofit research organizations, which in practice are simply extensions of the ministries and, therefore, resemble state research units. Foremost among them is the National Institute for Research Advancement, which was established in 1974 and

provides policy makers with relevant and unbiased research information. There are also a few for-profit scholarly research units, such as the Mitsubishi Research Institute, financed by industry or banks. But the bulk of policy research is conducted by state research units (Ueno 1998).

TOWARD AN IDEAL-TYPICAL ACCOUNT OF KNOWLEDGE REGIMES

Based on the country descriptions presented above, we now draw some tentative conclusions about the nature of knowledge regimes in different types of political economies. The discussion is summarized in table 8.1, which presents four ideal-type knowledge regimes corresponding to the four types of political economies we have just discussed. Our intent here is to develop some initial propositions to guide future research.

We suspect that in liberal market economies with decentralized, open states, knowledge regimes will be characterized by many privately funded scholarly

Table 8.1 Typology of Knowledge Regimes

	Liberal Market Economy	Coordinated Market Economy
Decentralized, Open State	*Market-oriented knowledge regime*	*Consensus-oriented knowledge regime*
	Large, privately funded research unit sector in civil society	Moderate, publicly funded research unit sector in civil society
	Scholarly and advocacy research units dominate	Scholarly, party, and state research units evenly balanced
	Highly adversarial, partisan, and competitive knowledge-production process	Consensus-oriented, relatively nonpartisan knowledge-production process
Centralized, Closed State	*Politically tempered knowledge regime*	*Statist-technocratic knowledge regime*
	Small, publicly and privately funded research unit sector in civil society	Large, publicly funded research unit sector in civil society
	Scholarly, advocacy, and state research units evenly balanced	Scholarly and state research units dominate
	Moderately adversarial, partisan, and competitive knowledge-production process	Technocratic, nonpartisan knowledge-production process

and advocacy research units, some state research units, and no party research units to speak of. This *market-oriented knowledge regime* represents an intensely competitive marketplace of ideas. It is marked by partisan and adversarial contests among knowledge producers trying to influence both public opinion and policy makers. Of the four types of knowledge regimes, this one is probably the most competitive and the most heavily reliant on private financing, both corporate and philanthropic, although there are certainly plenty of government contracts and grants. The state research units also compete against one another and against research units in civil society for the attention of policy makers.

In contrast, we hypothesize that knowledge regimes in liberal market economies with centralized, closed states will have fewer scholarly and advocacy research units. These will be supported by a mixture of public and private funds. It will also have a much more substantial set of state research units. There will be few significant party research units. So, compared with the liberal market economies with decentralized, open states, the mixture of types of research units will be a bit more balanced. And like its decentralized, open state counterpart, this knowledge regime will be a partisan, adversarial, and competitive marketplace for ideas. However, the importance of the competitive marketplace for ideas will be tempered by the significant role that state research units play, particularly within the well-established civil service. As a result, we call this type the *politically tempered knowledge regime*.

Insofar as coordinated market economies are concerned, we suggest that those with decentralized, open states will have a moderate-sized set of research units in civil society, dominated primarily by scholarly research units rather than advocacy research units. These organizations will be heavily dependent on public funding. There will also be an important array of party research units and a reasonable number of state research units. The comparative absence of advocacy research units is indicative of the fact that this knowledge regime is less oriented to competitive, partisan, and adversarial competition and more oriented toward the production of knowledge for a consensus-oriented policy process. This *consensus-oriented knowledge regime* is consistent with the surrounding corporatist institutions and system of proportional representation in electoral politics, which puts a premium on consensus building and moderation in policy making. Whereas ideational competition is tempered by the state in liberal market economies with centralized, closed states, ideational competition is tempered in coordinated market economies with decentralized, open states by a generally accepted and institutionally supported concern with compromise.

Finally, we come to the *statist-technocratic knowledge regime* found in coordinated market economies with centralized, closed states. We anticipate that these will have few advocacy or party research units. There will be more publicly funded scholarly research units and state research units of various sorts. Economic coordination depends far more on the state in this type of political economy than

in the rest, which is why the production of policy-relevant knowledge will also be left largely to the state. Again, the absence of advocacy research units signals that knowledge production in these countries is relatively nonpartisan. But in contrast with coordinated market economies with decentralized, open states, where knowledge regimes exhibit tendencies toward ideational consensus building, coordinated market economies with centralized, closed states will likely have knowledge regimes that are highly technocratic in orientation. Ideational competition that might occur otherwise will be tempered by technocratic fiat.

CONCLUSION

This chapter represents an attempt to map the topography of knowledge regimes in different types of political economies. Our intent has been to specify the various organizational actors and institutional mechanisms by which policy-relevant ideas are generated in different political-economic types. Attention to these sorts of actors and mechanisms has been lacking in much of the literature on how ideas affect the policy-making process. The chapter represents an improvement on the general literature on ideas and policy making reviewed earlier. It also contributes to the scholarship, such as that represented by Vivien Schmidt (chapter 2) and Robert Lieberman (chapter 10) in this book, that seeks to integrate an analysis of ideas with an analysis of institutions, both political and economic.

In this regard, the chapter also reunites two literatures in comparative political economy—the work on policy-making and production regimes—to suggest how different institutional forms of political economies affect how knowledge regimes are organized and operate. In this regard, it begins to rectify the curious separation in the work of some comparative political economists where, on the one hand, they discuss the importance of ideas and, on the other hand, they examine how policy-making and production regimes operate, but without much attention to the role of ideas and knowledge regimes.

We recognize the limitations of typologies, particularly those based on simple dichotomous distinctions. Notably, the literature on production regimes, which provided us with the concepts of liberal and coordinated market economies, has been criticized for being overly simplistic and neglecting the proliferation of hybrid forms (Campbell and Pedersen 2007; Crouch 2005). As a result, there might be important differences in production, policy-making, and, therefore, knowledge regimes among countries that fall into any of the four political-economic types we have discussed. Hybrid forms are likely.

Moreover, we understand that typologies tend to represent the world in static ways. Knowledge regimes change over time. And, as suggested above, we suspect that this will affect the content of the knowledge that they produce. It behooves

us to better understand the forces that cause change in knowledge regimes. This is a subject far beyond the scope of this chapter. But it is worth mentioning that several factors might precipitate such change. For instance, based on interviews we conducted recently with various policy research organizations inside and outside the state in several countries, we found that the rise of the Internet and desktop computing has made all sorts of information, including sophisticated quantitative databases, readily available. In turn, this has facilitated the increased sophistication of policy research in the types of organizations we have discussed. In particular, since the 1980s, there has been an increase in cross-national comparative research conducted by these organizations as a result of these technological changes. One would assume that this also affects the content of the policy analyses and recommendations that knowledge regimes produce. After all, access to new data affords policy research organizations the opportunity to examine countries that often pursue different policies from their own.

Another factor that can precipitate change in knowledge regimes is a change in either the policy-making regime, such as election of a new administration, or the production regime, such as a prolonged recession. France is a case in point. In our interviews, we found that during the 1980s and the early 1990s, France's economic performance was weak, and several administrations rose and fell from power. Eventually, in 1997, the prime minister's office decided that France needed fresh policy ideas and began financing new policy research organizations, thereby triggering a transformation of its knowledge regime by helping to create new policy research organizations in the private sector, albeit with public funding. Here, then, we see that changes in the surrounding institutional arrangements can have profound effects on a knowledge regime. But we also see that the nature of ideas themselves—in this case, their inability to provide clear ways out of the recession—lead to changes in the policy-making regime, which then feed back in ways that transform the knowledge regime itself. Thus, ideas are both dependent on and constitutive of their surrounding institutional environments. Thinking about the relationship between ideas and institutions in this way helps to transcend rigid dichotomous distinctions between the two—a rigidity about which we are cautioned in the editors' introduction and the work by Mark Blyth (chapter 1) and Vivien Schmidt (chapter 2) in this book.

We have said very little about content and impact. But one thing is striking. Since the late 1970s, among the countries discussed here, the most radical neoliberal policy advice emerged in two liberal market economies, the United States and Britain, where privately funded advocacy research units enjoyed influential positions in the knowledge regime, and knowledge production and dissemination were comparatively contested and competitive processes with fairly clear winners and losers. Other types of knowledge regimes with different processes did not produce such radical advice. This suggests that different

processes might affect the content of the knowledge produced. Our hunch is that coordinated market economies tend to produce policy knowledge that favors comparatively incremental policy reform, whereas liberal market economies tend to produce policy knowledge that favors more radical policy reform. This is because knowledge regimes in coordinated market economies are more consensus- or technocratically oriented and, therefore, cautious, while in liberal market economies, they are more partisan, adversarial, and acrimonious and, therefore, prone to more extreme policy recommendations. This, of course, also raises the issue of what impact knowledge regimes actually have, or not, on policy makers—a methodologically vexing question that scholars have only begun to address (e.g., Abelson 2002).

We recognize that specifying particular policy areas might confound our fitting of cases to political-economic typologies. For instance, if we examined the knowledge regime associated specifically with U.S. defense policy, it might be difficult to fit this case into the category of a liberal market economy with a decentralized, open state, because defense policy making is considerably less public and more insulated within the Department of Defense and a few congressional committees. And historically, defense policy makers have relied heavily on only a few scholarly research units operating on government contracts, such as the RAND Corporation. But if we looked at the knowledge regime associated with economic or trade policy, the fit might be better. The point is that the more closely one specifies the policy area, the more likely it is that there will be variation within countries in terms of how their political economies and, therefore, their knowledge regimes are organized and operate. This suggests that there is plenty of room for new empirical studies about think thanks such as the one that Andrew Rich puts forward here in chapter 9.

NOTES

Support for this project was provided by a grant from the U.S. National Science Foundation (SES 0813633).

9

Ideas, Expertise, and Think Tanks

Andrew Rich

For all that is known about the important role of knowledge and ideas in policy and political settings, we know relatively little about the practical trajectories of concrete policy ideas. When do they succeed, and when do they fail? What contributes to their success? As John Campbell and Ove Pedersen outlined in Chapter 8 of this book, expertise is produced in knowledge regimes that vary greatly from one country to another. In this chapter, I examine the production and dissemination of policy expertise and ideas within the American knowledge regime—and, in particular, what accounts for the success of conservative ideas relative to those of political liberals during the past four decades. Even at a moment when the United States has a Democratic Party–controlled presidency and Congress, the legacy of conservative ideas seems to hold great sway in what is possible in Washington policy making. Focusing on the dissemination of conservative ideas in the United States since the 1970s, this chapter explores key features of this trajectory. It is about the organizational infrastructure for conservative and liberal ideas in American policy making and about one of its most central components: public-policy think tanks. Think tanks are independent nonprofit research organizations with a century-long history in the United States. In the past three decades, during the period when conservative ideas gained dominance in American policy debates, the number of think tanks has more than quadrupled, with the greatest number of new organizations reflecting explicitly ideological and particularly conservative missions (Rich 2004).

Think tanks have been a driving force in what is often called a war of ideas in American politics, a war that conservatives are winning. They are a key organizational vehicle for promoting ideas. They offer a way for ideas to gain adherents and to inform the substantive underpinnings of policy debates. And think tanks are not just an engine for ideas. They are also a reflection of ideas.

Think tanks follow a variety of missions and pursue many strategies, some more successful than others. As much as ideas and differences in ideology inform the content and direction of American policy making, ideas also inform

differences in the missions and strategies of the organizations that aim to promote them. Think tanks that might express loose commitments to the same goals—informing policy making through research and ideas—understand these goals in different ways and follow far from uniform strategies for achieving them. Knowingly and not, differences in their missions and strategies are informed by aspects of the very ideas that they seek to advance as organizations. In fact, there are important differences in how conservative and liberal think tanks are organized, staffed, and run, even when the leaders of both conservative and liberal think tanks seem to have the same broad objectives. These differences have serious consequences, not just for think tanks themselves but also for the power of their efforts—and ideas—in American policy making. Because space is limited, this chapter focuses primarily on conservative ideas and think tanks and their impact on contemporary American politics.

In the end, to understand the influence of conservative ideas in the United States in the last three decades, it is essential to consider think tanks, the basic infrastructure for promoting these ideas. To understand the differences between conservative and nonconservative think tanks, it becomes important to consider the ideas they promote. Ideas have power both in policy making and in the organization of policy making. Conservatives organize and develop strategies for think tanks that reflect the value they place on think tanks as engines for policy ideas. By contrast, even when they profess to be attracted to think tanks for the same reasons, liberals often fail to achieve the same organizational effectiveness. The leaders of liberal think tanks are often preoccupied by deeply held commitments to producing objective research, on the one hand, and to grassroots activism, on the other hand. Both are at odds with realizing the organizational success of conservative think tanks.

The effective organization of conservative think tanks is among the important reasons conservative ideas have been successful in American policy making. Conservatives are winning in the war of ideas, not just because conservatives have *more* think tanks but also because they have *more effective* think tanks, think tanks better equipped to be engines for ideas. Conservative ideology, along with the commitments and inclinations of those supportive of conservative ideas, have bolstered the effectiveness of the organizational infrastructure of which conservative think tanks are a significant part. By contrast, liberals have been distracted by tenets of ideas—and an ideology—less helpful to fighting a war of ideas.

I begin by assessing work about how ideas are influential in politics and policy making and then consider the ways ideas might influence the organization of politics and policy making as well. The empirical focus of the chapter is on the diversity among think tanks, those operating at both state and national levels in American policy making. I review findings from a national survey of state think-tank leaders and from in-depth interviews with the leaders and staff

of nationally focused think tanks. I identify patterns salient to understanding think tanks, both state and national, and organizations of many types that promote ideas in American politics.

IDEAS AND POLITICS

As evidenced in the introduction and other contributions to this book, in the past two decades, a range of work in political science and sociology has reasserted the important ways that ideas affect and, at times, drive political change. Ideas inform and constrain political preferences and institutional arrangements (Schmidt, chapter 2 in this book; Campbell 2004; Pierson 2004; Weir and Skocpol 1985; Goldstein 1993). They motivate political activity (Mehta, chapter 1 in this book; Hall 1989). Ideas are important in all of these ways, independent from the material position of actors in politics and policy making (Derthick and Quirk 1985; Thelen and Steinmo 1992).

In order to illuminate the independent force of ideas on institutions and policy decisions, for example, a number of scholars have conducted insightful empirical research. Derthick and Quirk (1985) illustrate the compelling role of neoliberal economic ideas and neoliberal economists in setting the terms for trucking and airline deregulation in the 1970s and 1980s. Goldstein and Keohane (1993) point to the power of ideas in the formulation of foreign policy. What this work makes clear is that the power of ideas is not only constrained by institutions but, at least as important, that ideas themselves can constrain and influence institutional and policy design.

The ways ideas are understood varies in this work, and at times, what constitutes an idea seems either taken for granted or assumed. As this book suggests, ideas range from discreet policy prescriptions—the idea of medical savings accounts or drilling in ANWR—and the broader worldviews or belief systems that justify the specific prescriptions. For my purposes, in a perspective similar to the one Sheri Berman outlines in chapter 5 of this book, I focus on political ideologies. The assumption here is that concrete policy ideas are embedded in broader ideologies, and ideologies make space for particular ideas to gain traction in policy and public debates (Béland 2005; Freeden 2003).

In this context, it is more appropriate to think of the war of ideas as really a war of *ideologies*, with discreet policy ideas as important ammunition in the war's battles. For example, conservatives are pursuing the advancement of core values expressed in the form of policy prescriptions (Freeden 2003). Conservatives are not united in their policy preferences or in all of the tenets of their ideology. But they are more consistent than liberals in working together on the values they hope to realize in public policy.

Conservatives in the 1960s and 1970s expanded the appeal of neoliberal economics, notions that unfettered free markets are best and that competition develops efficiently out of them. Individuals and individual firms succeed, and the broader goals of growth and opportunity are best achieved, when people (and firms) are left to their own devices to pursue interests without substantial government intervention. Conservatives embedded this neoliberal belief system in a historical account of the country and wrapped it in core "American values" of freedom and individualism. From that point, conservatives could derive and pursue specific policy ideas that were supported by the narrative and the belief system.

In the past thirty years, American liberals have had a more difficult time connecting ideas to a core ideology. Of course, by "liberal" here, I mean not classical liberals but rather conventional liberals in the sense in which it applies to contemporary American politics. Liberals have remained basically committed to the benefits of government interventions aimed at leveling the playing field for individuals and firms. For liberals, tackling inequality (rather than promoting unfettered free markets) has been a central concern, and government has been an essential vehicle for correcting the excesses and unjust results of the private sector (Best 1990; Lazonick 1991; Edwards 1997). But liberals have failed, and by and large have been reluctant, to embed this core belief in a broader narrative and history in ways that reflect a coherent or compelling ideology (see, e.g., Beinart 2006).

Alternative ideologies have power in contemporary politics. There is little question that in the past three decades, conservatives have been more influential than liberals in advancing their broad ideological formulations and, in turn, many of their component policy ideas. To be sure, ideological differences fall along more than one dimension in the United States, and reducing an analysis to a consideration of only "conservatives" versus "liberals" misses variation within each worldview and ignores other ideological perspectives altogether. But when a focus on ideology translates into the concrete, real struggle for political change, that struggle in the United States is most often between conservatives, broadly defined, and liberals, broadly defined, which makes these labels relevant; between the two, conservatives are winning.[1]

To some, the ascendance of conservative ideology reflects the innate power of the ideas themselves; the ascendance of conservative ideas was all but inevitable, because efforts to support free markets, limited government, and strong families simply resonate more powerfully than alternatives. Conservatives' success stems from the innate power of the worldview, the ideology itself. Its resonance draws on the strong roots of modern-day conservative ideas in the history and traditions of the country's founding (see, e.g., Micklethwait and Wooldridge 2004; see also Weaver 1971).

This view has supporters, especially among conservatives, but for ideas to gain political influence, they require not just believers but also organized promoters, and these promoters must go beyond current elected officials. Ideologies require an infrastructure for their ascendance in policy discourse and decision making. By infrastructure, I mean a set of organizations—an institutionalized and sustainable mouthpiece for ideas and a place that provides secure employment for their purveyors. Think tanks have provided a significant portion of that infrastructure.

THINK TANKS AS AN INFRASTRUCTURE FOR IDEAS

In the past three decades, think tanks—conservative think tanks, in particular—have played a substantial role in popularizing and legitimating ideas about the role of government and the proper organization of society. As one close observer noted as early as 1989, "If there is a new politics of ideas in the United States, these organizations [think tanks] are certainly the primary participants in it" (Smith 1989, 178). In fact, the formation of the Heritage Foundation in 1973 marked the beginning of a new generation of explicitly ideological—particularly conservative—marketing-oriented think tanks. Scores more conservative think tanks proliferated in the three decades that followed, with their growing numbers reflecting the practical realization (in line with what scholars were also realizing) that ideas matter.

The first think tanks emerged at the turn of the twentieth century, as part of the Progressive Era program to replace political patronage with a more reasoned and objective public administration. They were formed with the initiative of businessmen who felt threatened by the system of patronage and who believed that the burgeoning social sciences could provide solutions to accumulating social and political problems that affected their (increasingly disgruntled) employees (Critchlow 1985). The Twentieth Century Fund, the National Bureau of Economic Research, and the Institute of Government Research, forerunner of the Brookings Institution, were among the first of these new institutions.

Up through the mid-1960s, several dozen additional think tanks were formed with similar missions to produce policy-relevant expertise generally based on rigorous, original research. Beginning with the Heritage Foundation in 1973, the missions of many new think tanks focused far more on the promotion of ideological policy ideas than on the production of research and expertise. The overall number of think tanks operating in American policy making more than quadrupled to more than three hundred, and the greatest number of new think tanks represented identifiable, especially conservative, points of view (Rich 2004, 15). Nationally, new ideological think tanks became engines for ideas.

Identifiably conservative think tanks came to outnumber identifiably liberal think tanks by two to one. By the late 1990s, conservative think tanks were outspending liberal think tanks by a ratio of three to one (Rich 2004, 18–24).

The central role of think tanks in providing an intellectual and organizational infrastructure for conservative ideas has been recognized by several close observers (Covington 1997; Blumenthal 1986). Nevertheless, overall, as conservative (and libertarian) think tanks—such as the Heritage Foundation, the American Enterprise Institute, the Cato Institute, and the Manhattan Institute—have provided conservatives with the organizational means to promote their ideas with policy makers and the general public, think tanks have remained the subject of little scholarly attention. Fewer than a dozen books published since 1970 focus on American think tanks.[2] No articles specifically about think tanks have appeared in major social science journals in the past thirty years. By contrast, scores of books and articles have been published about interest groups. Beginning with Bentley, Truman, and Dahl (and confounded by the work of Olson), an extensive interest-group literature has evolved through the last half-century and continues among political scientists and sociologists. Think tanks rarely, if ever, receive even a mention in this work.[3]

THE EFFECTS OF IDEAS ON ORGANIZATION

If think tanks provide an infrastructure for promoting ideology and policy ideas, how do they do it? And if the much-expanded ranks of conservative and liberal think tanks appear to have the same basic goals of promoting research and ideas in American policy making, why has their success differed so considerably? What role do the ideologies to which their leaders subscribe play in shaping the variation among think-tank missions and strategies?

In the few instances where think tanks have been noted as promoters of political ideas, the role of ideas and ideology in shaping the styles and strategies of their organization is ignored. Ironically, these accounts tend to suggest that organizations—all organizations—straightforwardly pursue the "rational" strategies most appropriate for pursuing their "interests." Indeed, the language of interests and rationality creeps into explanations of organizations in work by scholars who want to explain larger political trends in terms of the role of ideas. Campbell concludes, for example, that "the cognitive clarity and simplicity of supply-side ideas and thus their appeal to policy makers as well as the public depended not just on the nature of the ideas themselves, but also on the self-interested, strategic efforts of actors deliberately to render them in this way as an endogenous part of the struggle over public policy" (1998, 398).

However, the missions and strategies of think tanks might be influenced by the very worldviews they seek to advance, which, in turn, lead them to organize

and act in radically different ways. I find that conservative and liberal think tanks embrace very different conceptions of their core currencies: ideas and expertise. In fact, conservatives place primacy on the importance of ideas, while liberals focus on the value of expertise. For conservatives, all research and all research organizations are ideological; their work is informed by ideas. For those who start, support, or work at conservative think tanks, this means that their research is a means to a more important end: the promotion and ascendance of conservative political ideas. Conservatives appreciate the power of ideas. Liberals, by contrast, are typically committed to notions of the "disinterested expert" and the pragmatic value of rigorous, objective, well-designed policy research. For liberals, policy research is the appropriate end product for think tanks, a product with independent value.

At the same time as there are differences in how conservatives and liberals view the relative importance of ideas versus expertise, there are additional differences, rooted in ideologies, in how each organizes the production and dissemination of both at think tanks. Conservatives are both hierarchical and entrepreneurial in their approach. The promotion of ideas requires a highly coordinated strategy of marketing and communication. For liberals, the dissemination of expertise can follow two different, and not always compatible, approaches. "Pragmatic liberals" place their confidence in the disinterested experts housed at think tanks. They are committed to the view that it is someone else's job to disseminate and promote research, certainly not that of the researcher. Good research will find its own audience, and if it requires promotion, that work will be done by others, perhaps by policy makers convinced by the merits of the research.

Another approach for liberals is that of the "progressive activist." For the progressive activist, research actually poses an irritating dilemma, because for the progressive activist, political change should originate and resonate from grass-roots citizen-based constituencies, not (elite) researchers. This view rejects the legitimacy of the power of experts in politics as antidemocratic. Unlike on the right, where hierarchical and entrepreneurial approaches to think-tank organizing are compatible, there is a tension between these two visions of organization on the left, which presents added problems for liberals trying to win battles in American policy making.

THINK-TANK STRATEGIES FOR AFFECTING POLICY MAKING

These trends persist among think tanks working at both state and national levels in the United States. In July 2003, I administered a mail survey to leaders of the 115 think tanks focused on state policy making.[4] The findings suggest that even among conservative and liberal think tanks that aspire to play a

similar role and have a similar influence in policy making, significant differences exist that are rooted in the alternative worldviews.

First off, the leaders of conservative and liberal think tanks have very different backgrounds. The survey asked, "What type of job did the first leader of your organization have immediately before forming or joining your organization?" Respondents had ten answer choices, plus the option of writing in another description of the founder's background.[5] Among conservative think tanks, a significant plurality, almost 40 percent, of those who were the organizations' first leaders came from the private sector; they were either former lobbyists or business executives (38.2 percent). By contrast, almost two-thirds of those who formed liberal think tanks came out of state government or from the nonprofit advocacy community (63.1 percent).

I included a third category of think tank in the survey, which I call think tanks of "no identifiable ideology." These are think tanks that are not identifiably conservative or liberal as organizations. They often go to great lengths to be perceived as balanced, objective, or neutral. At the national level, the RAND Corporation and the Brookings Institution are in this category. Even though these institutions have individual staffers who might be easily classified as liberal or conservative, as institutions, they and others in this category of think tanks avoid being perceived one way or another.[6] They want to be understood as research organizations, first and foremost, institutions that strive for an objective truth. Among think tanks of no identifiable ideology, the results with respect to leaders' background were mixed across the ten response categories, with the most think-tank leaders coming from state government (24.0 percent) and the second greatest percentage coming from the private-sector lobbyist community (12.0 percent).

These results about the leadership of state think tanks are unsurprising insofar as we typically think of the nonprofit advocacy community in the United States as dominated by liberal voices—"progressive activist" voices, in particular. This community of organizations provides a counterweight to the representation of usually conservative business actors (especially by lobbyists) in policy making. Nevertheless, the results illustrate fundamental differences among think tanks, differences that exist as well in the operation and decision making at state think tanks.

The survey asked think-tank leaders about the criteria they use when selecting or promoting full-time staff. Out of nine response options (along with an option to write in a response not listed), leaders of conservative think tanks most often named political or ideological orientation as the most important consideration when hiring staff.[7] Almost three-quarters of the leaders of conservative think tanks named political or ideological orientation as most or very important in making decisions about whom to hire (73.6 percent). By contrast, less than half of the leaders of liberal think tanks named ideology as most or

very important (42.2 percent). Among the other top priorities for the leaders of conservative think tanks were issue expertise (61.8 percent), media and public affairs experience (35.3 percent), and a record of publication (32.3 percent).

By contrast, in addition to being less concerned about political or ideological orientation, the leaders of liberal think tanks expressed less concern with media and public affairs experience (21.1 percent) and a record of publication (5.1 percent). Instead, liberals place a premium on advanced degrees (either policy degrees, 42.1 percent, or PhDs, 31.6 percent) and experience in government (36.9 percent), along with issue expertise (57.9 percent). Leaders of conservative think tanks show far less interest in advanced degrees (23.5 percent for policy degrees and 8.8 percent for PhDs) and with experience in government (20.5 percent).

These results about the hiring preferences of think-tank leaders are consistent with whom think tank leaders report they actually hire. The survey asked the leaders of state think tanks to characterize where their full-time staff worked before joining the think tank. Almost three-quarters of conservative think-tank leaders indicated that all or some staffers came from the business community or the private sector (73.5 percent). By contrast, liberal think-tank staff came from the nonprofit advocacy community in similar proportion (63.2 percent).[8]

So, conservative and liberal think tanks have markedly different priorities for how to staff their organizations and for the kinds of backgrounds they desire among those they hire. One more difference between survey responses from conservative and liberal think tanks is worthy of note: how they rank the significance of different kinds of staff activities to their organizations. Respondents were asked, "How do you rate the importance of the following activities in relation to fulfilling your organization's mission?" They were given ten choices, along with the option to write in an additional response.[9] Leaders of both conservative and liberal think tanks most often named advising policy makers and the news media about their research products as either most or very important to fulfilling their mission.[10] But from there, differences in their responses quickly emerged.

The leaders of conservative think tanks were significantly more likely to name "advising legislators on immediately pending policy issues" and "shaping public opinion on policy issues" as high priorities compared with the leaders of liberal think tanks. Three-quarters of the leaders of conservative think tanks named advising legislators as most or very important (76.5 percent), whereas just more than half of liberal think tanks named that as important (57.9 percent). Likewise, three-quarters of the leaders of conservative think tanks named shaping public opinion as important (73.5 percent), while only half of the leaders of liberal think tanks reported that as important (52.6 percent).

The leaders of liberal think tanks, by contrast, named informing nonprofit advocacy groups about their research as important at much higher rates than those at conservative think tanks. More than three-quarters of the leaders of liberal think tanks named the nonprofit advocacy community as very or most important (78.9 percent), whereas only one-fifth of the leaders of conservative groups named it as a priority (20.5 percent).

DIFFERENCES IN THE STRATEGIC PRIORITIES OF THINK TANKS

Overall, these findings begin to illustrate important ways that differences in ideology result in differences in the staffing and activities of idea-promoting organizations. They reveal differences in the professional development and trajectories of leadership for conservative versus liberal think tanks, differences in their staffing decisions, and differences in how these organizations act to be influential in policy making.

The variation in their priorities with respect to staffing decisions offers perhaps the starkest contrast between conservative and liberal think tanks. Consistent with a view that ideas matter and that differences in ideology are important, the leaders of conservative think tanks place substantial importance on the ideological and political predilections of those they hire. Conservative think tanks are interested in hiring politically conservative people above all else.

Next in importance for conservative think tanks is that those they hire be prepared to make a contribution to the war of ideas. Conservative think-tank staffers need to have an issue expertise; they need to have experience in media and public affairs and should have a record of publication. The leaders of conservative think tanks were much more likely than their liberal counterparts to express a preference for staff who are ready to hit the ground running in the public battles to shape the terms of American policy debate. Responses to the question about staff qualifications were wholly consistent with the view that conservatives see think tanks as ideological idea promoters. This finding is supported by in-depth interviews with the leaders of national think tanks. David Boaz, executive vice president of the Cato Institute, observed in a 2005 interview: "We have always tried to be noncredentialist in our hiring. We want people who are qualified but not necessarily credentialed. So we look for knowledge of the issues, but we really want a commitment to libertarian values and a commitment to communicating ideas. We are not just a university, and we do not want people who just study. We want people who were thinking about policy changes that the country needs and how their research might fit into that. So we clearly do have, in that sense, a different orientation from a university, where the supply of pure knowledge may be more valued."

The legitimacy of this understanding of the role of think tanks among conservatives was also confirmed in a final survey question. Think-tank leaders were asked to choose from among three descriptions of think tanks: as places for (1) public intellectuals, (2) policy researchers, or (3) issue activists. The majority of conservative think-tank leaders (56.0 percent) selected the response that described think tanks as a place "for public intellectuals—for those with well-formed ideas about the role for government and talents in producing and organizing policy research about these ideas in ways that might inform policy making." For conservatives, think tanks are important as promoters of ideas; the research that takes place at think tanks is in the service of their broader ideological agenda.

By contrast, the leaders of liberal think tanks selected the description of think tanks as places for public intellectuals, as places for those with well-formed ideas, least often among the three choices offered. Instead, they were split between those who described think tanks as "for policy researchers—for those with interest in the researchable dimensions of particular issue areas and talents in producing applied policy research that might inform policy making" (31.6 percent) and those who saw think tanks as "for issue activists—for those with concerns about specific policies and populations and talents in producing research and organizing citizens in ways that might inform and affect policy making" (36.8 percent). The leaders of liberal think tanks view their organizations first and foremost as research organizations, not as idea promoters.[11]

Consistent with this result, the leaders of liberal think tanks are most concerned with hiring staff with issue expertise and with research/academic credentials, rather than staff with media experience or with records of popular publication. For the leaders of liberal think tanks, it is most important that the organization be able to produce credible, rigorous research, rather than promote that research or fit it into a broader ideological agenda. Research is the product of think tanks, and its completion is the core purpose of the organization. With these commitments, liberal think tanks—often formed with explicit interest in being ideological counterweights to explicitly conservative organizations—seem to more closely resemble think tanks of no identifiable ideology than their conservative counterparts.

Further evidence of this last point is provided by how the leaders of liberal think tanks characterize the importance of different types of staff activities. The leaders of liberal think tanks consistently placed priority on informing different audiences about their research products, rather than on shaping the broader terms of policy debate. They describe informing policy makers, nonprofit advocacy groups, and the news media about their research products as most important to fulfilling their organizations' missions.

The leaders of conservative think tanks also describe informing policy-making communities about their research products as important. But they place

equal emphasis on shaping public opinion on policy issues (regardless of their research products) and advising legislators on immediately pending policy issues (again, regardless of research products). These findings suggest that conservative think tanks place much more importance on finding a receptive audience for their ideas—separate from their research—than do liberals.

WHEN IDEOLOGY IMPEDES ORGANIZATION

The findings begin to support the conclusion that ideology matters not just to the outputs of policy making but also to the organization of political change. Many of the differences in how think tanks approach their missions are closely related to differences that come out of the ideologies they seek to promote. As a practical matter, this conclusion suggests a far bigger problem for liberals than for conservatives in American politics. To the extent that this war is ongoing and think tanks are important to it, conservatives have the advantage, first, because they have more think tanks that are better funded, and second, because even when liberal think tanks are active, they are not nearly as effective in advancing ideas.[12] Liberal think tanks might do many things well (e.g., the production of research, the convening of workshops), but they are not organized to be effective counterweights to conservative think tanks in the war of ideas.

Liberals approach think tanks from a century-long tradition of investing in the production of objective policy research. Since the creation of the social science disciplines during the Progressive Era, liberals have been committed to the view that research is essential to an informed policy-making process. Research might lead to ideas, but ideas that are pragmatic and well reasoned, not value-laden. The tradition for research among liberals is one that "tends to minimize disagreement over political values, and at times seems to ignore underlying values if not wish them away altogether" (Smith 1989, 191).

In the early and mid-twentieth century, at the height of liberal dominance in American politics, the policy sciences came of age. As one leading advocate of them wrote in 1951, the rigorous techniques of policy research equipped the policy maker "with a sufficiently sharp image of the full implications of given postulates to enable him to avoid conflicts of principle within the program of action" (Rothwell 1951, ix). Being "liberal," then, became defined not just by vague commitments to social justice but equally by deeply held commitments to principles of neutrality and value-free research. Liberals believe in research. But they have a hard time believing in *liberal* research.

In this context, liberals are inclined to approach a war of ideas in American politics by, in some sense, denying its very legitimacy. Ideological battle is political nonsense; the results of rigorous, objective research can, and should, best inform the appropriate possibilities for government and society. Politics

should not be about winners and losers so much as it should be about building consensus, and research can point the way toward that (pragmatic) consensus. To view the role of research—and research organizations—in any other way would be inappropriate. It is the obligation of the disinterested expert to develop optimal policy-based solutions. These are attitudes pervasive not just among think-tank leaders at the state level but also among those who run national organizations. As the president of one national liberal think tank put it in a 2001 interview: "I work very hard to maintain a posture of nonpartisanship and nonadvocacy. If you look at an organization's agenda, you can form a view—without knowing anything about what they're doing or who they are. You look at the agenda and say, 'Well, look, these people are working on income distribution, health insurance, welfare, public housing. They've got to be liberal Democrats. After all, who works on those things?' That's not so true here. . . . We do honest work. That is our charter—to do objective work, to put it out there, to try to get it in the hands of people who need it, when they need it. But not to push an agenda. Well, you might say that the work may lead to an agenda. That's true. But we're not an agenda organization. We don't think there is a political bias."

Conservatives begin their thinking on these issues in a very different place. They begin from the perspective that ideas and values motivate, rather than result from, research. In their view, all research is ideological insofar as ideas or ideology at least inform the questions that so-called neutral researchers ask. There is no such thing as disinterested expertise or the disinterested expert. Instead, there are "permanent truths, transcending human experience, [that] must guide our political life" (Smith 1989, 192). These truths motivate research, and research is a means to a more important end: realizing the ideas that are a reflection of this core truth.

Conservatives believe at a fundamental level that ideas have power (Weaver 1971). Ideas inform preferences and behavior far more than research. And ideas not only are but *should be* more powerful than expertise. One engages in (or supports) policy research for the same reasons one supports political advocacy: because both contribute to the larger causes of shifting the terms of debate in American policy making and to amplifying the power of conservative ideas.

For conservatives, the war of ideas provides the rationale for creating think tanks. Think tanks are the engines for conservative ideas. And conservatives apply an entrepreneurial spirit to their organizations with the view that in a war of ideas, conservative ideas need machinery, artillery, to promote and disseminate them from every angle possible. Conservative think tanks should operate across the full range of issue domains, and they should be poised to interject ideas into any issue debate that captures the attention of policy makers or the public.

Operating in this guise, research is only one part of their activities. The promotion of that research is equally important. Reflecting on this point, Herb

Berkowitz, the Heritage Foundation's former vice president for communication, observed: "Our belief is that when the research has been printed, then the job is only half done. That is when we start marketing it to the media. . . . We have as part of our charge the selling of ideas, the selling of policy proposals. We are out there actively selling these things, day after day. It's our mission" (Rich 2005, 25).

Conservatives view think tanks as an integral part of a strategy for effecting broad-based political change, with the promotion of ideas at the center of that project. As Blumenthal observes, "The conservative elite has been built by individuals who believe strongly, plan strategically, and move collectively" (1986, 11). It is essential to conservatives that everyone who works at conservative think tanks be committed to the cause of political change; research is part of the activity of think tanks, but the purpose of conservative think tanks is to advance in a single-minded way the core elements of conservative ideology each and every day.

Thanks to their alternative views about research, liberals have a much more difficult time reconciling the formation of research organizations with the promotion of ideas in American policy making. Rather than approaching think tanks with a commitment to advancing a particular worldview as their main priority, liberals view think tanks with a pragmatic eye, relying on them to produce research that might speak to the policy needs of different issue domains.

This pragmatism is reflected in liberals' tendency to organize think tanks on an issue-by-issue basis. At both the state and national levels, liberal think tanks are far more often focused on only one or a couple of issues than on the full range of issues considered by policy makers. One liberal group focuses on poverty; another focuses on the environment. One group focuses on women's issues; another focuses on racial minorities.[13] In addition, on the liberal side, each of these think tanks is first and foremost a research organization, each one with issue specialties. The production of rigorous policy research is their end goal, their ultimate mission. Despite their stated objectives, liberal think tanks end up behaving little differently from think tanks of no identifiable ideology. As the president of one liberal think tank who both recognizes and, to some degree, laments this dilemma put it in a 2005 interview: "The important thing for us, and it's not true—and I don't say this purely out of a spirit of rivalry and competitiveness—but it's not true, for example, for the Heritage Foundation. They don't really care whether their numbers meet academic standards. For us, it's a question of survival. We know that we can't make it unless we continue to be credible to places with our numbers. So we try to be bold politically, but we spend a lot of energy making sure our numbers are right."

But liberals were split in their answers to this survey question about the role of think tanks, and liberals are split more generally in American politics with

respect to how they view the appropriate role of research and research organizations. If one group of liberals takes the approach just described—a pragmatic liberal approach—another group comes at it from a very different direction. They are "progressive activists" (describing think tanks as "issue activists" in the think-tank survey), and they reject the elitist proclivities of social science research organizations and the very notion that research can best reveal the appropriate directions for public policy.

An expert-based politics is problematic in their view, not because progressive activists embrace a politics of ideas over expertise (as conservatives do). Instead, progressive activists reject the elitist, antidemocratic features of the liberal pragmatists, an approach in which research reveals truth and the policy researcher knows best. Progressive activists prefer a politics that relies on "the people"—on grass-roots mobilization and a mass base for political change. For progressive activists, any effort to turn research into advocacy requires grass-roots popular mobilization. In order for research and ideas to have legitimacy, they must reflect the real preferences of citizens, who are actively supportive of, and engaged in, their promotion.[14]

The progressive activist is closer than the pragmatic liberal to the entrepreneurial conservative's view of think tanks as primarily important because they might influence political change (rather than simply produce policy expertise for its own sake). But whereas *ideas* are the most powerful currency for conservatives, the *people*—organized as grass-roots constituencies—are more important for progressive activists. It is the perceived *interests* of the people—rather than any set of ideas—that progressive activists seek to pursue, with their help and on their behalf. This attention to perceived interests and to public mobilization is largely incompatible, or at least in tension, with the war of ideas in American politics and with the central role and purpose of think tanks in that war.

Ideas are really not important to liberals—pragmatists or activists—in the ways that they are to conservatives. As a result, the translation of liberal ideas into the organization of think tanks seems to have followed not only a different trajectory from that of conservatives but also a far more difficult one. Through much of the twentieth century, when liberals dominated political discourse, policy making seemed to reflect a certain pragmatism. Think tanks—and policy researchers more generally—could behave pragmatically as well. They could remain focused on expertise and ignore ideas and ideology. In the 1950s and 1960s, many became convinced that there was a consensus behind a pragmatic (but basically liberal) political order. With the terms of debate and the boundaries of decision making far more contested today, think tanks that reflect a liberal pragmatism have a hard time competing. Researchers who do not take seriously the power of ideas can be sidelined in policy debates.

To be sure, think tanks are not the only types of organizations that have contributed to the success of conservatives in the war of ideas, and so creating

think tanks need not be the only response by liberals. Additional research is required about how other types of organizations—grass-roots and elite, both on the right and on the left—engage American policy making and the war of ideas. Still, in recent years, foundations and individual donors have shown fresh interest in supporting new liberal think tanks (Edsall 2005). There are a range of new efforts under way, from the Jamestown Project to the Tobin Project, New Vision to the Rockridge Institute. My research suggests that new support should be directed not just toward the straightforward development of more think tanks but also toward think tanks of a particular type. If they are to be effective in the war of ideas, these think tanks need to adopt missions and reflect strategies at odds with what has been the typical approach for liberals. As conservatives and liberals have pursued very different ideas about the role for government, they have also followed very different ideas of how best to promote these roles for government from think tanks. Liberals, and the leaders of liberal think tanks, might do well to appreciate the power of these ideas, both in policy debates and in the organization of their efforts to influence policy debates.

As this book illustrates, scholars should recognize the same. Following what Colin Hay reminds us in chapter 3, ideas can play a powerful role in defining how people and organizations perceive their political and policy interests. And as the example of think tanks illustrates, ideas—and ideologies—can play a crucial role as well in affecting how people and groups organize to pursue their perceived interests. This point complicates not only how we approach think tanks but also how we evaluate the far from uniform structures, organizations, and effectiveness of interest groups, advocacy organizations, and all manner of political activity in Washington and beyond.

NOTES

1. In a February 2005 meeting of conservative intellectuals organized by the conservative Bradley Foundation, conservatives of all stripes were present—traditional conservatives, social conservatives, neocons, etc. Speakers from all brands of conservatism made clear, repeatedly, that despite their differences (differences worth fighting over), conservatives stand united as a movement against liberals in the United States. See *Vision and Philanthropy: A Bradley Center Symposium* (Washington, D.C.: Hudson Institute, 2005). Conservative ideas have become more salient than liberal ideas both with the American people and with policy makers. Political observers began recording the power of conservative ideas with the election of Ronald Reagan in 1980. Reagan's election was quickly seen as a victory for ideas in a political environment that seemed "more open to ideological appeals" (Blumenthal 1986, 11). Conservatives who were convinced that their ideas were gaining ground in the 1980s viewed the election of a Republican Congress in 1994, followed six years later by the election of George W. Bush as president, as thorough confirmation of their dominance (Viguerie and Franke 2004; Edwards 1997). Liberals lamented that American society was getting "out of

kilter, [with the] right in full cry, the left defeated and listless" (Stefancic and Delgado 1996, 4; Alterman 2003).

2. Six of these are authored by political scientists: Ricci 1993; McGann 1995; Abelson 1996; Stone 1996a; Abelson 2002; Rich 2004.

3. For a review of the interest-group literature, see Baumgartner and Leech 1998.

4. The survey inquired about the organizational histories, missions, and strategies of state-focused think tanks. I received seventy-eight responses, a 67.8-percent response rate. The seventy-eight returned surveys were primarily from think tanks focused exclusively on state policy making and were broadly representative of the larger population of state think tanks with respect to geography, ideology, and size. I received responses from thirty-four conservative think tanks, nineteen liberal think tanks, and twenty-five think tanks of no identifiable ideology.

5. The response choices were (1) worked in politics (e.g., elected office, campaigns, party organization), (2) worked in state government (e.g., government office, agency, or department), (3) worked with a nonprofit advocacy group, (4) worked as an academic/college professor, (5) worked as a journalist, (6) worked in politics outside of the state, (7) worked on government outside of the state, (8) worked as a lobbyist, (9) worked in the private sector, (10) worked at another think tank.

6. Organizations are coded for ideology based on assessments of their self-portrayals in mission statements, codings that are highly correlated with how think tanks are perceived by the news media and policy makers. See Rich 2004 for a detailed description of the coding process.

7. The response choices for this question were (1) specific issue expertise, (2) media/public affairs experience, (3) coherent/appropriate political or ideological orientation, (4) record of previous publication, (5) advanced policy degree (MA, MPA, MPP), (6) advanced research degree (PhD), (7) experience working in politics, (8) experience working in/around government, (9) academic experience.

8. State government was the second most frequently named background for think-tank staff by both conservative and liberal think tanks. And government was the most often named background characteristic raised by leaders of think tanks of no identifiable ideology.

9. The response options were (1) advising legislators on immediate pending policy issues, (2) advising legislative staff on immediately pending policy issues, (3) advising executive branch officials on immediately pending policy issues, (4) advising the news media about immediately pending policy issues, (5) informing the news media about research products, (6) informing nonprofit advocacy groups about your research products, (7) informing lobbyists and/or trade groups about your research products, (8) informing policy makers (legislators and executive branch) about your research products, (9) informing the policy research community (e.g., other think tanks, academics) about your research products, (10) shaping public opinion on policy issues.

10. For conservative think tanks, 85.3 percent of leaders named this response as most or very important, and 79.0 percent of liberal think tanks did the same.

11. The results on this question for liberal think tanks and think tanks of no identifiable ideology were virtually indistinguishable.

12. The population numbers for state think tanks suggest that liberal think tanks and think tanks of centrist or no identifiable ideology are not just substantially outnumbered by conservative organizations, but in seventeen of the states, a conservative

think tank is operating without any liberal counterpart; in fifteen states, a conservative think tank is operating with no counterpart of centrist or no identifiable ideology.

13. Based on my analysis, more than four-fifths of conservative state-focused think tanks (83.3 percent) are full-service, in the sense that they provide attention to a variety of issues, often selecting which topics to highlight based on the immediate priorities of state policy makers. By contrast, only one-quarter of liberal think tanks (25.0 percent) are full-service, with the rest working on only one or a couple of issues (75.0 percent). Another explanation for this trend for liberal think tanks to be specialized relates to the pressures that emanate from the liberal and mainstream foundations that provide much of their support. See Rich 2005.

14. For some discussion of this approach, see Bhargava and Gragg 2005.

10

Ideas and Institutions in Race Politics

Robert C. Lieberman

Most observers of the politics of racial inequality—including, most famously, Gunnar Myrdal (1944)—have viewed the question of the persistence of racial inequality as a dilemma between color-blindness and race-consciousness. This contest between color-blindness and race-consciousness is waged at the levels of both ideas and policy. Advocates of color-blindness reject both the legitimacy of race as a political category and policies that confer benefits and burdens on the basis of racial categories (regardless of whether their purpose is to protect or exclude a disfavored group). The race-conscious approach recognizes racial groups as legitimate targets of public policy. From this position, it is reasonable to frame policies explicitly to address racial differences in order to remedy the effects of past discrimination, equalize future opportunity, and ensure diversity in institutions such as schools and workplaces.

But despite its importance, this framing of race politics and policy as a conflict among ideas is not sufficient to account either for the outcomes of these policy debates or for the consequences of the resultant policies themselves. This is true for at least three reasons. First, different kinds of policies can have very different consequences for racial equality and incorporation, even in a context where similar ideas about race prevail in society. For example, over the same period in which employment-discrimination policy has been relatively successful at integrating the American labor market, American welfare policy has been strikingly unsuccessful at addressing African American poverty and incorporating African Americans and other minorities into effective social provision (Lieberman 1998; Lieberman 2005b; Schram, Soss, and Fording 2003; Schram 2005). Second, it is equally hard, in comparative perspective, to show a connection between framing ideas about race and race-policy outcomes. Although there appears to be a connection between national political and cultural traditions and national race-policy choices, that causal link is hard to sustain on closer inspection (Favell 1998; Bleich 2003). Countries with color-blind political traditions, such as France, have generally, although not exclusively, adopted color-blind antidiscrimination policies. Countries whose

political traditions more readily embrace racial group identities, such as Great Britain, are more likely to adopt group-conscious, multicultural policies. But these apparent tendencies do not fully account either for policies or for racial incorporation outcomes across countries (Lieberman 2005b). Moreover, this direct ideas-based explanation breaks down altogether when applied to the United States, whose political culture displays profound ambivalence between the color-blind and race-conscious traditions. Finally, the ideas about race that animate public policies do not always (or even usually) determine their consequences. Officially color-blind policies often have the effect of reproducing highly unequal patterns of racial incorporation.

Given the centrality of ideas in accounts of race politics, both in the United States and abroad, it seems natural to begin with ideas in any attempt to explain the curious and maddening mixture of success and failure in American race policy. Nevertheless, despite their recent resurgence in political science, ideas-based explanations have long taken a decided back seat to institutional approaches, especially in the study of the American politics and policy (Lieberman 2002). Race policy is an ideal medium in which to examine this book's central premise, that ideas are a primary cause of political behavior. Race is, by common assent, a cultural phenomenon, a socially and politically constructed set of categories that has often been nurtured and maintained by commonly held ideas and cultural consensus. The majorities and minorities that struggle over race policy, then, are almost by definition on opposite sides of the defining debate between color-blindness and race-consciousness (although, to be sure, there have been ample disagreements within racial groups about the relative merits of color-blindness and race-consciousness as both framing ideas and political strategies).

At the same time, an examination of race policy draws particular attention to the limits of explanations that rely purely on ideas. It inherently involves a clash of interests between majorities and minorities, and its outcomes hinge on matters of access to power and group cohesion, mobilization, and strategy—in short, the stuff of institutional politics. As Vivien Schmidt suggests in chapter 2 of this book, ideational approaches to explaining political outcomes build on and overlap with more conventional institutional approaches. Schmidt suggests that such an intersectional approach, which takes account of both the content of political ideas and the structure of variously conceived political institutions, can fruitfully address concrete empirical puzzles, especially about political dynamics and change, that might confound other kinds of explanatory strategies. The intersection of race with politics and policy making offers a case in point. As a policy-making arena, the politics of remedying racial inequality necessarily entails a process of coalition-building that combines what Hugh Heclo has called "powering" and "puzzling" (1974, 305–306)—clashes of both power and ideas between majorities and minorities.

In the remainder of this chapter, I show that neither ideas nor institutions form a sufficient basis for explaining the trajectory of American race policy and also develop a more general critique of the common limitations of these two analytical modes. I offer an alternative framework for both ideas and institutions as necessary elements of the formation of political coalitions. I then deploy this framework to highlight both the powering and the puzzling at work in one of the most perplexing developments in American race politics in the late twentieth century: the rise of affirmative action in employment.

IDEAS AND INSTITUTIONS: COMPETING EXPLANATIONS?

The conjunction of institutions and ideas in American race politics raises an important methodological question, which lies at the heart of this book's project. If race-policy outcomes are plausibly the result of both the structure of decision-making institutions and the status of competing ideas about race and discrimination, then we are left at the boundary between two modes of explanation that, as Craig Parsons (2007) points out, entail fundamentally different logics. Institutional arguments, Parsons suggests, derive fundamentally from external conditions and constraints that shape human behavior, while ideational explanations are rooted in the ways in which people understand the world and express their interpretations of it to others. Although the boundary between these two modes of explanation is fuzzy—ideational explanations can also involve institutionalized patterns of understanding that operate as external frameworks for action—this distinction neatly characterizes two dominant approaches to policy making in general and race policy in particular. What the case of affirmative action will show, however, is that this methodological distinction obscures as much as it reveals about the causal logic of American race policy. Each approach contributes an important element of a more satisfying explanation that involves both substantive content of policy disputes and the means by which power is deployed. The methodological challenge, as Schmidt argues here in chapter 2, is to weave together two explanatory modes whose analytical vocabularies and technologies have largely diverged.

Political analyses of policy making typically and convincingly view policy choices as the consequences of institutions, a set of regularities in political life (such as rules and procedures, organizational structures, norms, or taken-for-granted cultural understandings) that shape political behavior, allocate power and regulate its exercise, and therefore affect political outcomes.[1] By "institution," I mean here the formal structures, organizations, and processes that order and define national decision-making authority (see Orren and Skowronek 2004, 81–87). There are, of course, other kinds of institutions and institutionalism (see Hall and Taylor 1996; Shapiro, Skowronek, and Galvin

2006; Schmidt, chapter 2 in this book), but I adhere for the sake of this argument to the one that tends to dominate studies of American politics and policy making and that is, for clarity's sake, the most distinct from ideas-based accounts of political processes and outcomes. This is, in essence, a hard case. If this rendering of institutions—which is often devoid of any ideational content at all—can be deployed in a common analytical framework with ideas, the claim for the analytical fusion of ideas and institutions will emerge all the stronger (see Smith 2006).

Institutional accounts of race in American politics typically focus on particular features of American politics—federalism and localism, pluralism, the fragmentation inherent in separated powers, the party system, the distinctive pattern of state formation—to explain the persistence of racial inequality in American politics. Such works generally argue that these enduring features of American governing arrangements systematically shape the access of minorities to political power and thus affect political and policy outcomes that limit minority incorporation (Key 1949; Riker 1964; Katznelson 1981; Katznelson 2005; Lieberman 1998; Marx 1998; Frymer 1999; Kryder 2000). From this baseline, however, the challenge for institutional approaches is to account for any progress at all.

Some institutional perspectives on American race politics and policy fare somewhat better at explaining the pattern of racial progress in the United States, particularly by pointing to changes in the institutional context of race policy both over time and across policy areas. Jennifer Hochschild (1999), for example, argues that success has come most firmly and permanently when policies are relatively straightforward and self-enforcing and when they are promulgated by policy makers with strong institutional bases to induce others to change their behavior. Taking a somewhat broader historical view, Philip Klinkner and Rogers Smith (1999) have shown that certain political conditions—wars requiring full-scale national mobilization, adversaries that inspire the invocation of America's egalitarian liberal tradition, and social movements—have been necessary to produce racial progress throughout American history.[2]

But even these more supple institutional works pose knotty analytical questions. First, how do we account for institutional changes that might, in some contexts, produce more favorable policies and prospects for minorities than the common patterns of American politics? With their emphasis on order, institutional models of politics are often better at explaining stability than at accounting for important change that goes beyond normal and regular variation. Second, institutional approaches are limited in their capacity to account for the substantive course of politics. Given the raw material—assumptions about actors' beliefs, preferences, knowledge, understanding, and expectations—institutional theories can generate remarkably accurate predictions

about which outcome from among a range of contemplated outcomes is likely to occur. But precisely because institutional approaches tend to take these things as given, they are often at a loss to explain the appearance at any given moment of any particular menu of substantive choices. Why, for example, when the institutional conditions for policy change obtain, do policy makers reach for color-blind or race-conscious strategies to address racial inequality and promote incorporation? And finally, the same fundamental question that challenges ideational approaches bedevils institutional theory as well: how to explain variations in outcomes even in the same institutional context.

Given these limitations of institutional approaches, explanatory strategies rooted in political ideas—their substantive content, cultural rootedness, and political reach—have become increasingly prominent in the discipline in recent years. The idea that matters most in attempting to explain incorporation is racism. Perhaps these outcomes simply reflect prevailing public beliefs and attitudes about the proper place of minorities in American society. Some observers believe that racism dominates white Americans' beliefs and that the express desire to exclude African Americans and other minorities accounts for the persistence of racial inequality (Bell 1992; Hacker 1992). More sophisticated versions of the racism thesis suggest that even though out-and-out racist expression is frowned upon, racial stereotypes remain a powerful framing device that can shape political behavior and policy debates, often in ways that remain hidden behind a norm of color-blind equality (Kinder and Sanders 1996; Gilens 1999; Mendelberg 2001; Bobo and Smith 1998). Others suggest that racial prejudice per se is less important than other kinds of beliefs in shaping white Americans' opinions about policies such as affirmative action and others that are designed expressly to benefit minorities (Sniderman and Piazza 1993; Sniderman and Carmines 1997; Sniderman, Crosby, and Howell 2000; Krysan 2000).

At issue, then, in the formulation of race policy is not just the extent of racist beliefs (as in the biological or cultural inferiority of African Americans or other racially defined minorities) but also disagreement over the interpretation of persistent racial inequality and the very definition of discrimination. Does inequality result from overt acts of discrimination by people who do not want African Americans to succeed? Or does it result from practices of apparently equal treatment whose effects are systematically different on differently situated groups? If the latter, is the disfavored group disfavored because of its members' own shortcomings or because it lacks access to needed social and economic resources? These are questions, as Jal Mehta (chapter 1 in this book) suggests, of problem definition (whence inequality? what is discrimination?) that have clear implications for policy solutions (color-blindness vs. race-consciousness) and that cannot be reduced to power struggles between groups with predetermined interests.

What these analytical perspectives share, despite their disagreements about the prevalence of certain kinds of racist beliefs, is the view that what matters in shaping political and policy outcomes is ideas, not just opinions but also more deeply rooted cultural beliefs that inform the goals and desires that people bring to the political world and, hence, the ways they define and express their interests; the meanings, interpretations, and judgments they attach to events and conditions; and their expectations about cause-and-effect relationships in the political world. This view has long pervaded the most sophisticated analyses of American race relations, from Gunnar Myrdal's (1944) magisterial survey in the 1940s to President Bill Clinton's national initiative on race and the *New York Times*'s Pulitzer Prize–winning series on "How Race Is Lived in America" in more recent years ("One America" 1998; Correspondents of the *New York Times* 2001). It is undoubtedly true that over the course of American history, racist ideas about the inherent inferiority of racially designated groups have often played a decisive role in policy decisions (Smith 1997; King 2000). It might be the case, moreover, that modulations in American beliefs about race over time can account adequately for variations in race policies and outcomes. Moments of more racism, that is, might produce more exclusionary policies and lower levels of incorporation, whereas periods when racism abates might produce the opposite.

This explanatory approach, however, faces a number of important challenges. First, racism has declined dramatically as a central force in American life. How is it, then, that incorporation has not steadily improved? Second, racial incorporation varies from policy to policy. Dramatic successes and catastrophic failures coexist. How is this possible if the general level of societal racism is the primary barrier to incorporation? We know a fair bit about how particular kinds of racial stereotypes contribute to policies that limit incorporation, as in welfare policy, or about how voters distinguish between general principles such as the idea of integrated schools and policies such as busing that might actually enforce integration (Gilens 1999; Bobo and Smith 1998). What is generally lacking in such accounts, however, is an explanation of how Americans choose among competing ideas and beliefs, resulting in concrete political and policy choices—or, as Sheri Berman puts it, "why some of the innumerable ideas in circulation achieve prominence in the political realm at particular moments and others do not" (2001, 233).

IDEAS AND INSTITUTIONS IN THE POLITICS OF COALITION BUILDING

The answer to this question, and one approach to the general methodological challenge of combining ideas and institutions in a more capacious analytical framework, lies in the idea of coalitions. Because policy making involves simul-

taneous attention to multiple issues, policy often emerges from coalitions—often unexpected conjunctions of political forces, the proverbial "strange bedfellows" of politics—rather than simple majority agreement on particular policy. Race policy is especially prone to this logic, because it inherently pits minorities against majorities in situations where each seeks its own advantage. Race, in fact, has at times emerged as a distinct dimension in American politics that commands the attention of policy makers not to the exclusion of other issues but in combination with them, and these are precisely the circumstances in which race-policy change is most likely to occur (Riker 1982; Katznelson, Geiger, and Kryder 1993; Poole and Rosenthal 1997; Weingast 1998). Analyses of American politics, however, tend to presume that race policy is a matter of moral suasion rather than strategic action. I depart from this view by exploring the role of such strategic coalition politics in the evolution of race policy and racial incorporation. Coalition building entails the convergence of purposive and strategic political actors operating under common rules on particular courses of action that are collectively decided on and implemented. The process of forming coalitions necessarily involves both ideas (actors' goals) and institutions (the rules that bind them) (Lieberman 2005b, 9–12).

Desmond King and Rogers Smith have convincingly argued that struggles over American race policy have been part of an overarching conflict between two competing "racial institutional orders," which they define as "coalitions of state institutions and other political actors and organizations that seek to secure and exercise governing power in demographically, economically, and ideologically structured contexts that define the range of opportunities open to political actors"—a very reasonable and succinct summary of the ways institutions operate to shape race politics (King and Smith 2005, 75). In this scheme, the civil rights revolution of the late twentieth century amounted to the triumph of the "transformative egalitarian" order as multiple actors and institutions—the presidency and the executive branch, the courts, Congress, civil society organizations—joined in a self-reinforcing cascade of actions that undermined much, though not all, of the nation's white-supremacist legacy (King and Smith 2005, 82–83). In a subsequent refinement, Smith, drawing on Karen Orren and Stephen Skowronek's persuasive definition of political development, rightly stresses the centrality of institutions—as "carriers of ideas," among other things—in defining racial orders and their role in fostering and cementing durable political change (Smith 2006, 98; Orren and Skowronek 2004, 83). This formulation nicely appreciates the interconnection between institutions, understood as governing arrangements and the structures and organizations connected to them, and the ideas that animate political actors, shape their goals, and thus give content to the patterns of behavior that institutions construct (and without which the very labels that King and Smith attach to their hypothesized racial orders—"white supremacist" and "transformative

egalitarian"—would make little sense). As Skowronek (2006) has recently argued, ideas can be mobilized in different political contexts and at different times for very different purposes. So, although ideas themselves are not sufficient to account for substantial political change, they can catalyze the institutionally grounded political action that is itself a necessary component of political development.

In changing policy contexts, then, ideas and institutions have interacted differently to shape both interests and race-policy coalitions. Race-policy coalitions emerge from conflicts over ideas about the legitimacy of race as a political category, the place of racial identities and groups in national political life, and the role of the state in addressing racial conflict and inequality. These ideological debates, in turn, occur in institutional settings that shape actors' strategies in pursuit of their goals, privilege certain actors over others, and influence policy choices. It is these processes of coalition formation that ultimately determine the paths that race policy follows: color-blind or race-conscious, coercive or voluntary, unified or fragmented, backed by strong or weak state authority. How, then, did coalitions form around particular solutions to common racial challenges, at particular moments, in a variety of settings? Accounts of these processes of coalition formation, maintenance, and change and their consequences for racial incorporation are the hinge that connects ideas and institutions in a broader causal argument about change and variation in race policy.

Forming policy coalitions involves collective and authoritative decision making in a context where potential participants in the decision might not only disagree about the preferred outcome but also approach the situation with altogether different motives (Riker 1962, 9–12, 21; Hinckley 1981, 4–7). Thus, explaining the formation of coalitions requires an account of both the motivations of the disparate actors who come together to back particular outcomes and the decision-making structures and processes that allow them to do so. As in a criminal investigation, familiar to readers of mystery novels or viewers of police dramas, this means finding motive and opportunity—ideas, which underlie the goals and interests of political actors, and institutions, which shape how they can and must act to realize those goals.

Ideas, especially widely shared and often taken-for-granted cultural beliefs about public affairs, surely shape the beliefs, understandings, and goals of citizens and policy makers and help to frame public problems and determine which policy solutions seem reasonable and rational in a given situation (Powell and DiMaggio 1991; Sewell 1992; Dobbin 1994; Skrentny 2002; Hall and Taylor 1996). Much of the most prominent and convincing work on race politics and policy in the United States (and elsewhere) has taken this approach (Smith 1997; Favell 1998; Bleich 2003; Lamont 1999; Lamont 2000). But ideas and cultural dispositions by themselves are not decisive in explaining political and policy outcomes, especially in race policy, which generally

involves conflict and contestation among competing ideas—particularly between varieties of race-consciousness and color-blindness. The question for race policy is which among these competing ideas win by being enacted into policy or otherwise carried into action by states, and these are questions that cannot be resolved simply by understanding the substantive content of national approaches to race. Ideas, in short, give us motive but not opportunity.

Political institutions provide the opportunity, by constraining political behavior through the operation of rules, norms, and organizational settings, as well as by structuring political openings for group mobilization and the articulation of interests. Institutional analysis typically begins with basic structural features of national governing arrangements—separated powers or parliamentary government, federalism or centralization, and the like—that frame strategic possibilities for political actors seeking particular goals. But actors' goals, while often adjusted to fit institutional circumstances, are likely to be products primarily of enduring national patterns of ideas and culture, as well as shifting political and policy circumstances. Precisely because institutional approaches tend to take these things as given, they are at something of a loss to explain the appearance at any given moment of any particular menu of substantive choices. Institutions, then, can provide opportunity but fall short on motive.

The need to connect motive and opportunity in a more complete explanation suggests viewing policies not as merely the projections of national culture or as the mechanical outcomes of institutional forces but as the results of political conflicts in which particular elements of national repertoires of culture and ideas are mobilized and enacted into policy. These political struggles take place within historical and institutional contexts that shape policy making not simply by organizing power but also by acting as gatekeepers for political ideas and cultural dispositions. Policy making in democratic government is not simply a process of optimizing the choice of policy instruments to solve readily identifiable social problems (Lindblom 1959; Kingdon 1984; Stone 1997). Rather, it entails the formation of coalitions among actors who represent both interests vying for power and diverse policy ideas.

I have elsewhere explored this dynamic extensively in a comparative context, showing how national configurations of racial ideas and political institutions combine to define distinctive national paths toward racial incorporation, with varying outcomes for both national policy and the fortunes of racial and ethnic minorities in the United States, Britain, and France (Lieberman 2005b). In the comparative context, the puzzle of affirmative action in the United States stands out as a particularly vexing one. The coalition-building compromise that led to the passage of the Civil Rights Act of 1964 produced an ideologically color-blind and institutionally fragmented antidiscrimination law. When contrasted with the British and French laws against employment discrimination (and the coalitions that produced them), the

Civil Rights Act seemed to create very infertile ground for strong minority incorporation into the labor force. Both the British and French acts seemed stronger on paper than Title VII of the Civil Rights Act, the former in particular because it embodied a more self-consciously color-blind definition of discrimination and empowered the state more directly to enforce the law. But the color-blindness and institutional fragmentation of the Civil Rights Act paradoxically gave way precisely to an active, race-conscious program of antidiscrimination enforcement in the form of affirmative action, while the British and French antidiscrimination efforts foundered despite their nominally stronger state apparatuses.

THE CASE OF AFFIRMATIVE ACTION

Here, I focus more closely on the antidiscrimination story in the United States, particularly the puzzling rise of affirmative action, to demonstrate the utility of arguments that combine ideational and institutional factors to explain outcomes that appear curious from either perspective by itself. The rise of affirmative action during the 1960s and 1970s is ideally suited to test this argument. Both ideas (the apparent triumph of color-blindness in the Civil Rights Act of 1964) and institutions (the apparent weakness and fragmentation of the American state) would lead us to expect anemic enforcement of employment-discrimination law. What emerged, however, was a race-conscious policy backed by the strong and consistent arms of law and state (Skrentny 1996; Dobbin and Sutton 1998). The general question that these and other episodes raise is how ideas and institutions, which separately tend to be associated with regularity and stability in politics, can combine to produce such different outcomes.

Affirmative action is perhaps the paradigmatic race-conscious policy; it depends on the recognition of groups and grants benefits to individuals on the basis of their group affiliations (Skrentny 1996, 7–8). And yet affirmative action emerged as a means to enforce what appeared to be a resolutely color-blind law, the Civil Rights Act of 1964. Title VII of the act outlawed deliberate, individual acts of discrimination such as the refusal to hire or promote individuals because of their race. In doing so, the act appeared explicitly to rule out an alternative race-conscious approach to antidiscrimination policy, which some civil rights advocates had wanted. After the grand compromise that broke the legislative logjam and allowed the act to pass, its strongest supporters voiced this color-blind interpretation. Senator Hubert H. Humphrey famously promised to "start eating the [law's] pages one after another" if it proved susceptible to race-conscious interpretation (*Congressional Record* 1964, 7240). And yet, within ten years of the act's passage, the United States adopted precisely the approach that Humphrey had denied was possible: a set of race-conscious,

group-based policies and practices offering compensatory advantages in employment (as well as education and other fields) to members of historically or currently disadvantaged groups, policies and practices that came to be known collectively as affirmative action. The evolution of race-conscious affirmative action from color-blind legislative origins provides an instructive elaboration of the general argument.

Several factors made this surprising development possible. One was the status of the idea of color-blindness in American politics at the high point of the civil rights era. The debates over civil rights policy in the late 1950s and early 1960s represented the culmination of a long-standing debate in American political institutional life between color-blind and race-conscious visions of society. Not surprisingly, the color-blind vision dominated the field, while the race-conscious tradition was not deeply popular among civil rights advocates during this period. But as civil rights activists pursued mobilizing strategies rooted in growing racial solidarity and race-conscious empowerment and as civil rights lawyers addressed the practical challenges of enforcing antidiscrimination rules, it became increasingly clear that a straightforward color-blind approach would not suffice to meet the rising expectations of the civil rights era. As Mehta (chapter 1 in this volume) suggests, the challenge for civil rights advocates in this era was one of problem definition: what once seemed to be the goal—color-blindness—was now, when too rigidly applied, the problem. Simply treating race as irrelevant, civil rights advocates argued, would not outweigh the effects of past discrimination that left many, if not most, African Americans ill equipped to take advantage of the opportunities that color-blindness might hypothetically offer (Burstein 1985; King 1995b, 208–209; Kryder 2000, 22–132; Skrentny 1996, 114–117; Chen 2009). The debate surrounding the Civil Rights Act thus took place on turf defined by two competing ideational paradigms, each of which had both a deep intellectual legacy and institutionally powerful proponents.

Some of the institutional conditions that prevailed in American politics in the early 1960s were not especially favorable for the emergence of race-conscious policy. As they had for a generation, Southern Democrats wielded disproportionate power in Congress through a variety of procedural and organizational means (such as the filibuster in the Senate). Race increasingly structured the party system, dividing the majority Democratic Party along regional lines and leaving Republicans in the pivotal position in the policy-making process. Finally, civil rights policy making was situated in a chronically fragmented state, in which what little authority there was was divided among several different, mostly weak, federal agencies, as well as state and local governments.

And yet there were in the 1960s also institutional factors that were more favorable to the development of race-conscious policy. One was simply the fluid electoral incentives facing national political leaders. Both John F. Kennedy

and Lyndon Johnson needed to balance the electoral demands of Southern whites and Northern blacks, for each group was an essential piece of their electoral coalition. Consequently, civil rights legislation posed both challenges and opportunities for building a reelection coalition (Miroff 1981; Milkis 2008). Civil rights posed similar challenges for Richard Nixon as he sought to pry the South loose from the Democrats' grip while also competing for minority votes in his quest to forge a majority coalition (Frymer and Skrentny 1998).

The second factor was the civil rights movement itself, which was cresting just as the Civil Rights Act was taking effect (the act's passage came almost exactly halfway between two of the movement's high-water marks, the march on Washington of August 1963 and the march from Selma to Montgomery in March 1965). The movement embraced race-consciousness in a double sense, both embodying it in its embrace of race as a collective political identity and championing it as a policy paradigm (Horton 2005, 141–147). It was not only the mere fact of the movement's existence and national strength that was important for the advancement of race-conscious policy but also its organizational structure. While it was indeed a national movement, it was also deeply rooted and active in local political and economic arenas through local organizations (Young and Burstein 1995). The flourishing of local African American political organizations was, in fact, one of the most enduring consequences of the civil rights movement, and these organizations gave African Americans enhanced political leverage in many parts of the country (Greenstone and Peterson 1973; Button 1989; Morone 1990, ch. 6). The National Association for the Advancement of Colored People (NAACP), among other groups, was organized in a federated structure, which mirrored the structure of the American state—with federal, state, and local branches—and so was able to operate in multiple venues and with varied strategies simultaneously to exploit the political opportunities that were available to exert political, legal, and social pressure (Skocpol 2003).

In particular, federated organizations such as the NAACP and the NAACP Legal Defense Fund (LDF), collaborated closely with the Equal Employment Opportunity Commission (EEOC), the agency principally charged with enforcing the employment-discrimination provisions of the Civil Rights Act. While the EEOC spent a long time getting itself organized after its launch in 1965, the LDF in particular—along with other civil rights organizations such as the Congress of Racial Equality, the Student Nonviolent Coordinating Committee, the Lawyers' Committee for Civil Rights under Law, the American Civil Liberties Union, as well as the NAACP itself—already had established and highly coordinated organizations, both in the South and around the country. These organizations supplemented the field organization of the chronically understaffed and organizationally challenged EEOC and effectively provided it with an extra set of finely tuned eyes and ears. These organizations

brought important patterns and practices of job discrimination to the EEOC's attention. The LDF, which took the lead in representing plaintiffs in antidiscrimination suits, worked closely with the EEOC to share data, strategize about cases, devise courtroom arguments, and work out legal interpretations. These relationships between the state and movement organizations were the linchpin in advancing collective, race-conscious enforcement of apparently color-blind civil rights law, resulting in the unraveling of the momentary color-blind consensus of 1964 and the consolidation of a race-conscious policy approach backed powerfully by the state by the early 1970s, an outcome that neither ideational nor institutional theory would have predicted from the vantage of the earlier moment (Lieberman 2005b).

In contrast to the American experience, Great Britain and France, which also adopted antidiscrimination policies in the 1960s and 1970s, both seemed in important ways better poised to develop a race-conscious antidiscrimination-enforcement strategy (Lieberman 2005b). This was particularly true in Britain, whose law (the Race Relations Act of 1976) explicitly embraced a more race-conscious definition of discrimination based on group imbalances rather than an individual action and clearly anticipated some form of "positive discrimination." France's antiracism law of 1972, in keeping with the country's republican tradition, which disdains subnational group identities such as race or ethnicity, hewed more narrowly to a color-blind understanding of antidiscrimination-enforcement regime. The British case is particularly instructive, because, even though race-consciousness was effectively written into law, the window of political opportunity that made the law possible shut decisively when Margaret Thatcher's Conservatives swept into office and promptly stifled the possibility of race-conscious enforcement, underscoring the importance of institutional and political backing for the development of race policy (Lieberman 2005a). The French case, too, carries lessons, particularly concerning the limitations of single-minded color-blindness as a policy paradigm and a strategy for addressing racial inequality, as the recent racial violence in the wretched *banlieues* of Paris and elsewhere in France so starkly showed.

AFFIRMATIVE ACTION'S AFTERMATH

The evolution of affirmative action has been accompanied by a growing countermovement that has sought to reestablish color-blindness as the basic stance of American liberalism and the fundamental paradigm for antidiscrimination policy. This battle has been waged on numerous fronts, ranging from the relatively technical arcana of professional policy analysis and legal discourse to mass politics and popular culture (Horton 2005, ch. 8). The practical policy implications of this view are that the treatment of individuals in all realms of

political, economic, and social life should be equally color-blind and that just as the government should protect individuals against racial discrimination, it should not itself discriminate by pursuing race-conscious policies such as affirmative action or targeted benefits on the basis of race. Such policies, according to analysts such as Stephan and Abigail Thernstrom (1997), not only violate the liberal norm of equal treatment but also tend perversely to perpetuate rather than ameliorate racial division in society (see Hirschman 1991).

Much of this debate has been played out in a series of legal and political battles over affirmative action, particularly in higher education. In the 1970s, Alan Bakke, a rejected white applicant to the medical school of the University of California at Davis, sued the university to challenge its set-aside program for minority applicants. His case reached the Supreme Court, which, in a somewhat convoluted 1978 ruling, accepted his claim about Davis's rigid quota system while also approving the general principle that race could be used as one factor among others in university admissions (*Regents of the University of California v. Bakke* 1978.). Although the case validated a range of affirmative-action practices that quickly took hold in American colleges and universities, it also validated the claim that Bakke had been the victim of "reverse discrimination," in which race-conscious preferences for minority candidates had the effect of denying members of the majority equal protection under the Constitution. This claim, amplified in American political discourse in the ensuing decades, came to play a central role in the new color-blind discourse of American liberalism.

The political and legal challenge to affirmative action continued in the 1980s and 1990s. A series of court cases challenged the practices of minority set-asides in government contracting, resulting in a set of exacting conditions that governments must meet in order to justify procurement schemes that target minority-owned business (*City of Richmond v. J. A. Croson Co.* 1989; *Adarand Constructors Inc. v. Peña* 1995). The assault on affirmative action in higher education continued. Opponents of affirmative action won an important victory in *Hopwood v. Texas* (1996), in which a federal appeals court struck down the admissions program of the University of Texas law school. The legal challenge to affirmative action reached its climactic moment in 2003, when the Supreme Court ruled on two lawsuits over admissions policies at the University of Michigan. Although these cases amounted to an important victory for affirmative action and for the race-conscious approach to maintaining diversity in public institutions, they also highlighted the nuances and complexities that are often obscured in the simplistic renderings of the color-blind/race-conscious dichotomy. In these cases, a closely divided Court upheld the principle of race-conscious admissions for the purpose of ensuring diversity (*Grutter v. Bollinger* 2003), while restricting the appropriate policy instruments through which race-conscious purposes could be achieved (*Gratz v. Bollinger*

2003). The plaintiffs were white applicants to the university's college (*Gratz*) and law school (*Grutter*) who claimed that the university's admissions processes, which were designed to produce a racially diverse student body, violated their rights to equal protection because they identified applicants by race and thus violated the precepts and practices of color-blindness. But in her majority opinion in the critical case (*Grutter*), Justice Sandra Day O'Connor explicitly rejected the premise that race-neutral policies had an absolute claim to priority, arguing instead that racial diversity is an acceptable, even compelling, goal for public institutions (a premise accepted even by the Bush administration, not generally a friend to race-conscious policies, in its brief in the cases). Moreover, she argued that race-conscious policies are a reasonable way of achieving that diversity—especially given that race-neutral alternatives, such as those adopted in Texas in *Hopwood*'s wake, have demonstrably failed to achieve comparable levels of diversity as the affirmative-action policies they replaced (Tienda et al. 2003). Still, the Court's majority in favor of race-conscious admissions expressed considerable ambivalence about race-conscious policies, suggesting that affirmative action and other race-conscious policies are permissible only as temporary deviations from color-blindness, which is understood to be not only the correct policy but also the natural state of affairs for American politics and society, and more recently, the Court's new majority pulled back somewhat from this position in a case about occupational tests that disproportionately, if inadvertently, disadvantage minorities (*Ricci v. DeStefano* 2009). But paralleling this judicial ambivalence has been a growing trend toward public hostility toward race-conscious policies, expressed in successful anti-affirmative-action ballot initiatives in California in 1996 and Michigan in 2006. (One of the principal architects of the Michigan referendum, which was adopted by a margin of 58 percent to 42 percent, was Jennifer Gratz, the lead plaintiff in one of the Michigan lawsuits.)

The recent dominance of the color-blind paradigm, however, is more than simply a matter of its disproportionate hold on the American political imagination. There are also institutional reasons for the growing dominance of color-blindness in American race politics since the 1960s. During the generation between Lyndon Johnson and Bill Clinton, no president or other leading figure of either party had strong political incentives to address issues of race or racial inequality directly or to advocate race-conscious policies strongly. For one thing, since the 1970s, there has been a trend toward conservatism in public opinion and toward the Republicans in partisanship. This growing conservatism is associated with antipathy toward targeted, race-conscious policies such as affirmative action (Erikson, MacKuen, and Stimson 2002; Sniderman and Piazza 1993). Color-blindness, in fact, almost by definition, means a relatively passive or reactive state, limited to proscribing overt discrimination rather than actively intervening in the labor market or other putatively private

spheres to rectify racial imbalances. It is a rather peculiar irony that once laws such as the Civil Rights Act of 1964, which outlawed most forms of racial discrimination, were on the books, color-blindness, which formed the basis for a transformative social movement that truly revolutionized American life, became effectively the conservative position. As the American electorate has become more conservative, pivotal voters in national elections are less likely to be attracted to race-conscious policy appeals.

The racial dynamics of partisan support have also changed over the last generation. During the ascendancy of the New Deal in the mid-twentieth century, the Democratic and Republican parties did not diverge substantially from each other on civil rights or other racial issues, although Democrats were deeply divided among themselves. Democratic dominance depended on an inherently unstable coalition of Southern whites, committed to white supremacy and segregation, and Northern urban workers, who supported expanded social protection. Republicans, committed principally to limited government, made their peace with the New Deal when they needed to but made common cause with Southern Democrats when it suited them, particularly on the closely intertwined issues of race and labor (Katznelson, Geiger, and Kryder 1993; Farhang and Katznelson 2005). This fragile equilibrium was rattled in 1948, when President Harry Truman, in order to court pivotal African American voters in Northern cities, took moderate steps toward support for civil rights initiatives, driving Southerners to walk out of the party's national convention and nominate a rump ticket. But Southern whites returned to the Democratic fold in the 1950s, and there they remained until 1964, when mainstream Republicans joined with Northern Democrats to pass the Civil Rights Act. The key holdouts from the civil rights consensus were not only Southern Democrats but also a vocal group of conservative Republicans led by the party's presidential nominee, Senator Barry Goldwater, who dissented not because he objected to civil rights per se but because he fear the extension of national state power. Johnson trounced Goldwater in the 1964 election, but Goldwater won five states in the Deep South that had been safely Democratic since Reconstruction, inserting a wedge between two key components of the Democratic coalition: Southern whites, whose voting behavior and partisan allegiance began migrating toward the Republicans, and African Americans, whose voting rights would be protected the following year thanks to Johnson and the Democratic-led civil rights revolution (Carmines and Stimson 1989; Lublin 2004).

Given this divergence of the parties on civil rights and other racial issues in the last forty years, African American voters consistently vote in overwhelming numbers for Democratic candidates. As a result, Democrats have little incentive to campaign for Democratic votes on grounds that might alienate more racially conservative voters, except in concentrated areas of black population,

where Democratic victory tends to be a near certainty. Nationally, however, Democratic candidates and policy makers must appeal for moderate white votes in order to form winning electoral and governing coalitions (Frymer 1999). This electoral imperative has increasingly led Democratic candidates to emphasize universal and ostensibly race-neutral themes in campaigns but has also left them increasingly open to implicit racial appeals (Edsall and Edsall 1991; Williams 1998; Mendelberg 2001).

CONCLUSION

If color-blindness has come to seem the norm in American politics, it is as a consequence of these structural trends in American political development and not solely because of the substantive triumph of color-blind over race-conscious political ideas. This stylized history of affirmative action is important because it offers a set of observational guidelines that can tell us where to look to understand the contemporary political dynamics of race and to parse the reciprocal roles of ideas and institutions in political developments. The affirmative-action story suggests that ideational framing by itself—the perpetual battle between color-blindness and race-consciousness—is not sufficient to explain policy outcomes. Analyses of American race politics tend to presume that ideas and beliefs about race—racism or egalitarianism, race-consciousness or color-blindness—matter most in making race policy; in other words, that race policy is essentially a matter of moral suasion or political will. But what this history reveals is that race policy is equally a matter of strategic action and depends on the formation of coalitions. Coalition building, moreover, is not a straightforward matter of reaching agreement on ideas but, rather, entails the convergence of purposive and strategic political actors operating under common institutional rules on particular courses of action—agreement not necessarily on ends but primarily on means. Ideas about race interact with political institutional factors at particular historical moments to shape the possibilities available to political actors to form and maintain race-policy coalitions and to pursue their various and often incommensurable goals.

The possibilities for successful racial incorporation, then, seem to depend on historical circumstances in which political institutions support the adoption and implementation of inclusive and egalitarian policies that overcome historical inequalities. These circumstances, in turn, often emerge because of the opportunities posed by political institutions for mobilized groups to challenge prevailing policy ideas and to push particularly for more race-conscious approaches to inclusion. Thus, the possibilities for promoting racial incorporation in tandem depend not on institutions or ideas alone but also on the existence of conditions under which ideas find expression in institutional

contexts that give them and their bearers opportunities to wield transformative power. Such openings for movement toward greater incorporation are especially wide when configurations of ideas and institutions are discordant—both with one another other and with received structures of exclusion and inequality. These circumstances generate new imperatives for political actors to break out of familiar and settled habits and seek new paths. Explaining these pathways requires examining both idea-driven motives and institutional opportunities and their interactions, a task that is imperative if we seek to understand the cultural and institutional ingredients of coalition building in diverse societies.

The framework I have advanced for understanding American race policy also contains hints of broader applicability, across a wide range of policy domains and political settings that engage both the material distribution and mechanisms of power *and* conflicting beliefs and understandings about how the world works. The notion that ideas matter in politics and policy making is now well established. But the burden of this chapter, and of this entire book, has been to suggest *how* they might matter, especially in interaction with the institutional settings and processes that lie at the heart of contemporary social science and policy studies, and to offer an analytical vocabulary with which we might rigorously explore the causal role of ideas in politics. American race policy offers a case in point. Ideas about race provided not just normative guidance for political actors but also the medium for those actors to develop and articulate their own preferences in a rapidly changing political situation (see Hay and Blyth, chapters 3 and 4 in this book). Ideas about race, discrimination, and equality also shaped actors' definitions of the policy problems they faced and the solutions to those problems that seemed rational (Mehta, chapter 1 in this book). And finally, ideas played a critical role in framing the strategic calculations that actors made, given the opportunities for action that institutions posed (Schmidt and Campbell and Pedersen, chapters 2 and 8 in this book). Together, these explanatory elements begin to sketch a new understanding of the ways ideas matter in politics and the circumstances under which ideas succeed or fail and a new framework that should serve as an invitation to pursue the investigation of ideas and their role in politics more deeply.

NOTES

1. Institutional studies of policy making are legion in political science. An exemplary, although by no means exhaustive, list might include Skowronek 1982; Skocpol 1992; McCubbins, Noll, and Weingast 1987; Krehbiel 1998; and Cameron, 2000. These works represent a wide range of methodological approaches rooted in a variety of disciplinary backgrounds, from neoclassical microeconomics and the theory of games to macrohistorical sociology. But despite their differences, these varieties of institutionalism are united by a common set of concerns and assumptions about the role played

by patterns of ordered regularity in social and political life. See Thelen and Steinmo 1992; Immergut 1998; Lieberman 2002; Orren and Skowronek 2004.

2. Others note the importance of the international context in shaping American race policy, particularly during the Cold War, without going as far as Klinkner and Smith in proposing cataclysmic international (or, worse, internal) conflict as a necessary condition for progress. See Dudziak 2000; Borstelmann 2001; Skrentny 2002, ch. 2–3.

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