Networks of Privilege in the Middle East: The Politics of Economic Reform Revisited

Edited by

Steven Heydemann

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Acknowledgments

This volume is the product of a collaborative research project sponsored by the Mediterranean Program of the Robert Schuman Center of the European University Institute. Participants in the working group were selected through an open competition that drew more than 50 applications. Beginning in December 1999, participants took part in three workshops at the European University Institute where drafts of papers were presented, discussed, and—in the intervals between meetings—revised. Some participants in the group also took part in two thematic conversations held at the 2001 and 2002 meetings of the Middle East Studies Association, in San Francisco and Washington, respectively. Final revisions were completed in 2002–2003.

The origins of this project, however, are almost entirely the result of serendipity. Working at home one day in 1998, I received an e-mail from Yves Meny, then Director of the Robert Schuman Center and now President of the European University Institute. The e-mail invited me to submit a proposal to a competition for funding to direct a research working group. It noted, rather sternly, that if my proposal were selected I might be required to travel to Florence from time to time to take part in workshops and to give presentations about the project at EUI's request. It took about nine seconds to conclude that this was a condition I could live with and prepared a proposal for a working group on the role of informal networks in the politics of economic reform in the Middle East, a topic I had been moving toward for some time in my work on economic reform in Syria. I e-mailed it in and, some six weeks later, through means no less mysterious than those surrounding the original invitation, was informed that my proposal had been approved.

Thus began a collaboration that ends, formally, with the publication of this volume. But my connection to the EUI has been important for reasons that go well beyond this project. Not least, it afforded me the privilege of

working at the Robert Schuman Center and getting to know Yves Meny for the remarkable scholar and administrator he is—a description that is equally applicable to Helen Wallace, his successor as the Schuman Center's director. It offered to me and other participants in the working group a chance to take part in the activities of EUI's Mediterranean Program, and to benefit from the professionalism, support, and exceptional competence of its staff, especially Imco Brouwer and his assistant Anne-Charlotte Svantesson. I am also deeply grateful for the support and assistance of Filipa di Sousa, who has the unenviable task of helping EUI's visiting scholars to cope with the bureaucratic and other demands of daily life in Italy. Valerie Amiraux, who served with Imco Brouwer as co-scientific secretary of the Mediterranean Program, became a good friend and an invaluable source of information about EUI, Florence, and Italy.

The project also gave me the chance to work with and get to know a remarkable group of scholars. Participants in the project represented diverse disciplines, approaches, and intellectual concerns. Some were well established in their careers, others just starting out. They work in a variety of national and institutional settings, including the United States, Turkey, Italy, France, England, and Germany. Yet they all brought to this project a deep commitment to and insightful engagement with the issues at stake in our collective endeavor. I am grateful to all of them, including those who took part in and helped shape our discussions at one stage or another but whose essays do not appear in this volume. Two such participants deserve particular recognition: Ayse Bugra and Bryan Daves. Our editor at Palgrave, David Pervin, occupies a different but no less important position in the collaboration that this volume represents, not least in finding readers whose anonymous comments helped to guide us in revising these chapters. We were also fortunate to benefit from the sometimes sharp critique of two anonymous reviewers who assessed our papers at an earlier stage. At Georgetown, Alan Treat, a graduate student in the School of Foreign Service, performed wonders as multilingual copy editor, bibliographer, and grammarian. As in all my own endeavors, however, the collaboration and support for which I am most thankful is that of my wife, Gail David, and my daughters, Sarah David Heydemann and Julia David Heydemann, to whom I dedicate this volume.

INTRODUCTION

Networks of Privilege: Rethinking the Politics of Economic Reform in the Middle East

Steven Heydemann

Fairness. A level playing field. Accountability. These remain the elusive but sought after ends of a massive effort to change institutions, organizations, and practices on a global scale, transforming inward-looking, inefficient, and corrupt economic arrangements and policies into market-oriented economies that create incentives for individuals and firms to invest, save, trade, and work. Arguably, this wide-scale movement to strengthen markets, and to create them where they did not previously exist, has led to significant improvements in macroeconomic performance. Broad trends over the past two decades suggest that where economic reforms have taken hold, investment and employment rise, trade expands, and economic growth rates increase. The social and economic disruptions associated with economic reform are far from trivial. In many early reformers, criticism of market-based development strategies is becoming more visible. Yet the longer-term, macroeconomic benefits of liberalizing an economy are often seen as sufficiently compelling to justify the short-run costs.

Nonetheless, the experience of reform also makes clear just how intangible the aims of fairness, accountability, and transparency remain. Without exception, economic reform programs either reproduce or create anew arrangements that give a privileged few unequal access to the benefits of new economic policies. Whether this happens by intent, as progressive critics of economic liberalization programs allege, or through politically motivated distortions of the

reform process, economic liberalization and reform have everywhere been accompanied by the persistence or restructuring, if not deepening, of practices that they were intended to overcome. One practice stands out in this regard as especially resistant to reform: the pursuit of rents, an activity broadly understood as nonproductive or directly counterproductive efforts to capture "excess" profit by creating or exploiting government regulation to avoid competition.

Despite the intent of reformers to reduce or eliminate rent seeking, and to increase transparency and accountability, economic reform and the pursuit of rents go hand in hand.[1] Certainly, the effects of reform on rent seeking vary from place to place, as do the effects of rent seeking on economic performance. Nonetheless, it would not be inappropriate to describe economic reform as a process that, among other things, reorganizes the structures and practices of rent seeking through which some economic actors will capture a disproportionate share of the benefits of new economic policies.

The chapters in this volume present the efforts of a diverse, international group of scholars to shed light on the politics that have shaped the experience of economic reform in the Middle East. However, our interests are less generic than this implies. The intent of the chapters is not to provide an overview of reform processes and their (all too visible) limits in the Middle East. Our aim, instead, is to account for variation and patterns in the outcome of economic liberalization programs where outcomes, for our purposes, are defined in terms of (a) shifts in the policies and institutions that regulate extraction and distribution (fiscal policy and the domain of fiscality more broadly), and (b) shifts in the organization of politically mediated economic arrangements, notably but not exclusively the organization of rent seeking and other networks that mobilize to influence the form and content of new economic policies and exploit new regulatory environments created by policy reform. This is the focus of the research presented in chapters by Cammett, Haddad, Leenders, Sfakianakis, Wils, and Wurzel. The following chapters also investigate the strategic uses of reform and reform discourses by incumbents and economic elites to advance their political interests, focusing on fiscal policy as a domain in which these strategies have been especially important. This is the concern of chapters by Cassarino, Hibou, and Kienle.

These chapters, in other words, use a range of approaches to explain variation and patterns in the organization and reorganization of what might be called "networks of privilege" during and after economic reform in a select set of Middle East cases. We do not examine every country in the region. In fact, some cases are the subject of more than one chapter to bring different perspectives to bear on a single experience of reform. Wurzel, Sfakianakis, and Kienle offer quite different accounts of how networks functioned in the course of economic reform in Egypt, and how reforms in turn affected networks of economic privilege. Overall, however, these chapters explore whether and how the organization of economic privilege before reform, in particular the organization of state–business relations, explains the outcome of reform programs themselves, including the ways in which economic privilege is (politically) reorganized as a result of reform. They seek to account for outcomes in which the results of reform could not easily be predicted by the structure of interests prior to reform, and to identify the factors that produce variation in the outcomes of reform across a universe of cases that are, in many ways, quite similar in the organization of their political economies and in the kind of economic reform programs they have experienced.

Moreover, we approach this task with the understanding that "reform" in the Arab Middle East falls short of an ideal-typical transition from state to market. In the region studied here, policy reform has been partial and selective, at best. This reality has led some observers to question whether reform is an appropriate characterization of the shifts that have taken place in Middle East political economies over the past ten to twenty years.[2] And, needless to say, whether economic reform has happened in the Middle East has obvious and important implications for this volume. Not least, it has a direct bearing on our claim that existing frameworks do not adequately explain the politics of economic reform in these, and perhaps other, cases. If regional experiences don't qualify as reform in a meaningful sense, surely this claim must be invalid. Why, after all, should theories of economic reform be expected to account for processes that, whatever we might call them, do not meet some minimally accepted definition of that process?

These are appropriate concerns. Yet it would be wrong to view our cases as outliers whose experiences place them beyond the scope of the literature on economic reform. The relevant question in determining the analytic standing of our cases isn't whether they satisfy an ideal-typical conception of reform, but where they can be situated on a spectrum ranging from no reform to full reform and, more important, whether they can be situated far enough along that spectrum to exhibit the kinds of political and social processes that bring our cases within the compass of existing theoretical frameworks. The issue is not whether economic policy reform in the Middle East resembles that in Chile or the Czech Republic, but whether it generated the kinds of politics that permit comparison between Middle East cases and those that are situated further down the spectrum of reform experiences. As the chapters that follow demonstrate, this is clearly the case.

What the authors in this volume argue, therefore, is that despite the limits of economic reform in the Middle East, and despite ongoing debates about

whether and how much the economies of the region have changed, we find ample evidence that local actors experience shifts in economic policy as consequential to their interests, and as a meaningful basis for political mobilization. These dynamics have tended to be concentrated within sectoral or policy-specific domains that become relatively bounded "islands of reform." This phenomenon is evident, for example, in Cammett's case of bargaining over regulations that affect textile exports in Morocco, the dynamics of tax reform in Jordan discussed by Wils, and the privatization of state-owned enterprises in Egypt that are covered by Wurzel. Certainly, the politics of economic reform are as pervasive, volatile, and diffuse in the Middle East as in other developing regions, constituting a central element of public life. However, the strategic interactions of most interest for this volume appear most powerfully within what are often described as "policy domains," spaces within which delimited sets of actors actively engage in shaping the outcomes of struggles over policy making.[3] Within those domains we find the kinds of distributional politics that are in keeping with those that accompany economic reform anywhere. On these grounds, the experiences discussed in the following chapters can not only be considered to fall within the ambit of the literature on the politics of economic reform, but to challenge that literature in ways we hope will be seen as productive.

If our agenda is thus more limited than trying to account for reform outcomes in the Middle East in general, it is nonetheless ambitious. We hope to explain how reform is shaped by, and in turn helps to reorganize, the structures and practices through which select actors capture, politically, positions of economic privilege, and why a largely similar process applied to similar political economies has exhibited a range of outcomes in how this reorganization of privilege has occurred. Where do we begin in constructing such an explanation? In general terms, the chapters in this volume focus on three factors. First, they explore from a variety of perspectives how institutional and regulatory arrangements mold structures of economic privilege and organize rent-seeking arrangements in the pre-reform period, with a particular interest in state–business relations. Second, they assess how structures of economic privilege and the organization of rent seeking affect the bargaining and negotiation that accompany institutional and regulatory reform. Third, they examine the impact of this bargaining on the design of new regulatory regimes and institutions, and thus on the structure of rent-seeking opportunities and other forms of politically mediated economic exchange in the period after reform. Collectively, evaluating these three factors makes it possible to identify whether there exist correlations among them that explain not only why economic liberalization—fiscal policy reform in particular—reproduces or creates new possibilities for

politically privileged economic actors to collude in capturing the benefits of reform, but also to explain how these possibilities are organized, and why they differ in significant ways across cases.

The rationale for adopting this particular approach—highlighting the importance of formal and informal networks in bargaining over policy reform—rests on three claims that need to be made explicit. One is that processes of reform in the Middle East (and elsewhere) exhibit hybrid characteristics, take place through forms of bargaining and negotiation, and produce outcomes that do not fit well within existing theoretical frameworks. The second claim is that closing gaps between existing theories and what we see happening on the ground is critical to the development of an adequate explanation of the politics of economic reform in the Middle East, and to a satisfactory account of why reforms look the way they do, that is to say, partial, uneven, and selective.

In addition, hidden not so deeply within this project is a third claim: that the dynamics we observe are not unique to the Middle East but have implications that extend beyond a single region. In particular, as I develop in more detail below, these cases suggest, with some reservations, the benefit of adopting network-based forms of analysis in accounting for the politics of economic reform. In this sense, the experience of economic reform in the Middle East points us toward larger theoretical concerns about agency in the process of reform; and about the relationship among actors, institutions, and networks. It challenges existing accounts of the politics of economic reform by unpacking the basic unit of agency—distributional coalitions—that is central to research in the comparative political economy of reform. It also brings into closer, and I hope more productive, contact the more fully socialized approaches to institutional formation and change that have emerged within economic sociology and the material–rationalist accounts that tend to prevail in economics and parts of comparative politics. The volume thus holds out potential for generating approaches that could change the way we think about the politics of economic reform not only in the Middle East cases discussed here, but more broadly. I develop these points further in subsequent sections of this introduction, focusing in particular on the logic underlying the view that network approaches can advance our research on the politics and political economy of economic reform.

Policy Reform and the Reorganization of Rent Seeking

What is it, then, about the dynamics of economic reform in the Middle East for which we do not yet have an adequate explanation? Of the many possible

answers to this question, this volume is organized around one in particular. As indicated earlier, our starting point is the simple and perhaps even self-evident proposition that in the Middle East, as in other settings, economic liberalization is a process that reorganizes opportunities for rent seeking.[4] I do not claim that this is the only thing economic liberalization does. Certainly, economic policy reform has had quite wide-ranging consequences, even in the Middle East where heterodox reform strategies dominate, and the Washington Consensus has a weak foothold. Yet everywhere economic reforms have been introduced in the Middle East (and elsewhere) some privileged economic actors have been effective in preserving their positions from the presumed consequences of the transition from state to semi-market economies. Wherever reforms have been implemented, they have produced outcomes that continue to provide significant opportunities for privileged economic actors, old or new, to collaborate in capturing the gains that can be extracted from a particular set of regulatory arrangements and economic institutions.

What these chapters all take as central questions, therefore, is how a given configuration of privileged economic actors mobilize in response to shifts in economic institutions and regulations, to preserve if not enhance their access to politically mediated economic resources; why some "winners" seem more adept than others at doing so, and how shifts in economic policy do or do not create space for the organization of politically mediated forms of economic privilege. Our intent is to develop a better understanding of how such actors and the networks through which they often mobilize politically both shape economic reforms and how shifts in economic policy, in turn, reshape the organization of rent-seeking opportunities that they wish to exploit; to identify the variables that account for the specific ways in which economic reforms reorganize rent-seeking opportunities in specific cases; to explain patterns and variation in outcomes across cases in the Middle East; and to develop explanations that will permit us (though we do not do so in this volume) to test our models against the experiences of economic reform beyond the Middle East.

Situating the Volume Theoretically

Following sections of this introduction "situate" this volume in two distinct ways. The first, presented here, focuses on where these chapters fit in a larger literature on the political economy of economic policy reform. It addresses where we think current frameworks fall short, and why they do so. The second, developed in the subsequent section, suggests a strategy for responding to gaps in existing models, by more closely integrating network approaches into research on the politics of economic reform.

Over the past two decades, a dense literature has grown up around efforts to explain the politics that shape economic liberalization programs. Throughout this period, the role of privileged actors—their impact on reform processes and the effects reform has on them—has received particular attention, for good reasons. For many analysts, these actors are believed to hold the fate of reform in their hands. This power, whether real or not, has accorded them a special place in the literature on reform—and not necessarily a place of honor. In its earliest phases, this literature tended to assume that economic policy reform would (appropriately) disrupt and destabilize existing political coalitions and displace the privileged economic actors that circulated within them.[5] Economic reform, it was argued, depoliticized access to resources,[6] causing those who benefited from privileged economic positions to become losers in the more competitive environments created by economic reform.[7] Indeed, the breakup of privileged economic networks was viewed as critical for the success of reform.[8] Managing this process, building coalitions of winners and excluding losers from undermining new policies, was seen as central to the political dynamics of economic reforms.[9]

For much of the 1980s and well into the 1990s, international financial institutions such as the World Bank and International Monetary Fund viewed pre-reform economic networks as an obstacle that had to be overcome to ensure the success of new economic policies.[10] Creating the conditions for growth required dismantling economic arrangements that served the interests of political power holders, and replacing them with arrangements that reflected the "logic of economics."[11] In this view, liberalization was seen as causing a shift from cronyism, patronage, and rent seeking to transparency, accountability, and well-defined property rights.

Moreover, because it threatened the position of entrenched interest groups, liberalization was perceived to carry tremendous political risks. Those who benefited most from pre-reform economic arrangements were expected to mobilize in opposition to reform. This opposition not only jeopardized the success of reform, it threatened overall political and social stability. To avoid such an outcome, researchers worked to identify strategies for managing reform that minimize the political risks associated with dramatic changes in the organization of an economy.[12]

This view of how liberalization works shaped much of the early thinking about the politics of economic reform in a huge range of cases. Outside the Middle East, it has been applied to such cases as Indonesia, Kenya, and Japan, among many others. Within the Middle East, it has been used to explain the dynamics of economic policy reform in Egypt, Turkey, Jordan, Syria, Tunisia, and Morocco. It would, in fact, be hard to overestimate the influence of this

perspective on first-generation theories of how economic policy reform operates. The idea that economic reform represents the replacement of political logics by economic logics as the basis for policy making was a touchstone of comparative political economy for much of the 1980s and well into the 1990s.

Yet the actual experience of reform has raised important questions about the accuracy of these assumptions. In fact, as a number of scholars have pointed out, pre-reform economic networks have proven to be quite resilient. Both in the Middle East and elsewhere, privileged economic actors have been effective in preserving their positions from the presumed consequences of economic liberalization. Even when policy reforms are designed to increase transparency and eliminate opportunities for rent seeking, such actors continue to dominate important arenas of economic exchange. This is true of the experience of privatization in Russia, which transferred public assets to Soviet-era managers at huge discounts to their true value;[13] of Israel, where privatization has enhanced the economic position of dominant business families and reduced competition;[14] of privatization in Latin America;[15] and of reform in Arab states such as Egypt, Tunisia, Jordan, and Morocco where pre-reform business and political elites occupy privileged positions within newly liberalized sectors of the economy.[16] Even where economic liberalization improves economic performance, strengthens markets, and broadens the boundaries of economic inclusion (which, according to the World Bank, is happening in Egypt and Morocco) we find that privileged economic networks capture a disproportionate share of the benefits of reform.

Moreover, if all processes of reform in the Middle East seem alike in providing opportunities for "networks of privilege" to survive, there are clear and significant variations in the political processes through which structures of rent seeking are reorganized. We find meaningful variation in the extent to which patterns of economic inclusion and exclusion are transformed, and in the degree to which pre-reform networks are forced to adapt, adjust, or regroup in the face of new economic policies. In some cases, such as Jordan, Lebanon, and Morocco, private business interests may exert meaningful weight in shaping reform outcomes, as indicated by Wils, Leenders, and Cammett (chapters 4, 5, and 8). In others, such as Syria, they may be almost entirely excluded from policy making even though peak business elites benefit disproportionally from economic liberalization, as Haddad demonstrates quite clearly (chapter 1). In yet others, such as Egypt and Tunisia, the peak business elites occupy more ambiguous positions in the reform process, as shown by Sfakianakis, Hibou, and Cassarino (chapters 2, 6, and 7).

Moreover, the influence of international financial institutions (IFIs) and foreign investors varies across cases in defining reform agendas and ensuring local compliance once reforms have been enacted, ranging from high levels of interaction between IFIs and local policy-makers (in cases such as Egypt, Tunisia, Morocco, and Jordan) to the almost complete insulation of local actors from the influence of IFIs (again, Syria is an illustrative case). Moreover, organized labor and other interest groups vary in their reactions to reform, and in the strategies they adopt to resist or exploit it,[17] while bureaucrats and politicians vary in their responsiveness to pressures for reform and their commitment to liberalization.

Rent Seeking as Normal Politics?

These diverse reform outcomes have provoked a range of reactions from interested parties, including efforts by the World Bank and other IFIs to develop strategies for overcoming what are now widely seen as shortcomings in earlier reform efforts.[18] Nonetheless, for some researchers, the persistence and reorganization of rent-seeking arrangements after reform is neither unanticipated nor, necessarily, unintended. Thus, if the old school Washington Consensus perspective might consider our starting point—economic reform as a process that reorganizes opportunities for rent seeking—to be undesirable and theoretically controversial, other approaches are more likely to view the persistence of rent seeking as the predictable result of conditions in which economic policy is driven by private rather than public interests. To situate this volume effectively, it is important to establish how the chapters here respond to the challenges posed by this work.

Perhaps the most significant challenges to this volume can be found in the claims of scholars who write on rent-seeking behavior and on predatory and developmental states, including the work of economists ranging from James Buchanan, Mancur Olson, Anne Krueger, Gary Becker, Alice Amsden, and Douglass North; to political scientists and sociologists including Margaret Levy, David Waldner, Stephan Haggard, Robert Kaufman, John Waterbury, Thomas Callaghy, Robert Bates, Barbara Geddes, Miles Kahler, Joan Nelson, and Peter Evans, among many others. While I do not wish capriciously to lump together methodological approaches as different as various forms of new institutionalism, rational choice theories of predatory rule, and public choice economics—much less arbitrarily draw connections between them and critics of public choice such as Evans, Boyer and Hollingsworth, or Kahler—the wide-ranging literatures I refer to share certain minimum assumptions.[19]

Most centrally, arguments about developmental success or failure tend to share the view that where property rights are not clearly specified; or where state institutions lack particular kinds of qualities (embedded autonomy, for

Evans; the capacity to function as a neutral third party, in North's work; or the capacity to manage the transition to markets, in Kahler's case); or where the time horizon of state elites is too short to permit them to view investment in development as a matter of self-interest (as in Olson's more recent work[20]) the result will be the capture of the state and its policy-making apparatus by coalitions of public and private actors who then use their positions to extract from the economy the maximum possible amount of resources.

Further, as Olson, Callaghy, Haggard, Bates, and Evans have all demonstrated in different contexts, once such rent-seeking networks control the state apparatus, they resist or undermine reforms that might jeopardize their privileged economic positions. Dani Rodrik makes this familiar case with respect to trade reform: "from the perspective of policy makers, the pure reshuffling of income must be counted as a political cost. In politics, rents and revenues that accrue on a regular basis create entitlements. Whether viewed as desirable or not, taking income away from one group is rarely easy for a politician to accomplish."[21]

It is worth noting that whether public and private interests are compatible—that is, whether certain forms of rent seeking are consistent with economic development or necessarily lead to destructive forms of predation—is not as clear as some of the above-named scholars might believe, North and Olson in particular. Amsden's description of corruption, bribery, and rent seeking in her account of developmental success in Korea exhibits a number of similarities to Bates's description of bribery, corruption, and rent seeking in his account of developmental failure in Africa.[22] More recently, research on rent seeking and development in Asia has worked toward a more articulated understanding of when such activity is or is not developmentally productive.[23] It is also worth noting that whether it is seen as developmentally productive or not, much of this literature locates the origins and persistence of rent seeking not among a powerful class of entrepreneurs that dominates state institutions (the starting point of much dependency literature), but among strategic state elites responding to the requirements of short-term political survival. Capitalists are more often policy takers in this literature than they are policy makers.[24] Nonetheless, the core point remains. In Evans's terms, the embeddedness of a state bureaucracy in networks of private capital "will degenerate into a super-cartel, aimed, like all cartels, at protecting its members from changes in the status quo."[25]

This view of the negative relationship between rent seeking and reform is being qualified in useful ways, but it nonetheless poses a meaningful challenge for this volume. It suggests that we are focusing on a well-understood phenomenon and, as a result, that the extent of our contribution will be to

apply the insights of existing literatures to a broader range of cases. Even if this were so, and the chapters in this volume went no further than to test these literatures against the dynamics of bargaining over reform in Middle East cases, both the disciplines and the field of Middle East studies would benefit. My sense, however, is that the volume does more than this, contributing to three ongoing debates within the broad field of comparative political economy. First, the volume provides the basis for moving toward more systematic explanations of how predation and economic efficiency interact, and reinforce the view that the relationship is more varied than is often assumed in the literature on economic reform. We know that in some cases, Korea for instance, these practices were consistent with high growth. We know that in others, such as tropical Africa, they were not. Similarly, we know that in some cases, such as Kenya, Russia, and Indonesia the prevalence and power of rent-seeking networks has impeded possibilities for economic reform, while in others such as Egypt, Morocco, and Tunisia such networks have posed more limited obstacles to reform. Yet to date we do not have good systematic explanations of the specific conditions under which rent-seeking networks disrupt reform, promote reform, or are neutral in their impact on economic policy change.[26] This volume sheds light on such questions and thus brings us closer to such systematic explanations.

Second, as indicated earlier, the experiences of Middle East states complicate claims about the negative correlation between rent seeking on one hand, and the capacity to liberalize economic policy and improve economic performance, on the other. They open up the possibility that rent seeking may be a condition for, rather than an obstacle to, "market-rational" shifts in the organization of a political economy. A number of hypotheses suggest why this might be the case. One is that policy makers might preserve privileged economic arrangements to induce compliance with reforms that, in general, broaden the scope of markets and of economic inclusion even if they do so imperfectly. The structure of such inducements will, obviously, differ depending on their targets. With respect to business, this might mean that reformers use their control of access to rents as inducements they can provide to business elites, in the form of side payments, in exchange for their willingness to invest under new rules and thus increase the likelihood of improvements in overall economic performance. With respect to labor, this might imply that policy makers structure reforms in ways that preserve the privileged access of workers to public resources, whether in the form of employment guarantees for public sector workers, the maintenance of corporatist labor institutions, or a commitment to sustain certain levels of welfare expenditure in the national budget.

A second hypothesis is simply that rent-seeking skills may well be portable across policy regimes; those who are most adept at rent seeking before reform will have an acquired advantage—in personal ties, information, expertise—in securing privileged access to new arenas of rent seeking created as a result of reform. A third is that policy makers seeking to establish a coalition in support of reforms may use their regulatory authority in a way that creates new alliances among rent seekers whose interests were previously at odds. In other words, economic reform is not simply a process in which pre-reform networks bargain with policy makers over the terms of new economic arrangements on the basis of fixed and predetermined interests. Reform is also *constitutive* of new networks, distributive coalitions, and political alliances. As a result, policy makers may exploit their regulatory authority to induce, through the distribution of economic privileges, the formation of coalitions and networks that will enhance the overall prospects for reforms to move forward. And new rent-seeking networks may emerge from the bottom up as social groups organize to exploit the possibility reforms offer for securing new or additional access to state resources.

These hypotheses suggest the possibility that there exists a positive-sum relationship between rent seeking, capacity for reform, and improved economic performance—under some conditions. They also highlight a problem that brings us to the third major debate these chapters engage: the question of agency in the politics of economic policy reform. They underscore that actors in the reform process do not fit neatly within the conventional categories to which they typically are assigned. While researchers tend to invest categories such as worker and capitalist with their own agency and autonomy—predicting responses to reform, for example, based on an individual's presumed identity as a worker, capitalist, or technocrat—one of the implications of our focus on networks is precisely that the boundaries of these categories are porous, flexible, and often ambiguous, including, notably, the boundary between the categories of public and private.

This claim includes but goes beyond the observation that categories should not be permitted to obscure the diversity of interests of those they lump together. We take as a given that the interests of capitalists, like those of workers, bureaucrats, IFIs, or politicians will inevitably be fragmented and will inevitably produce conflict as well as collaboration. Rather, our claim is that networks disrupt, undermine, and cut across the categories that appear regularly in the research literature as the principal agents of reform. Networks permit the formation of unexpected coalitions of actors. They promote patterns of bargaining and interaction that appear counterintuitive based on a less flexible reading of the categories that actors are presumed to occupy.

Networks, in other words, contribute to environments in which policy reforms take shape as regulatory hybrids, compromise formulas, half-measures, and unwieldy amalgams of seemingly conflicting interests.[27]

Two implications follow, though of different kinds. First, any effort to develop a systematic explanation of reform outcomes based on how relations among a given set of actors are configured must come to terms with the presence of such crosscutting networks and the messy policy environments to which they contribute.[28] Second, a better-theorized approach to the role of networks in reorganizing structures of rent seeking may force us to rethink how we conceptualize the messy policy environments that define the reform process almost everywhere in the developing world. In other words, by constructing our independent variables as agents who are not bounded by their position within fixed categories, we will almost inevitably be forced to rethink the bargaining and politics through which our dependent variable—reform outcomes—are produced.

Reform as a Social Process: Networks and the Problem of Agency

As research on the political economy of economic reform has developed, it has moved through three broad phases or "generations" of work, summarized in table 0.1. Like any taxonomy, this one no doubt fails to do justice to the complexity and variety of the thing it classifies. My purpose, however, is precisely to simplify and sharpen distinctions and elaborate on why it makes sense to bring network analysis more centrally into research on the politics of economic reform—not as a synonym for informality or corruption (since much rent-seeking activity is entirely legal), but as an alternative way to conceptualize how we think about actors, agency, causality, and the nature of economic reform as a *social process*. In brief, and I elaborate in the following, this volume resides a bit uneasily within what I would define as a third generation of work on the politics of economic reform, adopting some of its premises but challenging others.

For the purposes of this volume, a useful way to think about generations of research on the political economy of economic reform is to organize them in terms of three main attributes: (1) who the relevant *actors* are and how their *interests* are conceived; (2) what the *process* of reform is about; and (3) what kind of *outcome* is expected. I've added to this a fourth category, what each of these generations (or research programs) felt needed to be explained—in other words, what each one takes as a core analytic problem to be solved.

Table 0.1 Mapping generations of research on the politics of economic reform

Generations	*Actors*	*Process*	*Outcome*	*To be explained*
First	Reformers versus distributive coalitions Interests of "winners" and "losers" largely fixed by their position in pre-reform political economy	Deregulation State shrinking State withdrawal	Markets Forms of economic liberalization	Conditions that promote or obstruct movement from state to market Strategies of coalition management
Second	Reformers versus distributive coalitions Interests of "winners" and "losers" largely fixed by their position in pre-reform political economy	Deregulation State shrinking State withdrawal	Semi-markets Partial, "incomplete," or even dysfunctional forms of economic liberalization	Patterns and variation in hybrid market forms Redistributive consequences of partial reform, including how "winners take all"
Third	Reformers versus distributive coalitions Interests of "winners" and "losers" largely fixed by their positions in pre-reform political economy	Reregulation	Reorganization of state capacity Reorganization of state–market relations	Patterns and variation in reregulation Redistributive consequences of reregulation
Our project	Reformers and other actors embedded in networks that reflect complexity of interests Distributive coalitions coexist with other modes for organizing bargaining among actors	Reregulation Reorganization of structures of rent seeking	Reorganization of state capacity Reorganization of state–market relations Shifts in the organization and possibly, though not necessarily, in the distribution of rents	Conditions that determine modes of bargaining over reform: when networks matter and when they don't Redistributive consequences of reform Patterns and variation in reregulation and the reorganization of rent-seeking arrangements

What stands out right away in table 0.1 is that much of the movement from one generation of literature to another has taken place through shifts in the way we think about process, outcomes, and the resulting analytic problems on which we need to focus. Actors, on the other hand, have been conceptualized in much more static terms. In table 1.1, this shows up as shifts in the content of cells in the column on the right and moves left with each generation of work. Thus, we have moved from what might be called a "Washington Consensus" view of reform in the late 1980s and early 1990s, to a "Neoinstitutional" approach of the mid-1990s to what might be called the "Reregulation" approach of the last couple of years. These generations are not neatly bounded, discrete approaches that progress cleanly from one to another. Nor does the arrival of one signal the disappearance of its predecessor. Nonetheless, there are important differences.

First-generation work on reform, heavily influenced by the theories (and neoorthodox politics) of public choice theory (via Olson, Tollison, Tullock, Riker, and many others), conceptualized the state largely as a problem—a source of regulation and thus distortion in the efficiency of markets—to be overcome by diminishing it. If, as Beker asserts, "participants in the political sector" are as narrowly self-seeking as their private sector counterparts, than the assumption that economic policy reform will create environments in which rent seeking and the pursuit of economic privilege are overcome is highly questionable.[29] Indeed, Michel's "iron law of oligarchy" might well be rephrased as an iron law of rent seeking: where there is regulation, there is rent seeking, and where there is rent seeking, there is inefficiency. On this logic, the possibility for developmentally effective forms of regulation, or for variation in the effects of rent seeking on economic performance, essentially disappear. The most productive approach to economic reform, in this case, becomes a straightforward process of "state shrinking," the transfer of assets and functions from state to market. The biggest obstacle to success is the presence of interests that organize in the form of distributional coalitions to defend their privileges.

In this work, which has been largely superceded (not least because it proved an unreliable guide to the design and management of reform programs),[30] institutions were pushed to the background out of a neoorthodox conviction in the naturalness of markets. The aim of reform was simply to get the prices right, ensuring that as few distortions as possible could arise. Reform outcomes hinged, most centrally, on the capacity of reformers to overcome the efforts of "winners" who would organize to resist the loss of rents. As Eggertsson notes:

> The interest-group theory of government has supplied important insights, but it has weaknesses that are particularly apparent in many of its applications in the rent-seeking literature. The theory does not make clear what the state is, except presumably an aggregate of competing interest groups who somehow reach an equilibrium in the political market. Yet in much of the rent-seeking writings there seems to be a presumption that the state will somehow supply output-maximizing property rights if only special interest groups can be contained. This is evident in Olson's (1982) famous book, *The Rise and Decline of Nations*, which is perhaps the best known manifestation of the rent-seeking literature.[31]

This negative image of interests and of the role of distributional coalitions is pervasive in first-generation work on economic reform. It has spilled over into subsequent work, which continued to view interests as an impediment to an idealized notion of economic efficiency.

In the second generation a positive view of institutions reemerges, along with an appreciation of the link between state capacity and the ability of reformers to construct and consolidate market-supporting institutions. This has obvious and important implications for how reform is defined, for the sorts of tensions associated with it, and for the analytic issues it raises. It provided the basis for a powerful critique of neoliberal development theory by scholars as diverse as Peter Evans, Colin Leys, and Robert Bates. For this generation of work (which is ongoing) the question is not about state withdrawal, but about state transformation—reshaping state capacity to promote market-supporting forms of intervention and regulation. The state may well shrink, but it remains centrally involved in the production and maintenance of markets. The 2002 World Development Report, *Building Institutions for Markets*, is a good example of sophisticated second-generation work.[32]

However, though the state, institutions, and regulation are central to this generation of research the process of reform itself is still conceptualized largely in neoliberal terms. In particular, markets are viewed not simply as apolitical, but as domains in which politics need to be held at bay if reform is to be effective. The rationality of markets is conceived of as distinct from, and incommensurate with, the logic of interest-group politics. Thus the emphasis in work such as Waterbury's on "change teams," on insulation and delegation, or in Evans's work on how embeddedness must nonetheless preserve the autonomy of the bureaucracy from politics for a developmental state to come about. Outcomes in which "winners take all," to use Hellman's term, are cast as dysfunctional distortions of the reform process.

The conclusion of this literature is not that the underlying conceptions of markets or of reform as a process should be changed, but that new strategies are needed for managing interest groups in ways that prevent some actors from capturing (privatizing) its benefits. Partly for this reason, we see the

focus of the literature shift in recent years to what the World Bank now calls "second-generation reforms," and to what kinds of polities, presidential or parliamentary, are most likely to give politicians the autonomy to manage markets unconstrained, or minimally constrained, by the logic of politics.

In third-generation research, reflected in the work of Snyder, Schamis, and others, conceptions of both process and outcome have changed. Markets are now conceptualized as political, reform has been recast as a process of reregulation, and questions of insulation or delegation have moved to the background, if they are present at all.[33] Certainly, the understanding remains that reform outcomes differ in their effects on economic productivity and growth, but the idea of markets as a domain that can plausibly be isolated from the logic of politics is no longer viable. Political logics infuse markets no less than they do other domains of life, and economic policy reform is an inherently political process. Questions about how to ensure the "integrity" of a reform process, about evaluating success or failure by the extent to which interests are kept at arms length, become less meaningful. Here too, institutions matter. As in second-generation work a core interest of this research is how institutional forms affect the politics and distributional consequences of various processes through which economic policies are renegotiated. Although institutions tend to be conceptualized in terms that second-generation work would find familiar (heavily influenced by Douglass C. North and new economic institutionalism), the process of reform no longer carries with it the neoliberal assumptions of earlier work.

Each shift from one generation of research to another has produced movement toward a richer, more fully theorized account of economic reform as a social as well as political process. From this taxonomy, moreover, the elements that this volume shares with third-generation work become clearer. The chapters in this volume share a sense of markets as politically constructed and maintained. They view reform as a negotiated process of reregulation that is shaped by the interaction of political and economic interests, including, notably, the interest of incumbents in remaining in power. They share a concern with the redistributive consequences of reform, while giving more weight to the effects of reregulation in the reorganization of rent seeking than is common in third-generation literature.[34] The chapters also share, to varying degrees, a concern with institutions and how institutional forms affect the bargaining and conflicts that define the reregulation and determine its outcomes.

At the same time, one category has changed very little in the movement from one generation of work to another: actors and how they are conceptualized. With few exceptions the political economy literature defines actors in similar terms, taking interest groups, notably distributive coalitions, as the unit of agency. Underlying this reliance on distributive coalitions is the (reasonable) commitment of most in political economy to some notion of rationality and methodological individualism.[35] However, the reliance on distributive coalitions as a near hegemonic category for theorizing about the organization of interests is problematic. This is not to say that each generation of work conceptualizes actors in precisely the same way. First- and second-generation work tend to view group interests with a negative bias, as a source of market distortions via rent seeking. Third-generation work focuses more on the distributional consequences of group interests with less concern for whether these distort an ideal or maximally efficient allocation of resources.[36]

Starting with rationalist assumptions about interests and the role of distributive coalitions as the key unit of agency, it becomes possible to construct a variety of deductive hypotheses about the politics of reform as a struggle among actors who (usually but not always) get assigned to one of two binary categories: winners or losers. Shared interests among those allocated to one or another category provides the basis for distributive coalitions to form—though as we know and as the literature recognizes (much of the time), collective-action problems are not resolved quite this easily. It is often argued, for example, that losers organize more readily than winners because the certainty of losing is a more potent source of mobilization than the possibility of winning, though Synder and others have challenged this view quite effectively. Still, outcomes of reform are believed to be dependent, in part, on the skill with which politicians manage coalitions and in part on the relative power of coalitions. Institutions come into play at almost every stage, especially in second- and third-generation literatures. They shape the organization of interests, influence opportunities for coalition formation, affect the strategies of winners and losers, and partly determine whether new policies and practices can be consolidated.

Thus, public choice work on rent seeking (first generation), Waterbury's book on public enterprise reform (second generation), and Snyder's work on reregulation (third generation) are all explicit in adopting distributional coalitions as their core units of agency. Haggard and Webb also note, in *Voting for Reform*, the widespread use in work on policy making, including the politics of economic policy reform, of a "simple" interest-based model.

> The most common approach to policymaking in democracies—both among academics and nonspecialists—is to focus on the role of interest groups. The model is a simple one. Policy reform has distributive consequences for different groups, which organize to protect their incomes and rents. Politicians respond to constituent pressures because they seek to

> remain in office, and they exchange policy distortions for political support. The fate of reform thus hinges on the political balance of power between the winners and losers in the reform effort.[37]

While the model as they describe it is indeed simple, it should not be underestimated. Interest-based models and the concept of distributional coalitions are quite powerful analytically. They capture important elements of reality and as Cammett's essay in this volume shows (chapter 8) can lead us to interesting and counterintuitive findings. Indeed, this volume is more a critical extension of earlier work rather than an outright rejection of it. In some important respects, these chapters are responding to gaps that the literature itself has acknowledged but not resolved. For instance, in their introduction to *The Politics of Economic Adjustment*, Haggard and Kaufman include an important discussion of challenges to interest-group-based models.

Summarizing the chapters in their volume, Haggard and Kaufman note that while they use "interests as their main point of departure," they also "raise serious questions about the ability to explain economic policy by reference to distributional outcomes and group interests alone." They elaborate in ways that are worth quoting. Their comment is useful in framing the questions that this volume focuses on and why they matter, as well as moving us toward a justification for our interest in network-based approaches.

> One limit on societal explanations . . . concerns the ubiquity of collective action problems. The political power of economic actors depends on their capacity to organize, but organization does not follow from shared economic interests alone. Compared to those who gain from the status quo, the diffuse beneficiaries of reforms may have substantial difficulty organizing, particularly when the gains from the policy reform are ambiguous and uncertain.
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> An equally important set of limitations on interest group analysis concerns the problem of deriving interests from a group's position in the economy. *The ambiguity of groups' policy interests stem from a number of different sources. . . . [I]ndividuals and groups occupy a number of positions in the economic structure simultaneously, and may well have a variety of other identifications such as religious or ethnic ones that cut across their economic interests.* Incomes are affected through a variety of channels . . . Moreover, the distributional consequences of policy are highly sensitive to [a number of factors.] It may therefore be difficult for groups to calculate the costs and benefits of reform or to weigh the tradeoffs between short-term losses and longer-term gains. [Emphasis added]

These concerns are far from trivial, but Haggard and Kaufman go on to add a couple more. First, they note, interests are dynamic, not static—they change in response to policy shifts at an earlier point in time. This is consistent with my view of reform as constitutive of interests. Distributional analyses frequently assume that actors' interests are fixed, and thus can't accommodate the sequencing effects of reforms that play out over time. Yet what Haggard and Kaufman consider the most "damaging limitation" of interest-based models, the one we engage most directly, "concerns the mechanisms through which interests are translated into policy outputs . . . Most coalitional models of politics rest on very simple aggregation rules, or simple decision-making mechanisms such as lobbying, voting, or pure capture of office. The essays in [their] volume concur that the influence of social groups is a more complex function of the way legal and institutional setting provide or limit opportunities for influence."[38] These are critical problems that raise important questions about the adequacy of interest-based accounts. Such approaches don't acknowledge the multiplicity of forms that political influence can take. They allocate interests to actors based on a rigid and narrow conception of the positions they occupy in an economy, and thus fail to take into account the socially embedded, plural, and hybrid quality of interests, or what this means for the politics of economic policy-making and policy reform.

Does it matter that we misread or oversimplify actors' interests and how they are expressed politically? I would argue it does, for both analytic and empirical reasons. For instance, how would such approaches contend with the problem confronted in Cammett's case, in which the positions of winners and losers are destablized in the process of bargaining over economic policy reform, along lines that do not reflect a simple reading of their interests or political influence? Could a more narrowly interest-based account explain Tunisian strategies for exploiting reform to restructure systems of privilege as discussed by Hibou or Cassarino? How well would it account for the shifts in the relationship between business and state relations in the cases covered by Haddad, Sfakianakis, and Wils, or for the difficulties Kienle raises in determining where agency should be located in his case, Egypt, despite the willingness of most observers unproblematically to assume that it rests with a small economic superelite? Based on these kinds of experiences, my sense is that narrowly interest-based approaches provide fewer insights into the dynamics that most interest us in the politics of economic reform than we might wish. It conveys a partial and in some respects distorted image of the process of economic restructuring, reregulation, and the reorganization of politically mediated forms of economic privilege.

Nonetheless, despite the acknowledgment that they are problematic in their conception of actors and how they organize, surprisingly little has been done about it. The problems have been recognized for some time, their implications are serious, but they have been largely ignored.[39] In fact, the concerns expressed by Haggard and Kaufman have largely been set aside by subsequent research. Certainly this is the case in literature that adopts a strategic-rationality approach to the political economy of reform, where the emphasis has not been on actors and agency, but on developing new conceptions of reform as a process, its outcomes, and what it is that needs to be explained (in other words, the second, third, and fourth columns in table 0.1).

Moreover, these concerns have faded at a time when the gap between narrow, interest-based conceptions of actors and the conditions to which they are being applied has grown wider and more problematic. Research programs have developed more complex models of economic reform. Processes of reform themselves have exhibited trajectories that earlier models might not have predicted. Not least, research on reform long ago moved beyond the electoral democracies in which interest-based models originated into authoritarian settings, like those in the Middle East, where they are not so readily applicable. Much has changed, in other words, both empirically and analytically, but our conception of actors has not kept pace.

This, in my view, is where the current volume can help to fill a gap. Research on the political economy of reform has come a long way. It has moved from the somewhat stilted approaches of the first-generation literature to the much richer views embodied in third-generation work. One of our aims is to push the boundaries of third-generation work even further by developing new conceptualizations of actors. Our contribution, or one of them, is to develop and test approaches for studying the politics of economic reform that provide a more complex understanding of actors, identities, interests, and how these are expressed politically. The authors in this volume are especially well suited to make this contribution because they have all carried out the extensive fieldwork that is essential for the construction of more fully socialized and hybrid conceptions of agency.

In other words, we are trying to move beyond the notion that interests express themselves politically in one generic form, the distributional coalition. We are trying to move beyond the binary categorization of actors as winners or losers, recognizing that such labels don't begin to capture the range of possible relationships that define how actors experience the process of economic reform; are inadequate in providing a way to understand actors' interests; and thus operate with a partial sense (as reflected in table 0.1) of what the process of reform is about and what needs to be explained.

The important question, of course, is: how do we do this? What kind of analytic tools can we adopt, adapt, or create that reflect a more complex conception of actors, interests, and politics, and ultimately give us a more adequate account of the process of economic policy reform? As I suggest here, one plausible strategy concerns the integration of network approaches into work on the political economy of economic reform.

The Role of Networks

There are, of course, all kinds of ways to tell more complex stories about the world. Why networks? In large part network approaches are appealing conceptually because they express an understanding of economic behavior as both social and political. They move us toward an alternative to the current reliance on distributive coalitions as the sole meaningful units of agency. I am not alone in viewing network analysis in this light. In fact, one strand of network theory in economic sociology grew out of a critique not terribly dissimilar to the one I make of the literature on the political economy of reform. As Granovetter and Swedberg stress:

> Economic action is socially situated and cannot be explained by reference to individual motives alone. It is embedded in ongoing networks of personal relationships rather than being carried out by atomised actors. By *network* we mean a regular set of contacts of similar social connections among individuals or groups. An action by a member of a network is *embedded*, because it is expressed in interaction with other people. The network approach helps avoid not only the conceptual trap of atomized actors but also theories that point to technology, the structure of ownership, or culture as the exclusive explanation of economic events.[40]

This observation is at the core of the emergence of economic sociology as a field, and of the development of network analysis as an alternative to the materialist conceptions of behavior associated with mainstream economics and new institutional economics (NIE). And while the tension between network analysis, with its socialized conception of economic behavior, and rational choice or new institutionalist approaches that view behavior in terms of means–ends efficiency, have been seen as an obstacle to their integration, there are few intrinsic reasons for not doing so (for example, see Mustafa Emirbayer and Jeff Goodwin, "Network Analysis, Culture, and the Problem of Agency," *American Journal of Sociology* 99, no. 6 [1994]: 1411–1454).

> Every domain encompasses a diversity of controversial policy matters and numerous claimant groups and public authorities, each seeking in varying degrees to influence the ultimate decisions about matters of importance to them and to their constituencies. Burstein (1991) argued that each policy domain also develops a logically coherent substantive or functional basis for framing its policies and that its participants usually construct a common culture about how society does and should work.[45]

Elements of this definition may be more or less appropriate for the cases included here. Certainly, policy domains can be expected to take different forms in advanced industrial democracies than they do in the late-developing states of the Middle East. For instance, the authors of *Comparing Policy Networks* view policy domains as autonomous, with important implications for the degree of specialization we can expect among networks within any given domain. I suspect that policy domains are far more interconnected in the Middle East, reflecting the density of elites, their concentration in positions of power, and the interlocking relationships that bind them to one another. Still, we have adapted the idea, not uncritically, to suit our purposes.

With these caveats in mind, the relevant "policy domain" for this volume is fiscal policy reform, or fiscality more broadly. The networks of interest to us are those that have a direct stake in the outcomes of fiscal policy reform. To narrow it down even further, the networks of immediate interest to us are the ones that mobilize to influence the outcome of fiscal policy reforms, recognizing that some actors or networks who have an interest may not mobilize (for reasons that may be worthwhile exploring in their own right). It is true, of course, that chapters encompass a range of policy domains, from privatization to reform of trade policy to more general conceptions of economic reform as an overarching instance of a policy macro-domain. However, there are some important elements of coherence that run throughout. One, as I noted earlier, is the extent to which chapters reflect the general orientation of third-generation work on the politics of economic reform, in particular, a concern with the redistributive consequences of reform and the notion of reform as a process of reregulation that provides ample scope for the reassertion and reorganization of economic privilege. A second is our shared commitment to more complex notions of actors, interests, and agency than is typical in work on reform, including third-generation work. A third is our general interest in networks (units of agency, structured relationships of different kinds) that cut across one particular boundary: the boundary separating the nominally private from the nominally public.

Finally, there is also a useful degree of consistency in the way network approaches are used to guide research, again, not uncritically, throughout the volume. For many, impressions of network analysis have been shaped by one component of such work: the technical forms of micro-level modeling that require data that are largely beyond the reach of those who study Middle East cases. What comes to mind in thinking about network analysis are techniques that use detailed inductive data to locate the positions of individuals within social networks and produce spatial maps of network forms as the starting point for understanding how variation in network forms affect outcomes. However, and without getting into a definitional debate about what constitutes network analysis, these are not the only models available to us. Nor is micro-level modeling of social ties the only kind of work that "qualifies" as network analysis. For instance, Wasserman and Galaskiewicz go to some length to point out the value of network models for linking micro and macro levels of analysis.

> ...a central item on the network agenda is to bridge the gap between the micro- and the macro-order. ...One way that network analysis provides a "bridge" between the micro- and macro-orders is that successive levels are "embedded" in one another. Individual relational ties are the crucial components of dyads; dyads constitute triads; triads are contained in higher order subgraphs; and all are embedded in complete networks. The network itself is often embedded in a larger institutional context (whether social, political, economic, epidemiological, or whatever); further even the institutional order is embedded in myriad networks that connect to other institutional sectors in a national and international context. The beauty of network analysis is that it allows a researcher to tie together so many interdependent parts that constitute micro- and macro-social orders.[46]

Granovetter and Swedberg make a similar claim about the capacity of an economic sociology that is rooted in network analysis to shed light on macro-level questions, especially concerning institutional formation, maintenance, and transformation. And the breadth of work that is often used as examples of network analysis is in fact broader than a narrow definition of the approach might suggest. In other words, we have to be careful not to conflate the methodological commitments that define network analysis, which the authors here share, with one but only one of the *techniques* through which those commitments have been operationalized. The methodological commitments are consistent with a wide range of applications, and can be operationalized across different levels of analysis. While there is certainly a strong "scientistic" bent among some network analysts, the core commitments that underlie it are methodologically pluralist.[47]

This doesn't fully address the questions that Kienle poses in chapter 9 about how we can tell, definitively, when agents operating through specific network ties cause a particular outcome, but it helps us refine them in ways that may bring solutions closer to hand. In pushing researchers to be clear about the causal role of networks, Kienle was expressing, appropriately, the concern that researchers who work on the Middle East don't have the kinds of data that permit productive use of network approaches. My response is that data needs are not given, but are a byproduct of the particular kinds of techniques that scholars use. By my reckoning, Cammett, Hibou, Sfakianakis, Wils, and others here do indeed possess the data they need to advance the aim of reconceptualizing actors, interests, and units of agency. The issue isn't whether they can satisfy the demands of spatial modeling techniques, but whether they have been attentive to the standards of evidence required to support the particular kinds of causal claims they are making. All things being equal we might prefer that we had more fine-grained data to work with concerning the social positions of actors. But not having this data only means that we need to make use of approaches consistent with the level of data we do have, and apply to them the appropriate standards of evidence. We can't make causal claims of the sort that require micro-level spatial mapping of social networks, but this is only a problem if we pretended we could.

What this volume has tried to do, therefore, is to begin a process of rethinking the politics of economic reform based on a more complex, fully socialized conception of actors, one that looks beyond their formal position in a political economy to take the hybrid and multiplex construction of interests into account. While these chapters represent initial and in some respects tentative moves in this direction, we hope they will be seen as useful contributions to a larger reconception of the politics of economic reform.

Notes

1. Recently, a new research literature has begun to develop a more nuanced understanding of rent seeking and its effects on development, and to address the interactions of rent seeking, "re-regulation, and economic restructuring. For example, see Timothy Mitchell, *Rule of Experts: Egypt, Techno-Politics, Modernity* (Berkeley: University of California Press, 2002).
2. On the other hand, there is also a literature that views the political economies of the region as dominated by neoliberal policies associated with international financial institutions and their supporters (see Mitchell, *Rule of Experts*).
3. See David Knoke, Franz Urban Pappi, Jeffrey Broadbent, and Yutaka Tsujinaka, *Comparing Policy Networks: Labor Politics in the U.S., Germany, and Japan* (New York: Cambridge University Press, 1996).

4. By rent seeking I am referring here to any activity directed toward the pursuit of politically mediated economic gains.
5. See, e.g., Ilya Harik, "Privatization: The Issue, the Prospects, and the Fears," in Ilya Harik and Denis J. Sullivan, eds., *Privatization and Liberalization in the Middle East* (Bloomington: Indiana University Press, 1992), 1–23.
6. *World Development Report, 1996: From Plan to Market* (New York: Oxford University Press, 1996), 22, makes the (dubious) claim that liberalization depoliticizes access to resources. This view is also expressed in Volker Perthes, "The Syrian Private Commercial and Industrial Sectors and the State," *International Journal of Middle East Studies* 24, no. 2 (May 1992): 207–230.
7. As one recent study emphasizes, "it is commonly argued that losses from economic reform are concentrated among specific groups—namely, those who were privileged or subsidized by the previous status quo . . ." Joel S. Hellman, "Winners Take All: The Politics of Partial Reform in Postcommunist Transitions," *World Politics* 50, no. 2 (January 1998): 206.
8. My use of the term "network" is drawn from recent work in network theory in sociology, in which units of analysis cut across what are typically recognized as institutional or juridical boundaries. Networks of business and state actors who collaborate to manage access to economic benefits—disregarding their formal juridical separation into private and public actors—are a prominent example of this phenomenon. Much of the literature on the political economy of liberalization presents an image of state and private actors as existing on opposite sides of a boundary. This project takes into account that they often are interconnected in ways that overwhelm the idea that they represent distinct and coherent categories of actors. See Walter W. Powell, "Neither Market Nor Hierarchy: Network Forms of Organization," *Research in Organizational Behavior* 12 (1990): 295–336.
9. The importance of managing groups of winners and losers to ensure a winning coalition in support of reform is the theme of John Waterbury, "The Political Economy of Economic Adjustment and Reform," in Joan M. Nelson, ed., *Fragile Coalitions: The Politics of Economic Adjustment* (Washington, DC: Overseas Development Institute, 1989), 39–56.
10. The gap between the Bank's theory of what liberalization is supposed to accomplish and what it actually does is painfully apparent in *From Plan to Market*, 13. After pointing to the achievements of liberalization experiences in some 26 countries in transition, the report acknowledges that these countries have not adequately reformed the critical areas of tax administration, public administration, fiscal controls, or legal institutions, and that they are "plagued" by serious conflicts of interest.
11. On the tension between the logic of politics and the logic of economics, see Thomas Callaghy, "Lost Between State and Market: The Politics of Economic Adjustment in Ghana, Zambia, and Nigeria," in Joan M. Nelson, ed., *Economic Crisis and Policy Choice: The Politics of Adjustment in the Third World* (Princeton: Princeton University Press, 1990), 257–319.

12. One common strategy is delegation—shifting responsibility for economic policy-making to technocrats insulated from political pressure. A second involves strategies of coalition management to balance the political influence of either winners or losers. For an "exclude the winners" approach, see Hellman, "Winners Take All." For an approach more concerned about the political impact of losers, see John Waterbury, "The Political Management of Economic Adjustment and Reform," in Joan M. Nelson, ed., *Fragile Coalitions: The Politics of Economic Adjustment* (New Brunswick: Transaction Books, 1989), 39–56.
13. See Ira W. Lieberman and John Nellis, eds., *Russia: Creating Private Enterprise and Efficient Markets*, Studies of Economies in Transformation Series No. 15 (Washington, DC: The World Bank, 1995). See also, Steven L. Solnick, *Stealing the State: Control and Collapse in Soviet Institutions* (Cambridge: Harvard University Press, 1998). Michael McFaul estimates that "insiders—that is, Soviet-era enterprise directors, in cahoots with trade-union officials loyal to them—gained controlling shares at three-quarters of all large enterprises." "Russia Needs True Reform, Not Higher Taxes," *New York Times*, August 4, 1998.
14. For a recent account describing Israel's privatization program as having increased the concentration of share ownership among a small economic elite and reduced competition, see Judy Dempsey, "Doubts Exist Over Benefits of Sales," *Financial Times*, sec. 2, March 26, 1998.
15. Luigi Manzetti, *Privatization South American Style* (New York: Oxford University Press, 2000).
16. Note that the World Bank regards Morocco, Tunisia, and Jordan as examples of successful, if incomplete, reform. See Nemat Shafik et al., eds., *Claiming the Future: Choosing Prosperity in the Middle East* (Washington, DC: The World Bank, 1995). On the experience of economic reform in the Middle East, see Henri Barkey, ed., *The Politics of Economic Reform in the Middle East* (New York: St. Martin's Press, 1992); Nemat Shafik, ed., *Economic Challenges Facing Middle Eastern and North African Countries: Alternative Futures* (New York: St. Martin's Press, 1998); Christian Morrisson, *Adjustment and Equity in Morocco* (Paris: Development Centre of the Organization for Economic Co-operation and Development, 1991); and Iliya Harik, *Economic Policy Reform in Egypt* (Gainesville, FL: University Press of Florida, 1997).
17. This variation in labor's response to reform is a central theme in Marsha Posusney, *Labor and the State in Egypt: Workers, Unions, and Economic Restructuring, 1952–1996* (New York: Columbia University Press, 1997).
18. Anne O. Kreuger, ed., *Economic Policy Reform: The Second Stage* (Chicago: University of Chicago Press, 2000).
19. See Evans, *Embedded Autonomy: States and Industrial Transformation* (Princeton, NJ: Princeton University Press, 1995); Robert Boyer and J. Rogers Hollingsworth, eds., *Contemporary Capitalism: The Embeddedness of Institutions* (New York: Cambridge University Press, 1997); Miles Kahler, "Orthodoxy and Its Alternatives: Explaining Approaches to Stabilization and Adjustment," in Joan M. Nelson, ed., *Economic Crisis and Policy Choice: The Politics of Adjustment in the Third World* (Princeton: Princeton University Press, 1990), 33–61.

20. Along these lines, see Mancur Olson, "Why Poor Economic Policies Must Promote Corruption: Lessons from the East for All Countries," in Mario Baldassarri, Luigi Paganetto, and Edmund S. Phelps, eds., *Institutions and Economic Organization in the Advanced Economies* (New York: St. Martin's Press, 1998), 9–51.
21. Dani Rodrik, "The Rush to Free Trade in the Developing World: Why So Late? Why Now? Will It Last?" in Federico Sturzenegger and Mariano Tommasi, eds., *The Political Economy of Reform* (Cambridge: MIT Press, 1998), 209–239.
22. Alice Amsden, *Asia's Next Giant: South Korea and Late Industrialization* (New York: Oxford University Press, 1989); and Robert H. Bates, *Markets and States in Tropical Africa: The Political Basis of Agricultural Policies* (Berkeley: University of California Press, 1981). Interestingly (if a bit of a digression), for Amsden financial liberalization is a tool that state elites use to discipline industrialists who threaten to exit from the privileged economic arrangements in which they are embedded. Although Peter Evans relies heavily on Amsden for his own account of Korea in *Embedded Autonomy*, he seems not to realize how powerfully Amsden's argument challenges his claims about the impact of developmental success on state autonomy. Evans argues that success creates new possibilities for industrialists to develop alliances with multinationals, and thus undermines the capacity of autonomous states to manage economic growth. He tells us that developmental states become their own "gravediggers." Amsden, on the other hand, argues that industrialists prefer embeddedness, view market liberalization as a threat, and that state elites will manipulate industrialists' fear of competition to preserve their control over economic decision-making.
23. See Mushtaq H. Khan and K. S. Jomo, eds., *Rents, Rent-Seeking and Economic Development: Theory and Evidence in Asia* (Cambridge: Cambridge University Press, 2000).
24. This is certainly the view, e.g., in John Waterbury, *Exposed to Innumerable Delusions: Public Enterprise and State Power in Egypt, India, Mexico, and Turkey* (New York: Cambridge University Press, 1993).
25. Peter Evans, *Embedded Autonomy: States and Industrial Transformation* (Princeton: Princeton University Press, 1995), 58.
26. For example, there is a widely held view that the size of the state is negatively correlated with economic performance, based on the assumption that large bureaucracies create greater possibilities for predatory and rent-seeking behaviors. However, as one author has recently argued regarding Africa: "with respect to larger bureaucracies being associated with lagging national incomes, the inverse comes closer to the truth." Arthur A. Goldsmith, "Africa's Overgrown State Reconsidered: Bureaucracy and Economic Growth," *World Politics* 51 (July 1999): 520–546.
27. Within the Middle East, which is not the only region characterized by such outcomes, this is reflected in Béatrice Hibou, "From Privatising the Economy to Privatising the State: An Analysis of the Continual Formation of the State," in

Hibou, ed., *Privatising the State* (London, Hurst, and New York: Columbia University Press, 2004), 1–46; and Dieter Weiss and Ulrich Wurzel, *The Economics and Politics of Transition to an Open Market Economy: Egypt* (Paris: Development Centre of the OECD, 1998).

28. I use the term "messy" here in a technical sense, to refer to environments characterized by a high number of interactions over time among a large set of actors. On messy research agendas, see Peter Evans and John D. Stephens, "Studying Development Since the Sixties: The Emergence of a New Comparative Political Economy," *Theory and Society* 17 (1988): 713–745.
29. Gary Becker, "Political Competition Among Interest Groups," in Jason F. Shogren, ed., *The Political Economy of Government Regulation* (Boston: Kluwer Academic Publishers, 1989), 16. See also Becker, "A Theory of Competition Among Pressure Groups for Political Influence," *The Quarterly Journal of Economics* 98 (1983): 371–400.
30. For instance, see Anne Kreuger, ed., *Economic Policy Reform: The Second Stage* (Chicago: University of Chicago Press, 2000).
31. Thrainn Eggertsson, *Economic Behavior and Institutions* (New York: Cambridge University Press, 1990), 279.
32. *Building Institutions* includes a section on "Norms and Networks," conflating the two and calling them both informal institutions. The concern to extend neoinstitutional economic modes of analysis to norms and networks is interesting and in some ways productive. In other ways, however, it seems a bit limiting. More on this later.
33. In her politicized view of markets, there's a sense in which Chaudhry's book falls into this generation of literature as well. See K. A. Chaudhry, *The Price of Wealth: Economies and Institutions in the Middle East* (Ithaca: Cornell University Press, 1997).
34. For a kick (well, it's not that much fun), check out the following website for the newsletter of a very poorly named movement to reform economics: post-autistic economics: www.paecon.net.
35. The commitment to rationality is nonproblematic when rationality is understood not in the reductionist terms that very few scholars now accept, but in a thicker sense that is now quite widely accepted. This latter view takes adequate account of such issues as altruism, self-sacrifice, and emotion, as well as the implications for rationality of incomplete information, the influence of norms, institutions, and ideas (culture) as constraints on individual behavior, and so on. This is not to say that the nature of rationality is a settled problem, only that we can use the concept without worrying about adopting the baggage associated with earlier conceptions.
36. One way to think about this is the trade-off between growth and equity in the allocation of resources and in definitions of efficiency. For first-generation work, there is a clear preference for arrangements that privilege growth over equity. Thus, bargaining over redistribution of resources is seen as a key source of distortion in an economy. For third-generation work, the trade-off is less clear-cut. In some cases, equity may be preferable to growth, and the politics of defining and allocating resources in pursuit of equity become a legitimate means for determining how economic arrangements will be organized.

37. Stephan Haggard and Steven B. Webb, "Introduction," in Haggard and Webb, eds., *Voting for Reform: Democracy, Political Liberalization, and Economic Adjustment* (New York: Oxford University Press, 1994), 8. The link between democracy and interest-group models is explicit, and poses questions about exporting such models to nondemocracies that are relevant and important. They have been dealt with to some degree in the literature, but are beyond the scope of this work.
38. All of the text quoted is from Stephan Haggard and Robert R. Kaufman, "Institutions and Economic Adjustment," in Haggard and Kaufman, eds., *The Politics of Economic Adjustment: International Constraints, Distributive Conflicts, and the State* (Princeton: Princeton University Press, 1992), 27–28. It is striking how little impact this very insightful and self-reflective critique—now a decade old—has had on subsequent research.
39. I am open to alternative views of the literature, including the possibility that the problems I have identified here have in fact been dealt with in the literature. I do not believe so, but am open to persuasion.
40. Mark Granovetter and Richard Swedberg, "Introduction," in Granovetter and Swedberg, eds., *The Sociology of Economic Life* (Colorado: Westview Press, 1992), 9
41. This tension between the "undersocialized" view of individuals in economics and the tendency of some economic sociologists to construct "oversocialized" view has received a fair amount of attention. See Mark Granovetter, "Problems o Explanation in Economic Sociology," in Nitin Nohria and Robert G. Eccles eds., *Networks and Organizations: Structure, Form, Action* (Cambridge: Harvard Business School Press, 1992), 23–56.
42. For instance, networks based on extended families may be harder to mobilize fo tightly defined purposes, but easier to discipline. On the other hand, networks o members in a professional association may be easier to mobilize around an issu with clear financial consequences for the profession, but harder to discipline These kinds of distinctions matter in understanding variation in network eff cacy, among other things.
43. Stanley Wasserman and Joseph Galaskiewicz, eds., *Advances in Social Networ Analysis: Research in the Social and Behavioral Sciences* (California: Sage, 1994), xi
44. Alain Degenne and Michel Forse, *Introducing Social Networks* (London: Sag 1999), 2–3. For me, the emphasis here on the *inductive* foundations of netwo analysis is another reason the approaches are useful for the kind of data-rich pr jects that are represented in the working group.
45. David Knoke, Franz Urban Pappi, Jeffrey Broadbent, and Yutaka Tsujinak *Comparing Policy Networks: Labor Politics in the U.S., Germany, and Japan* (Ne York: Cambridge University Press, 1996), 9.

46. Wasserman and Galaskiewicz, *Advances in Social Network Analysis*, xiii.
47. This is true of Wellman and Berkowitz, eds., *Social Structures: A Network Approach* (New York: Cambridge University Press, 1988), despite the inclusion of several chapters that don't lend themselves to formalization. I wonder, though, whether the linkage of economic sociology and network analysis, which postdates the chapters in Wellman and Berkowitz, reflect the methodological opening up of an approach that had previously been largely formal in its orientation.

PART 1

State–Business Networks and the Politics of Economic Reform